NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

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NOTES AND COMMENTS.

Research Degrees at Oxford.

On November 27, in a congregation of the University of Oxford, the first stage of a new statute respecting the conferring of degrees on persons who have gone through a "course of special study or research," was passed by a large majority. The subsequent moulding of the statute possibly may make this new provision less practicable and less useful than its friends desire; for there seems to be a lamentable absence of definiteness in the proposals and a lamentable presence of conflicting interests among the supporters of the change. Still it would be difficult to overestimate the possible importance of this new movement in the old University. It opens the door to the conferment of the distinction of a degree upon students who are not merely receiving instruction of the same order and imparted by the same methods as are customary in the higher forms of public schools. It affords the University the opportunity of being something more than a continuation school.

Of the dangers that seem to hang round the new statute we may point out two or three. The first is connected with the management of the new arrangements. It is proposed that a new delegacy be appointed. This is the merest fatuity; there already exists in the University the proper authority. The Boards of Faculties, as constituted by the University Commission, are not only the legal bodies, but by their composition contain exactly those people best qualified to judge of the fitness of individuals to pursue research and of the quality of the research carried out. The Boards of Faculties represent the interests of studies. Delegacies invariably contain persons who see in their duty to the University the mere safeguarding of the vested interests of colleges. All who know Oxford know that the vested interests of colleges are connected, not with education, but with education as known in the boarding-house trade. Moreover, the most important duty of the delegacy will be the selection of the persons who are to work for the new degree. That can be done satisfactorily only by the representatives of the different departments of study—in fact, by the Boards of Faculties.

Next we think that it is a great blunder to include under the new provisions "courses of special study." This is merely to open wide the avenues to an ordinary degree. If a man is competent to earn the distinction of a degree by a special course of study, he is competent to attain the degree in the ordinary way by reading for honours in one of the final schools. If the statute is intended simply to make it possible for men to obtain an equivalent of the degree in honours without going through an undergraduate career in the ordinary fashion, the idea of research should not have been connected with it at all. What the University should have done is sufficiently plain. There is a certain learned pursuit known as research, and most familiar in the natural sciences, but equally possible in any subject. It demands special aptitude, the guidance of those who are themselves investigators in its initial stages, at least, and it results in definite additions to our knowledge. If these additions, the results of research, are really additions to knowledge, they should be published. What is wanted is that men who have been taught at Oxford, at any university, or in any shape or form whatever, should be able to come to Oxford and to say: "We desire, and think we have the capacity, to pursue original investigation in such and such a subject." It is then for the accredited representatives of the subject in question to decide whether or no the offer be plausible. If the candidates are accepted, it is for the University to give them their opportunity. Next for the Boards of Faculties to decide after a year, or what time they may deem necessary, whether or no it be worth the candidate's while to pursue his work. Finally, when the student has finished his piece of work and prepared it for publication, it is again for the Boards to decide, with the aid of a specialist where necessary, whether or no the definite addition to knowledge made by the candidate be of an order entitling him to be distinguished by a degree.

And this leads to our last point. Such a degree would be a distinction different in kind from B.A. or M.A., and it should be a different degree. We think, ourselves, that it should be B.Sc., and that if a bachelor of science continues to make additions to knowledge at Oxford or elsewhere it should be within the power of the Boards of Faculties, upon their own initiation or after his application, to recommend the university to make him a doctor of science. And the degree of Doctor of Science of the University of Oxford should be a sign to all the world of high capacity for research and of very considerable achievement in research.

Research Degrees at Cambridge.

The Cambridge movement for the establishment of Research Degrees is running a course parallel with the movement at Oxford. As at Oxford, a large body of graduates approve in a vague way of a proposal that the University should take cognisance of and afford opportunities to a new class of students; but there is the same confusion of aim, and resulting confusion of suggestions. It is worth repeating in other words part of what we said in the preceding paragraph. There are many men, trained for the most part at provincial or colonial universities, who have the desire and, some of them, the capacity to undertake research work. At present, many of them go to Germany, many of them, from lack of opportunity, abandon their hopes of advancing knowledge. It is desirable and urgent that the two great English Universities should open their doors to such men without making them, as at present, begin from the beginning, and labour through a second undergraduate career. The Universities should select these men, prove their capacity for research, and, after they have accomplished certain definite pieces of work, sit in judgment on the quality of their additions to knowledge, and in satisfactory cases give them a degree which should be a stamp of their special abilities and achievements. It may be a matter of opinion whether or no only graduates of Oxford and of Cambridge, or of other universities, should be accepted as research students; our own opinion is that, if the new degree be confined strictly to those who have shown this special capacity, their previous career is a matter of no moment. Keep up the standard of the special degree: let it be clearly understood that it is to be a definite sign of special qualities, and not of mere attainments, and then let all the world come and try. After a very short time of trial, a competent board will be able to weed out the greater number of those who have deceived themselves as to their abilities in this direction; but even those who have escaped this weeding after a short trial must be given to understand that a high measure of success is necessary.

Unfortunately, at Cambridge, as at Oxford, there are signs that a definite and intelligible course like this may not be adopted, but that by slumping "special courses of study" and so forth with research, the new degree will be available for persons whose only difference from the ordinary undergraduate will be, possibly, a greater age, but certainly an absence of the ordinary university training. To do this is merely to overthrow the university system, in response to the clamours of University Extension students and their like. That may or may not be an excellent purpose, but it has nothing to do with research.

A Central Zoological Bureau.

In our last number we gave details concerning a new Zoological Record, in the form of an Author-index on separately printed cards, which has been started by the University of Minnesota. We pointed out the great value of such a work, but at the same time could not help regretting that the undertaking seemed likely to suffer from having its headquarters at so remote a city as Minneapolis, where so very few of the almost innumerable zoological publications are to be found. From this and from other points of view, it would be perfectly legitimate to criticise the scheme more severely than we cared to do; the one thing on which we laid stress was that a beginning had at last actually been made, and in this as in other matters, "Once begun is half done."

It seems, however, to judge from the letters which our advocacy of the scheme has brought us, that we were unintentionally and most excusably unjust to a very perfectly conceived plan that has been incubated in certain quarters for some time, and that will, it is expected, very soon be hatched in a full-fledged state. The plan concerns a central zoological bureau, and with it the name of Dr. H. H. Field is intimately connected. Had we been aware, as we now are, that this plan was something more than a castle in the air, we should certainly have recommended our readers to await its completion before sending their dollars to Mr. Clarke Barrows. Knowing, however, the extreme apathy of the public in matters that are everybody's business, we do not suppose that a single dollar has yet crossed the Atlantic on this errand, so that no great harm will have been done.

We are now able authoritatively to inform our readers that those associated with Dr. Field have been laying their foundations broad and deep, and that it is this alone which has prevented them from premature self-advertisement. Among other preparations that have been made are the following. All the journals for the last ten years have been gone through, with the view of finding out exactly what is needed; thus details have been ascertained with regard to 160 German periodicals, 150 French, 100 English, 44 Italian, and so on, in somewhat striking contrast to the list of 42 submitted by Mr. Clarke Barrows. Correspondents have already been secured in nearly all the countries of the civilised world, in which we are glad to see Japan included. On the Russian portion of the bureau especial stress should be laid, as it promises to supply the entire national literature free of expense. Similar coöperation is hoped for from the equally difficult countries, Poland, Hungary, and Bohemia. In France, a national committee binds itself to provide either all the French journals, or at least titles and abstracts of all papers. A like help is expected from Italy and Germany. Many other arrangements that are approaching completion need not be referred to. Enough to say that no pains are being spared to find out the precise needs of zoologists, which term naturally is assumed to include palæontologists, and to meet those needs with all possible completeness.

Now in this gigantic and increasingly important matter of scientific bibliography there is only one course open to any self-respecting journal or to any responsible scientific body, and that is absolutely to lay aside all personal feeling, whether of the individual, or of the society, or of the nation. The work is one for the whole world; it is one in which every nation that calls itself civilised should share. One thing is perfectly certain, as proved by many sad experiences, namely, that there is no money in this business: all the account will be on the side of expenditure. Consequently the question of any pecuniary vested interest is not one that ought to be raised. Those, therefore, who, because they have themselves embarked on some bibliographic plan of their own, refuse to cast their fortunes in with a truly international undertaking, will be acting only in the despicable spirit of the dog in the manger. We do not know that there are any such; we trust not. It is certainly not to be feared that those who have so honourably borne the burden and heat of the day—those, for instance, who have carried on the *Zoologischer Jahresbericht* and the *Zoological Record*—will be prevented by any petty jealousy from joining their forces to those of the central zoological bureau. Their alliance may be taken for granted; what must be urged is that the smaller local organisations should also help in the one great work, neither giving up nor continuing in semi-opposition. Work and place will, without doubt, be found for all.

Here, then, is an opportunity for the University of Minnesota. Let it confine itself to the publications of North America, or even to those of the United States; but let its list of those be complete. It would not be difficult to enumerate at least seventy serial publications, appearing in the United States and even in Minnesota, but not in the list of Mr. Clarke Barrows. Let all these be obtained and indexed, and let the slips and abstracts of the contained papers be sent in to the central bureau, and then a really worthy contribution will have been made to the work of the world of far more value than Mr. Barrows, or even the University of Minnesota, can hope to accomplish by their unaided efforts. For American Botany such a work has just been started. A bibliography is published in the *Bulletin of the Torrey Botanical Club*, under the direction of the Bibliography Committee of the Madison Botanical Congress; then slips, similar to those suggested by Mr. Barrows, though not quite so clear, are printed and supplied by the Botanical Supply Co., of 418A, Harvard Street, Cambridge, Mass. Why should not American Zoologists imitate this plan? For many years Mr. J. S. Kingsley has published in the *American Naturalist* what purports to be a complete list of papers on recent zoology published in America. If Mr. Kingsley will continue his good work, only making it really complete by the addition of palæontology, and if the University of Minnesota will print catalogue slips and furnish abstracts, or collect the papers for the central zoological bureau to distribute them to specialists for indexing—then no labour would be wasted and all parties ought to prove happy in the consciousness that they are helping the zoologists of the world.

THE NOMENCLATURE OF SPECIES.

NATURALLY the recording of species is closely associated with their nomenclature. M. Gustave F. Dollfus has some "Observations à la note de M. Jousseaume sur les fossiles de Corinthe" in the thirteenth volume of the *Bulletin de la Société géologique de France* (1894, pp. 286–294). In his paper he makes the following remarks on the nomenclature of species:—

"It is certain that instances of binomial nomenclature are found in many authors before Linné, but more often animals were designated by a Latin phrase, a kind of diagnosis of which the terms were more or less modified, transposed, and rearranged by authors. It is certain that the philosophical idea of a binomial nomenclature formed of two constant terms belongs truly to Linné, and came even to him only after some time. In the tenth edition of the 'Systema Naturæ' . . . the new nomenclature does not appear; it only exists in the twelfth edition, of 1766, and it was with a thorough knowledge of the matter that the British Association, when it established the 'Rules of Nomenclature,' decided not to make them apply before Linné, recognising the futility of a search before that date, and considering as fortuitous the instances that could be quoted from earlier authors."

This is very admirable so far as the fixation of Linnæus is concerned, but we should like M. Dollfus to point out to us what Latin phrases in the place of names are to be found in the tenth edition of the "Systema," and also how the binomial system is better established or better carried out in the twelfth edition than in the tenth.

TRINOMIAL NOMENCLATURE OF PLANTS.

WHEN international coöperation has secured a proper recording of animals and plants, many current abuses and difficulties will disappear. In the *Bulletin of the Torrey Botanical Club* for October, 1894, two systematic papers, one on the Smilaceæ of North and Central America, by T. Morong, the other a Revision of the genus *Lathyrus* in the same countries, recall some remarks on nomenclature to which, not long since, we gave utterance. We mean the use of a trinomial designation for a plant. Whatever may be the relation between a "variety" and a true "species," that relation is certainly made no clearer by tacking the varietal on to the specific name without a break. Moreover, when we come to run down Mr. Morong's citations of Smilax, for instance, we find that the meaning of the trinomial is by no means constant. Thus *Smilax invenusta armata*, A. DC., implies, or should imply, *S. invenusta*, Kunth, var. *armata*, A. DC.; but *Smilax Bona-nox Wrightii*, A. DC., cited correctly, is *S. Bona-nox*, L. Subspec., *Wrightii*, A. DC.; while *S. Bona-nox senticosa*, A. DC., turns out to be *S. Bona-nox*, L., Subsp. *polyodonta*, var.

senticosa, A. DC. Without expressing any sympathy with the practice of splitting a species into subspecies and then piling up an intolerable number of varieties, we cannot but condemn the shortened method of citation employed in the present instance, which is meaningless, as well as incorrect.

THE DISPERSAL OF TYPE-SPECIMENS.

THOSE zoologists and botanists who occupy themselves with the determination of species must often waste wicked words on the arrangement that permits the type or first described specimens of a species to be far removed from the land of its origin. Thus the ornithologists of Australia must voyage to Philadelphia to study the birds of the famous Gould collection; and the systematic botanists of North America, when working on the flora of their own country, are continually at a loss for the means of correlating the specimens in its various herbaria with the descriptions of the plants sent or brought to Europe by early explorers. The September number of the Californian journal *Erythea*, in an article entitled "Professor Greene's Mission in Europe," supplies a case in point. Professor Greene has devoted a considerable portion of the last twenty years to the elucidation of the flora of the Pacific Coast. Nuttall was collecting there nearly sixty years ago, but as his descriptions—like most of those published in the earlier days of systematic botany—are extremely meagre, it becomes necessary to refer to his types, the original set of which is at the British Museum. Professor Greene is now in London busily examining these and other types at the two national herbaria before proceeding to examine the—at any rate, from his point of view—less important continental collections. In this case, then, the chief advantage of the dispersal of type-specimens is that English botanists make the acquaintance of Mr. Greene.

THE PLANT-TYPES OF LINNÆUS.

To the *Journal of Botany* for November, Mr. C. B. Clarke communicates a valuable account of the Cyperaceæ contained in Linnæus' Herbarium, now the property of the Linnean Society. It is often necessary to know exactly what is to be understood by a certain Linnæan name, and to this end the plant-sheets written up by Linnæus himself are of very great service. As Mr. Clarke points out, they are not, however, "types" in the sense understood by most modern botanists—that is to say, they are not necessarily the plants from which the description was wholly prepared. The "species" of the "Species Plantarum" is made up of several parts, namely (1) the citations of predecessors, (2) the citations of pictures, (3) Linnæus' own diagnosis, (4) the authentic examples in his own herbarium. Of these, Mr. Clarke regards the first as the most important, since Linnæus, meaning that his species should be the one generally accepted, drew up his diagnosis by directly copying from his predecessors, altering it just so far as to make it include his authentic specimens. Where, as sometimes happened, the latter were wrong, these alterations introduced difficulties which can only be dealt with in each individual case. As regarded the pictures cited, the writer finds them to be of small value, since in the first place the pictures of the time were poor and it is frequently hard to decide which of half a dozen or more allied species is represented, and secondly, "even when the figures were good, Linnæus seems to have allowed for a good deal of variability in the plant and imagination in the artist."

From the above data we have to decide in each case to what plant we will apply each Linnæan specific name, and how the name is to be quoted. For instance, by *Cyperus Haspan* Linnaeus meant the plant now generally known under that name, including, however, several which are now regarded as distinct species, but the sheet marked *haspan* in Linnæus' hand is *C. Ina*. Again, *Scirpus supinus*, Linn., is a widespread species, the naming of which has never been questioned, but the plant in the herbarium so named has nothing whatever to denote it, belonging, in fact, to a different family. As, however, from the citations of his predecessors and his diagnosis, it is clear that Linnæus meant our *S. supinus*, the name must be retained for the plant to which it has always been applied. In spite of all these difficulties, Mr. Clarke is still able to mark a large majority of the species as good.

CASUARINA.

ANATOMICAL characters would not seem to furnish a very sure guide to the systematic affinity of a plant, unless notions of systematic botany are first to be completely revolutionised. It will be remembered that Treub, after working at the mode of pollination and embryology of that strange-looking Australian plant genus *Casuarina*, decided that it must have a subdivision all to itself, which he styled Chalazogams, comparable with one to be known as Porogams, in which the rest of the Angiosperms were contained. The point of distinction, among others noted, on which the name Chalazogams was based was the path taken by the pollen tube along the chalaza and up through the base of the embryo-sac, and not as in the Porogams a direct one through the micropyle. Some recent investigations into the anatomy of *Casuarina*, carried out by Messrs. Boodle and Worsdell, and published in the "Annals of Botany" (viii., pp. 231–264), do not afford much matter for argument either for or against the position assigned to the plant by Treub. In the structure of the bast or phloëm, "it shows no important departure from the dicotyledonous type," while in the wood portion of the vascular tissue "it agrees pretty well with the Cupuliferæ and other dicotyledons." This latter resemblance is of some importance in the light of recent work (to which allusion was made in NATURAL SCIENCE, vol. iv., p. 333) by Miss Benson on the reproduction of the Amentiferæ; an importance, however, which is diminished by the occurrence of points of agreement in the wood of Rosaceæ, Saxifragaceæ, and others.

Geotropism in Plants.

In the same number Dr. Pfeffer describes an ingenious means by which certain proof is obtained of Darwin's assumption that only the tip of a growing root is sensitive to the stimulus of gravity. Darwin's assumption was correct, but his investigations, like those of subsequent workers, were inconclusive, since they involved cutting off the tip of the root, and the results were in a sense pathological. In Pfeffer's experiments, the roots of a bean or lupin are allowed to grow into short tubes of thin glass, bent at a right angle. The growing root follows the bend, and pushes on to the closed end of the tube. The terminal portion of the root, from one to five mm. long, thus becomes bent at right angles to the remainder. Experimenting with roots thus prepared, Pfeffer finds that, so long as the tip remains vertical, no geotropic curvature takes place in the rest of the organ; but when the tip is directed horizontally or at an acute angle with the normal vertical position, the rest of the root endeavours by a curvature to re-establish the normal relation to the force of gravity.

Rootlets Penetrating Living Tissues.

Mr. G. J. Peirce, who has previously studied the penetration of living tissues by the modified roots or haustoria of the Dodder, has also shown (see *Botanische Zeitung*, lii., part i., p. 169) that this power of making a way through plant-tissue is also present in the roots of the pea, bean, turnip and white mustard. Seeds of the two latter enclosed between halved potato tubers forced their roots in twelve days through the substance of the potato, and even pierced the external cork layer. No root-hairs were developed, and the passage made by the root was surrounded by torn, dead, brown-walled cells. As no corroded starch grains were found, the author concludes that the penetration is due to mechanical pressure and not to the action of a diastatic ferment. These experiments recall an account of the production of internal tubers in a potato (Rendle in *Journ. Bot.*, 1893, p. 193, t. 336). In the case described, shoots had grown inwards from the base of the eyes or buds, pushing their way through the substance and giving rise to a number of thread-like roots as well as to small tubers. The latter, by their growth, eventually burst the skin of the mother-tuber, which was thus seen to be actually bringing forth young from its interior. It was also pointed out that the layer of cells lining the channels and cavities in the old potato were losing their contents, and their walls had become corky, while on the surface of the young shoots and tubers layers of cork cells were being formed. Mr. Peirce also found that the roots of seedling peas would penetrate the external tissues of branches of an *Impatiens*, a leaf of *Echeveria*, a leaf-stalk of rhubarb, a leaf of an aloe, and a stem of a *Euphorbia*.

Apospory in the Hart's Tongue.

As our readers are aware, one of the most interesting phenomena of plants is the occurrence of an alternation of generations. In higher plants, such as the flowering plants, this alternation is so far reduced that the intermediate generation is a mere rudiment; but in many lower plants, such as the Ferns, the alternation is complete. In a typical fern, the conspicuous plant as we know it is the asexual or vegetative generation. It produces in due course a number of spores, the organs in which these are formed being the brown spots on the under surface of the fronds. Each spore when ripe is discharged from the fern and, if it falls on suitable soil, produces the inconspicuous prothallus, or sexual generation. On the prothallus, male and female cells are produced in definite organs, and from the union of such male and female cells arises again the conspicuous sexless generation.

But in some cases the phenomenon known as Apospory occurs. In such, the sexual generation is produced directly from the tissues of the asexual generation without the intervention of a spore. In the last issue of the Linnean Society's *Journal* (vol. xxx., no. 209), Mr. C. T. Druery describes the phenomenon of Apospory in a variety of the hart's tongue (*Scolopendrium vulgare*), a species in which it has not hitherto been noticed. The variety (var. *crispum Drummondæ*) has very long, narrow fronds which are, moreover, finely frilled. In the plant in question the tips of the fimbriate projections presented in many cases certain features characteristic of the vegetative production of a prothallus, and when cut off with a portion of the fronds and cultivated, distinct prothalli were produced bearing sexual organs. This constitutes the fourth British species in which apospory has been observed.

The Histology of Fossil Plants.

In a recent communication to the Royal Society by Williamson and Scott, an abstract of which has been sent us, we have an interesting example of the steady progress of palæobotanical histology.

A few years ago the "true fructification" of the well-known Carboniferous *Calamites* was fully described by Williamson, and, in 1885, Renault expressed the opinion that the root of this genus was to be found in *Astromyelus*. Renault's opinion has now been verified in the case of English specimens of *Astromyelus*, and thus another step is made towards a thorough knowledge of the morphology of *Calamites*.

The name *Astromyelus* was instituted by Williamson in 1885 for certain plant fragments which in some respects resembled *Calamites*, but at the same time showed special characteristics which justified another generic title. A detailed examination of French and English specimens has conclusively proved the organic connection between *Calamites* and *Astromyelus*, and the anatomical characters of the latter appear to be in all respects typical of root structures. In younger examples of the Calamitean roots, alternate strands and primary xylem and phloëm have been recognised, and those of the former show the characteristic centripetal development. The secondary wood agrees in structure with that of the stem, and in some cases Cambrian and secondary phloëm have been detected. There seems to be some evidence that these adventitious roots of *Calamites* possessed a double endodermis, such as characterises the roots of *Equisetum*. A constant feature of the cortex is its lacunar zone, and immediately internal to the epidermis a protective epidermoidal layer has been made out, thus recalling another feature characteristic of recent roots.

Fossil Liverworts.

Fossil remains of cellular plants are rarely discovered, and their reported discovery is generally looked upon with suspicion. This is not surprising, as their soft delicate body, lacking always the core or skeleton of fibrovascular tissue which gives rigidity to the higher plants, and often without even a tough external membrane, is not adapted for preservation. Mr. F. H. Knowlton seems, however, to have got hold of an undoubted Liverwort from the Lower Yellowstone of Montana, which he describes in a recent issue (October, 1894) of the *Bulletin of the Torrey Botanical Club.* The species, which represents the only extinct form known from North America, is allied to the genus *Preissia*, and a new genus, *Preissites*, has been made for its reception. In Europe, if we except some more or less doubtful species of Jungermanniaceæ preserved in the Baltic amber, only six fossil species have hitherto been discovered, all of which are closely allied to the recent genus *Marchantia*. Of the six, five are French, while one from the English Wealden was recently described by Mr. Seward in the British Museum Catalogue of Mesozoic Plants. The American fossil was collected by Professor Lester Ward, to whom the species is dedicated.

Nuggets of Gold.

The existence of gold nuggets is a problem which has been debated no less keenly than the existence of sin. Some experts believe in the doctrine of original sin; Professor Liversidge, of Sydney, who has devoted much attention to the former subject, inclines to the theory of original gold; that is to say, he believes nuggets to have been washed out of quartz or calcite veins in which they previously existed as reef gold. On the other hand, many authors believe that particles of alluvial gold may have increased in size by electroplating themselves in the drifts where they lie.

Alluvial gold is generally supposed to be richer and more pure than reef gold, and is therefore credited with a different origin. Gold is soluble in solutions of alkaline sulphides, carbonates, and silicates, and it is suggested that the nuggets have been derived from percolating auriferous waters. Sea-water undoubtedly contains gold, and the precious metal is being deposited at the present day from the hot springs of Nevada and California.

Professor Liversidge, as he narrates in a paper read before the Royal Society of New South Wales, has himself caused gold nuggets to grow by immersing them in photographers' gold toning solution, together with sand, pyrites, and other minerals. Yet he concludes that such has not been the origin of the large gold nuggets, for they are found with other auriferous pebbles which are admittedly derived from reefs; further, he denies that there is any essential difference in composition between reef and alluvial gold, and asks why the latter, if formed *in situ*, does not occur in strings and plates.

We may add that nuggets occasionally bear impressions of crystallised minerals upon which they must have been deposited, and therefore in a vein: this is the case with one of the largest nuggets in the St. Petersburg Museum.

On the whole it does not appear that gold will grow any more rapidly if buried in alluvial deposits than when invested in the more ordinary way.

Sterilised Foods.

These are the days when the bacteriologist, advertising or other, cannot sleep the sleep of the just unless he has suggested a new terror to the hypochondraical public. Italian ices, water-cress, oysters, milk, and kissing—each dear delight or domestic necessity in turn is declared a poison. We cannot reassure the timorous by declaring the warnings quite unfounded. We are certain, even, that as the number of those on the quest for microbes increases, the number of places where microbes associated with disease are found will also increase. For microbes have an astonishing vitality: they themselves, or their spores, frequently are unaffected by any application of heat save prolonged boiling or the higher temperature of roasting, by antiseptics, save in concentrated form and after prolonged application, or by alcohol in any of its potable forms. Their tenacity of life, their power of rapid multiplication, and the facility with which they spread from place to place, secure their presence in almost every conceivable spot.

In this omnipresence of microbes the timorous may take refuge, and pursue their wonted habits of eating and drinking and kissing untroubled by the sinister suggestions of bacteriologists. They may be certain that, except in special cases to which we shall allude, they run no more risk than the risks of breathing the common air or coming in contact with the outer world in the most necessary fashion. All animals and plants inevitably are subject to a bombardment of bacteria, and the few taken in casually with articles of food are of little moment. The living body itself in most cases is able to dispose satisfactorily of the most unwelcome bacterial intruders. Otherwise in the struggle for existence man and the higher animals would perish before the onslaught of bacteria.

Those who are in a feeble state of health may do well to take special precautions. They may protect themselves partially against inhaling bacteria by wearing respirators; they may avoid all but the most carefully cooked food. But as few could resist the continued immigration of disease-causing bacteria, certain definite precautions must be insisted upon.

The first and most obvious is the destruction, by burning, of all matter that has been in contact with a person suffering from an infectious disease. No doubt in towns this is fairly well seen to; in the country it is apt to be totally or partly neglected. This is not a precaution that should be left to the spasmodic or ill-instructed efforts of local authorities; there are so many ways in which bacteria, or spores, may spread all over the country from the remotest focus that the Local Government Board should have a staff to see that proper disinfection is carried out wherever the necessity arises. The timid man, fearing his cigarettes or his water-cress, should insist upon obtaining a pledge from every candidate who seeks his vote for Parliament that no effort, in season or out of season, shall be spared until a staff shall be appointed adequate to cope with every source of infection as it arises in the country.

Milk and Infection.

Proper disinfection of every case of infectious disease is the only means by which the spreading of bacteria may be stopped literally at the fountain-heads. But of palliative measures the most important is the proper supervision of the milk trade. When pathogenic bacteria are distributed by clothing, by fruit, or by means of the wind, no multiplication of their number occurs. Milk, on the other hand, is a fluid containing nearly exactly such ingredients as would be chosen in a laboratory when a fluid was being prepared for the artificial cultivation of microbes. Again and again epidemics have been shown to have gone round with the milkman. In a recent number of the *Forum*, Dr. Nathan Straus has shown how the death-rate of New York was lowered by the establishment of proper precautions for the sale of milk; above all, success attended the sale of sterilised milk. Young children are peculiarly unable to resist the attacks of bacteria, and when fed upon milk that has incurred the inevitable risk of contamination from the time it leaves even a healthy dairy, a large percentage become infected. This is a matter that should receive the immediate attention of local authorities; but it is to be remembered that it is a precaution necessary only because of our national carelessness in securing the disinfection of existing cases of disease.

A peculiarity of the typhoid bacillus, which is specially common in milk, is its affection for acids and its dislike of alkaline fluids. A common "history" of infection by typhoid is that the patient went for a long country walk and feeling hungry had a drink of milk at a farm-house. When the stomach is empty and hungry its reaction is acid, and it is therefore a suitable medium for the growth of the microbes of typhoid. The obvious practical moral is to drink only boiled or sterilised milk when you are hungry, or to avoid it. This explains the importance of boiling milk for invalids or babies who are being fed upon it. On the other hand, after a full meal, when the stomach is no longer acid, milk is less dangerous.

The Tubercle Commission.

The appointment of a commission to investigate the relation of tuberculous meat to the spread of tubercle among human beings is a timely provision of Government. Anyone who has assisted at post-mortems upon animals that have died in menageries or in farmyards, or that have been killed in the ordinary way by the butcher, must have been astonished at the frequency of tubercle. Except in extreme cases, the tuberculous nodules occur most frequently in the viscera, and the carcass as it reaches the butcher's shop shows no trace of the infection. No doubt all but the worst cases are destroyed as unfit for human food; but many cases are so slight that they might be overlooked, and there is an opinion widespread among veterinary surgeons that tubercle is not transmitted from a diseased animal to an animal eating its flesh. This opinion is so general that it appears in an "Advanced Agriculture" published a few weeks ago, and it is natural that "vets." should have ready listeners among butchers and farmers. We welcome, then, the appointment of a commission that should give a final judgment upon which legislation, if necessary, may be based. In the meantime, despite the protests of the vegetarians, those who confine their attentions to meat that has been well roasted, boiled, grilled, or stewed, may remain complacent devourers of animal proteid.

Strange Food.

One of the essays in Mr. Cornish's recently published volume, "Life at the Zoo," contains a number of amusing quotations from the author of the "Englishman in Paris." In the days of the siege the Parisians had the opportunity of making a number of experiments on the qualities of animals not usually eaten. Besides such smaller cattle as rats and mice, they ate up nearly all the animals in the "Jardin des Plantes." Many of these proved excellent; they compared very favourably with the harmless necessary dog. Dogs, indeed, were largely eaten. At the end of the siege the sight of a dog or a cat was a rarity in Paris. But it presented difficulties even to the Parisian *chef*. "You may disguise anything with saffron except dog's flesh. His meat is oily and flabby; stew him, fry him, do what you will, there is always a castor-oil flavour remaining, which cannot be got rid of. The only way to minimise that flavour—to make him palatable, is to salt, or rather, pepper him, to cut him up into large slices and leave them a fortnight, bestrewing them very liberally with pepper-corns. Then before cooking them put them into boiling water for a time and throw the water away."

Additions to the Zoo.

The collection of animals in the Zoological Gardens recently has been enriched by a number of valuable acquisitions—an unusual number, perhaps, for the time of year, which is obviously not favourable to new-comers. As the Regent's Park Gardens have been open to the public for over sixty years, it is not so very often that an animal "new to the Society's collection" arrives. But within the last week or two a flourishing colony of Surinam toads (*Pipa americana*) have been accommodated in one of the tanks in the Reptile-house, and, better still, have already begun to breed. It seems to be uncertain at present which sex carries the nursery about in the curious way so familiar to those who possess any illustrated work upon Natural History; but, in any case, the interesting phenomenon can now be seen at the Gardens, one individual being pitted with holes with developing eggs in them. An equally interesting recent acquisition is the Tree Kangaroo (*Dendrolagus benettii*)—a remarkable instance of a creature that has changed its habits radically without any appreciable and corresponding alteration in structure. A pair of that singular bird, the Horned Screamer (*Palamedea cornuta*), has been placed in one of the large aviaries. A pair has never been on view, and it is not often that a single individual has been exhibited. One of the two specimens is provided with the curious median frontal horn so distinctive of this bird—differentiating it from its near ally, the Screamer Chauna—the other is hornless; presumably one is a male, and the other a female. It is a matter for speculation whether the horn of this undoubtedly archaic form of bird has any relation to the horns occasionally found among the Dinosaurs. The bird, however, has a decided tendency to develop horny appendages; for it has two strong ones on the wings, useful for fighting purposes, which cannot be said of the rather frail horn on the forehead.

The British Lion in America.

We reprint the following amusing complaint from the *American Naturalist* for November:—

"We have frequently complained in these columns of the exclusive conduct of scientific enterprises by persons not acquainted with the sciences and not engaged in their pursuit. We will not enumerate the blunders committed by such persons under such circumstances, as they have recently come under our observation; but only refer now to a question of taste in which some of these well meaning persons have immortalised themselves in stone. A new building for the use of the collections of the Academy of Natural Sciences of Philadelphia was recently erected, chiefly from money appropriated by the Legislature of Pennsylvania. An entrance doorway was devised, and in order that it should represent the uses of the building, it was adorned with figures and reliefs of animals. Persons possessed of the least spark of originality would have seen the propriety of representing in these figures something appropriate to the country, and if possible the institution. Nothing would have been easier than to have placed at the entrance of the Museum, figures of some of the forms of life discovered by its members. The idea was suggested to the gentlemen in charge of the construction, but to commemorate in so conspicuous a manner the services of the naturalists of the Academy it did not strike them favourably. So it came that the apex of the entrance was surmounted by, not even an African lion, but an official British lion, with his mane brushed into a collar like Punch's dog, such as one sees on Government buildings in Great Britain. On each side is a lioness similar to those seen on buildings all over the world. At the summit of one lateral column is a head of a hound, and on the other side a ram with very unsymmetrical horns, both foreign importations. Of the animals in relief above the door, the only American animal is a crab, *Lupa diacantha*, which is indeed, very appropriate to the building commission, as it generally goes backwards, and pinches its nearest neighbours."

THE FOLDS IN THE GASTROPOD SHELL.

DR. DALL appears to have been on an exploring expedition through one of his own papers, and to have realised that an idea of great value contained therein was likely to receive less attention than it deserved, because its environment afforded it too great facilities for concealment. At any rate his theory as to "the mechanical cause of folds in the aperture of the shell of Gastropoda" has been reprinted with adaptations from the "Transactions of the Wagner Free Institute of Science" (vol. iii., 1891), and now appears in the *American Naturalist* for November. The theory is that the folds, or ridges, on the columella, or central pillar, of many spiral gastropod shells, owe their origin to the circumstance that the mantle of the animal, as it contracts and withdraws into its house, is thrown into longitudinal folds, and the shelly matter secreted by the mantle naturally collecting in the furrows between these folds gets deposited in the lines of their passage over the columella, thus layer by layer building up the shelly ridges. The idea is so simple, yet apparently so satisfactory, that its republication in an accessible form will, we hope, lead to its discussion by competent observers, with a view to its proper testing by further researches, and its establishment or rejection as a working hypothesis.

I.

The Study of Existing Glaciers.

IN NATURAL SCIENCE for last November (vol. v., p. 335) allusion was made to the formation, by the International Geological Congress, of a committee that should study the changes now occurring in glaciers, and organise research in this important subject all over the world. The following article is written to give some further details, to show the kind of work that is being done, and to enlist the coöperation of fresh workers.

Any consecutive series of records of glacial movements, from different countries, cannot fail to throw much light on the subject of ice-action in modern as well as in pre-historic times. And it is as well to emphasise the canon that, since we know of not the slightest variation in physical laws, the action now-a-days must be of the same nature as when the scale was very much larger. It is, therefore, very necessary that writers on the theory of such matters should gain a personal familiarity with existing glaciers. Thereby they would be saved from sundry enunciations, which are at times positive enough in all conscience, but which bring a frequent smile to the sunburnt face of the student of nature. And, be it remarked, this study does not demand in its followers special knack or strength, or, in fact, that they should be mountaineers, since climbs up stiff rocks and steep snow-slopes bear but a small proportion to the easy excursions ordinarily needed for the examination of glaciers.

There is a branch of this investigation that has as yet been very little worked at; namely, the connection of meteorological conditions with variations in the bulk of the ice-mass and with its advance or retreat. By a careful collection and comparison of data, much unlooked-for light might be thrown on these matters, and many crude notions as to the influence of heat, wind and moisture, might be brought to bit and bridle. To this study may be added that of the modifications due to latitude and the influence that that factor has upon continental and insular climate. It is easy to see the immense importance of a concurrent epitome of local meteorology, whether we consider the varying amount of snowfall, or the prevalent winds and clouds, or the effect of variations of temperature upon the melting of the ice or upon its evaporation direct from the solid state, or the similar influence that these changes have upon the *névé*. It would, indeed, hardly be going too far to say that these matters have a bearing, as yet unestimated, on sundry astronomical questions, which need not be discussed here.

It was with a sense of the value of such a study in different countries, a study which may, somewhat barbarously, be termed Comparative Glaciology, that a futile attempt was made in 1890 to induce the British Association to create a committee (without any money grant) for the study of glaciers in Canada, New Zealand, the Himalayas, and other districts under what we now-a-days term British influence.

Although nothing came of this, it was not long before an appeal was made to the Alpine Club, a body not professedly or exclusively scientific, but including men perfectly capable of taking part in such investigations. Fortunately the President, Mr. Douglas W. Freshfield, happens to be a man distinguished in the annals not only of pleasant climbing, but of exploration, and notably as a pioneer in the Caucasus, while he is at the same time Secretary of the Royal Geographical Society. He took the matter before the committee of the Alpine Club, and a committee of three resulted, of whom one member was requested to undertake, for the *Alpine Journal*, the preparation of an epitome of such information as should be collected. Mr. Freshfield also, as President, drew up a memorandum asking for records and observations, and drawing attention in a very able and well considered manner to those points upon which information seemed to be most needed. This memorandum is reprinted towards the close of the present article, in the hope that it may reach a yet wider circle of readers.

Our Colonial Office was next applied to for help, and especially asked to transmit copies to Colonial Governments, for the purpose of securing local coöperation on the part of officials, surveyors, and travellers. A favourable reply was sent, and these requests complied with. It is as yet too soon for much to have been done, except in one notable case, that of New Zealand. There the circular arrived and was distributed through the authorities in time for the exploring season; whereas in this Northern Hemisphere summer work was over for the year. As a result, several New Zealand glaciers have already been surveyed, and points have been trigonometrically determined with a view to recording motion and variation in mass. Indeed, in more than one instance actual observations have already been sent us; and materials exist for an interesting report in the *Alpine Journal*. These are referred to later on in this article.

Something of a beginning having been made as regards what one may term "English-speaking" glaciers, the next step was to address the International Geological Congress at its recent meeting in Zurich. The idea was that each nation concerned should be represented by some one charged with organising and reporting in his own country, and that the reports should be issued in collected form. The scheme, ably seconded by Professor F. A. Forel, was adopted without opposition, and Prince Roland Bonaparte removed the only serious difficulty by offering to take the necessary expenses upon himself. The committee consists, so far, of the following members, besides whom it is intended to be completed by representatives of Denmark, Norway, Sweden, Russia and Italy:—

Austria	Professor Dr. S. Richter	Graz.
France	Prince Roland Bonaparte ..	Paris.
Germany	Professor Finsterwalder	Munich.
Great Britain	Captain Marshall-Hall	Easterton Lodge, Parkstone, Dorset.
Switzerland	Professor F. A. Forel	Morges.
United States of America	Professor Dr. F. Reid	Baltimore.

Professor Forel is the organising secretary; the committee is empowered to make its own regulations, and is to report to the general committee of the next Congress, wherever that may be held. The late Tsar invited the Congress to St. Petersburg; but whether that invitation will be renewed by his successor remains to be seen.

So far the Glacier Committee has not had time to arrange further details than those here mentioned. But, at any rate, a tolerably complete machinery exists, since each country has the means of reporting on its own matters without any outside interference; whilst it is to be hoped that, once in several years, a comparative statement of ice-history may be published in one volume. Of course the cordial coöperation of the various Alpine Clubs is important, and there is every reason to expect it. The Glacialists' Association may also contribute. But it is to be hoped that we may be recruited by younger men, to whom we suggest that many branches of this enquiry open comparatively new ground, as, for instance, the motion of *névé*. And the names of men coming promptly to the front, like Messrs. Harper, Douglas, and Brodrick, of the New Zealand Alpine Club, are not likely soon to be forgotten.

Some examples of the kind of work accomplished or suggested may be of service.

In a short paragraph, the *Alpine Journal* for November, 1894, mentions some important facts, which will be given with more detail in the May number. One thing bears upon a theory which the Rev. J. F. Blake recently brought before the British Association, to the effect that the bottom layers of a glacier are squeezed out in advance of the higher ones. Messrs. Harper and Douglas report that in the Franz Josef glacier the upper ice is pushing over the lower, and overlapping and breaking off at the terminal face; and in support of this statement they send photographs. The same gentlemen have carried out measurements to ascertain the speed of ice-movement at different levels. This has also been done for glaciers in the Canterbury district by Mr. T. N. Brodrick.

Again, Professor Forel thinks that the cycle of increase and diminution in the size of a glacier may be somewhere about thirty-five to fifty years. Since this is a periodicity of more than a generation, history may be consulted with advantage; but at the same time one must not forget to study the parallel meteorology of the district. It may, for instance, be enquired for how many years after a snowy winter, or a sequence of such, glaciers show increase in their lower portions; or how far local and prevalent warm winds may mask glacial advance in a valley. Without being able to give precise figures, we suspect that in several cases the advance or retreat of glacier snouts has had too great an importance attached to it.

The evidence as to the pace of the same glacier at different heights is none too copious, whilst the motion of the *névé* and of the snow-masses which feed the ice is very little known. Professor Forel's action in causing lead plates, engraved with the date, to be buried near the summit of Mont Blanc, is one that might be followed elsewhere.

The comparative amounts of rocks and detritus resting upon glaciers, and of that which has found its way beneath, as well as the amount removed by torrents from under the ice, are difficult points on which some good work has recently been done, but on which still more is required.

A few other subjects, as well as methods, of investigation are summed up in the circular by Mr. Freshfield, to which allusion has already been made, and which runs as follows.

"The recent movements of glaciers may be noted by the following signs:—

"When the ice is advancing the glaciers generally have a more convex outline, the icefalls are more broken into towers and spires, and piles of fresh rubbish are found shot over the grass of the lower moraines. Moraines which have been comparatively recently deposited by advancing ice are disturbed, show cracks, and are obviously being pushed forward or aside by the glacier.

"When the ice is in retreat, the marks of its further recent extension are seen fringing the glacier both at the ends and sides in their lower portions, the glacier fails to fill its former bed, and bare stony tracts, often interspersed with pools or lakelets, lie between the end of the glacier and the mounds of recent terminal moraines.

"Where a glacier has retreated to any considerable extent, careful observations of the form of its bed are of value. What is the nature of the rock surfaces exposed—convex or concave; are they rubbed smooth on their lee-sides; how far have the contours of the cliffs or slopes, or the sides of any gorge, been modified where they have been subjected to ice-friction? Is there any evidence that the ice has flowed over large boulders, or loose soils, such as gravel, without disturbing them? How has it affected rocks of different hardness, for instance, veins of quartz in a less hard rock? Generally, do the appearances indicate that the glacier has excavated, or only abraded and polished its bed; that it has scooped out new rock-basins, or only cleaned out, scratched, and preserved from filling up by alluvial deposits or earthslips, existing basins? What is the general character of the valley bottom and slopes above and below the most conspicuous ancient moraines?

"The depth of mountain lakes and the position of the point of greatest depth should be ascertained wherever possible. The marginal rock-structure of lake basins, particularly near their outlet, is of much importance with a view to ascertaining whether they are true rock basins, or whether they are reservoirs formed by ancient moraines, earthslips, or alluvial deposits.

"The traveller or surveyor should, if possible, paint a mark and date on any conspicuous rock *in situ* parallel with the termination of the glacier at the time of his visit, marking the distance in yards of the ice from it. The next visitor will then be able to measure the movement that has taken place since his predecessor's visit. Leaving out of question elaborate trigonometrical methods, such, for instance, as have been carried out on the Rhone Glacier in Switzerland, the following plan gives very valuable results, and demands no other instruments than a small jar of paint, a brush, a measuring tape, and a pocket compass. To ascertain the recent retreat of a glacier, measure the distance from the end of the ice in front of the longitudinal axis of the glacier to the most advanced terminal moraine, where vegetation first shows itself. The bare ground recently left by glaciers is easily recognisable. The diminution of volume is best measured by ascertaining the height of bare soil left on the sides of the lateral moraines in the portion of the glacier within the zone of vegetation. All photographic representations of the glacier-end, and of the ground which has been freed from the glacier-ice, are of great value. Those will be of most service that show the position of the glacier-snout with relation to some conspicuous rock or other feature in the local scenery. Each photograph should be dated, and the bearings and distance of the camera with reference to any such feature accurately noted.

"It is very important to investigate the state of various glaciers as regards advance or retreat. Neighbouring glaciers often furnish very different results in this respect, owing to the fact that steep glaciers anticipate in their oscillations those of which the beds are less inclined. To ascertain the oscillations of glaciers, it is necessary to fix the actual position of the ice-snout at the end of the glacier with the greatest accuracy. Two methods can be employed for this purpose, either of which may be selected according to circumstances.

"Paint some signs on large boulders, not too far from the end of the glacier, and measure their distance from it by a tape (Richter's system), or build a low wall of stones of a few yards in length, and, say 15 to 20 inches in height, some distance from the ice-end, and measure this distance (Gosset's system). It is to be recommended that the stones of these walls should also be painted. If the traveller himself returns after some interval—even after only two or three weeks—he will be able to judge of the movement of the glacier, and he will have laid down a basis for future observations by future travellers.

"One of the results most to be desired is an exact knowledge of the dates: (i.) Of the maximum extension of the ice. (ii.) Of the commencement of retreat. (iii.) Of the minimum. (iv.) Of the commencement of fresh increase. In dealing with a mountain group, therefore, the traveller should note (where he can get the information as to the past) the date of the commencement of the actual movement of *each glacier*, and in all cases whether the ice is in advance, or retreat, or stationary. Of course the rate of forward movement, or velocity of the ice, and the oscillations in the extension of the ice must be kept carefully distinct.

"Should time and circumstances permit, a series of observations of the velocity of the ice is of value. These may be made after Tyndall's method, by planting a line of sticks across the glacier, or by painting marks on boulders, the position of which relatively to ascertained points on the mountain-side has been accurately fixed. The size of the glacier, that is, the area of its basin and its length, as well as the slope of its bed above, as well as at the point measured, should be noted. The rate of movement of the ice appears to be connected both with the volume of the glacier and the inclination of its bed."

With regard to the suggestion in the last paragraph it will be as well to caution surveyors that, if stakes are made use of, they should be of good size and planted deeply; surface ablation is rapid in hot weather, and Herr Escher von der Linth, having, on July 8, 1841, inserted stakes in holes 3 feet deep on the Aletsch Glacier, found, on August 16 (39 days later), all the stakes fallen, and no trace of the holes. Where available, rocks on the glacier surface, even though not likely to be in a straight line, are the best for surveying purposes. Even they have a bad habit of tumbling into crevasses.

Those who are not experts and who may wish for a few further simple suggestions as to procedure and instruments, will find them in an article by the writer of this, in the *Alpine Journal* for February, 1891, no. 111. Not further to multiply these details, it is evident how much there is to be done. And it is also an evident duty for Alpine Clubs, for the International committee, and indeed for all bodies that can influence observers, to systematise the methods employed, leaving of course a perfectly free hand on all matters outside the forms that they may draw up.

More need not be said to urge the importance and the interest of this study. But, besides all this, there is a constant pleasure in the questioning of Nature amid her most glorious scenes, which is by no means confined to the professedly scientific tourist. The interest grows with each day's excursion, while the shorter scrambles, which alone are prudent in unsettled weather, vary the holiday of the hard-working climber after a fashion that becomes more and more fascinating as day after day passes. Then, after the return home, recollections crowd upon one, and the light of past experience is thrown upon many a problem which had before appeared to be too obscure to solve.

MARSHALL-HALL.

II.

The Mammals of the Malay Peninsula.

THE mammal-fauna of the Malay Peninsula is strictly a forest-fauna. There is but little open country such as one sees in many tropical regions, and consequently it is in the thicker parts of the forests that one must wander to see the wild animals at home. The jungle, however, is so dense that it is exceedingly difficult to get near enough to the larger animals to watch without disturbing them; nor are there many persons who, having good opportunities for observing them, will take the trouble to do so and to record their observations. The consequence is that at present there are many unsettled points of importance in the habits even of the bigger beasts, which require the attention of those who have opportunities of penetrating the jungles in pursuit of game, or in exploring or surveying expeditions. Such questions are the presence or absence of the Orang-utan in the Peninsula, or the number of species of wild cattle and of rhinoceros. The natives often talk of the Mowas (a variant of the Bornean word *Mias*, the real name of what Europeans call the Orang-utan) as occurring in the Peninsula; but I have never met anyone who ever saw one. It is always "further away beyond the hills on the horizon," and so on. Marvellous stories are told of it, and it is almost reckoned as an unearthly creature. The name Orang-utan, as applied to this animal, appears to be a European fiction; it merely means a man who lives in the woods or inland, as opposed to one who lives on the sea-coast or river-bank, and is never applied to the Mias, except by natives who have got the name from the English. It would be better to drop it altogether. The Mias is said to be very local in Borneo and Sumatra, though apparently abundant where it occurs, so that it may still be possible that it does inhabit some unexplored corner of the Malay Peninsula. This and many other questions must be answered as opportunities occur, when the interior of the Peninsula is opened up more fully to the naturalist.

In the following notes I propose to collect such observations as seem of interest, chiefly relating to the mammals of the island of Singapore, adding here and there notes on those of the Malay Peninsula which I have kept in captivity.

Though much of the island of Singapore has been cleared for cultivation, patches of jungle of considerable size remain, which are haunted by numerous mammals; and it is surprising how difficult it is to see or get near to the larger ones, how close they lie hidden in the bushes or fern, how quietly they slip off when disturbed. You may ramble through the woods and, except for the cries of the monkeys and the twittering of the squirrels and tupaias, you will not be aware of any mammals within the jungle. The best times for observing the jungle denizens are the early morning and the dusk. Then all are brisk and on the alert. The wild pigs are making onslaughts on the pine-apple and tapioca fields, the deer come out to crop the shrubs on the edge of the woods, the tiger is moving quietly through the bushes in pursuit of the pigs and deers, the weird loris is creeping towards the banana trees, the large red flying squirrel is taking its surprising leaps from one lofty tree to another, the monkeys are squabbling for the best seats on the branches, the great fox-bats are flapping slowly overhead, and the civet cats are climbing briskly about on the trees in pursuit of roosting birds, or trotting off to the coffee bushes and fruit trees in search of fruits. The greater part of the Malayan mammals are nocturnal, the vegetarians to avoid the carnivores, the latter to chase the vegetarians.

Primates:—There are two or possibly three kinds of monkeys indigenous to Singapore. The commonest is *Macacus cynomolgus*, called, from its cry, the Kra, a very common monkey in the more open parts of the woods, seldom going into the larger tracts of jungles. They travel about in small flocks, living chiefly on fruits, and often descending to the ground to pick up those that are fallen. They are very amusing pets, being very intelligent and easily tamed. They breed readily in confinement, producing one young one a year. It is commonly stated that no monkey stands flat on the sole of its feet; indeed, this has been cited as an important difference between man and the quadrumana. It is true that the anthropoid apes do not do so, but they are strictly arboreal and never descend to the ground, so that their feet are adapted for grasping firmly; whereas in the smaller monkeys which often come down to the ground the soles are flatter. The Kra may often be seen walking for a short distance erect on the flat of its foot, its arms hanging by its sides. The only difference between this walk and that of man is that the great toe stands at an angle with the sole. All that I have seen doing this have either been the leaders of a troop or exceptionally intelligent monkeys. The Kra has a more extensive vocabulary than most of the other monkeys, having apparently distinctive cries for all events that may occur in its jungle life: thus the alarm-signal for a tiger is quite distinguishable from that for a man or dog. In this respect the smaller monkeys are very different from the oriental anthropoids, which have a very limited vocabulary.

The Kra will not admit strangers into the family troop without a fight. If a female is put into the same cage with a number of other Kras, the other females will attack and bite her; if a male, he is set upon by the males. If he manages to hold his own pretty well he is admitted into the family, but he is sometimes killed. In fighting, they attempt to bite the top of the head and the ribs. If a monkey on the ground is attacked by a dog, all the rest of the troop comes to its rescue, and usually a severe fight is the result. My fox-terriers are very keen on attacking the monkeys which live wild in and round the Botanic Gardens, and desperate fights often take place, in which monkeys sometimes get killed. On one occasion a battle had taken place in the Garden jungle, resulting in the serious injury of one of the combatant Kras. The others having retired, the dogs went away, and shortly after a Tamil coolie, who was weeding a path nearly a hundred yards from the jungle, saw three big monkeys coming across the open grass plot towards him bringing with them the injured one. They ran, as usual, on all fours, two holding the wounded one by its arms in their mouths, the other pushing it behind. The man was frightened at the monkeys, and ran away for help, whereat the monkeys laid the wounded one on the grass and retired to the jungle. The monkey was taken care of, but soon died. There could be little doubt that the animals really intended to bring the wounded one for help to the man, as there was no reason for their carrying it from the safe shelter of the jungle across the open lawn, where there could be no escape if the dogs did return. Nor did they do anything with it, but put it down where they last saw the man, as if to ask his aid. On a former occasion one of the men had rescued a wounded monkey during a fight and carried it off, but it also was too injured to live. The other monkeys saw this, and probably looked on man as a defender from their enemies the dogs, but it is difficult to see how they managed to concert the action.

Mr. H. Lake told me that on one occasion when travelling in Johore he came upon a brisk fight between a large number of Kras and about half a dozen Lotongs (*Semnopithecus*) for the possession of a Rambutan tree (*Nephelium lappaceum*) in fruit. For some time before he came in sight of the tree the cries of the combatants could be heard, and for two hours the battle raged, and was raging when he went out of hearing. The Lotongs were in the tree defending it from the rushes of the Kras, which they repulsed by biting them and throwing them out of the tree and into the river, sometimes falling with their assailants to the ground, and again climbing up to carry on the defence. Now and then the attacking party would draw off to rest, and arrange simultaneous attacks from different sides. But the Kras had not got a footing in the tree when the observer left.

There is a form of the Kra which frequents the mangrove swamps, feeding on the fruits of these trees, and on crabs and shell-fish, and which may often be seen running about on the mud at low tide in search of food. They are usually more grey in colour, but do not appear to be distinct specifically. The mangrove monkeys are very fond of the water, remaining a long time beneath the surface. I have seen one leaping off the boughs of a tree into the water, climbing up and leaping off again and again.

The Bĕrok (*Macacus nemestrinus*) is not really wild in Singapore, but it is frequently kept in captivity, and, often escaping, remains in a half-wild state, usually near town. It is common in the Peninsula in the denser jungles. This monkey is the one trained to gather coco-nuts by the Malays, who take them round to plantations to pluck the nuts at one or two cents per tree. The Berok is led to the tree, which it quickly ascends, and at the top awaits instructions. At a signal or word it places three of its paws on the nut required, holding on to the tree with one hind-foot, and then twists the nut round rapidly till it becomes detached and falls. A properly-trained Berok will thus pick any nut on a tree or all those that are ripe, or even select an old nut or younger one according to order, judging of the age by shaking the nut and listening to hear whether the milk rattles or not inside. At a jerk of the string by which it is attached the monkey descends and sits quietly on the ground. A trained monkey is valued at twenty dollars or more. The Berok when young is an intelligent pet, but adults are liable to be vicious. I have never known one breed in captivity.

The Lotong (*Semnopithecus femoralis*) is not very common in Singapore, but a few occur in most of the larger jungles. They appear to live in small flocks of six or seven, but are commonly seen singly or in pairs. They are strictly arboreal, never descending to the ground, and are very active, taking immense leaps when frightened, and uttering their wild laughing cry. This cry can be heard far off, and when heard at night is an intimation of the neighbourhood of a tiger or other large wild beast. The Lotong is very rarely to be seen in captivity, and is very short-lived when captured. It appears to eat buds and other green-stuff as well as fruit. I once saw one devouring the flowers of a big wild nutmeg (*Myristica superba*). There are several other species of *Semnopithecus* in the Malay Peninsula with apparently similar habits to that of *S. femoralis*, but it is not easy even to secure specimens, still less to observe their habits.

The Wa Wa (*Hylobates agilis*) is common in the Malay Peninsula as far south as Johore, but is not wild in Singapore. Their strange chant, going gradually up the scale and ending in loud wails, can be heard at a long distance in the early morning at sunrise, seldom much later. Their habits are tolerably well known, as they are constantly kept as pets. They are exceedingly affectionate and playful. Beside the above-mentioned song, except a kind of low crooning expressive of sorrow, I never heard them utter any sound.

The Kongkang (*Nycticebus tardigradus*) is common in Singapore, and often caught by the natives. It remains concealed all day, and at night creeps out and visits the plantain trees to eat the fruit. It is an uninteresting pet, but is often kept as a curiosity. Here, as in

India, it is supposed to have magical properties, being able to see ghosts, and every part of it, including its tears, is used in medicine.

The Galago or "Kubong" (*Galeopithecus volans*) is plentiful in some parts of Singapore. It frequents the thickest jungles, remaining all day closely clasping the trunks of trees, which its beautiful soft mottled green and grey fur resembles, and moving about at night. Beside the one coloured as above mentioned, there is another less common form with red fur. It appears to live on leaves, as the stomach is always full of nibbled up foliage, but in captivity it will eat plantains. It is a very delicate animal, and difficult to keep, suffering much from cold. During the day it makes at times a noise like the quacking of a duck. I have twice obtained females with single young ones clinging to them.

Cheiroptera :—There is a considerable number of species of bats in Singapore, but it is difficult to get much information on their habits.

The large fruit-bats (*Pteropus vampirus*), "Kelawang" of the Malays, are abundant at certain times, appearing in vast numbers and taking up their abode in some chosen spot, remaining there for some two or three months and then scattering again and disappearing utterly. One year the Garden jungle was a favoured spot, and it was roughly calculated that seventy thousand roosted there every day. The reason for this flocking of the fruit-bats I cannot guess, as it appears to take place at no particular time of year, nor does it bear any relation to the fruit season. They devour large quantities of fruit of all kinds, but appear to do less damage than the smaller *Cynopteri*, which are permanent residents and, occurring in great abundance, destroy a great quantity of fruits. To keep the fruits from the bats, the natives often enclose the bunches in bags of cloth or matting, and also attach to the trees the long thorny flagella of rattans, in which the bats entangle their wings and tear them when they attempt to attack the fruit.

The smaller fruit-bats hide during the day beneath plantain leaves, and in such places, and also in caverns and holes in rocks, often in enormous numbers. The Bat-caves of Selanger are well known. They are large caverns in the limestone rocks, in which myriads of bats, apparently of several species, roost during the day. The floor of the caves is ankle deep in bats' dung, containing myriads of beetles' wings and exhaling a strong pungent odour, while the squeaks of the bats and the whirr of their wings can be heard at some distance from the mouth of the caves.

These fruit-bats play an important part in the dispersal of seeds, for when they devour a fruit they often bear it to some distance to eat, throwing away the seeds as they eat the flesh. Once, when sleeping in a rather open bungalow at Ayer Panas, in Malacca, I was kept awake for some time by the dropping of seeds of *Elæocarpus parvifolius*, the "Medong Kelawang" (*lit.* Bats' *Elæocarpus*) of the

Malays, which is a hard green globose fruit containing a round woody seed as big as a marble. This the small fruit-bats flying in under the roof brought in and nibbled as they hung suspended from the roof poles, dropping the seeds with a sounding noise on the wooden floor.

The large owl (*Ketupa javanensis*) catches these little bats. I have seen one swoop by me in the gardens in the evening, bearing in its claws its prey, which emitted the piercing shrieks of this bat.

The houses in Singapore being very open, bats often fly in at night, the commonest being the little fruit-bat (*Cynopterus marginatus*). The fruit bats, being clumsy and stupid, have much difficulty in finding their way out again, and are easily caught; but the insectivorous bats are much more clever, and fly in and out with ease.

The insectivorous bats, "Kelawang," are more difficult to catch and to observe than the fruit-bats. They seem to live in ones and twos, and not in numbers together as the latter do. The Horseshoe bats (Rhinolophidæ) usually live in the jungles, suspended to boughs of shrubs all day. I observed one in the Gardens at dusk which darted from its perch on a twig in pursuit of insects, returning again and again to the same twig, where suspended it spun round and swung about till it darted off again. Even when alarmed it only left the twig for a few minutes and returned again. The curious hairless bat (*Chiromeles torquatus*) is not a rare species. When it comes into the house at night, unlike most insectivorous bats, it is confused by the light, and is easily caught.

Insectivora:—The commonest insectivora are the Tupaia (*T. ferruginea*) and the Musk-Shrew (*Crocidura murina*).

The name Tupaia really signifies a squirrel; and this insectivore so closely resembles one in form and habits that it is considered as one by the Malays. The common species is very destructive in gardens, as it is almost if not entirely frugivorous. It bites holes in the chocolate pods to eat the pith which encloses the seeds, strewing the latter all over the ground, and even digs up the seeds planted in flower boxes. It varies much in colour at different times of the year, but is usually of a dark brown, and then so closely resembles the common squirrel that, except for the longer snout, it is difficult to distinguish it when running about. If this resemblance is to be reckoned an example of mimicry, it is not easy to decide whether it is the Tupaia which mimics the squirrel or the squirrel the Tupaia. Possibly the resemblance is accidental, both animals having taken on the most inconspicuous colouring and the most suitable form for their environment. The curious long-nosed squirrel, *Sciurus* (*Rhinosciurus*) *laticaudatus*, which so closely resembles a Tupaia that to distinguish it one has to look at the teeth, lives apparently almost entirely on the ground, darting about among the fallen logs, as some Tupaias always do; and this animal seems really to be adapted for the mimicry of a Tupaia. *Tupaia ferruginea* is more terrestrial in its habits than a squirrel. When alarmed it darts up a tree, but never very high, and turning its head downwards utters a series of little scolding grunts, which sound like some one talking at a considerable distance. When a stone is thrown near it, it usually immediately jumps to the ground. It is evidently as yet only half accustomed to an arboreal life. In confinement it is very nervous, dashing about the cage when approached, and it never lives long in captivity.

The Musk-Shrew (*Crocidura murina*) is most abundant in gardens and near houses, and often perfumes the lower part of the house with its strong musky smell. Notwithstanding this, the dogs and cats constantly kill them, though of course they do not eat them. It is nocturnal, and being of a dusky grey is very inconspicuous. When disturbed it darts away uttering loud, piercing shrieks, probably to warn its pursuer that it is not a rat but an uneatable shrew, and it may thus often escape being killed by mistake. It is useful in a house for keeping down cockroaches and other insects, but is commonly reported to taint wine and other liquors by running over the corks.

H. N. Ridley.

(*To be continued.*)

III.

The Distribution of Food-Fishes in Relation to their Physical Surroundings.

SOME twenty years ago the Scottish Meteorological Society appointed a Committee to coöperate with the Fishery Board for Scotland in investigating the question whether a definite relation could be established between the movements of herrings off the east coast of Scotland and the temperature of the surface of the sea in the neighbourhood of the fishing grounds. The results of the Committee's labours were embodied in three Reports, published in the Journal of the Society; and they are summed up in the conclusions that, while there is apparently no direct connection between the number of herring caught and the surface-temperature of the sea, there is evidence that the herring-fishing begins as soon as the surface-temperature rises to $55\frac{1}{2}°$ F. Further, a majority of cases showed that great catches were obtained in patches of water colder than the mean temperature of the surrounding area, while shoals of herring were seldom found in relatively warm patches.

Since that time, our knowledge of the physical conditions obtaining in the sea round our coasts has considerably increased; and especially is this true of the coast of Scotland, thanks to the work of Mill, Gibson, and others in connection with the Scottish Fishery Board and the Scottish Marine Station. At the same time, our acquaintance with the distribution of marine animals has greatly extended. The institutions just mentioned have done good work in Scotland, the Marine Biological Association and the various coast laboratories in England, and the Royal Dublin Society and the Government charter "Harlequin" in Ireland. But up to the present no systematic attempt has been made in this country to follow up the lines suggested by the Committee of the Scottish Society for combined investigation.

Meanwhile the matter, in so far as it relates to the Baltic Sea and the entrances thereto, has been taken up by Sweden; and the Swedish oceanographers have achieved brilliant results. With what appear to be practically unlimited resources, extensive explorations have been carried on for several successive years, and a full account of the work by Professor Pettersson, of Stockholm, the chief of the scientific staff, has just been published, in English, in the *Scottish Geographical Magazine*.

The first question arising in such investigations is, naturally, What special physical or chemical element is it which primarily affects various forms of animal life? The actual influence exerted upon different animals directly, favouring their healthy existence and reproduction or otherwise, or indirectly, affecting their food supplies, is a biological problem: the chemist or physicist must for the present assume that, given a certain chemical or physical variation, some corresponding change takes place in the biological conditions, and that the success or failure of certain fisheries is in some sense a measure of that change. The first and most obvious element is, of course, that of temperature. But, while it is undoubtedly true that most animals are found within regions having more or less definite temperature-limits, and that certain forms flourish under a wider range of temperature-conditions than others,[1] it is also true that direct hard and fast relations between temperature and the constant or periodic occurrence of certain animals in certain localities cannot be found. The same statement holds for salinity, and for density, the joint function of salinity and temperature. In the absence of any simple relation, it remains to explain the coincidence of the occurrence of certain changes of temperature or salinity with migrations of animals, which has been observed over and over again; and we obtain a hint from the fact that it is just these two physical elements upon which the oceanographer chiefly relies in tracing the circulation of waters, and in identifying the sources from which the water occupying any given region is derived. The presence of a body of water at a particular place is in general indicated by peculiarities of temperature or salinity, which, although easily enough recognised by proper physical methods, are certainly too slight to seriously affect animal life; and if the arrival or departure of such water coincides with important changes in the fauna, we are almost forced to conclude that it has some other property to which we must look for the direct relation required.

It is impossible to detail here the steps by which the Swedish oceanographers have, by observations in and around the Baltic, built up their case; but a study of Professor Pettersson's papers in the *Scottish Geographical Magazine* shows that he and his colleagues have collected a mass of evidence to prove that the migrations of herring and mackerel, and other variations in the distribution of not only fishes but Plankton, are dependent on the amount of oxygen present in the sea-water. Jacobsen, the chemist of the "Pommerania" expedition in the North Sea in 1872, showed that the amount of air absorbed by sea-water depended solely on the temperature and barometric pressure to which it was exposed when at the surface. Water passing from the surface to lower layers retains the amount of nitrogen unchanged, while the oxygen may be diminished by the action of organic matter or animal life. Sea-water shut up in enclosed

[1] *See* the paper by Dr. Otto Maas in NATURAL SCIENCE, vol. v., p. 276.

areas is accordingly liable to become, at least in its lower layers, deficient in oxygen; and fresh supplies must be obtained by mixture with water from the ocean, if animal life is to remain healthy and abundant.

Having found that, in the cases referred to, the Swedish fisheries depended on the influx of ocean water, rich in oxygen, at the entrance to the Baltic, Professor Pettersson was naturally anxious that the oceanic streams should be explored beyond Swedish waters, and his representations in this country led the Fishery Board for Scotland to undertake the survey of the region to the north and east of Scotland and round the northern and western borders of the continental plateau. During the whole of August, 1893, H.M.S. "Jackal" was engaged upon this work, and further expeditions were made in November, 1893, and in February and May, 1894. The results of the investigations, which were made under the scientific direction of the present writer, are published in the Twelfth Report of the Fishery Board (part iii.). Observations were made by Swedish, German, Danish, and Norwegian vessels simultaneously with those of the "Jackal," in different parts of the North Sea, and a joint report on part of the work is at present in preparation.

The work of the "Jackal" was unfortunately restricted to a study of the water circulation by means of observations of temperature and salinity. Gas analyses and observations with the tow net were beyond the resources at our disposal, and it is chiefly by assuming that Professor Pettersson's results hold good, to a greater or less extent, in the open sea, that the circulation of water disclosed by our observations gains marked significance in relation to fisheries. It will be seen, however, that the presumptive evidence in favour of such an assumption is so strong as to make complete investigations of urgent importance.

Our attention was first directed to the narrow, deep channel between the Faeroe and Shetland groups, which forms a submarine gulf opening from the basin of the Norwegian sea, and is cut off from the Atlantic basin at its southern end by a barrier rising to within 300 fathoms of the surface—the Wyville Thomson ridge. It has long been known that the lower strata of this channel consist of water from the Norwegian Sea, recognisable by its low temperatures (31° to 33° F.), and also that Atlantic water flows over the ridge, northwards along the channel, with considerable velocity; but it seemed necessary to ascertain whether the relations of the Atlantic stream to the underlying cold water varied from year to year. On comparing our observations with those of the "Porcupine" and "Knight Errant" it was found that in 1893 cold water rose nearer to the surface at the southern end of the channel, and warm water descended nearer to the bottom at the northern end than had been the case during the earlier expeditions; a result indicating that the Atlantic stream was stronger than usual, and the "reaction current" or undertow in the cold under-layers consequently greater. The cause of this was probably the great excess of surface temperature over the average in the north-eastern part of the Atlantic basin during the summer of 1893, and the consequent high level of the sea surface in that region. It may be supposed that the surface current in the Faeroe-Shetland Channel is, in winter at least, chiefly controlled by the winds; but when winds are weaker than usual, differences of temperature must have increased influence in maintaining currents.

The stronger the current over the Wyville Thomson ridge, the greater the undertow of cold bottom water. A compensating arrangement is accordingly set up, such that the Atlantic stream is always cooled by mixture with the underlying water, and tends to sink below the surface, losing its horizontal motion at the same time. Hence, except when strong south-westerly winds tend to continue the bulk of the current north-eastwards into the Norwegian Sea, there to be absorbed into the general cyclonic circulation of that basin, a mass of Atlantic water collects at the northern end of the Faeroe-Shetland Channel; and the influence of the earth's rotation, deflecting the current to the right, will make it bank up against the continental plateau to the north-west of the Shetland Islands. We have thus a body of water, partly derived from the Atlantic, and partly from the Norwegian Sea, seeking admission to the German Ocean; and the volume of this must vary from season to season and from year to year. Further, the relative proportions of Atlantic and Norwegian Sea water composing it must vary; and since the surface temperatures of the two basins differ widely, the amount of air contained must be different at different seasons and in different years, being smaller the greater the proportion of Atlantic water.

We may next discuss the mode in which this water is admitted to the North Sea. The "Jackal" observations showed that between the Orkney and Shetland islands tidal influence reigns supreme. It is known that in that region strong tidal currents set north-west and south-east; and it was accordingly found that water from the Faeroe-Shetland Channel is, as it were, pumped into the North Sea, in a south-easterly direction. This probably occurs at all seasons, but the volume of water actually introduced cannot be great, inasmuch as it is only the amount substituted by mixture for water already in the North Sea which is withdrawn in its place. Taking next the water introduced by surface currents, we know that the prevailing winds over the whole region are westerly and south-westerly. When the wind influence is strongest, during winter, the tendency will be to drive the surface water either north-eastwards towards the Norwegian coast, out of the North Sea altogether, or eastwards towards the Continental side; there will be little or no current down the western or British side. In summer again, winds are light and variable, and at the same time the surface of the North Sea is warmer than that of the Faeroe-Shetland Channel, owing to the greater warmth of the land. The level of the North Sea is therefore higher, and its surface waters will tend to flow out northward and westward. The outflow will be greater the warmer the summer; its occurrence was clearly shown by the "Jackal" observations in August, 1893.

It appears therefore that, if the western coasts of the North Sea

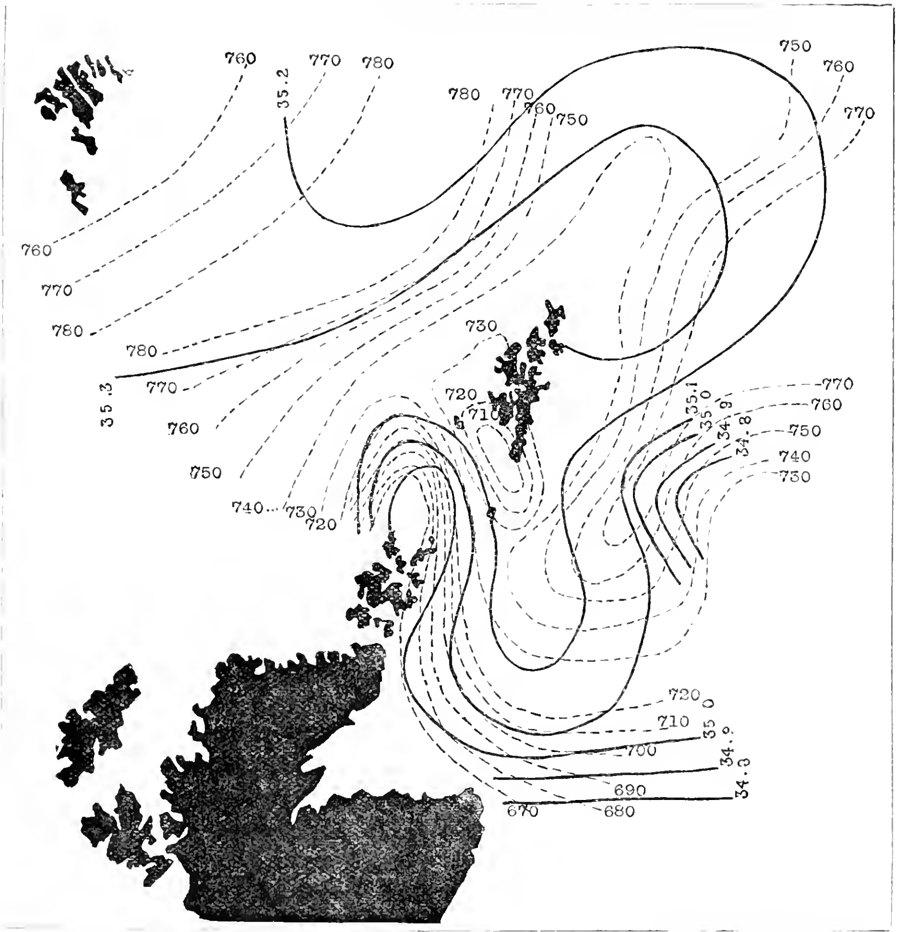

FIG. 1.—Chart of the Faeroe-Shetland Channel and the North Sea. The solid lines show the salinity, in grams per kilogram, at the bottom or at 200 fathoms; the dotted lines show the specific gravity at the same depth (in this case the first three figures are omitted, *e.g.*, "1·02760" is printed "760").

are to receive any part of the oceanic water, it must be by means of a current flowing under the surface. In order to understand the mechanism of such a current, it is necessary to study the conditions obtaining in the North Sea itself in somewhat greater detail. At the end of winter or in early spring, the greater part of the area is occupied by water which, although its salinity has been lowered by the addition of large quantities of fresh water from rivers, is nevertheless on account of its low temperature—due to land influences—markedly denser than the oceanic waters outside. As spring progresses and the atmosphere becomes warmer, the surface layers of the North Sea gradually rise to a temperature higher than that found in the ocean; and as they thus become specifically lighter, they tend, as we have already seen, to flow outwards. The warm surface layers at the same time protect the lower strata from being warmed by the atmosphere, and the latter retain their winter cold, except in so far as they are warmed by conduction. In early summer we have, therefore, at the meeting place to the north of the Shetlands, the following conditions: a cold under-layer of North Sea water, somewhat fresher than the oceanic water, but denser than it because of its lower temperature; a warm layer of North Sea water, fresher and lighter than the oceanic water, but tending to flow out over it; and lastly the oceanic water, salter than either of the others, but intermediate in density because of its temperature. Under these circumstances a gradual process of mixture is set up, with the result that between the oceanic and bottom North Sea water a ridge of maximum density or heaviest water is formed, due to the saltness of the one and the cold of the other. In the earlier stages the position of the ridge is probably nearly north and south from the extreme northern point of the Shetlands; but as the season advances and mixture progresses, the axis gradually retires eastwards. Its position in August, 1893, is shown by the dotted lines in the chart (Fig. 1), the salinity being as represented by the solid lines.

As soon as the axis of maximum density has moved eastwards clear of the Shetland Islands, it is evident that a path is open for oceanic water to move southwards between it and the land, as a bottom current. Guided by the ridge, the water will penetrate gradually southwards along the east coast of Scotland; and as the process of mixture goes on, the ridge itself will slowly retire, and oceanic water spread more and more to the east into the North Sea. As winter approaches, the falling temperature and increasing wind will gradually destroy this distribution, and cut off further influx from the ocean until the following season.

Obviously the conditions required to bring about the circulation just described are exceedingly complex. The amount of oceanic water admitted to the North Sea in this way, and the date of its admission, must depend on the meteorological conditions prevailing in the north-east of the Atlantic, in the southern part of the Norwegian Sea, and in the North Sea itself, during at least six months previously; and it may be that in cold and stormy seasons some entirely different circulation takes place. The observations of the "Jackal," however, leave almost no doubt that the movements of water described actually took place in the summer of 1893. Probably the unusually fine season, and the great cold of the preceding winter, gave an exceptionally good opportunity of observing the circulation; but the observations of the "Drache" and other vessels in former years indicate a similar arrangement.

It is certanly remarkable that there should be so much evidence that oceanic water reaches the east coast of Scotland, as a bottom current, just at the time the herring-fishing usually begins, and penetrates southwards at a rate which appears to agree closely with the progressively later date of the fishing as we go southward. It has already been stated that, in the course of the work of the Committee of the Scottish Meteorological Society, Dr. Buchan found that the herring appeared off the east coast of Scotland about the time the surface temperature of the sea reached $55\frac{1}{2}^{\circ}$ F. This would usually occur about the middle of August, the time at which we should expect the oceanic water to reach the fishing grounds. The further conclusion that large catches of herring were most frequently made in cold patches of water becomes extremely interesting from the fact that, as was shown by the present writer from a discussion of extensive materials in the hands of the Scottish Meteorological Society (*Journ. Scott. Met. Soc.*, vol. viii., p. 332), the great fishing grounds occur where the prevailing winds are off the shore, and that the cold patches are therefore patches of water welling up from the bottom. If our view of the circulation be correct, the bottom water is, for the time being, oceanic water.

Whether the fresh supply of oceanic water reaching our coasts really does contain a markedly greater amount of oxygen than the water whose place it takes, can only be ascertained by analysis of actual samples; and the further question, whether the herring comes *with* the oceanic water or *to* it must be dealt with by zoologists. These investigations could be carried on concurrently with inquiries into the circulation of the waters, depending on the ordinary physical methods. In the light of the results obtained by the Swedish oceanographers, it can scarcely be disputed that the work of the "Jackal" places the scientific investigation of the whole subject in a position of the highest importance to our fisheries. To carry out the work satisfactorily two ships would be necessary, both fully equipped with the apparatus devised by Professor Pettersson for use in shallow waters. One steamer should be of at least 1,200 or 1,500 tons, able to carry on work in the Faeroe-Shetland Channel, even in moderately rough weather; and she should be provided with apparatus for sounding in deep water, and with a launch to enable tow-netting and similar work to go on during the deeper soundings. A smaller vessel, capable of steaming at a fairly high rate of speed, and specially handy in turning and manœuvring with sounding lines and nets, could be devoted to the in-shore waters, keeping a special look-out in the neighbourhood of the fishing grounds. Both vessels should remain constantly at sea during the fishing season, so that no change might pass unnoticed; and favourable opportunities at other times of the year should be made use of to observe the remaining features of the water-circulation. If this work were continued for a few consecutive years there can be little doubt that a method would be found of ascertaining, and even forecasting with considerable accuracy, the distribution of food-fishes at different seasons and in different years. The value of the work would of course be greatly increased by coöperation in the investigations carried on by Sweden and other countries; indeed a complete solution of the problems affecting any one of the parties concerned can scarcely be looked for without some such mutual help.

H. N. Dickson.

IV.

The Problem of the Primæval Sharks.

If the earliest true fish could be found, it would almost certainly fall within the sub-class Elasmobranchii. The progress of discovery of the primæval sharks in Palæozoic rocks is thus followed by ichthyologists with increasing interest. The remarkable fossils described within the last few years have in many respects revealed the most generalised type of fish of which we can conceive; and at the same time it has become apparent that there was great variety among these old genera, several of their parts attaining an excessive development in different directions.

One imperfectly known group, for instance, in which the dermal armour has the microscopical structure of that of Elasmobranchs, is gradually becoming recognised as a characteristic type of the Upper Devonian and Lower Carboniferous periods. It is remarkable for the excessive development of this armour, which assumes the form of great plates and more or less triangular spines, the latter attached to the skin only at their base. The Devonian examples are described chiefly under the name of *Psammosteus*, while most of those of Carboniferous age are termed *Oracanthus*. *Gyracanthus*, known only by paired spines, is also probably a closely related genus.

Psammosteus has just been discovered in the Upper Old Red Sandstone near Elgin, by Mr. William Taylor, of Llanbryde, and new interest has thus been aroused in the fish (6). Hitherto it has been found only in corresponding strata in Russia and Spitzbergen, and in boulders scattered over the North German plain (8). The largest collection of its remains is preserved in the Geological Museum of the University of Dorpat; but even here there is not enough evidence to show how the armour of the fish was arranged. A brief notice of the various plates and spines of *Psammosteus*, which the present writer has had the privilege of examining in that museum, may, however, now prove opportune.

The most interesting fragment of armour from the Upper Old Red Sandstone of Neuhausen, Livonia (where an *Asterolepis* much like *A. maxima* occurs), is shown of one-quarter the natural size in the accompanying Fig. 1. The original specimen at Dorpat is much abraded, but it still bears traces of having been ornamented externally in the manner characteristic of *Psammosteus paradoxus*; and there is a second specimen in the same collection having the ornament well-preserved, with a smooth margin which was evidently overlapped either by skin or by another plate. The inner surface is only marked by indications of blood-vessels; and the external surface is not traversed by any grooves for sensory canals, unless the network of impressions happens to have served this purpose. The plate is bilaterally symmetrical and a little tumid on each side; and it is quite likely that the original outline of the anterior and posterior ends is not correctly given in our illustration of the actual fossil. Another fragmentary example of the same plate may be seen in the School of Mines, St. Petersburg; and it is a homologous piece of armour which has lately been discovered in Scotland.

Another form of dermal plate at Dorpat, also ornamented only on the convex side, but this ornament being the granulation characteristic of *Psammosteus arenatus*, is more difficult to describe. There are several specimens, but that best preserved measures about thirty centimetres in length, while its transverse arching is so great that the narrow dome it forms is about fifteen centimetres in depth. It looks,

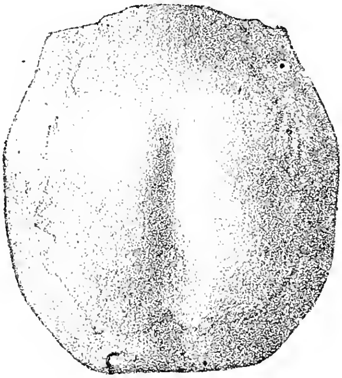

FIG. 1.—Median shield of *Psammosteus paradoxus*, wanting external ornamentation; one-quarter nat. size.—Upper Old Red Sandstone, Neuhausen, Livonia.

indeed, like one end of a keel-less boat with the maximum bendings thickened; and where it tapers towards what would be the middle of the boat, the centre of the floor is excavated by a long re-entering angle. The ornament is abraded on the thickened bends.

The fragments of *Psammosteus arenatus* from Spitzbergen, now in the State Museum, Stockholm, are less satisfactory than those from Russia; but here, too, it is clear that the fish was provided with a bilaterally symmetrical armature, while some of the plates were overlapped (8).

The spines of *Psammosteus* are of two kinds, and these, so far as known, are always ornamented on the plan either of *P. arenatus* or *P. mæandrinus*, never of *P. paradoxus*. The bilaterally symmetrical spines are the smallest, and shaped much like the rostrum of *Pteraspis*.[1] They

[1] One of these spines is figured in Pander's "Placodermen des devonischen Systems" (1857), pl. vii., fig. 16.

always exhibit the "*arenatus*" ornament, and are often much abraded at the free extremity. The paired spines are triangular, resembling those of the Carboniferous *Oracanthus*, but with a less extensive internal cavity, and these are also often abraded at the tip. They may have been arranged along the lower margin of the body as in certain Acanthodian sharks (*e.g.*, *Climatius*); or the animal may have had only a single pair of these spines at the back of the head, as described by Dr. Traquair in *Oracanthus* (5).

Nothing is known of the teeth of the old armoured sharks, but there is reason to believe (5) that some of them at least formed a crushing pavement such as occurs in Cochliodonts (10). Modern researches, however, are tending to show that the nature of the dentition among early Elasmobranchs does not differ so much in different genera and families as has sometimes been supposed. The tooth named *Diplodus*, for example, does not differ much from that of the existing *Chlamydoselache*, although these two fishes belong even to distinct orders. Moreover, it is now proved that the tooth named *Cladodus* is common to more than one kind of Palæozoic shark. Teeth of this character are merely the result of the fusion of the primitively separate conical cusps into little clusters of varying form and extent, each cluster fixed on a single base. The tendency to fusion seems to have been the same in all the old sharks, and it is thus now time to admit that we can no longer determine Palæozoic "genera" and "species," or even "families," on the evidence of detached teeth. The earliest and simplest known form of shark's tooth is *Protodus jexi* from the Lower Devonian of Canada, this exhibiting merely a single cusp on a comparatively large base. The Lower Carboniferous teeth named *Dicentrodus* are only sometimes equally simple; and there are, of course, several later sharks in which some of the teeth at least are unicuspid, while the base remains much expanded.

In short, it may now be definitely stated that the primæval sharks differed from their modern representatives in the marked tendency to fusion of their hard skin-structures. The shagreen granules merged into great plates of armour. The pointed teeth fused at their bases into small clusters of cusps, such as we know under the names of *Diplodus* and *Cladodus*. The obtuse teeth amalgamated into crushing plates, like those described as *Cochliodus*, *Psephodus*, etc. The dermal fin-rays grouped themselves into cut-water spines, as among the Acanthodians.

Of greatest interest, however, is the internal skeleton of these old sharks. We know very little about it at present, but progress is gradually being made; and our knowledge of the fins, at any rate, has been much advanced within the past two years. Until the description of the pectoral fin of *Cladoselache* ("*Cladodus*") *fyleri* in Natural Science for March, 1892 (9), the most primitive type of paired fin known was Gegenbaur's "archipterygium"—a paddle with a jointed central axis of cartilage, fringed on either side with little cartilages—a kind of fin observed both in the living mud-fish, *Ceratodus*, and in the Palæozoic shark, *Pleuracanthus*. The description of the fin in question from the Lower Carboniferous of Ohio revealed a still more primitive type; that is, assuming (as most speculators do) that fishes originally possessed on each side of the body a continuous fold of skin, strengthened by parallel cartilaginous rods extending outwards from the body-wall, this fold becoming sub-divided into the pairs of pectoral and pelvic (ventral) fins as we commonly know them. The researches of Dr. Bashford Dean (2, 3) now show quite clearly what the paired fins of *Cladoselache* really are. They are mere "balancers" with a more extended base-line than is customary. They exhibit the series of parallel cartilaginous rods which once supported the lateral fin-fold, practically unmodified in the pelvic fins, simply

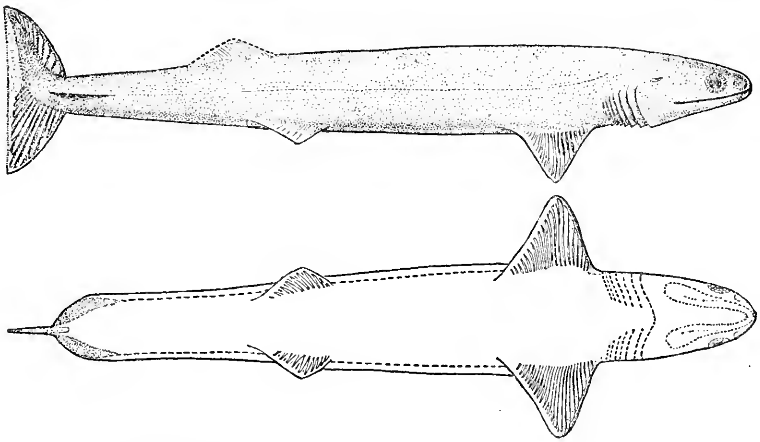

FIG. 2.—*Cladoselache newberryi*; lateral and inferior aspect, restored by Dr. B. Dean. Lower Carboniferous, Ohio.

clustered and partly fused within the body-wall in the pectoral fins. Dr. Dean, followed by Professor Cope (1), thinks that there is a tendency in the pectoral fin for the hinder end of the row of basals in the body-wall to rotate outwards—a process which would reduce the point of attachment of the fin to what was originally its front angle. The outwardly-turned row of basals would in this case correspond with the median axis of the well-known paddle in *Ceratodus* and *Pleuracanthus*; and neither Dr. Dean nor Professor Cope sees any difficulty in the development, quite secondarily, of a fringe of cartilaginous rays on the hinder border of this axis. As a result, these American authors conclude that the modern tribasal or dibasal shark's fin cannot have evolved from the paddle-like "archipterygium," but that these two kinds of fin must have arisen independently from the "ptychopterygium" (as Professor Cope aptly terms the arrangement in *Cladoselache*).

To recapitulate and discuss all the evidence for this theory would lead here into too many technicalities, and we must be content to regard it at present merely as a convenient guide for further investigation. The present writer is by no means convinced that it better explains the known facts than the theory to which he expressed adherence in 1892 (9). It is definitely known that among fringe-finned fishes the originally long lobate fin gradually become sshortened; and the abbreviate lobate pectoral fin of the modern representative of the order (*Polypterus*) differs in no essential respects from the corresponding fin of a typical modern shark. The facts of Palæontology, as at present understood, still seem rather to favour the idea that the same kind of evolution has taken place among Elasmobranchs.

In conclusion, one word of protest against the American idea that the paired fins of *Cladoselache* (Fig. 2) can be compared with those of

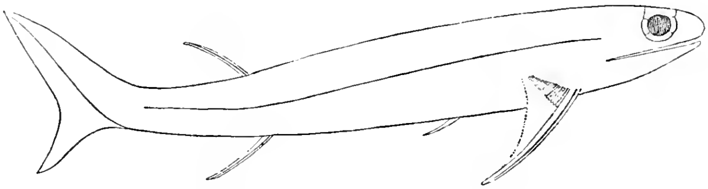

FIG. 3.—*Acanthodes bronni*; outline restoration by Dr. A. Fritsch (4). Lower Permian, Germany.

an Acanthodian (Fig. 3). We venture to maintain that these fins are fundamentally different in every respect. In *Cladoselache* the cartilages of the internal skeleton are well developed and support the whole fin-membrane; in Acanthodians, whatever view we may adopt as to the naming of the parts, these cartilages are as much reduced as in a modern herring. Dr. Dean (3) speaks of the "radials" of *Cladoselache* as if, by fusion, they might readily become a fin-spine like that of the Acanthodian *Parexus;* but the former are cartilage and endoskeletal, the latter is merely the ordinary dentine and therefore presumably exoskeletal. The problem of the primæval sharks continues to present endless difficulties, but these are only multiplied by such comparisons. In the present writer's opinion, the pectoral of *Cladoselache* is more remotely connected with that of the Acanthodians than is that of a modern Siluroid with the pectoral of the Devonian *Holoptychius*. Everything still tends to show that the very highest Elasmobranchs lived simultaneously with almost the lowest in late Palæozoic times; while sharks and skates nowadays are a comparatively degenerate race.

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A. SMITH WOODWARD.

V.

Musical Boxes in Spiders.

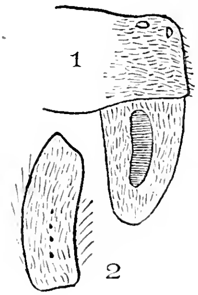
APART from such unavoidable sounds as the buzzing which results from the rapid vibration of wings, or the pattering made by the striking of feet against hard surfaces, nearly all the animals belonging to the group which zoologists call the Arthropoda are mute.

But in each of the great divisions of this class, namely in the Crustacea (Crabs and Shrimps), the Arachnida (Scorpions and Spiders), the Myriopoda (Centipedes and Millipedes), and the Insecta (Flies and Beetles), there are some species which are provided with organs specially set apart for producing sounds. These sounds are produced in various ways; but in most cases they result from the rubbing together of two mutually roughened surfaces. In the common house-cricket, for instance, the under surface of the wings of the front pair is furnished with a fine saw, and the familiar chirrup of this insect is caused by the rubbing of one wing over the other in such a manner that the saw of the one that is uppermost scrapes over the ridge-like nervures of the other lying beneath.

Organs constructed on the same principle exist in some Millipedes, some Crabs, several Beetles, and a few Spiders. Their presence in spiders was first announced in 1843 by Westring, who found them in the males of some small European species, which were then referred to *Theridium*. The instrument consists of a finely-toothed area upon the forepart of the abdominal or hinder region of the body; and the sound is produced by the scraping of the teeth against a set of grooves upon the hinder end of the carapace or dorsal shield, which covers the forepart of the body. About forty years later Mr. Campbell published the description of an analogous organ in some species of an allied genus, *Lephthyphantes*. But in the latter the stridulating organ is connected with the mouth-parts, and consists of a series of ridges, accompanied by a knob, upon the outer surface of the mandible, and of a corresponding series upon the adjacent surface of the third segment of the palp. Moreover, in all other spiders in which similar instruments have been found, they are placed in substantially the same position. Consequently, a few words to explain the structure of the jaws and their relation to one another may be added, in order that the working of the organs now to be described may be made intelligible to all readers.

Beneath the forepart of a spider's head there is, on each side, a stout jaw, which ends in a long, moveable, claw-like fang. Immediately behind this jaw, or mandible as it is called, there is a short limb, formed almost exactly like one of the walking legs. This limb, known as the palp, is never used for locomotion, and is directed straight forwards in such a manner that the inner surfaces of its basal segments are closely applied to the outer surface of the mandible. In most cases these surfaces are smooth, so that they slide over each other with but little friction, and practically no noise; but, as has been already described, in some species of *Lephthyphantes* the opposed surfaces are finely ridged. Consequently, friction and sound must ensue when the surfaces are rubbed together. But as a matter of fact, owing to the small size of the spider and the fineness of the ridges, the sound produced is, according to Mr. Campbell, too slight for human ears, even when supplied with a microphone.

In another spider, however, named *Thomisoides*, which is found only in the southern hemisphere, and is about equal in size to our largest British species, the stridulation is, according to M. Simon, distinctly audible. The instrument in this case resembles in structure and position that of *Lephthyphantes*, except that the scraper consists of a series of sharp tubercles on the inner surface of the third segment of the palp (Fig. 2). These tubercles, by being scraped up and down over the ridged area on the outer surface of the mandible (Fig. 1), give rise to a sound which is said to resemble the buzzing of a bee. Lastly, it must be added that, whereas in *Lephthyphantes* it is the male that is supplied with the more perfect instrument, in *Thomisoides* the two sexes possess it equally well developed.

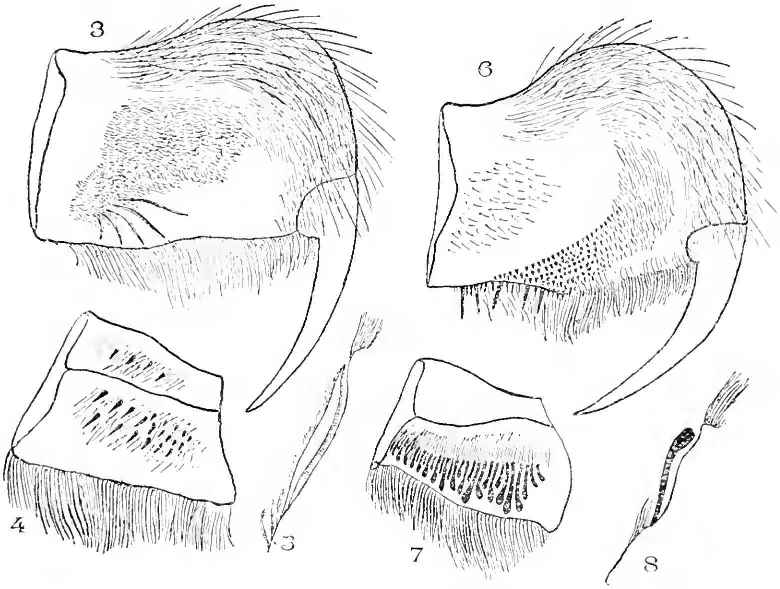


Figg. 1 and 2.—Mandible and palp of *Thomisoides*.

In the last group that has to be considered the organ is also present in the males and females; but it is more complicated, and differs greatly in structure from those that have been just described, since it consists exclusively of modified hairs. The organ was first noticed by the late Professor Wood-Mason, who discovered it in a huge Indian spider, to which he gave the name *Mygale stridulans*. Most of our readers are probably aware that the Mygalidæ, or Aviculariidæ as they are more properly named, are the largest spiders known, being in fact those that are often called the "bird-eating spiders." Still the sounding organs are not found in, by any means, all the members of this group. Indeed, since 1876, when Wood-Mason's paper was first published, no author has until now taken the pains to re-investigate the question, or to discover in which species the organ is present and in which absent. But a recent examination of the spiders of this family contained in the British Museum has shown that all the large kinds that are found in the countries lying between

India and Queensland are furnished either with Wood-Mason's type of musical instrument or with another type which has never yet been described.

In both cases the instrument consists of a series of strings or keys, and of strikers, which set them vibrating. In Wood-Mason's organ, the strings, which are placed upon the inner surface of the basal segment of the palp, are composed of strong, horny, club-like rods, which vary in length, thickness, and shape (Figg. 7 & 8). Most of them are slightly curved, and all of them lie in a general direction, parallel to the surface of the segment that bears them. This surface is curved in a way that suggests the sounding-board of a piano or violin; and overhanging the bases of the strings there is a thick fringe of hairs, which may possibly perform the office of a



FIGG. 3, 4, and 5.—Mandible, palp, and vibrating hair in *Phormingochilus*.
FIGG. 6, 7, and 8.—The same in *Musagetes*.

"mute" or "damper." At all events, since the strings vary in length and form, it seems clear that they must give rise to different notes when thrown into a state of vibration; but whether, like the rats of Hamelin Town, the spiders are capable of "squeaking in fifty different sharps and flats," we are at present not in a position to say. The strikers, or scrapers, placed on the outer surface of the mandible, consist of longer and shorter spines, as in the S. Indian genus *Pœcilotheria* and the Burmese *Musagetes* (Fig. 6), or of long, stout, spiniform hairs, as in the Javan *Selenocosmia* and the Austro-Malayan *Phlogius*.

In the other kind of musical instrument which some of these oriental spiders possess, the strings are situated on the outer surface of the mandible, and the scraper, which consists of several short spines, is set upon the inner surface of the basal segment of the palp (Fig. 4). The organ is thus structurally the exact opposite of Wood-Mason's, and the differences between them point to divergent evolution of the two groups of spiders. The strings in the kind we are now describing are few in number, generally five or less, and each consists of a long, tough, blade-like, feathery hair, which is nothing but an enlarged and slightly modified representative of the hairs composing the carpet which, in these spiders, clothes the middle of the outer surface of the mandible. Fig. 3 shows the outer surface of the mandible of one of the spiders of this group, named *Phormingochilus*, from Borneo, with the carpeting of short hairs and the strings below it; and Fig. 5 is a side view of one of the strings.

It may be asked, however, what direct evidence there is that the function of this organ is to make sound, and we are bound to admit that there is none, except such as the argument from analogy is capable of supplying. But it must be confessed that the assumption is justified by the well-ascertained fact that in the allied group possessing Wood-Mason's organ, which is made on the same principle, at least one species has been heard to stridulate. In fact, it was hearing the sound made by the animal that induced Wood-Mason to look for its cause and led to the discovery of the organ above described. And since the spider when stridulating was observed to rub its mandibles and palpi together, the fact that the sound was produced by the friction is practically beyond dispute.

But although we may thus claim clearly to understand the mechanism by which the sound is made, we are by no means so sure of its object. Since the organ is equally well developed in both males and females, there is no reason for supposing, with Wood-Mason, that the sound is used as a sexual call, as in the case of the small spiders (*Lephthyphantes* and *Theridium*) mentioned above, where the organ is found perfected only in the male. Moreover, although the last-named spiders belong to a group of which the members respond readily to such sounds as those produced by a tuning-fork, all experiments that have been made upon cursorial species, like *Thomisoides* and the large bird-eaters, have failed to reveal the existence of any sense of hearing. Consequently, in spite of Dr. McCook's argument, which is clearly fallacious, that "of course the existence of stridulating organs, if they be sound-producing organs, naturally infers the presence of auditory organs," it may be safely stated that there are no grounds for supposing that these spiders can hear the sounds that they themselves produce.

What is actually known about the question is briefly this. A Mr. Peal, who was the first to notice the sound, had his attention drawn to the spider by hearing it stridulate loudly, when threatened with death from the luckily ill-directed blows of a gardener's hoe. Moreover, the rescued animal subsequently repeated the performance when worried by a cat, standing up the while and brandishing its fore-limbs, as shown in Fig. 9. From this it seems clear that the sound is emitted under the stimulus of danger. And if it be asked what benefit the spider can derive from the stridulation, it must be remembered that animals that are rendered noxious by the possession of stings and poison-glands, or of juices distasteful to others that would prey upon them, are very commonly made conspicuous by some readily-perceived character. Some, for instance, like bees and wasps and many centipedes and scorpions, are brightly coloured with black and yellow bands, which immediately strike the eye. Others, on the contrary, like the huge black scorpions of India and

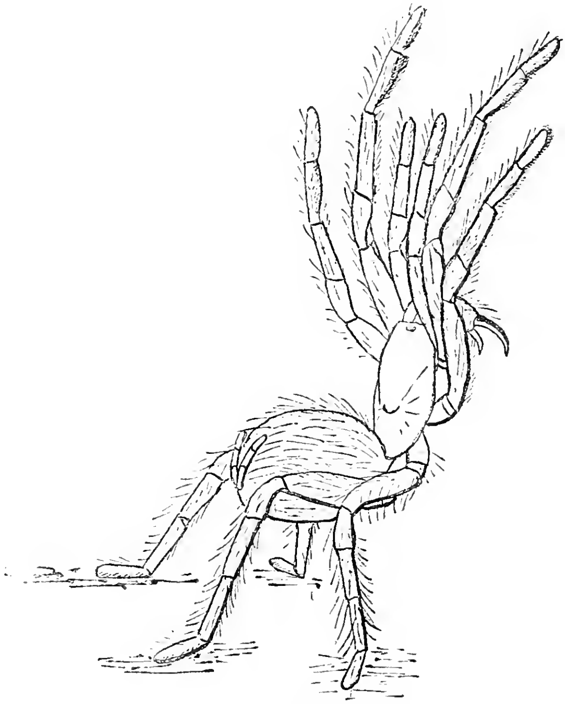


FIG. 9.—*Phormingochilus* stridulating.

rattlesnakes, possess an apparatus specially designed to make a rattling sound, which is believed to be a danger-signal, warning other animals to let them alone. And this, as Wood-Mason has suggested, is probably the function of the stridulating organs in these large poisonous spiders.

But Dr. McCook, when criticising, somewhat sceptically it must be admitted, Wood-Mason's conclusion, appears to have fallen into a very popular misapprehension of the whole question. For he gravely asserts that he can scarcely give credence to the theory that the stridulating organs [in these spiders], like the rattles of a rattlesnake's tail, are intended to give warning to *victims* of their dangerous and deadly nature. With this belief we most cordially agree; but, in justice to the memory of the late Professor Wood-Mason, we must add that he never accredited his spider with such unnatural and altruistic intentions. As a matter of fact, he said just what we have stated above, namely, that the spider stridulates to warn *animals that would prey upon it* of its deadly nature, in order that they may leave it in peace, from considerations of their own safety. It is true that both parties to the question are benefited by the spider's rattling; but it is also true that the development of an organ like the stridulator above described could only be brought about by Natural Selection through the gain that the spider alone experiences from possessing it.

But if the object of this stridulating organ be what has been suggested, it may well be asked, What can be the function of the analogous organ that is found in *Thomisoides*? For, seeing that in this spider the instrument is equally well developed in both male and female, and that there is no evidence that either sex possesses an auditory sense, the belief that the sound is used during courtship as a sexual call is without foundation. Moreover, as compared with the Aviculariidæ, *Thomisoides* is a small and feeble spider, which, so far as we can see, could in no sense profit, as the large spiders do, from the possession of a stridulator unless there were something terrifying or repellent in the sound pure and simple. Otherwise, the device would be speedily seen through as a transparent fraud, and the stridulation would prove detrimental to the spider by drawing the attention of enemies to its whereabouts. But M. Simon has told us that when discovered this spider, instead of attempting to escape by speed of foot, or by lying *perdu*, or by showing fight, starts stridulating, and that the resulting sound resembles the buzzing of a bee. This author, indeed, makes no suggestion respecting the object of this resemblance; but we venture to think that he has accidentally furnished us with the key to the whole question. For just as many harmless flies profitably mimic poisonous bees and wasps in appearance, and possibly also in the nature of their buzzing, so, too, does it seem permissible to believe, in the absence of any other explanation of the fact, that *Thomisoides* is protected from enemies by its power to imitate the sounds produced by poisonous insects.

To recapitulate—stridulating organs in spiders seem to exist for two distinct and definite purposes.

In the case of *Lephthyphantes* and of the species referred originally to *Theridium*, the organs are either better developed in the males than in the females, or are exclusively confined to members of the former sex. From this fact, coupled with the circumstance that the spiders of these two kinds are, judging by analogy, possessed of auditory organs, we may infer, although as yet without direct proof, that the male makes use of his sounding organs during the period of courtship. This, as is well known, is a time which to the male of very many species is beset with exceptional difficulties and dangers, owing to the irascible and voracious disposition of the more powerful female, who will often rather make a meal of her suitor than a mate. Consequently, to soothe her temper and reduce her to a proper frame of mind, he is often driven to practice various charms and ingenious wiles. The males, for instance, of some species of the Attidæ or jumping spiders execute fantastic dances and antics, as described by Mr. and Mrs. Peckham, and, like peacocks, make a display the while of whatever plumes or bright colours they possess. So that, since it seems to be established that the females of the Attidæ have in all probability an æsthetic sense which is gratified by the sight of gaudy ornaments on members of the opposite sex, there appears to be no great extravagance in the supposition that the females of the species of *Theridium* and *Lephthyphantes*, like the females of our song-birds, not to mention nearer allies, may be charmed by the musical efforts of their lovers. But whether this be the object of the stridulation, or whether it be merely used as a sign of the specific identity of the approaching male, the evidence indicates that it only serves its possessor in his relations with the females of the same or of closely allied species.

In the second case, namely, that of *Thomisoides* and the Aviculariidæ, in which the stridulating organ is brought to the same state of perfection in the females as in the males, the spiders themselves being by analogy unable to hear, the available evidence shows that the sound has nothing directly to do with the relations of one sex to the other, but that it functions solely as a danger signal, warning enemies to keep their distance. While, however, the signal is genuine enough in the large and powerful Aviculariidæ, in *Thomisoides* it is altogether fraudulent, since this spider, which is relatively a feeble animal, by aping the humming of a bee, succeeds in deluding its hearers into the belief that it possesses certain dangerous qualities to which in reality it has no claim.

Lastly, since all the activities of a spider, as of every living organism, are directed towards one of three ends, that is, either towards procuring food or avoiding enemies or reproducing its kind, it seems that the stridulating organs, so far as is now known, are adapted to the furtherance only of the second and third of these three vital principles. Up to the present no spider is believed to use its stridulation as a means of procuring food. Perhaps, however, in the future some species will be found in which the sound acts as a lure to entice victims within reach. If so, we shall then be able to say with some confidence that we have discovered in spiders all the purposes for which sounding organs could be evolved.

R. I. POCOCK.

SOME NEW BOOKS.

"THE BRITISH CUVIER."

THE LIFE OF RICHARD OWEN. By his grandson the Rev. Richard Owen. With the scientific portions revised by C. Davies Sherborn. Also an essay on "Owen's Position in Anatomical Science." By the Right Hon. T. H. Huxley, F.R.S. Portraits and illustrations. 8vo. Pp. 409 and 392. Two vols. London: John Murray, 1894. Price 24s.

THE Rev. Richard Owen is greatly to be congratulated upon these excellent volumes, which deal fully with the life of his illustrious grandfather. His task was rendered both easier and more difficult by the fact that Owen had a habit of keeping letters. The amount of material therefore permitted of an almost exhaustive analysis of the life of the great anatomist, but an *embarras de richesses* must have presented hindrances. Mrs. Owen's careful diary also facilitated the work of her grandson; this diary is indeed the principal feature of the book; for everything, serious and frivolous, is jotted down. We find there the jokes of Sidney Smith cheek by jowl with accounts of lectures at the Royal Institution. It carries one back far into the past to read about conversations with Abernethy, Dean Buckland, Cuvier and Oken. Nearly all the heroes of science of this century figure in the interesting pages before us. We begin with Sir Astley Cooper and Sir Everard Home, who signed Owen's certificate from the College of Surgeons, and come down to eminent living persons such as Sir Henry Acland. Owen's acquaintances and friends were many and different; he by no means restricted himself to those who followed the same paths; Dickens was one of his chief friends, and got him to contribute to "All the Year Round" a series of papers upon Zoological Gardens, aptly entitled, "The Private Lives of Public Characters." Sidney Smith cut jokes at him, speaking of the bones of the *Dinornis* as Owen's *magnum bonum;* while Carlyle wrote him characteristic epistles. We have dealt so fully with Owen's position in science in previous numbers of this Journal that it is unnecessary to recapitulate here the admirable accounts given by Dr. St. George Mivart and Mr. Smith Woodward which are duly quoted from in the two volumes before us. We shall endeavour rather to present the reader with samples of the lighter matters in these volumes. Much pleasant gossip has found its way into the Rev. Richard Owen's biography; and we confess to being interested in anecdotes of great men, even those that are not germane to their life work. The following is a characteristic specimen of Mrs. Owen's diary :—

"March 14.—R. at the day meeting of the Royal Society. Enlivened the evening when he returned by reading Chadwick's 'Report on Burials.'

"April 4.—Miss Edgeworth came to take leave before going back to Ireland. R. was making ready to go in to lecture when she came in, but had time to stop and have a talk with her. She admired greatly the professorial gown with its red silk.

"10th.—R. drew the outline of diagram which I am to colour for to-morrow's lecture. Afterwards he dissected a Chimpanzee. Will watched his father dissecting till he himself smelt like a specimen preserved in rum."

Among the Professor's varied experience was a curious request from a business firm which was worded as follows:—"We have been for a few days embalming the remains of the late William Beckford, Esq., of Fonthill Abbey. Will you oblige us by giving us your opinion what we ought to charge? We are entirely at a loss to know the value with a family of such wealth of our process. It has never been done in the West of England." They were advised by Sir Richard to ask 100 guineas. A great deal of the Professor's time was spent at the Zoo, and a highly interesting entry in Mrs. Owen's diary refers to the first arrival of giraffes at that institution; it runs as follows:—

"R. and I started at four, and after waiting about near the garden till about five, saw the most lovely procession imaginable. The four graceful bounding playful giraffes attended by M. Thiebaut and four Africans in native costume. Two policemen were there to clear the road, but in the neighbourhood of the Gardens there was nothing to clear except an early cart or two. The procession had walked from Blackwall—eight miles—and passed through Gloucester Gate to the Gardens. When the giraffes got on to that part of the road in which the trees were on both sides, they could scarcely be held in by the attendants. One animal got so excited that M. Thiebaut called out 'Laissez aller,' &c., and they allowed the pretty creature to bite off some of the young shoots of the tree. . . . The giraffes had to have a light at night, as they would not rest quietly without it." At this time all the animals which died in the society's menagerie were dissected by Owen; some of them he used to take home; and the material was supplemented by occasional windfalls from travelling menageries. For instance, he received one day a present of a rhinoceros from Wombwells, which was accommodated in the back premises of his house in Lincoln's Inn Fields. Nothing came amiss to his diligent scalpel. And yet with all this vast amount of work he had plenty of time to attend meetings of all kinds and to read a prodigious quantity of novels, among which those of Charles Dickens were his prime favourites. These novels, as well as his numerous papers, kept him up till late hours; but nothing seems to have hurt him, not even comparatively unlimited theatre-going, another pleasure to which, we read, he was greatly addicted. The only thing, indeed, which appears to have frequently upset him was lecturing, of which he did a good deal. He used to lecture without notes, but was highly nervous, at any rate at first.

Professor Huxley, in his appreciative sketch of Owen's position in science which closes the second volume, places him as scarcely second to Cuvier. Like Cuvier his researches embraced the extinct as well as living animals. His restoration of the *Dinornis* from a fragment seized hold of the popular imagination, and as a consequence he was inundated with inquiries as to the true nature of such extinct beasts as the cockatrice, the phœnix, and the bunyip; the latter is a beast of which we had not heard before; it appears to be, not only a mythical, but also an Australian, monster founded upon an embryo sheep. As to the phœnix, Owen once had an amusing interview with an oriental gentleman from the Turkish embassy; this personage brought with him a ladle whose bowl was stated to be a piece of the phœnix, in fact the bill. After some search Owen was able to demonstrate to the unspeakable one that the bowl was in reality composed of the beak of a horn-bill. "The head and beak were brought into my study, and handed to the Oriental. He examined it very deftly, comparing the beak with the bowl, and then exclaimed, with astonishment and reverence, 'God is great! This surely is the bird!'" The surprised Turk proved to be "Mahommed Abu Said, Chief Spoon and Ladle Maker to the Commander of the Faithful." The subject of this memoir was largely enabled to do the vast amount of work which he accomplished by his great powers of concentration. This, however, was by no means confined to scientific matters, as the following anecdote shows. One day, "after having heard a lecture of Whewell's, he went on to the club, and took up Thackeray's 'Vanity Fair' to read. He became so deeply absorbed in the book that he sat on, oblivious of the fact that everyone else had disappeared one by one. He was also apparently deaf to coughs and hints of attendants, etc. At last, in desperation, the men came forward and began to take away the lamps. Then, having looked at his watch and found it considerably past 2 a.m., he rushed wildly out of the club, and, like a scientific Cinderella, left his umbrella and great coat behind." It will be inferred from the short notice which we give here that the Rev. Richard Owen's biography of his grandfather teems with interesting matter. It should have a wide circle of readers.

Organised Imbecility.

Apparitions and Thought-Transference: an Examination of the Evidence for Telepathy. By Frank Podmore, M.A. With numerous illustrations. London: Walter Scott, 1894. Price 3s. 6d.

This little volume, which is advertised with the sub-title, "The Communication of Sensations, Ideas, and Emotions otherwise than by the Known Senses," treats modern "spookery" with all the gravity befitting a volume in the "Contemporary Science Series."

Students of the longer established sciences are supposed, for the most part, to set aside with undue levity the propositions and arguments of the exponents of this *fin-de-siècle* pursuit. It is pleasant, therefore, to find ourselves in substantial agreement with an important statement made by Mr. Podmore in his introductory chapter. "The evidence, of which samples are presented in the following pages, is as yet hardly adequate for the establishment of telepathy as a fact in Nature, and leaves much to be desired for the elucidation of the laws under which it operates."

We differ, however, from the author as to the advisability of his suggestion that "any contributions to the problem, in the shape either of accounts or experiments, or of recent records of telepathic visions and similar experiences" should be sent to the Society for Psychical Research, for notwithstanding the great ability of several of the leading members of that society, we take it as proved that the society has shown itself unable to understand the nature of scientific evidence and the conditions of scientific experiment.

The evidence here presented rests upon the authority of persons falling into three classes. The first set conceal themselves under initials. Here, for instance, in this book are "Miss A.," "Mrs. B.," "Mrs. C." and "Mrs. G.," "Miss X.," "Miss Y.," and "Mrs. Z.," "Mr. J.," "Mr. J. T." "Dr. N.," and "Dr. F.," who have played guessing games and got "results." Next there are named nonentities who have done likewise. Lastly there are awful warnings who also have done likewise.

It is plain that the witness of the two first classes of people is worthless. We assume readily that the society is generally successful in excluding conscious fraud from its records. But consider the case of these people. If they see no visions, if they assist at unsuccessful guessing, they remain unknown and subject maybe to the intolerable laughter of those acquainted with their aspirations. But, if they succeed, they invest themselves with a mysterious superiority. They are no longer plain "Miss A." and "Mrs. B.," but Miss A. and Mrs. B., "Percipients." It is small wonder, then, that their minds should be strongly biased in favour of success. No wonder that, as instanced on page 48 of this book, "a cry of joy should unfortunately escape them" after a guesser has made a successful venture. Most of our readers have met some example of this class, either in the early stage of "thought-reading" or blossomed into the lamentable fruit of spiritualism, and no doubt they share our friendly contempt for the victims of a pardonable ambition to achieve the mysterious and the unknown, and our indignant reprobation of the people who encourage and exploit them.

The third class of persons who vouch for certain among the events recorded in this book are individuals of more or less scientific repute, whom we have ventured to denote as "awful warnings." We have known personally, or followed in public print, the careers of well nigh "four-and-twenty leaders of revolt," doctors of medicine, professors of chemistry, professors of physics, and professors of biology, who have begun harmlessly by taking a hand in the card-games of thought-readers. But the same bough is limed for them all: with the same fluttering protests that it happened in daylight and in darkness, that the medium had electric bells on her toes and her hands in their hands, they are caught by the spiritualists. They retire to secluded chambers or to lonely isles with a medium and a circle of enthusiasts, and they have their hair tweaked (and their leg pulled, although on this they are silent) by ghostly "Jacks" and "Marys" and "Indian boys."

We do not intend to offer arguments against spiritualism: we rejoice to blazon the fact that we have a prejudice so deeply rooted as to be ineradicable against the manifestations that appear in the vicinity of spiritualists; and we are content to base on this prejudice the dogmatic statements that there is always gross fraud connected with the banjo-playing, hair-pulling phenomena, and that the opinion upon occult phenomena is absolutely worthless of anyone who allows himself to write or say anything about such manifestations, without the clearest statement that he regards them as fraudulent, though the fraud may be beyond his detection.

Let the Society for Psychical Research have an end of their folly. The strange dilemmas and paradoxes of our senses, the bewildering problems of personality, the examination of the channels by which impressions reach us, and even the existence of incorporeal psychical entities, are matters for the experts of our laboratories of physiological psychology, and neither for straying professors of other subjects, untrained nonentities, nor above all for hired mediums. If they will insist upon continuing to collect and publish marvels for the open-mouthed, let them, in the wake of the astute Mr. Stead, publish such in shilling Christmas numbers

Two Books on Psychology.

Lectures on Human and Animal Psychology. By Wilhelm Wundt. Translated from the Second German Edition by J. E. Creighton and E. B. Titchener. Pp. x. and 454. Illustrated. London: Swan Sonnenschein & Co., 1894. Price 15s.

Introduction to Comparative Psychology. By C. Lloyd Morgan. With diagrams. Pp. vii. and 382. London: Walter Scott, 1894. Price 6s.

Even in our own country, so agape for mesmeric marvels, so inapt at intellectual novelties, there is evidence that psychology on its scientific or physiological side is receiving a considerable amount of attention. Of the two books before us the second or smaller is a purely English production. The translation of Wundt's lectures is an English issue of an American book.

As Sachs is to Botany, so is Wundt to Psychology. As Wundt says in the preface to the second edition of his lectures, "Thirty years ago the science was no more than a programme for the future." Fechner had begun to break the ground in psychophysics, but otherwise everything remained to be done. Psychology, so-called, was in the hands of dogmatists and metaphysicians. The application of the exact methods of an extended physiology to the senses and the mind met with distrust on all hands. Memories of the degradation of what was regarded as psychology in these days still linger in this country in association with the phrase "Psychological novel." Consider what is implied by calling "Une Cruelle Enigme" or "The Yellow Aster" psychological novels, as our esteemed literary contemporaries delight to do, and you shall know what psychology was when, thirty years ago, the first edition of these lectures was published. The means by which Wundt and his pupils have made psychology an exact science are simple; they are experiment and observation. In place of subjective analysis in these lectures we have the record of the subjection of vision and touch and hearing, memory and attention to a rigorous experimental method.

A comparison of the method of Wundt with that of Dr. Bain, the most physiological of the older school of psychologists, will make the change of attitude more apparent. Bain introduced his account of the great law of relativity by general considerations, like our knowledge of heat as an appreciation of a transition from cold, by a comparison of the contrast between health and sickness, leisure and toil, and so forth. Wundt starts from actual experiments on sensation-differences and associates definitely with the law of relativity the idea of quantitative apprehension of contrast. Bain describes anatomical structures, introduces physiological conceptions and correlates them with sensations, but does not embark upon the measuring of sensations at all. Wundt, at the outset, begins with the idea of units of sensation and of sensation-stimuli, to be fixed by definite experiment, and to be used as the bases of all future experiment. Thus, in the case of sound-stimuli, it can be shown that a normally sensitive ear can just hear the sound made by a pellet of cork weighing one milligramme falling through a height of one millimetre upon a sheet of glass, at a distance of ninety-one millimetres from the ear. "Given a sound, the intensity of which it is desired to measure, it is only necessary to remove it to the distance at which it just disappears." As a sensation-stimulus it then precisely equals the unit, it produces the just-noticeable sensation, and the distance tells us how much greater is the given sound at the place of its production than is the just-noticeable sound intensity.

It is this experimental method, familiar of course to every modern psychologist, that marks the difference between the old school and the new. It is necessary to do no more than to commend most heartily to all readers these lectures of Wundt. They were designed originally as an introduction to psychology, and they are so written and so translated as to be intelligible and interesting to those who are unfamiliar with the new science. We congratulate the translators on their performance of a difficult task; but we cannot forgive their slovenly omission of an index.

Professor Lloyd Morgan has written a very interesting book on a totally different side of psychology. He sets out from the philosophy of experience, which he claims as an aspect of Monism. Instead of cumbering himself with the dualistic hypothesis that subject and object are separate and independent, that the conscious being is placed in the midst of a separate and objective world, he simply assumes what we all know—things happen, and we are conscious of them. He makes the further assumption that Nature is wider than experience, that things happen when we are not conscious of them. Further, he assumes the Monistic view that the organism is the product of evolution; "that mind is not extra-natural nor supra-natural, but one of the aspects of natural existence."

On this basis, clearly stated in his 'Prolegomena,' he proceeds with the study of the mental conditions of animals with the clear view of tracing an evolution of mind parallel with the general evolution of organisms. A large part of his data are the result of his own careful investigations and experiments, some already published in our own columns or elsewhere, some appearing now for the first time.

Comparing man with animals, Professor Morgan believes that the emotional states resulting from sense experience are of the same nature in both. He distinguishes carefully between emotional states and "emotional tones" resulting from the perception of relations. In the case of pleasure, for instance, the mere emotion is a sensuous phenomenon resulting directly and sub-consciously in some form of action. Supposing that the common interpretation of sexual selection be true, the female does not consciously judge between two males and choose that which she decides more gaudy. The one produces a stronger sensuous state in her than the other, and she moves towards it, drawn by sub-conscious chains, without any deliberate choice.

The main difference he sees between the psychology of animals and of man resides in the perception of relations. An animal acts directly, using perhaps intelligent association of means and end, but not sitting in judgment on his own action nor acting consciously with intelligence. "A man perceives the particular relations among phenomena, and builds the generalised results of these perceptions into the fabric of his conceptual thought." A man in fact is intelligent, and conscious that he is intelligent.

FOR THE AMATEUR OF FERNS.

A MANUAL OF EXOTIC FERNS AND SELAGINELLA. By E. Sandford. 8vo. Pp. 286. London: Elliot Stock. Price 3s. 6d.

THIS little book should prove of considerable use to the grower of ferns; it contains not only a description of one thousand species, but various hints of a practical kind, which will put the beginner in the right way. The owner of a greenhouse who may be deterred by the alarming prices which have to be paid for the aristocratic orchid, may find some satisfaction in the cultivation of the much cheaper, if humbler, fern. These plants will thrive and flourish in the most unlikely and unpromising situations, such as in bottles, upon pieces of cork and "other things." In this, the only rival is the ubiquitous *Penicillium*, which is, according to Professor Huxley's "Elementary Biology," equally satisfied with jam or old boots as a home. The fern, too, can be readily transported from foreign parts; the thrilling stories of adventure which accompany the gathering of orchids—so graphically told by Mr. Frederick Boyle—seem to find no place in the peaceful history of fern discovery; a pinch of dust from the under side of the frond, and the fern is safely got. Though this dust does not now, as it did long ago, render the possessor invisible, it allows of the gathering together of a large amount of material in a small space. Mr. Sandford particularly dwells upon and recommends for cultivation the tree ferns. These magnificent plants were until lately a rarity in this country; but they will readily grow—at least a good many of them will—in the open air; and there can be no question in the minds of those who have seen them of their suitability as ornamental plants. Tree ferns do not grow quite so rapidly, perhaps, as might be desired; but we are told by the author that *Cyathea medullaris* will grow from spores to a height of ten feet in less than twenty years; another species of the same genus grew five and a half feet in two years, which is a respectable rate of growth, and should be sufficient to content the nomad inhabitant of our suburbs.

Another great advantage which the fern has as a plant for the amateur is its large immunity from insect parasites. Mr. Sandford is indeed able to dismiss this subject in half a page. "White and brown scale" are among the most disastrous foes of fern life; but the omnivorous woodlouse, like the slug, has a fancy for the tender fronds. These animals are so well-known to the gardener that no special directions are needed for their destruction.

The fern is not conspicuous from the economic point of view; litter for cattle and food for piglings seem to be its chief uses; but the author quotes from the traveller Huc a statement that the young fronds taste when cooked like asparagus—a vegetable which plays a part relative to strange and unknown plants much like that which veal plays in relation to the flavour of outlandish animals. Mr. Sandford's descriptions of the one thousand species are in some cases fairly full, and in all cases the derivations of the generic names are given. In describing colour he divides green somewhat curiously into "light" and "dark" and "pleasing." The book can be safely recommended as a useful handbook, the want of illustrations being, perhaps, its most serious drawback.

Plant Life.

Le Monde des Plantes, par P. Constantin, agrégé des sciences naturelles, professeur au lycée de Rennes. 2 vol. gr. in-8 de 750 pages; illustrés de 2,000 figures. Se publie en 48 séries à 50 c. ou en 8 fascicules à 3 fr. Paris: Baillière et Fils, 1894. Fascicule I., pp. 192, figs. 276.

"Le Monde des Plantes" forms the botanical contribution to the series of volumes issued by A. E. Brehm under the title "Merveilles de la nature." It is a somewhat popular description of the plant kingdom, in systematic order, family by family, starting with the dicotyledonous flowering plants with free petals and passing downwards, on the Candollean hypothesis, through those with joined petals (Gamopetalæ) to an apetalous division. This is not the most scientific classification; but, as the author's aim is especially to spread a knowledge of those plants which are useful from a horticultural or economic point of view, it may answer the purpose. The brief introduction to the flowering plants with which the first number opens gives a good idea of the scope of the book. Characteristics which are evident or easily to be understood by the fairly intelligent general reader are alone put forward, though it is stated that these do not comprise the whole story. It is accurate as far as it goes, and may very well serve to excite an interest in plants where a more strictly scientific account would only repel. The statement, however, that the stem of the Rolang palm may reach a length of 300 metres needs, we believe, confirmation.

In the systematic portion the author gives, under each genus, a good account of those species which are of general interest, either as cultivated plants or otherwise. This account includes the characters by which the plant can be distinguished from allied species, its geographical distribution, and its uses. Considerable help is afforded by the illustrations, which are numerous and good and form a notable feature on the whole. It is just the sort of book for people who take an interest in plants and want to know a little about them without going deeply into the scientific aspect of things.

PLANT PHYSIOLOGY.

PRACTICAL PHYSIOLOGY OF PLANTS. By F. Darwin, M.A., F.R.S., and E. H. Acton, M.A. 8vo. Pp. xviii., 321, with illustrations in the text. Cambridge: Published at the University Press, 1894. Price 6s.

MR. SHIPLEY has been fortunate in securing so sorely needed and excellent a practical manual for the biological series which the syndics of the Cambridge Press are issuing under his supervision. In olden times the botanical student gathered plants, learned to examine their floral structure, and to determine their position in a systematic classification. In more modern times he learned to cut sections and study the internal structure of plants which he usually did not gather, but found embalmed in spirit on the laboratory shelves. Within the last few years he has, here and there at well-equipped scientific centres, learnt to investigate the processes underlying the actual life of a plant, and of which life is but the expression.

The little manual now to hand will be welcomed by all exponents of the last-mentioned phase of botanical study and investigation. It is an extension and elaboration of the written instructions prepared by the authors for use in their respective classes, and contains, therefore, such a selection of experimental and analytical work as can be carried out by botanical students under the supervision of a competent demonstrator. The latter will be found a necessity, as the directions for experiments frequently assume a certain knowledge of manipulation and method which the student will in many cases not possess. The unpretending but clear illustrations will afford no little assistance in the arrangement of apparatus.

The book is divided into two parts. Part i., which deals with general physiology and occupies two-thirds of the whole, recalls Mr. Darwin's elaborate lecture demonstrations. It includes directions for an exhaustive series of experiments, 265 in number, on respiration, assimilation, nutrition, transpiration, growth, and movement. Part ii., on the chemistry of metabolism, is a practical study of those substances found in the tissues which are of interest from their connection with the constructive processes involved in the life and growth of a plant. The text throughout is exceedingly clear, and the index full and carefully compiled.

A Model Text-Book.

Outlines of Biology. By P. Chalmers Mitchell. 8vo. Pp. xvi., 297, with illustrations in the text, and an Index. London: Methuen & Co., 1894. Price 6s.

In a book written to introduce students to any branch of learning, there are certain qualities to be demanded. These are—first, accuracy, for nothing should ever have to be unlearned; second, clearness, so that no doubt as to the author's meaning can exist; third, decision without dogmatism, that is to say, there must be no ambiguity over doubtful points, but a definite acceptance of one explanation for working purposes, although the possibility of other explanations may be admitted; fourth, caution, no knowledge should be assumed that we do not actually possess, and statements that are only particular should not be made general. There are other qualities, not perhaps absolutely necessary, but eminently to be desired. These are an interesting method of presentment, correct language, and attractive illustration. When the branch of learning in question is one of the natural sciences, then still other qualities are either needful or desirable. Whether education or mere instruction be its aim, that book is most likely to succeed that leads its readers along the same paths as the discoverers of science must themselves have followed. Students should not have facts thrust at them, but should be shown how to find them out; steps should be made, not simply taken; conclusions should be drawn, not merely stated; definitions should be led up to, and not started from.

These are high ideals, and it will be acknowledged that they are very rarely attained. Indeed, one would hardly be very severe on a writer who fell short in a few of the above-mentioned qualities. When therefore we find a writer who fulfils our somewhat arduous requirements, it is both a duty and a pleasure to award him our highest praise. Such praise it is our pleasant task to offer to Mr. Chalmers Mitchell, the author of the fascinating little introduction to the study of living beings that now lies before us.

Let us prove, by instances, that this praise is well deserved. Written, as we are told, primarily for candidates preparing to be examined in elementary biology by the conjoint board of the Royal Colleges of Physicians and Surgeons of England, the book necessarily follows, in its main outlines, the syllabus issued by that body. The plants and animals chosen as types of structure are, therefore, *Protococcus*, *Spirogyra*, *Botrydium*, the Yeast-plant, Bacteria, *Amoeba*, *Vorticella*, *Hydra*, the Earth-worm, the Dog-fish, and the Frog. Of all these organisms the description is bound up with the practical work needed for its verification, so that the student seems not so much to be taught by the printed page as to learn for himself from the concrete facts before him. Further, as each fact is discovered, its meaning is sought for, and the fact, thus brought into line with other facts before and after, becomes more readily fixed in the mind of the student. Also, at intervals, and especially at the end of each chapter, the conclusions that have been gathered bit by bit are summarised, and the lessons taught by the scattered facts are packed into convenient compass for carrying away in the student's brain. A great deal is talked about the educational value of science, and the true inductive method of teaching by observation and experiment, but we rarely find books that put their precepts so well into practice as does Mr. Mitchell's "Outlines of Biology."

The method of teaching by types, so much in vogue, often repels the student by its absence of a connected story, and by a want of relation between the different chapters. No such complaint can be made here. The interest of the reader is caught at the outset, and is kept to the close, by a series of admirable chapters on the broader problems and conclusions of biology. All the earlier studies are thus united by the central idea of protoplasm; so that the thoughts of the student are thrown back to the first chapter by such sentences as "Ciliary motion, then, is a form of protoplasmic activity," and again, "The development of the animal consists in the organisation and specialisation of its protoplasm." Among these chapters we would specially instance those on "The Building up of Protoplasm," "The Differences between Plants and Animals," "Cell-structure and Cell-division," "The Gastrula, Coelenterata, and Coelomata," and "Embryology." But there are also interesting little excursus scattered through the more descriptive chapters; thus, *Amoeba* is made the text for talks on subjects so different as genera and species in general, and death and reproduction in single-celled animals.

Our author's accuracy is shown in such cases as his careful distinction between *Protococcus* and *Pleurococcus* on p. 16, or that between *Lumbricus* and *Allobophora* on pp. 138 and 152, since these are forms too often confused by our writers of text-books. The lucid and interesting qualities of his writing may best be gauged by the fact that a person absolutely ignorant of biology has seriously incommoded the present reviewer by constantly borrowing the book to read. The clearness of style is enhanced by a judicious use of solid and spaced type, though it must be confessed that a whole page of the latter is nearly as troublesome to read as bad type-writing. An additional advantage to the book is that the numerous figures have nearly all been drawn by the author himself. This proceeding naturally has its defects, for not all scientific men have mastered the difficulties of "process work." But it brings with it the counterbalancing gain of clearness and accuracy.

In a word, we know of no introduction to biology that we would more willingly place in the hand of a beginner, whether he intended to become a professed biologist, or merely to pass the examination of "the conjoint board"; while those who wish, not so much to study, as to get a clear and up-to-date idea of the broader questions of biology, may safely be advised to make themselves a Christmas present of these attractive "Outlines."

Rocks, Ancient and Modern.

Einleitung in die Geologie als historische Wissenschaft: Beobachtungen über die Bildung der Gesteine und ihrer organischen Einschlüsse. By Johannes Walther. Part III. Lithogenesis der Gegenwart. Beobachtungen über die Bildung der Gesteine an der heutigen Erdoberfläche. 8vo. Pp. viii., and 535–1055. Jena: Fischer, 1894.

Reference has been made on more than one occasion in the pages of Natural Science to the many interesting points raised by Professor Walther in the earlier parts of the above work. One is therefore quite prepared to find that the more strictly geological part of the book is treated in an original and unconventional manner. In the two previous sections of Professor Walther's "Introduction," an account has been given of the conditions of the "Bionomy" of the sea, and the conditions of life and action of marine animals. The concluding volume describes "Lithogenesis" or the method of formation of the various rocks that are now being built on the surface of the earth. Professor Walther begins with a plea for the more systematic study of this subject. He maintains that far more can be learnt from an examination of the sedimentary rocks than is generally thought. So far does he carry this, that he declares that the history of the earth could have been written from the evidence of the rocks alone, even if they had not contained any fossils. For this to be possible, however, much greater precision is necessary in the study of the methods of formation of rocks now in process of deposition, and of the agencies by which they are laid down and modified. He wishes, therefore, to raise this study into a science which he calls "phenomenology." According to Walther this subject has not been properly appreciated: he thinks that, just as there is comparative anatomy and comparative botany, so there ought to be a systematic study of comparative lithology. He draws a close analogy between the study of rocks and animals: phylogeny is studied by the methods of palæontology, embryology, and comparative anatomy, and Walther maintains that rocks should be regarded in the same way; thus petrography and stratigraphy are equivalent to palæontological history, the study of rock development as at present going on takes the place of embryology, and the comparison of rock structures and characters agrees with comparative anatomy.

The author defines Lithogenesis, or Lithogeny, as the interpretation of "fossil rocks" by the investigation of the recent rock-forming agencies (p. 537). This, he says, is only the application of the method of Ontogeny which has always taken the leading place in zoology and botany. The work is divided into three parts:—1st. General lithogeny; 2nd. The "Faciesbezirke" of the present, describing the different deposits of different areas on the earth; 3rd. The bases of a comparative lithogeny. In the first are described the different methods of the destruction of old rocks and the formation of new ones. The subject is well illustrated by numerous references and verbal illustrations which compensate for the lack of figures, of which there are only eight in the volume. The various agencies are systematically classified; thus the author divides denudation into four kinds, that by the wind, which he calls "Deflation," that by flowing water, or Erosion, that by glacier ice, or Exaration, and that by the sea, for which he accepts the term Abrasion. The second part of the work is the largest: it describes the different types of deposits formed in different regions on the earth. Dr. Walther takes first the four different zones that run round the earth, and describes their most typical deposits. There is the Polar region with its moraines, its bog-iron ores, and its guano; the temperate region with its different sets of deposits grouped according as they are formed by Exaration, by Erosion, or by Deflation. Then follows the zone of deserts, where the agency of the wind and deposits formed chemically in lakes undergoing desiccation are most characteristic. Finally come the Tropics, where red marl and laterite are the most widely-spread deposits. Then follows an account of the rocks formed on the mainland by volcanic action, or on its shores as sea-beaches, sand-dunes, mud-flats, delta-deposits, and talus-banks at the foot of cliffs. The next five chapters, dealing with sea-deposits, form one of the most valuable sections of the book: they are well up-to-date, especially in the treatment of coral reefs and deep-sea deposits. It is interesting to note that Professor Walther pronounces in favour of Darwin's theory of the formation of oceanic atolls.

The third part of the volume considers the bases upon which such a science of comparative lithology as the author desires must rest. But this part of the work is brief and general in its treatment. It shows that much has still to be done in the application of the methods advocated by the author, which many other geologists are also trying to use in their particular spheres of work. To have such prominent attention called to them as has been done in this work will, no doubt, lead to their yet wider consideration. The book is throughout a sign of the need of a knowledge of biology to the geologist, and of physical geography to the zoologist and palæontologist.

United States Geological Survey.

A large parcel of Bulletins of the United States Geological Survey reached this country at the end of October. Among some twenty volumes on different subjects it may be useful to draw attention to the following:—No. 115, "A Geographic [*sic*] Dictionary of Rhode Island," by H. Gannett; no. 116, "A Geographic Dictionary of Massachusetts," by H. Gannett; no. 117, "A Geographic Dictionary of Connecticut," by H. Gannett, all dated 1894. These three valuable gazetteers are "designed to aid in finding any geographic features upon the atlas sheets" of those States, published by the Geological (why not Geologic?) Survey. They contain all the names given upon those sheets, and no other—and this, we think, is a pity, because the Geological Survey of the United States, like the Geological Surveys of other countries, does not give the whole of the names used in a tract of country, because of the overcrowding it would compel. It is to be hoped that Mr. Gannett will continue this valuable series of reference books.

Bulletin no. 102 (1893) is "A Catalogue and Bibliography of North American Mesozoic Invertebrata," by C. B. Boyle, a work of immense importance to geologists and zoologists in every country. The catalogue is arranged under genera, the species following in alphabetical order; n. g. or n. sp., for "new genus" or "new species," is placed after each entry when the genus- or species-name stands as originally described by its author. When the species-name has become attached to another genus-name the original author's name is put into brackets, but, unfortunately, the compiler has not indicated the original genus in which the species was placed. This is a mistake, for the tracing of the original genus-name often causes much labour. The formation and locality (or, as our American friend pleases to call it, the location) are also given. A reference in the preface to the similar lists drawn up by Samuel George Morton would have been gracious, for Morton's works were of considerable value in the early days of geology.

Bulletin no. 100 consists of "Bibliography and Index of the Publications of the Geological Survey, with the laws governing their printing and distribution," by P. C. Warman, 1893. This runs to 496 pages, and will be of great value to bibliographers as well as to geologists. Each publication is chronicled in the most complete manner, *e.g.*, general title, special title, collation, contents, number printed, method of binding, and price. We learn from the Preface that no less than 12 Annual Reports, 20 Monographs, and 99 Bulletins (subsequently increased by this present batch) have been issued, a fact which is highly creditable both to the Geological Survey and to the United States Government. The generous methods of distribution are explained in full, and a comprehensive index is added to the volume.

Dr. Bowdler Sharpe and Mr. C. W. Wyatt have completed their "Monograph of the Hirundinidæ, or family of Swallows," the first part of which appeared in September, 1885. The last parts (xviii., xix., and xx.), issued as one, appeared in October, and contained in addition to the completion of the text, a detailed paper on the geographical distribution of these birds, and an account of the literature of the subject. In this bibliography the authors have carefully explained the books referred to, and given a critical account of the species of each author, as well as an identification when necessary of the birds figured. Careful and elaborate digests of literature such as these are invaluable, and until they are done in every group there will still be much groping in the dark.

Mr. Stanford has issued "A Catalogue of Atlases, Books, and other Publications," October, 1894. The Index to Authors supplied at the end is a most commendable institution. The firm of Nijhoff, of the Hague, also send Catalogue no. 255, containing books on Spain, Portugal, and the Philippines. Many of the items relate to works on the languages of the Eastern Spanish Possessions. We have received from Messrs. Dulau & Co., of 37 Soho Square, London, a Catalogue containing the titles of over 1,500 works on the Anatomy, Morphology, and Physiology of Plants; and a similar Catalogue devoted to Crystallography, Metallurgy, Mineralogy, and Mining.

Periodicals and Excerpts.

We have received from Dr. Michaelsen an interesting excerpt from "Die Tierwelt Ost-Afrika's" upon the earthworms of East Central Africa, in the neighbourhood of Albert Edward Nyanza. We fully agree with Michaelsen's prefatory remarks in which he says that this group of animals "offers the most weighty documents for the history of continents and oceans," and we sympathise also with his lament that it is so difficult to get people to collect them. To secure a worthless horn or two of some totally uninteresting antelope a travelling naturalist will cheerfully risk life again and again; but it is hard to induce him to stop and pick up the worm at his feet. There is an innate belief that nothing which is abundant and easy to get can be of interest. It so happens that tropical Africa has yielded quite the most interesting animals belonging to this class, to our knowledge of which Dr. Michaelsen has largely added on this as on former occasions. The curious family Eudrilidæ, which teem with new and unexpected anatomical characters, often of more than family importance, are the principal inhabitants of tropical Africa. both east and west. In the present paper two new genera of this family, which are named *Unyoria* and *Eminoscolex*, are described and well illustrated, besides new species of the characteristically East African genera, *Polytoreutus* and *Stuhlmannia*.

THE *Proceedings of the Royal Physical Society of Edinburgh* for 1894, comprising the papers read during the 123rd Session of that Society, commences with the address of the Vice-President, Mr. Kidston. It is an elaborate and important paper "On the Various Divisions of British Carboniferous Rocks as determined by their Fossil Flora." Besides this address, there are nineteen communications dealing with fossil fishes (Dr. Traquair), the Arachnid and Reptilian fauna of the environs of Edinburgh (by Messrs. Evans and Carpenter), and other subjects. A paper which will probably attract a large amount of attention is one upon "Geographical Distribution of Disease in Africa," by Dr. Felkin. It appears from the coloured map which illustrates the paper that one of the best parts of Africa in which to take up a permanent abode is in the middle of the Sahara; for there one has a choice of only six species of disease. The constant association between smallness of size and malignity of disposition is well shown by the island of Mauritius, which occupies almost the opposite extreme; here you may catch or contract, if you are lucky, no less than eleven distinct forms of disease. Some diseases are curiously local; to meet with "sleeping sickness," for instance, it is necessary to frequent the West Coast.

THE fourth yearly volume (1894–95) of that useful list of Universities and Professors, *Minerva*, published at Strassburg, was announced to appear towards the end of October. It contains a portrait of Lord Kelvin.

Mr. J. D. PAUL has a short paper, illustrated by a map, on the earthquake recorded at Leicester on August 4, in the *Transactions of the Leicester Literary and Philosophical Society* (vol. iii., pt. 8). The shock apparently affected an area of not less than two thousand square miles.

THE *Bombay Natural History Society* has given on the outside wrapper of the parts of the last volume of its journal the exact date of publication of each part. It has, moreover, repeated on the title page of the whole volume the information, and thus rendered it impossible for any error to occur in the future. This is most praiseworthy. The society has a membership of some 750.

OUR esteemed contemporary, *The Geological Magazine*, is following our example and changing publishers. It will in future be published for Dr. Henry Woodward and his able colleagues by Messrs. Dulau and Co., of Soho Square, London. We trust that the good fortune which has befriended it in the past will not desert it in the times that are coming.

OBITUARY.

F. BUCHANAN WHITE, M.D., F.L.S.

BORN MARCH 20, 1842. DIED DECEMBER 10, 1894.

DR. F. BUCHANAN WHITE died suddenly at Annan Lodge, Perth, in which city his father is a doctor, and in which he himself was born. Educated for the medical profession, he graduated with honours at Edinburgh in 1864; but, having a moderate competence, instead of entering upon the active duties of a doctor's life, he addressed himself to the study of Natural History and kindred branches of learning. His favourite subjects were Botany and Entomology, and in these he was one of the highest authorities in Scotland. He was the author of part i. of the "Fauna Perthensis," and for many years before his death had been engaged in the preparation of a "Flora of Perthshire"; the latter work, which would have been his crown had he been spared a little longer, will now form a fitting monument of his untiring industry in the laboratory and the field.

Dr. White's loss will be specially deplored by the members of the Perthshire Society of Natural Science, of which, along with the late Sir Thomas Moncrieff, he was the founder, and in whose affairs during the past quarter of a century he took the keenest interest. For a long succession of years he acted as President of this Society, which has done so much to popularise scientific studies in Perthshire. The Perth Natural History Museum also owes its origin and development chiefly to Dr. White's labours, and it is now justly reckoned one of the most complete and well arranged provincial museums in the country.

Besides the two books above referred to, Dr. White's contributions to the scientific literature of the day were numerous and varied. He was for many years editor of the *Scottish Naturalist*, and of the *Transactions of the Perthshire Society*; and these publications contain a large number of papers on scientific subjects from his pen. He published a monograph on the collection of *Halobates* brought home by the "Challenger" expedition, and lately contributed a memoir on British willows to the *Journal* of the Linnean Society. Of that Society he became a Fellow in 1873. He was also a member of the Entomological Societies both of London and France, Secretary of the Cryptogamic Society of Scotland, and Local Secretary of the

Botanic Society of Edinburgh. For some time he was Examiner in Natural History at Aberdeen University.

Dr. White had the valuable faculty of gathering around him in Perthshire a band of disciples, full of enthusiasm for natural history pursuits; but in late years, with somewhat impaired physical strength, he mainly contented himself with guiding their studies in the several departments for which they showed a special aptitude and liking. Many students of natural science, beyond the city and county of Perth, owe much to his kindly sympathy and ready help. He was the prince of field-botanists, and his friends in the north will long cherish happy memories of scrambles among the crags and corries of the Perthshire hills in the company of Buchanan White, ever the cheery comrade, fleet-footed leader, keen-eyed observer, and patient teacher.

FRIEDRICH BIDDER.

Born 1810. Died August 27, 1894.

BIDDER was born at Landohm, in Kurland. He became Professor of Anatomy at Dorpat in 1842, and from 1843 to 1869 Professor of Physiology at the same place. There also he died. His work lay chiefly in human physiology, but he paid some attention to the Amphibia—"Bidder's organ" in this group taking its name from him.

RICHARD MEADE.

Born 1827. Died September 12, 1894.

WE learn from the *Geological Magazine* that Richard Meade, of the *Mining Record* office, was born in 1827 at Dublin. His chief work was the preparation of the volumes of "Mineral Statistics"; but he is probably best known as the author of "The Coal and Iron Industries of the United Kingdom," which was published in 1882.

Mr. J. R. Wellman, the first president of the South London Entomological Society, died at Clapham on November 12, at the age of 62; Baron Gerhard Maydell-Stenhusen, the Siberian traveller, died at Bad Ems, on August 18 last.

The *Entomological News and Proceedings of the Entomological Section; Academy of Natural Sciences, Philadelphia*, publish in part ix. of their fifth volume a portrait and sketch of the life of Benjamin Fann Walsh, an American entomologist of note, who died in 1869. He was a native of Frome, and was a schoolfellow of Charles Darwin's.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Professor W. Kükenthal has returned to Jena from his zoological excursion in Borneo. Dr. M. von Davidoff requests that all letters for him be sent to Villefranche-sur-mer, France, till July 15, 1895.

Dr. John Murray has been awarded the "Prix Cuvier" by the Académie des Sciences, Paris. The last Englishman who received this honour was the late Sir Richard Owen. The Cothenius medal of the Royal Academy of Berlin has been awarded to Dr. Hans Bruno Geinitz, in Dresden On October 16, 1894, Dr. Geinitz completed fifty years' membership of the Academy, and celebrated his eightieth birthday.

Mr. Mathew Davenport Hill, late scholar of New College, Oxford, has been elected to the Oxford Biological Scholarship at Naples. Mr. J. H. Burkill, late scholar of Caius College, Cambridge, and Assistant Curator at the Herbarium, has been awarded the Walsingham medal.

Professor Charles S. Prosser, of Washington College, Kansas, has been appointed to the chair of geology at Union College, Schenectady, N.Y. Dr. Harrison Allen has resigned the directorship of the Wistar Institute of Anatomy in Philadelphia, and Dr. Horace Jayne has been appointed in his place. Mr. T. H. Kearney, jun., has become curator of the Herbarium in Columbia College, New York; Dr. S. Nawaschin, Professor of Botany and Director of the Botanic Garden at Kiev University; and Dr. K. Schilbersky, Professor of Botany and Vegetable Pathology at the Agricultural Institute, Budapest. The University of Chicago is forming a department of Botany, under Professor J. M. Coulter.

Mr. Horace B. Woodward has been added to the editorial staff of the *Geological Magazine*, and will be a strong addition to the working editors. Professors Pfeffer, of Leipzig, and Strasburger, of Bonn, have succeeded to the editorship of the *Jahrbücher für Wissenschaftliche Botanik*, formerly held by Professor Pringsheim. Communications should be sent to Professor Pfeffer.

Professor J. F. Blake, has been appointed to arrange the collections of the Gaekwar of Baroda, stored in the Baroda Museum. Professor Blake leaves England about the end of December. This will not in any way delay the issue of his well-known "Annals of British Geology." The last volume, that for 1893, is expected to appear early in 1895.

In issuing the twenty-sixth annual report of the Silesian Botanical Exchange Club, the director, Mr. S. Mayer, remarks on the increasing difficulty of satisfying the demand for new plants, and the limited means available for getting collections made abroad. He has accordingly resolved to make a personal expedition to the tropics, and has chosen Singapore as a base of operations, with the intention of exploring botanically the Malayan Peninsula and Siam, and visiting the Sunda Archipelago.

Mr. H. N. Ridley, the Director of the Straits Settlements Forest and Gardens Department, who is now in England, has done a good deal in the Malay Peninsula during his six years of residence at Singapore, but Mr. Mayer will doubtless find much left to collect if he can satisfactorily combat the difficulties and hardships incident to the exploration of the less known districts.

Letters have been received, dated the middle of November, from Prince Henri d'Orléans. He had then spent some time in Cambodia, and was leaving for the Upper Mekong. M. Gervais Courtellemont has returned to Paris after a visit to Mecca. He is an Algerian, and his knowledge of Arabic enabled him to travel as one of the faithful. He has brought back some photographs of the city of considerable interest.

Our knowledge of Africa increases. Count Götzen has telegraphed to Berlin his safe arrival at Matadi on the Lower Congo. He has succeeded in crossing the continent from east to west, following closely the line taken by Emin in his last and fatal journey. This route lies between those of Stanley and von Wissman. He started from Dar-es-Salaam about the end of 1893. Mr. F. C. Selous is starting on a shooting expedition to the Upper Nile. It is his intention to return to Matabeleland in March next. He will be accompanied by his wife.

We stated in our October number that more convenient arrangements were being made for students of the Hope Collections at Oxford. We can now add, on the authority of the *Entomologists' Record*, that Professor Westwood's study has been thrown into the large insect room, that a room has been set apart for photography, and another fitted up as a biological laboratory. As there is much material still to be overhauled, Professor Poulton hopes that students will avail themselves of these facilities.

The fund raised in memory of Professor Milnes Marshall reached £783 10s. 3d. Of this sum £650 have been invested in Manchester Corporation Stocks for the benefit of the Marshall Biological Library, which the relatives of the late Professor started by a gift of his books. The remainder of the sum, less some £20 expenses, has been invested in the same securities, and provides for a "Marshall Gold Medal" as an annual prize at the Owens College Athletic Sports.

The Epping Forest Museum Committee have received a contribution of £20 from the Drapers' Company.

At a meeting of the Bristol Corporation, held in December, it was decided to appoint a Curator for the Bristol Museum at a salary of £200 a year. Mr. Edward Wilson, who has, almost at his own expense, cared for the collections for years past, has been appointed to the office.

A second edition of the "Guide to the Field Columbian Museum" (Chicago) has just been published. The Museum consists of a Columbus Memorial Hall, and departments of Geology, Botany, Zoology, Anthropology, and the industrial arts. The guide is an admirable one, and is rendered still more useful by the insertion of numerous plans showing the arrangement of the rooms.

The Geologists' Association arranged an excursion to Elstree, under the direction of Professor J. F. Blake, on Saturday, November 10, to enable the members to examine a fossiliferous bed of London Clay, exposed while constructing a new tunnel. The heavy rain spoiled the expedition somewhat, but some typical fossils rewarded the searchers. The fact that the works will be closed in March accounts for the unusual date of the excursion.

The Ealing Microscopical and Natural History Society has changed its name to the Ealing Natural Science and Microscopical Society. It has also passed a new set of rules, founded mainly on those of the Geologists' Association.

The concluding part (iii.) of volume vi. of the *Transactions of the Shropshire Archæological and Natural History Society* has just appeared. We regret to observe that the index to this completed volume shows nothing but Archæology. Surely there is some interest taken in Natural Science in the county. We note also that the honorary curatorship of Zoology is vacant; but after all that is not of much consequence, for, to judge from the condition of the Museum during the last few years, "honorary curatorship" and "vacant" are synonymous terms.

The *Report and Transactions* for 1893 of the Guernsey Society of Natural Science, just published, show that there is much good material in the Channel Islands and many who are willing to work it out. The list of members numbers 100, and monthly meetings are held from September to July. Among the more interesting reports in the present volume is that of the Geological Section. This deals with the superficial deposits, and shows that the Society is always on the look out for new sections or cuttings, no less than twenty of these having been recorded during the year. In another paper the Diatoms of Guernsey are studied by Mr. E. D. Marquand, the president.

In the *Proceedings of the Philosophical Society of Glasgow* (vol. xxv.), Dr. John Ferguson's 1893 presidential address will be found. It is entitled "On the work of the Philosophical and other Scientific Societies," and treats of the general work done by the Society during the hundred years it has been in existence.

Those interested in artificial flight will find a detailed and illustrated report of Mr. H. S. Maxim's paper, read before the Society of Arts on November 28, 1894, in the *Journal* of that Society for November 30.

Some time ago we called attention to the curious announcements of the Palæontographical Society, in which works were advertised as "in preparation" for, in some instances, twenty-five years. The volume just issued shows but small improvement in this respect. Meanwhile, the Geological Survey of New South Wales has gone one better, for they have announced for some years, and still continue to announce, the fact that Dr. L. G. de Koninck is at work on the Palæozoic Fossils of New South Wales. We dare say they are right, but our information on this side of the world is not so peculiar. Dr. de Koninck died in 1887.

At a meeting of the Field Naturalists' Club of Victoria, held at Melbourne on October 22 last, a demonstration in practical taxidermy was given by Mr. G. A. Keartland. This is an excellent departure, and might well be followed by some of our own local societies.

The *Report* of the Madison meeting (1893) of the American Association for the Advancement of Science has just been published. It contains Professor Le Conte's address on "Theories of the Origin of Mountain Ranges"; C. D. Walcott's address on "Geologic Time; as indicated by the Sedimentary Rocks of North America"; and H. F. Osborn's address on "The Rise of the Mammalia in North America." Detailed lists are given of fellows and members, whether living or dead, a compilation of considerable value to librarians and others.

THE Belgian Academy of Science in Brussels has offered prizes to the value of six hundred francs for the best treatise on one of the following subjects: (1) Researches on the number of chromosomes, before fertilisation, in an animal or a plant. (2) New researches on the quaternary flora, especially on peat-mosses. (3) Is there a nucleus in Schizophytes? If so, what is its structure and the mode of its division? The author must give a critical *résumé* of all work hitherto published on this subject. Treatises should be written in French or Flemish, and sent with motto and sealed name to Chev. Edm. Marchal, Secretary of the Academy, before August 1, 1895.

THE difficulty which has been experienced in England of obtaining a sufficient supply of the new diphtheria antitoxin is likely soon to be remedied. Already a supply is forthcoming from the Institute of Preventive Medicine, and Dr. Klein is providing it in considerable quantities to such hospitals as care to apply for it. The munificent action of the Goldsmiths' Company in making a grant of £1,000 for the furtherance of research work in connection with the treatment, and for the supply of serum for use among the poor, will commend itself to everyone. The administration of the grant has been undertaken by the Conjoint Board of the Royal Colleges of Physicians and Surgeons, and the research will be carried out in their Laboratory on the Embankment. It cannot be doubted that much valuable information will be obtained as to the more precise nature and action of the antitoxin, especially as an accurate bacteriological diagnosis will be made in all cases. The Conjoint Board of the Royal Colleges has offered to make the needful examination in all cases of diphtheria admitted into the hospitals of the Metropolitan Asylums Board for a period of six months, and the latter body have agreed to an expenditure of £375 to this end—no very astonishing remuneration when we read further that the daily average of cases so examined is to be twenty. In this way a really valuable body of statistics will, it is to be hoped, be forthcoming, and one which should place the therapeutical value of the antitoxin on a firm basis, if basis it has. And accounts already to hand of the results of the treatment in this country fully confirm the favourable effects reported from the continent.

THE Bohemian Portable Biological Station, which we described in November, is now placed on the shore of the Black Lake in the Böhmerwald, and a report of recent researches is in course of preparation. The Director, Dr. Anton Fritsch, still devotes his attention, at the same time, to palæontological studies. A new part of his work on the "Fauna of the Gas-coal of Bohemia" has just appeared, and he is now occupied with the examination of a series of fossil Arthropods from the coal-field of Nyran.

ACCORDING to *Die Natur*, the marine laboratory which was founded by the exertions of Professor R. Wagner and the Archimandrite Mileti in 1881 at Solovetzk, an island in the White Sea, has now been improved into a two-storied building, with convenient fittings and work rooms. Numerous sailing boats are at the disposal of the workers, and efforts are being made to secure a small steam-launch. The director, M. Knipovitch, has been at the head for the past three years.

WE have received a circular from Dr. Vittorio Simonelli and Dr. Carlo Fornasini, stating that it is proposed to start a publication devoted to the Palæontology of Italy. The journal is to be called *Revista Italiana di Paleontologia*. The new periodical, which is intended to do for Italian geology what the Palæontographica Society does here and the Paléontologie Française does in France, will be published at Bologna, in octavo. Number 1 will appear on February 28, 1895, and the subscription price will be five lire annually. Subscriptions should be sent to Via Zamboni, 65, Bologna.

CORRESPONDENCE.

Names, Names, Names.

Will you allow a Philistine and a heretic to occupy a small portion of your space upon a matter of serious moment to those students of Natural History who are not systematic zoologists, namely, the burdens that are being created for them by the eccentric and apparently wanton multiplication of names? Two books on Reptiles have just been published, one a new volume of the great catalogue of the Reptilia in the British Museum, the other a volume of the special monograph on the Reptiles in Godman and Salvin's "Biologia Centrali-Americana." These two works have come out within a year of one another. They are written by specialists, who are both distinguished men, and both officers of the same Museum. It is assuredly a pity that in these two works, in a large number of cases, the same forms not merely have different names, but are put in different genera.

If nomenclature is to become more and more dependent on the personal equation of the particular writer, it will cease to have any use or meaning. At the best it is merely the index of our knowledge, and to multiply indices to the book of Nature is not to arrange but to confuse.

I hope I shall be forgiven for what I have said. The matter is a serious one to some of us, and the particular case I have selected is a good case, since it is an instance of two big men placing a heavy burden on a large number of small ones.

Henry H. Howorth.

[Before we comment on the remarkable state of affairs to which Sir Henry Howorth calls our attention, we invite explanations from the parties immediately interested.—Ed.]

Fresh-Water Fishes of New South Wales.

It is not so many years ago since the Murray River system of Australia was credited with the possession of a fish-fauna peculiarly its own; I speak, of course, exclusively of edible fishes, all the smaller forms of fish life being contemptuously ignored by our transmontane neighbours. The principal genera on which this distinction was conferred were *Oligorus*, *Ctenolates*, and *Macquaria* (=*Murrayia*, = *Riverina*).

Many years ago the Murray Cod was proved to be a native of the Richmond and Clarence rivers district, which rivers drain the north-eastern watershed of New South Wales; it was always, however, a disputed point whether the species was indigenous to these rivers, had been introduced thereto by human or other agencies, or had simply crossed the borderland between the two systems during a season of exceptional flood. Be this as it may, the discovery, by the naturalists of the "Challenger" Expedition, of both the Murray Cod (*Oligorus macquariensis*) and the Golden Perch (*Ctenolates ambiguus*) in the Mary River, Queensland, where there can be no question of introduction, at once nullifies the claims of the Murray River and its tributaries to the exclusive possession of these two species, while at the same time it strengthens the claim of our north-eastern rivers to the former species as truly indigenous.

These defections left *Macquaria australasica*, with its numerous Castelnauic synonyms, as the sole remaining species peculiar to the Murray system, which position it has held unchallenged from the time of the "Challenger" discoveries until now. However, in a small collection of fresh-water fishes, forwarded to me from the Nepean River at Camden, by Mr. H. J. McCooey, I found six fine specimens of Macquarie's Perch, and I am, therefore, compelled to break the last link of exclusiveness to which the Murray system could lay claim. That a fish of such size and excellence could exist in numbers so near to the metropolis, yet be quite unknown from that locality to Australian scientists, shows how sadly our fresh-water fauna has been neglected, and the necessity for paying more particular attention to this branch of zoological science henceforth.

Australian Museum, Sydney, J. DOUGLAS OGILBY.
6th Oct., 1894.

THE CLAWS OF ARCHÆOPTERYX.

IN one of his very interesting articles on *Archæopteryx*, Mr. Pycraft has suggested that the wing-claws were used mainly during youth, as the young Hoatzin uses his now, to assist him in climbing. This will hardly account for the maintenance of the great size of the claws after the attainment of maturity. May they not have been useful while moulting was in process? It is hardly likely that this very primitive bird had developed a perfect system of moulting, by which the quills were shed in pairs at considerable intervals, as they are by most birds now. Ducks shed their large quills almost simultaneously, and take refuge from enemies on the water. But a land bird, even if fairly strong in the leg, might find assistance from his forelimbs useful in climbing up trees, in order to get out of the reach of enemies of non-arboreal habits.

F. W. HEADLEY.

EVOLUTION OF A PROTECTIVE HABIT IN SEA-URCHINS.

WITH reference to the very interesting note in the September number of NATURAL SCIENCE, entitled "A Moving Grove," it may be pointed out that another species of the sea-urchin *Hipponoë*, namely *H. variegata*, has a somewhat similar habit to *H. esculenta*. That is to say, it carries portions of seaweed on the spines of its anal region. It is conjectured by Studer, who relates the fact, that this seaweed is for the animal to stick its eggs to—a kind of artifical brood-pouch. This forms an interesting stage in the development of a habit, and suggests that in habits as in structures, for which we are at first unable to account by mere Natural Selection, much may be explained by a change of function. The earlier stages may have been evolved in obedience to conditions quite different from those that have governed the later stages. B.

CHANGE OF ADDRESS.

In future the PUBLISHING AND EDITORIAL BUSINESS *of* "NATURAL SCIENCE" *will be carried on at the Offices of* MESSRS. RAIT, HENDERSON & CO., LTD., 22 ST. ANDREW STREET, HOLBORN CIRCUS, LONDON, E.C.

TO CONTRIBUTORS.—*All communications to be addressed to the* EDITOR *of* "NATURAL SCIENCE" *at the above address.*

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NOTES AND COMMENTS.

Science and Letters.

THE art of letters has no content of its own and stands in no contrast to science. All of us, in attempting to describe a fossil or to narrate the life-history of a fern, are engaged in the same pursuit as is the man of letters. The distinction, between him and ourselves, too often is this: we are incompetent craftsmen and we are persuaded of the untruth that if you have something to say it does not matter how you say it. Men of letters are not a class by themselves; not mere conjurors with words, amusing the rest of the world with the grace and ingenuity of their antics, by their skilful poise of the adjective and clever balancing of the phrase. They are historians, dramatists, novelists, poets, or, sometimes, parsons and men of science who have conquered not only ideas, but the expression of them. This salutary truth, which should be a truism, may serve as an excuse for reference in these pages to Robert Louis Stevenson, who, since last we wrote, has become but a memory.

In our poor opinion there is much of moment to scientific writers in the art of Stevenson. First, there is the method. Steep yourself in your subject, says the common adviser, then sit down and write quickly. But so doing, your matter will ooze out from you in the flamboyant periods of, say, the late Professor Kitchen Parker, or in the more distasteful prolixity of the average German. Not so does the expression of scientific fact take its appropriate place in the art of letters. The most careful selection and arrangement of the facts are needed, so that the salient points may be thrust into prominence, the subsidiary facts restrained into a decent subordination, and vain repetition suppressed. If one but consider; an account of the morphology of the tadpole's skull is as difficult to set forth well as the creature of a boy's story. Yet you read "Treasure Island" between London and York after a nice decision between it and the current *Truth*, and Long John Silver sticks in your mind, not to be rid of, a permanent possession. Yesterday you read a description, many pages long, of a new genus, anxious on the details, comparing and weighing: to-day you are running round to the library to read again an important point that failed to impress itself. This happy art of presentment comes not by grace or by knowledge; but by patience and labour.

Next, from the words and phrasing much also may be learned. To those unversed in the analysis of sentences, many lines of Stevenson seem whimsically peculiar, full of deliberate abnormality. But let such examine the easy transition from idea to idea, the orderly progression of the exposition, and they shall see how the words and phrases are chosen and arranged for the simple purpose of presenting the ideas in the directest and shortest fashion, which also is the intention, although not the achievement, of scientific writing.

Stevenson and Science.

For the mention of Stevenson a sturdier excuse than our need of the qualities of his style may be found in his excursions into the province of natural science. Of these, two are memorable; the essay "Pulvis et Umbra" in "Across the Plains," and a poem entitled "The Woodman" in the *New Review* for January.

The essay, and we commend it to all readers who do not know their Stevenson, is an attempt with a strongly ethical basis to express a monistic idea of man's relation to the universe, and to contrast with it his kinship to the dust, his thought of duty, and his ineffectual effort to do well. The essay is so short and so well-knit that quotation from it is not advisable. It is however interesting to note that while Professor Huxley in his Romanes lecture (*see* Natural Science, vol. iii., p. 62) laid down that the cosmic process was not only non-moral but immoral, Stevenson reads in it "a bracing gospel."

The poem, published last month, is practically an account of the struggle for existence among plants in the tropics, and much of it might be a paraphrase of Dr. Rodway's essay on the struggle for life in a Guiana forest that appeared in our columns. We quote a few lines:—

> I saw the wood for what it was—
> The lost and the victorious cause;
> The deadly battle pitched in line,
> Saw silent weapons cross and shine;
> Silent defeat, silent assault—
> A battle and a burial vault.

Thick round me, in the teeming mud,
Briar and fern strove to the blood.
The hooked liana in his gin
Noosed his reluctant neighbours in;
There the green murderer throve and spread,
Upon his smothering victims fed,
And wantoned on his climbing coil.
Contending roots fought for the soil
Like frighted demons; with despair
Competing branches pushed for air.
Green conquerors from overhead
Bestrode the bodies of their dead;
The Cæsars of the sylvan field,
Unused to fail, foredoomed to yield;
For in the groins of branches, lo!
The cancers of the orchid grow.

Eozoön: Requiescat in Pace.

The fierce battle which raged between zoologists and petrologists over the nature of that curious structure called *Eozoön* is too well-known to need recapitulation here; but it may be mentioned that Professors King and Rowney, of Queen's College, Galway, in 1865-66, were the first to question its supposed organic origin. Extreme obstinacy, for we may so call it, on both sides prevented an exact examination of the divided collections, and infused into the dispute an acrimony as unpleasant as damaging to all parties concerned. Dr. Moebius was the first zoologist seriously to dispute the organic nature of *Eozoön*, and his book, "Der Bau des Eozoön," appeared in 1878. No satisfactory result was obtained, however, until March, 1891, when, at the instance of Dr. P. Herbert Carpenter, the original Tudor specimen of *Eozoön*, said to be preserved in limestone, and to which the supporters of the organic nature of the structure had pinned their faith, was sent to England by Dr. A. C. Selwyn. At a meeting of the Geological Society in that month, Mr. J. W. Gregory showed that the so-called Tudor specimen of *Eozoön* was nothing more than a series of calcite bands of secondary origin in a rock of Huronian age.

It has fallen to Dr. Johnston Lavis, by reason of his intimate acquaintance with the formation of the Somma-Vesuvian area, and to Dr. J. W. Gregory, intimate with zoological as well as petrological structures, finally to work out and demonstrate, in the most conclusive manner, that the structure known as *Eozoön* is completely paralleled by some structures seen in the ejected blocks of Monte Somma. It is not a little singular that this parallelism has remained unobserved so long, for a specimen of ejected block from this volcano, which has been accepted without hesitation by some of those who have contended for the organic nature of *Eozoön* as true *Eozoön*, has been in the collections of the British Museum more than half a century, and formed part of the series brought by Sir W. J. Hamilton. This block, of which any quantity can be obtained, is an altered limestone which occurs fragmentarily in the pumice of the series of explosions that excavated the Atrio del Cavallo, known as Phase VI, period 4, and Phase VI, period 1 (Plinian eruptions) of Johnston Lavis. *Eozoön canadense*, therefore, in the view of these authors, is nothing more than the zonal alteration of blocks of limestone which have been enclosed in an igneous magma, a view also supported by its mode of occurrence in Canada and elsewhere. These specimens are fully described and beautifully illustrated by photographic plates in the memoir before us (" Eozoönal Structure of the Ejected Blocks of Monte Somma," *Scientific Transactions of the Royal Dublin Society*, vol. v., series 2, October, 1894), and to this paper we must refer the reader for the details.

Whatever may have been the past history of the dispute, we at present cannot regret it, for it gave rise to a long series of interesting papers, and called forth such an amount of research into organic and inorganic structures as no other object has succeeded in doing. At the same time, it teaches that extreme caution is necessary when dealing with structures difficult of explanation and presenting peculiarities at once characteristic of biological and petrological forms; and while it urges less dogmatism, it shows how necessary it is for the specialist in one branch to call to his aid specialists in other branches, even when examining a structure which appears so obviously to belong to forms with which he is familiar.

OOLITE.

THE singular resemblances which are seen between organic and inorganic structures were well exemplified at the Geologists' Association at its meeting on January 4. Mr. G. F. Harris read a paper "On the Analysis of Oolitic Structure," during which he showed upon the screen a series of photographs of microscopical preparations of oolitic granules. One of these was a silicious sphere formed in the warm waters of a pool in the Yellowstone district. Commencing as a solid body, the sphere gradually became more and more cavernous, and the outer layers bore a strong resemblance to the organism known as *Parkeria*, from the Cambridge Greensand. These cavernous modifications were probably due to the enveloping of small algæ by the silicious coats. Sections of granules were shown by Mr. Harris from the Great Salt Lake, the extinct Lake Lahontan, the recent oolite sand forming off the Bahamas, and from many localities of the oolitic formation in England. The majority of these granules had characters peculiar to their locality, and the lecturer was satisfied that, so far as the English oolites were concerned, the nature of the grains was sufficiently characteristic to enable him to refer with a considerable degree of certainty specimens of the rock to special localities. The paper dealt only with the structure of the grains, but the lecturer touched on many interesting points regarding the origin and growth of oolitic granules, full consideration of these latter points, however, being deferred to some future occasion. We shall be much interested to see this paper in print.

The Formation and Absorption of Skeletal Substance in Echinoderms.

In his valuable memoir "On the development of *Echinocyamus pusillus*" (*Nov. Act. Reg. Soc. Sci. Upsala*, ser. iii., 1892), Professor Hjalmar Théel showed that the calcareous spicules were deposited within a protoplasmic mass formed by the fused pseudopodia of several wandering, amœboid cells. The skeletal substance, or stereom, has therefore an intracellular origin. In a short note recently communicated to the Royal Academy of Science at Stockholm (*Öfversigt*, li., p. 345), Professor Théel confirms his former views, and shows that similar amœboid cells have also the power of absorbing the stereom; so that the two groups of cells correspond to the osteoblasts and osteoclasts of the vertebrates. The absorbent cells only differ from the formative cells in the greater activity of their amœboid movements, which give the impression that the process of absorption demands intenser labour on the part of the cell than does that of deposition. When a cell begins to absorb a spicule it strains to extend and flow round and over it, so as to take it whole into its protoplasm; hence the granular main portion of the cell moves incessantly, gliding slowly along the swallowed spicule until nothing remains of it. Meanwhile the pseudopodia are continuously extended and retracted. Since the calcareous particle attacked is often of considerable size as compared with the amœboid cell, we have to suppose either that the latter can dissolve an unexpectedly large quantity of salts and retain them in solution, or that, as is more probable, the dissolved salts are gradually transferred through the pseudopodia to other cells in the neighbourhood, which may either retain them till needed, or use them immediately for building up fresh calcareous structures in the growing Echinoderm. When the calcareous body is too large to be completely enveloped by the absorbent cell a portion of it is detached, apparently by the secretion of an acid, and this portion is then swallowed. A piece of stereom, so large that it can hardly be taken into the protoplasm of the cell, is fully dissolved in about two hours.

This process of absorption not improbably takes place, *pari passu* with the processes of growth, all through the life of an Echinoderm. A familiar instance of absorption is seen in the orals of many crinoids. The process is best observed, however, at the close of the larval period, when the skeleton of the larva is giving place to that of the adult form. In the pluteus or sea-urchin larva, for instance, at this period not only is the larval skeleton in the way, but a large number of structures requiring calcareous salts are being rapidly developed. The larval stereom, therefore, which has been gradually accumulated from the earliest embryonic stages, may be regarded as reserve material for building up the future complex skeleton of the young sea-urchin. A mere accumulation of such reserve material might conceivably have had a pernicious effect on the larva, but, as things are, the reserve stereom has become essential to the existence of the larva itself, since it has assumed the most suitable form possible for facilitating the flotation of the larva in the sea.

The Duration of Niagara Falls.

In an interesting paper, contributed to the *American Journal of Science* for December, Mr. J. W. Spencer discusses the history and prospects of the Niagara Falls. The paper is illustrated by an excellent set of charts and maps, and should be valuable to the physiographer and geologist. We reprint the conclusions to which he comes:—

"The computation of the age of the Niagara river,—based upon the measured rate of recession during 48 years; upon the changing descent of the river from 200 to 420 feet and back to 320 feet; and upon the variable discharge of water from that of the Erie basin only, during three-fourths of the life of the river, to afterwards that of all the upper lakes,—leads to the conclusion that the Niagara Falls are 31,000 years old and the river of 32,000 years' duration; also that the Huron drainage turned from the Ottawa river into Lake Erie less than 8,000 years ago. Lastly, if the rate of terrestrial deformation continues as it appears to have done, then in about 5,000 years the life of Niagara Falls will cease, by the turning of the waters into the Mississippi. These computations are confirmed by the rate and amount of differential elevation recorded in the deserted beaches. It is further roughly estimated that the lake epoch commenced 50,000 or 60,000 years ago, and there was open water long before the birth of Niagara in even the Ontario basin, and that under no circumstances could there have been any obstruction to the Ontario basin, if even then, later than the end of the Iroquois episode which has been found to have ended 14,000 years ago."

A New Bone-Cave.

In a recent letter to Sir Henry Howorth, Mr. H. C. Mercer, of the University of Pennsylvania, announces the discovery of an interesting bone-cave in that State. It was discovered during blasting operations, in a limestone quarry, and is close to a similar cavern which was opened in 1870, and some of the remains from which were described by Cope. The whole is filled to the roof with a stratified deposit, consisting of fragments of limestone, clay, and sand, in which numerous bones occur. These are all broken, and are said to include remains of mastodon, tapir, sloth, peccary, ox, bear, and, probably, of birds. No remains of man, fishes, or mollusca have yet been met with.

Mr. Mercer is carrying out the excavations with great care, and it is to be hoped that a valuable addition to our knowledge of the cave fauna of North America, at present very imperfect, may result.

THE JAVANESE SKELETON.

RECENTLY there has appeared a quarto of 39 pages with two plates published in Batavia and written by E. Dubois. It is entitled "*Pithecanthropus erectus*, eine Menschenaenliche Uebergangsform aus Java," and, not unnaturally, the title raised hopes that the great gap between the Anthropoid Apes and Man had been bridged over at least partially, and that the long-sought "Missing Link," so dear to popular imagination, had at last been found. One of our correspondents, of some authority in these matters, considers that the facts brought forward appear capable of a different interpretation from that put upon them by the author. The specimens described were obtained from a bed of andesitic ash in the neighbourhood of Trinil, on the river Bengawan in Java, and consist of the upper portion of a skull, a molar tooth, and a left femur, which are regarded as having belonged to a single individual. The femur was found some fifteen metres from the other remains, so that its association with them, though highly probable, is not certain. This bone is certainly human, for it agrees in all important respects with the femur of a man of average height. The most significant point about it is that it is diseased, a considerable irregular growth of bone having taken place on the inner side, a short distance below the head. The skull, of which only the upper and hinder portion is preserved, is described as dolichocephalic, and as being distinguished from the skull of the Anthropoid Apes by its larger size and more arched forehead. Its length is 18·5 cm., and the capacity of the brain case is calculated to have been 1,000 cubic cm., but the data for this latter measurement are insufficient owing to the complete absence of the whole of the lower portion. If, however, we suppose the cranium to have had approximately the cubic contents given, it would be in this respect about two-thirds the size of an average human cranium and about twice that of the skull of a middle-sized gorilla. The sutures are all closed and there are no crests for the insertion of muscles. Examination of the photographs of this specimen shows that the whole surface of the bone is rough, being covered with irregular pits, a condition indicating that, like the femur, it was diseased. It seems, therefore, most probable that the skull owes its peculiar form to the premature closing of the sutures, and that it belonged to a microcephalic human being. It may be remembered that the Neanderthal skull, to which great importance was attached at the time of its discovery, has been held by Meyer and Virchow to be pathological. Although Java is, perhaps, a not unlikely place to find the remains of an immediate ancestor of man, that discovery is yet to be made.

A Little Knowledge.

It is cheering to note the elevation of the masses by the spread of education. Even your journalist begins to have some glimmerings of a world beyond his own, and, like a child in the arms of his bathing-woman, splashes ineffectually in a sea of unfamiliar words. Monarchs and premiers are laid on the shelf, China and Japan bicker in obscurity, while he, the purveyor of novelties, rushes after that inspired creation of our prince of journalists—"the Pliocene Plesiosaurus." But, of a surety, all these penny-a-liners must now confess themselves conquered by Autolycus. These are some of the delicious sentences in which the erudite trifler of the *Pall Mall Gazette* introduces an article on the chair:—"Primitive man discovered the chair; not, of course, the chair as we know it, but the embryonic, nucleated chair. With the eye of sympathetic imagination we can see that poor untutored, unenfranchised ancestor of ours at work upon the task of discovering the chair. It is the evening of a day millions of years back; the deep primæval forest resounds with the bellowings of the mastodon, the howls of the plesiosaurus, the squeakings of the ornithorhynchus, and the gibberings of other creatures with names as terrifying as themselves; and the sun is going down in the west. Our ancestor has been chopping wood and swearing at that blunt stone adze of his." This is charming—but a little inartistic, is it not? to make the sun set in the west. There is a want of imagination about that statement, little in harmony with nucleated chairs and forest-loving plesiosauri. Seriously to consider it, this is as though Reuter were to inform us that the Queen of Sheba spent a karyokinetic afternoon riding down Broadway on a mammoth, and finished up at Delmonico's with a supper of fried trilobites. The truth and the humour of it are on a level.

It is to be feared that Autolycus derives his knowledge of prehistoric man, as many self-constituted critics make their sole acquaintance with Ibsen, from the pages of *Punch*. But we are sure that Mr. E. T. Reed does not, any more than Mr. Anstey, desire his humorous sketches to be regarded as even an approximation to the truth. Nevertheless, Mr. Reed will pardon us if we suggest that his earlier imaginings, such as "A Naval Battle" and "A Slight Difference with the Local Mammoth," were more truly humorous than those later ones in which impossibly post-dated caricatures "run riot o'er the land." There is a sesquipedalian humour about some of these monsters that is by no means convincing. It really is not right of this ingenious artist to confuse the minds of his public and his fellow-journalists, and above all to lay such traps for poor Autolycus.

But perhaps this is hypercriticism. What are a few million years more or less? "Aliquando bonus dormitat" even Lord Kelvin. And surely a slight mixture of periods is not to be imputed for a fault to those who, like Autolycus and Shakespeare, write "not for an age, but for all time."

A Progressive Learned Society.

The Royal Geographical Society has been planning certain measures of reform, some of them of considerable interest. Thus the Society has not only reconstructed its premises so as to afford greater facilities of reference in its Library: it has added a new Reading-room, and endeavoured to better the accommodation of its Fellows generally. They may use the Council-room for the purposes of reading or of writing letters, and in another room they have the privilege of conversation and tobacco. These efforts to give the Society some of the advantages of a club are most praiseworthy, not only from a social but from a scientific point of view; and they might well be followed by other learned societies. As a rule the privileges of Fellows of these Societies are practically confined to the consultation and borrowing of books, and to the attendance at evening meetings. Little or no social intercourse is possible owing to the want of a conversation-room; nor is any adequate provision made for the writing of letters, &c.

The Royal Geographical Society, it is true, performs some functions that other societies might find it difficult to adopt; its map-room, thanks to Government aid, is open to the public, so that anyone may go in and learn what is known about the geography of any portion of the world. Nevertheless the Society labours in other ways which serve to promote the study of geography, and which might be followed by some of the older but less energetic societies. It organises lectures both technical and popular, and aids travellers and explorers in a variety of ways. Perhaps the most important of its departures is its federation, at present informal, with other societies in Liverpool, Manchester, Newcastle, Edinburgh, and other large towns. This alone is a subject most deserving of the attention of scientific societies, for by such means, in due time, the great and accumulating burden of literature might, to a certain extent, be concentrated and generally lightened—to the great benefit of those now living, and to the greater benefit of those who come afterwards.

"The Zoological Record" in Parts.

We desire to direct the attention of such of our readers as may be engaged on original zoological research to the letter from Mr. S. Pace that we print in this number. The general complaint as to the present system of selling the *Zoological Record* in one bulky and expensive volume, to those who really need only a few pages maybe of it, was admirably voiced in our own pages not long ago by the Rev. T. R. R. Stebbing in his vivacious article, "On Random Publishing and Rules of Priority" (vol. v., p. 341).

No one has ever been able to understand why the authorities at the Zoological Society persistently refuse the persistent demand for the sale of the *Record* in separate parts. A few despairing students have at last put on Pace to break the *Record*, and we trust that their effort will meet with the success it deserves, especially as it cannot possibly injure the finances of this ponderously respectable publication. The fatuity of the present enforced alliance of diverse groups is obvious to any dipper into the volume; for the diversity of treatment meted out by the recorders to their respective groups is no less than that of the groups themselves.

In this connection we may state that, as at present understood, the Central Zoological Bureau, projected by Dr. Field and others, intends to issue its slips and its Record in parts, according to the various subjects. This alone would be enough to give it an advantage over the present *Zoological Record*. In reply to many enquiries, we take this opportunity of stating that Dr. Field may be addressed at No. 67, Rue de Buffon, Paris, but that, during February, he will be in England, intent on advancing his project.

Warning Colours.

Professor Felix Plateau, who is so well-known by his work upon the sense of sight in insects, has lately published an interesting paper upon the "Magpie moth." This insect has often been quoted as an example of "Warning colour" in all three stages of its existence. The white, yellow, and black of the caterpillar are repeated in the moth, while the pupa is brown, banded with yellow. Professor Plateau carried out his studies in so thorough a fashion that he not only caused other animals to eat the caterpillars, but ate them himself, "after some natural hesitation." The flavour, instead of being disagreeable, proved to be the reverse, reminding him a little of almonds. No less than 43 per cent. of the caterpillars are devoured by ichneumon flies, while spiders, beetles, a few birds, and, according to the experiments of Mr. Beddard, quoted by Professor Plateau, certain monkeys and other animals will eat them with pleasure. It is difficult therefore to see exactly where the advantage of the warning colour comes in. So small a percentage could be saved by this means. Professor Plateau's conclusions are very much the same as those arrived at by Mr. Beddard in an article upon the "Magpie caterpillar" in the *Gentleman's Magazine* for 1890, but they are based upon a larger series of experiments. It would be as well if all the examples of warning coloration were subjected to asc areful an examination. Professor Plateau's paper is to be found in the most recently issued part of the *Mémoires de la Societe Zoologique de France*.

Mimicry.

Professor Plateau, however, is not entirely against current theories of the kind. But he appears to think, and rightly, that they have been a little too much used as an universal explanation. In a paper in *Le Naturaliste* figures are given of the "Merveil du Jour" and of the "Scarse merveil du jour," two moths which have mottled green fore-wings, and which in a woodcut look very much alike. They are not, however, quite so much alike when seen alive; but the resemblance is just as good as that shown in many cases that are put down to "Mimicry." In this case, however, there can be no possible question of advantageous mimicry, for the two insects appear at different times of the year. The scarcer insect delights the collector in the New Forest in June, while the common "*Aprilina*" is abundant everywhere in early autumn. The advocates of Mimicry have shown a regrettable tendency to ignore examples of this kind, which are by no means rare.

"Letusimulation."

We are no unfriends of neology, holding that, when a new idea has to be presented, or an old idea divided, it frequently conduces to clearness and accuracy to coin a new word rather than to stamp a new meaning on an old word. A writer in a recent issue of the *American Naturalist* gives some pleasant notes on the well-known habit possessed by many animals: the feigning of death when they are touched. So far so good. He chooses to call this habit "Letusimulation," and tells us that the word is derived from *letum* death and *simulare* to feign. Could anything be more ridiculous? *Letum* really is not a Latin word at all, but is an obvious loan from the Greek, occurring in a few late authors. But even were it a Latin word, the English derivative would be letisimulation. Even were the form correct, what object in this world of trouble is to be gained by inventing a barbarous polysyllable for an idea that has its perfectly simple English form, and that under no possible circumstances could be confused with any other idea? The inventor has already taken a further step in the propagation of his nonsensical jargon; for he tells us that he has contributed to the *Atlantic Monthly* a paper on "Animal Letusimulants," meaning, of course, "feigners of death." If the editor of the *Atlantic Monthly* cannot project off his own bat such a title into the waste-paper basket, he should ask advice from some intelligent man of science before he accepts a new phraseology as ugly as it is futile.

An American Snake-eating Snake.

Mr. Angus Gaines, in a recent number of the *American Naturalist* (1894, p. 970), gives an account of the exploits of a specimen of *Ophibolus getulus*, captured and kept in captivity. This small snake rivalled a recent feat at the Zoological Gardens.

Mr. Angus Gaines writes:—"After he [the *Ophibolus*] had been in my possession for 25 days, I captured a *Eutænia radix*, which I put in the same enclosure. The other snakes paid no attention to the newcomer, but the *Ophibolus* roused at once, as if scenting a natural enemy, and seized the *Eutænia*. The fight was long and fierce, for the *Eutænia* was strong and active, and was five inches longer than his assailant, but the *Ophibolus* gained the victory and undertook the seemingly impossible feat of swallowing his victim. This task occupied the whole night, but he actually succeeded in swallowing the snake five inches longer than himself. This very hearty meal distorted him beyond recognition, and he gave no signs of life except by a slight twitching of the tail. After an absence of some 40 hours I revisited my terrarium, and found that he had disgorged his prey and resumed his proper shape.

"Since that time the *Ophibolus* has taken no food, though he is still strong and active; his spots, however, which were originally of ivory whiteness, have assumed a sulphur yellow hue.

"I tried placing a looking-glass in my terrarium, and the *Ophibolus* showed signs of excitement at the first sight of his reflection, but afterwards paid no attention to it.

"My *Ophibolus getulus*, 12½ inches long, after going fifty days without food, except the one snake which it subsequently disgorged, killed and ate a *Natrix sipedon* over eight inches long, and is doing well."

A New Plant.

New forms of life are always interesting; but they are specially interesting when they belong to the strange debatable land that lies between the lower confines of the animal and vegetable kingdoms. The recent issue of the *Annals of Botany* (vol. viii., no. 32) opens with an account of a new alga-like organism given by Mr. B. M. Davis, who found it in the salt marshes of the Charles River, Cambridge, Massachusetts, where it covered the stems of marsh-grass and other objects so thickly as to give their surface the appearance of a dark green velvet. The organism is a very lowly one, and its exact position is dubious. Mr. Davis, however, after a careful description of its structure and life-history, gives reasons for placing it on the plant side of the border, its nearest relations being some genera of the family Tetrasporeæ. *Euglenopsis*, as the author names his new genus, from certain resemblances to *Euglena*, shows an extremely peculiar structure and mode of growth. It consists of branching filaments composed of empty cell-cavities or compartments, the ends of the branches bearing green cells. The larger specimens reach about a fourth of a millimetre in height. The terminal green cells contain protoplasm, in which we can distinguish a nucleus, two spaces containing cell-sap, and a green band or chromatophore, in which is a bright red pigment-spot. The protoplasm may escape from the cell-wall and become a motile organism comparable with the large zoospores of many algæ. Each zoospore is provided with four motile filaments or cilia at the lower end, by means of which it swims. The change from a stationary to a motile condition occurs during the night. When specimens were kept in an aquarium, swarms of zoospores collected, each morning, on the sides of the glass towards the light, which, therefore, exerts a directive influence similar to that observed in the case of other algal zoospores. After a time a resting condition is resumed, the organism protecting itself by a new cell-wall.

The branched filament characteristic of the adult is the result of a unique method of growth. The cell has assumed the resting condition, and its protoplasm contracts, leaving the lower end of the cell but remaining attached above. This then is drawn entirely into the upper half of the cell and the empty half becomes shut off by a transverse wall. Similarly a second and a third empty compartment may be shut off immediately above the first. Numerous repetitions of this process occur, and the adult plant is characterised by being composed of a series of empty compartments ending in a true cell. Branching results from a longitudinal but oblique division of the protoplasm into two cells, each of which grows in length, the upper continuing its straight course, the lower being pushed out laterally and thus forming a branch which repeats the mode of growth of the main stem filament.

The Growth of Wounded Roots.

In a recent number of Natural Science (vol. vi., p. 9) we referred to some experiments by which Dr. Pfeffer had shown the root-tip to be the seat of sensitiveness to the stimulus of gravity. In the issue of the *Annals of Botany*, mentioned in the foregoing note, our readers will find an account of some work on the same lines by an American botanist, Professor V. M. Spalding, carried out in Pfeffer's laboratory at Leipzig. The subject is the investigation of the curvatures, styled traumatropic, which follow the infliction of wounds on the tip of growing roots. These phenomena were first studied by Charles and Francis Darwin and described in the "Power of Movement in Plants." Young seedlings were allowed to grow in moist air, and a small piece of card was fixed on one side of the tip of the short rootlet by means of shellac dissolved in alcohol. A large proportion of these rootlets became considerably bent, curving away from the side to which the object was attached. A similar result followed when nitrate of silver was used as an irritant, or when thin slices were cut off parallel to one of the sloping sides of the apex.

In their explanation of these experiments the authors concluded that sensitiveness resided in the tip of the root, a theory which Pfeffer's recent ingenious experiments have put beyond doubt; and also that extremely slight pressure or simple contact was a sufficient irritant to induce deflection. Detlefsen repeated the Darwins' experiments, and came to the conclusion that the curvatures were simply a mechanical result of an injury to the root-cap. When this is injured the tissues beneath are partly released from strain, and extend more rapidly than those on the opposite side, thus causing convexity of the side affected. Professor Spalding, however, observed that curvatures in the radicle of a bean follow a branding of the tip after the removal of the root-cap. He also finds, as Wiesner proved ten years ago, that the curvatures do not result from simple contact, and from several experiments adduces strong evidence that they are the result of an injury to the growing point. Thus they follow a small injury which extends to the growing point but fails to take place even after extensive injury in which the growing point is not involved. Some of the most interesting cases cited are those which show that the stimulus to a curvature may remain latent during an artificial suspension of growth. It was found that roots which have been wounded may have their growth in length stopped by confinement in plaster casts for several days, and that traumatropic curvature still takes place when they are released and growth is resumed.

Finally, the author claims that the experimental evidence adduced justifies the belief that the growing point of the root is sensitive, and that its irritation induces the curvature in the zone of rapid growth behind the root-tip. Such results are comparable with those obtained in the study of curvatures induced by the stimulus of light and gravity. For instance, in the case of the cotyledons of grasses the tip has been shown to be most sensitive to the directive action of light, the stimulus being transmitted from the tip to the lower part, where, after an interval of time, the corresponding curvature is observed.

SIR HENRY HOWORTH'S COMPLAINT.

In a letter, printed in our last issue (NATURAL SCIENCE, vol. vi., p. 71), Sir Henry Howorth complained that two specialists, who are both distinguished men and both officers of the same museum, have within a year of one another published two volumes on reptiles in which the same forms have "not merely different names but are put in different genera." We find that Sir Henry Howorth is quite right in his facts. Indeed, we are assured by those who know that some sixty per cent. of the species are called by different names in the two works. Certainly it is a matter for regret that divergence of opinion so extreme should exist between two high authorities from whom the public may expect guidance; and the public will be still more puzzled to find that in the case of the British Museum Catalogue the same authorities are in amiable conjunction, the one as author the other as editor. But it may be pointed out that the fact of both being officers of the same museum has nothing to do with the case. In the official Catalogue the two agree; but the trammels of office must not hinder free expression of opinion on scientific matters, even on specimens contained in the museum, when that expression is made in an unofficial and purely private publication, such as the *Biologia Centrali-Americana*.

On the general question of divergent nomenclature we think that Sir Henry Howorth's complaint is exaggerated. The whole tendency of modern nomenclature is to render it less and less dependent "on the personal equation of the particular writer." Existing confusion has arisen from two distinct causes. First, in old times different names were given by writers to the same species, because they ignored or were ignorant of each other's work; the name given by the most arrogant writer was as a rule the one that usurped the field, at all events until the law of priority was put in force. Second, ignorance either of facts previously published, or of facts as yet undiscovered, caused the reference of many species to the wrong genera. To retain for such species the names by which they were described originally, as Dr. D. Sharp once suggested, would be to render names of no meaning, to turn them into mere numbers in a catalogue, and to abrogate the Linnæan system.

These two facts account sufficiently for the existence of different names for the same animals. To complain of the regrettable fact that two persons should take different views of the same question is merely to cry out against human nature. Personally, we are on the side of the more modern view. We think that the law of priority, dating from an accepted standard like the tenth edition of Linnæus's "Systema Naturæ," must determine the specific name, and that the reference of species to genera must depend upon the completest and surest anatomical information, however much such may disguise familiar animals under unfamiliar names. But in these matters there is room for "personal equation"; in the matter of anatomy always; in the matter of the first specific name, when the type-specimen is unknown or the description vague. It is little wonder that there may be capable zoologists of conservative habit who prefer the names that they consider to be stamped by long usage to names that they might admit to be more philosophical. That differences of the kind mentioned by Sir Henry Howorth should occur in two publications, the one a private production, the other the catalogue of a museum, we take to be unavoidable and natural. On the other hand, were such divergence to occur in the nomenclature adopted in a single volume or in a single museum, unhesitatingly we should assert it to be a fault of the gravest nature.

His Own Petard.

The *Journal of Botany* has taken on itself to criticise a few fairly obvious misprints that lately crept into the *Geographical Journal*. In his eager anxiety to throw stones, the editor has omitted to test the walls of his own house. In three lines of the very paragraph in which he insists on the wickedness of spelling botanical names incorrectly, "*Hemichrysum*" is first misquoted as "*Hemichysum*," then blunderingly corrected to "*Helichysum*," when all the world knows that it should be "*Helichrysum*." By this time the editor of the *Journal of Botany* has probably learned that the wiles of the printer are not to be combated by the "Assistant-Secretary of the Royal Geographical Society, under the authority of the Council," any better than by his own learned self.

But if the editor of our esteemed contemporary is sincere in his wish to reform the orthography of scientific periodicals, he will not be offended if we suggest that he might supervise with greater care and consistency the spelling of geographical names in his own journal. In a single paper in his last volume, Lake Naivasha is spelt in one place "Nawasha," in another "Navaisha"; "Durama" we may guess to stand for Duruma, and "Nakuru" for Nakuro, while "Inhuyuni" may mean Mkuyuni—but if so, as it is said to be "inter Nawasha [*sic*] et convallem Baringo," its position has been altered even more than its spelling; finally the poor botanist who searches in the Ulu Mts. for a second specimen of the species recorded from the "Ulau" Mts., is not unlikely to be disappointed and to have to tramp off to the very different and distant range of Mau. With these little eccentricities all in one paper, it is hardly worth quoting from the rest of the volume; and doubtless it is only as a practical illustration of the advantages of "home rule" that the editor allows his contributors to spell the name of an important botanical district either "Cameroons" or "Camaroons" as each may prefer.

Mollusca as Purifiers of Water.

The following interesting note appears above the signature of Charles Hedley in the *Journal of Malacology* for December 12th :—

"A use, novel to me, of pond snails by the Chinese silk growers is described in an official work which caught my eye by chance. This waif of malacological information is so certain to escape recorders that I transcribe the passage.

"'Report on Silk. Imperial Maritime Customs of China, ii., Special Series, No. 3; *Shanghai*, 1881, p. 57.

"'The water used for reeling silk is taken from mountain streams, as being the cleanest; the water from wells is never used; and if mountain water cannot be had, river water is taken, which is cleaned by putting a pint of live shellfish to one jar of water. There is a special kind of shellfish, called the pure water shellfish [here follows the vernacular name in Chinese characters] (fig. xxiv.), found everywhere in ponds, wells, and creeks. They first of all sink to the bottom of the jar, and then by degrees make their way up its sides, consuming gradually all impurities in the water within half a day or so. After the clean water has been drawn from the jar, the shellfish are cleansed and put to the same duty again.'

"As the three coloured figures are drawn in Chinese perspective the species cannot be certainly identified. They are, however, sufficiently like Reeve's figure (Conch. Icon., vol. xiv., *Paludina*, pl. iv., f. 18) of *Vivipara chinensis*, Gray, to assume that this the artist endeavoured to portray."

I.

The Mammals of the Malay Peninsula.

Part II.

CARNIVORA :—Probably there is no part of the world which for its size contains so many animals of this order as the Malay Peninsula. Very little is known of the habits of most in the wild state, since almost all are nocturnal and most live concealed all day in the masses of creepers at the tops of the bigger trees.

The tiger or "Rimau" (*Felis tigris*) is still over-plentiful in Singapore, and few of the larger forests are without one or more for long; but so dense is the undergrowth, and so quiet is the animal, that it is rarely seen and very seldom shot. It inhabits the more open country and small patches of forest. In the dense hill-forests of the interior it is not usually found, and is replaced by the black panther. The animals are seldom very large, and are light coloured. They habitually swim over to Singapore, across the Johore Strait, usually by way of the intermediate islands of Pulau Ubin and Pulau Tekong. They make the passage at night, landing in the early morning. As so much of the coast is mangrove swamp, and the animals do not risk going through the mud, they are only able to cross where the shores are sandy, and thus they have regular starting and landing places. They often come over to Singapore for breeding, and the slopes of the wooded hill Bukit Timah and the sandy woods of Changi, near one of their favourite landing places, are the usual localities selected. The young are brought forth in December or January as a rule, and parents and young remain together in the same locality till about Easter, when, as by this time the food in the locality is exhausted, the family scatters, and either wanders over Singapore, or, if the young are old enough, swims back to Johore.

During the day the tiger remains sleeping in the shady parts of the jungle, unless the weather is wet and windy, when it becomes restless and wanders about. About four or five o'clock it rouses itself and begins to roam about in search of prey, continuing its walk till about eight o'clock in the morning, when it again retires to sleep. When very hungry it will seek food in the daytime, and has been known to attack bullock carts in broad daylight in Malacca. It lives for the most part on pigs, wild and tame, deer, dogs, fowls, and mousedeer, but it has also been seen in the mud of a mangrove swamp digging up and devouring shell-fish.

When hungry the tigers are very bold; thus, on one occasion, a tiger invaded the house of a European near Singapore in the night and stole a joint of beef which was in the kitchen. One night a tiger entered the open door of a Chinese hut on the edge of the jungle in Pulau Ubin, and walking through the ashes of the fire (where I saw its footmarks), broke through the lattice-work wall of the house and went away, to the relief of the Chinaman. The following night four tigers walked up the steps of another house close by, apparently in search of the owner or his dog; by breaking through the back of the house the inhabitants managed to escape, leaving the house to the tigers.

The tigers are usually quite harmless to human beings, but now and again take them. Wallace mentions that in his time a man a day was killed in Singapore. This is easily credible, the forests were then being cut down, and many Chinese were employed in this work, and being scattered over the jungle, were doubtless easily taken. In Singapore of late, till the last two years, the average was one native every two months. The number of deaths from tigers given by Wallace and Jagor has been ridiculed as improbable by some writers, who appear to have derived their information from the Police Reports, ignorant of the fact that many such deaths do not get reported to the police for the following reason. The chief people killed are the Chinese gambir- and pepper-coolies. Now, on a Chinese plantation, coolies are not allowed to talk of tigers, for fear of frightening each other. Even if a tiger is seen, a coolie is not allowed to mention it, and, if a man is killed, he is buried quietly and a false return of death given if possible. This is done to prevent the coolies from being frightened and leaving the plantation.

The two popular fallacies still to be found in some Natural History books, that a tiger when once he has attacked and eaten man becomes a man-eater and that it is only very aged and toothless tigers which devour men, have elsewhere been shown to be false. Sometimes one or a couple of tigers will take to man-eating regularly, but this is not common here, and has never happened as far as I know in Singapore, while those which have done so and have afterwards been shot have usually been found to be fine young beasts. The Malays often talk of the "Rimau Kramat," a sacred tiger, which is stated to be a very old hairless and toothless tiger, perfectly harmless and quiet. I have been shown footprints of very large animals said to be "Rimau Kramat."

As has been said, it is usually Chinese coolies who are taken by tigers. Working early and late in the gambir-fields, their bare brown skins are sometimes mistaken by the tiger for those of the deer which often come in the dusk or at dawn to browse on the gambir shoots. Rushing on the unsuspecting coolie from the long grass or scrub where he has been lying in ambush, the tiger strikes him dead with one blow of the paw on the shoulder. Sometimes the body is left untouched, often it is dragged to the jungle and partially devoured, the thighs and throat being first eaten. It appears to be rather an exception here for a tiger to return to the kill.

Sometimes a man is watched by a tiger apparently for some days before the latter can make up his mind to take him, and at times is stalked from a considerable distance. The following is an instance of a fairly common kind of occurrence which took place in Singapore in 1890. Two Chinese coolies were returning from a gambir-field at six p.m.; one, delaying for an instant, did not overtake his companion, who presently missing him, called out, but got no answer, and so went on to the house. After a short time, being alarmed, he, with his companions, returned to the spot with lights—for it was by this time dark—and finding a pool of blood, all ran back to the house. Next morning the body of the man was found in the jungle, lying on the face, with the thighs eaten. The tiger must have been stalking the men from the patch of jungle, and must have crept up to them through the gambir-bushes for fully five hundred yards, and then struck the coolie dead noiselessly by the side of his companion. The body was removed to the house, and I was informed that the tiger visited the house the next night and took a fowl away, and continued to come each evening till the body was taken to town for burial. Though the tiger remained for at least some weeks in the same jungle, it never attacked any more of the coolies.

The Malays have many superstitions and stories about tigers. Certain people are supposed to have the power of turning into tigers for a short time, and resuming their human form at pleasure. The transformation commences tail first, and the human tiger is so completely changed that not only has it all the appearance and actions of the tiger, but on resuming its human form it is quite unconscious of what it has been doing in the tiger state. A much dreaded form of demon is that of a headless tiger which is supposed to be seen rambling about at night.

The black panther (*Felis pardus* var.) is called "Rimau Akar" (*lit.* tiger of the Lianas) by the Malays, probably because it lives in the masses of creepers in the big trees, though I have no evidence of its being arboreal. It is said to have occurred in Singapore, but this appears doubtful. It is abundant in Johore, and formerly occurred in Pulau Ubin between Singapore and Johore. It appears to go further into the hill-woods of the interior than the tiger. Very little seems to be known of its habits. It is quite harmless to man unless wounded, and lives chiefly on goats, fowls, and dogs. In captivity it is always very ferocious, and never appears to be at all tamable. The spotted form is at any rate rare in the south of the Peninsula if it occurs at all, but it appears to be fairly common in Perak and the northern part of the Peninsula. The more slender form, commonly called the leopard in opposition to the short thick panther, is said to occur in the Peninsula. The relations of these forms or subspecies in the Malayan region are well worth the study of local naturalists.

The smaller cats are very numerous in the Peninsula, but owing to their nocturnal habits very little is known of them in the wild state. They appear to live all day in holes in trees or high up among the creepers, coming out at dusk in search of prey. In captivity they usually remain motionless all day. A *Felis planiceps* which I have in captivity, remains in one corner of its cage, without moving, till night, when it comes out to take its food. The commonest wild cat is *F. bengalensis*. I have seen it in Singapore, and it appears to be abundant in the Peninsula and to be often trapped. *F. tristis* has been taken in Malacca; and I had a fine golden cat, *F. temminckii*, from Pahang. The latter was very quiet in captivity, but was never at all tame. When taken young *F. bengalensis* becomes very tame and playful, and lives a long time in captivity, but trapped adults are always ferocious and ill-tempered. The native name for a cat is Kuching; wild ones are called Kuching Hutan (wood-cats) and the large ones Kuching Rimau (tiger-cats).

The Viverridæ are well represented here. The commonest species is *Viverra malaccensis*, the Musang. It inhabits hollow trees or masses of creepers, or very commonly takes up its abode in the roof of a house, leaving its hiding place in the dusk and rambling about in search of food. It is very regular in its habits. I have had no less than seven living in the roof of the house at one time. The animals used to leave the house about six p.m., descending by one of the posts of the house, and would return at nine o'clock, leaving again later in the night, and coming back at about five a.m. Once I saw Musangs moving about in the top of a tree at midday; they were an old one and one or more young, which the adult was apparently teaching to walk on the boughs. Musangs are very clever at climbing, far more so than a cat. A pet one, belonging to one of the officers, used to walk skilfully on a very thin string; put on the tightened twine, it would grasp it with its fore-paws and draw itself up, and then balance itself by waving its tail round and round, or even by clasping the string with its tail, which is slightly prehensile; when it had got its balance it would walk along the string briskly, carrying the tail free. The Musang feeds chiefly on fruit, but also devours birds, and is a great nuisance to pigeon- and chicken-fanciers. It is very serviceable, however, in keeping away rats from a house.

It plays an important part in the dispersal of seeds, eating a great deal of fruit, and dropping the seeds on paths and bare places on the ground, where they speedily spring up. It seems particularly partial to the fruit of *Strychnos tieute*, in which the seeds are enclosed in a very bitter pulp, apparently rich in brucine. It is most troublesome to fruit growers, and especially in the coffee-fields, where it devours the coffee-berries and passes the seeds uninjured. As it always selects the best berries, the seed passed by it is usually considered the best for planting, and indeed has fetched a high price in the coffee-markets. Musangs are usually easily trapped, but after a few have been caught the rest become wary. The common form of trap is a hemicylinder of sticks about 3 feet long and about 8 inches across. This is propped up with small sticks after the manner of a figure-of-four trap, and some heavy stones put upon it. A plantain is put inside as bait, and the civet on entering the trap to eat it touches the supporting sticks which let fall the cage; and the stones prevent the civet from lifting it up again. Other modifications of this trap are also used. The civet cat is very easily tamed, especially when caught young, and makes a clever and intelligent pet.

The larger civets, *V. zibetha* and *V. tangalunga*, the "Musang Jebat," do not, I believe, occur wild in Singapore, but are common in the Peninsula, and are often trapped and brought for sale. They are never at all docile, and seldom live long in captivity.

The Water-mungoose (*Herpestes brachyurus*) is very rare in the Peninsula. A living example was presented to the Gardens by Dr. Johnston, who obtained it in Tringganu. The natives called it "Musang Babi," Pig-civet, because it bristled up its hair when excited and resembled somewhat a very small wild pig. It lives exclusively on fish, refusing meat, and is very fond of bathing. I know nothing more of its habits, and few natives have ever seen it.

The Bear-cat (*Arctictis binturong*), the "Bintūrong" or "Menūrong" of the Malays, is generally obtained in Malacca, and is sometimes kept as a pet. It is easily domesticated, and becomes very affectionate, and will follow its master like a dog. It feeds on fruit, also taking small birds, and is of arboreal habits, climbing about well and aiding itself by its prehensile tail, which it uses chiefly to lower itself from branch to branch. When suspicious it growls fiercely, ending up with a kind of barking spit; when pleased it makes a humming noise. It appears very nervous of snakes, turning its face away and protecting it with its fore-paws, whence I presume it is not a snake-eater. From its enemies it defends itself by trotting quickly forward and biting viciously. When very happy it jumps about with all four feet off the ground in a very comical manner.

The Common Bear (*Helarctos malayanus*), "Bruang" of the Malays, is so well known that it is hardly necessary to say anything about it. It is tolerably common in the Peninsula, but is absent from Singapore. Formerly, rewards were offered for its destruction, but it appears to be quite harmless to man unless wounded, when it becomes dangerous. It is, however, a troublesome enemy to fruit growers near the jungle in which it lives, being very partial to durians. A tame one, when it got loose, would often climb up a tree, and breaking off the branches, make a kind of nest in which it would sit for a few minutes; but I believe that in a wild state it lives, at least usually, in holes dug in the ground, or among ferns and bushes. It is exceedingly powerful for its size, and the Malays say that if it can get its back against a tree it is a match for the tiger.

Two species of otter have been met with in Singapore, viz., *Lutra sumatrana* and *L. leptonyx*; but they seem to be rare, and little is known about them. The Malays often call them "Anjing Ayer" (water-dogs).

Very little is known about the wild dogs of the Peninsula. The natives say that there are two species, one larger than the other, and that the smaller one climbs trees. One species seems certainly to be *Cyon rutilans*. I have had three wild dogs in confinement, one of which seemed to be decidedly a larger-built animal than the two received later. It became tame enough to touch. The cry was a yapping followed by a howl. It was very active, running up the side-walls of its cage to a height of about 12 feet. It never wagged its tail, but in the presence of other dogs arched it gracefully. These animals are said to hunt in packs, but are very rarely seen. I once found the tracks of a single one following those of a deer. The native name for them is "Anjing Hutan" (wood-dogs).

Rodentia:—These are tolerably numerous; but the smaller ones, rats and mice, are not at all easy to collect or study. I have attempted to trap them in the jungles, but, when caught, wild cats or civets constantly devour them in the traps ere morning, and the Malays are not clever, like the Dyaks, in catching them.

The Common Porcupine, "Landak" (*Hystrix leucura*), is still common in Singapore, but a number must be destroyed by the burning of the open country, in which they chiefly live. They are very destructive to the pineapples. The tiger kills and eats them, and I have found the remains of one so destroyed. The brush-tailed Porcupine (*Atherura macrura*) is not a native of Singapore, so far as is known; it inhabits the limestone caves in Pahang.

Squirrels are very common and easily observed, but the number of species is not very great. Two species, *Sciurus tenuis* and *S. vittatus*, are very abundant in woods and gardens. *S. bicolor* is rarer, and inhabits the thicker jungles. I have seen several other species in Singapore which I was unable to secure. *S. tenuis* is a very small and active squirrel, very abundant and destructive in the Botanic Gardens. It feeds chiefly on acorns and chestnuts, and plays an important part in the dispersal of seed. When it gathers an acorn or a bunch of chestnuts, it runs off to some distance to eat it, holding it in its mouth. Hanging head downwards on the bark of a tree, it begins to nibble the acorn, which frequently slips from its paws and rolls away unhurt. Some of the acorns seem adapted for this slipping, being covered with a thin, smooth, silky coat; in other cases, the fruit is so smooth and rounded that the squirrel can only bite it at the base, and they frequently begin to bite the acorn cup to get at this part, with the result that the acorn slips suddenly out and falls to the ground. Fuller notes as to the action of squirrels on the dispersal of seed will be found in a paper on the Dispersal of Seeds by Mammals in the *Journal of the Straits Asiatic Society*. This squirrel utters a sharp, bird-like, twittering cry when playing about or when alarmed, and also a double-noted cry, "Atcheh, Atcheh," when pairing. I have seen a pair of squirrels fighting briskly; they gripped hold of each other and fell in a ball from the upper boughs of a tree, but before reaching the ground separated and clung to the lower boughs, rushed up to the top, closed again, and again fell, till at last one fell on the ground and rushed off, pursued by the other. The nest is often made in a hollow tree, and is a large structure made of strips of bark and thin twigs, and lined inside with soft bast, which the squirrel tears off boughs of trees with its teeth. I have also found nests in the roof of a shed, in a plant of the prickly *Bromelia pinguis*, and in an Elk's-horn fern (*Platycerium*) which was suspended by a wire in a plant-house. In this nest was a single young one, which, on the fern having been removed from the house and put in another part of the garden, was found to have been carried away next day by the mother squirrel. The nest in the *Bromelia* plant was quite exposed and only about two feet from the ground, the squirrel trusting to the thorns on the edge of the leaves to protect its young, of which there were two. The little ones were covered with short, smooth, grey fur, and looked very unlike squirrels. *S. notatus* is a bigger squirrel, grey with a red breast. It is less common than *S. tenuis*, but far from rare. It has much the same habits, but does not hang head downwards to feed. Its cry resembles the striking of two pieces of wood rapidly together, and can be heard a long way off. It is easily kept in confinement and readily tamable. *S. prevosti*, Desm., Raffles' Squirrel, one of the most beautiful kinds in the world, is common in the Malay Peninsula, but I never saw it in Singapore. Its brilliant colouring—black, red, and white—makes it very attractive, and it is easily tamed. All these squirrels are most destructive to coco-nuts. Biting round holes in them and getting inside, they soon clean out the flesh ; they even sometimes put their nests inside the cleaned-out nut. They also destroy a great deal of other fruits, especially durians. The small boys in Kedah shoot them with stones from pellet-bows made of bamboo.

S. bicolor is a strictly arboreal squirrel which lives in the tops of the higher trees in the thick jungles. It is very variable in colour. The commonest form in Singapore is black with a cream-coloured belly. In the Peninsula it is usually entirely cream-coloured. It is remarkably docile, but much less active than the smaller species. When eating, this species sits transversely on a bough, grasping it with its hind feet, the head and body hanging down on one side and the tail on the other. It eats fruits of different kinds and also buds and leaves.

The red Flying-squirrel, *Pteromys nitidus* (Tupai Belang), is still common in Singapore, inhabiting the thicker jungles. It remains quite quiet during the day, but at dusk begins to move about. It climbs with some clumsiness to a high point on a tree, and then dives off to the next, up which it climbs again, and again dives off, and so travels to its feeding ground. It appears to be very fond of coconuts, and will attack any that are near the jungles which it inhabits.

H. N. Ridley.

(*To be continued.*)

II.

Antarctic Exploration.

THE restless activity and love of adventure which characterised the mariners of the Elizabethan era, and urged them on through difficulty and danger to cross the threshold of the unknown and to open up the seas of the world to the commerce of their native land, has lost none of its intensity and is as powerful in the Victorian age as in that of the Virgin Queen. As the area for exploration became less and less, and sea after sea yielded up its secrets, there remained for conquest only the ice-bound waters which surround the poles; and the problem of the "North-West passage" was persistently attacked, until its ultimate solution proved the worthlessness of the quest. Then Nordenskjöld forced his way from the North Atlantic eastward to the Pacific, to be followed by Wiggins, who is still engaged in his life's object of opening a trade route through the Kara Sea to the mighty rivers of northern Siberia, while Nansen, after having conquered the Greenland ice-cap, has embarked on the daring attempt to enter a supposed polar current, and drift with the ice across the Pole itself. So far from the glamour of the Arctic having lost its spell, its votaries are more numerous than ever, and all the maritime nations are pressing northward in a race for the Pole.

Of all these aspirants to polar fame few think of winning distinction for themselves and their country by exploring the seas of the Antarctic regions. Since 1774, when Cook in his wonderful second voyage reached Latitude 71° 10′ S., the 70th parallel of South Latitude has, it is believed, been crossed only twice, namely, by Weddell in 1823 and by Ross in 1842; and, marvellous as were the discoveries of the latter, we cannot but believe that, under the exceptionally favourable circumstances which fell to his lot, had he been possessed of all the appliances of modern science and aided by steam-power, the result would have been infinitely greater. Since that time, with the exception of the brief visit of the "Challenger," which, although possessing steam-power, was totally unsuitable for ice navigation, these seas have been unvisited, and it has been left for commerce to take up the problem where Ross left it fifty years ago: the hope of gain, as on so many previous occasions, has furnished the incentive for work which science was powerless to prosecute.

Under these circumstances, the fitting out of a fleet of whalers for the Antarctic, commanded by experienced ice navigators, and in every way suited for meeting and surmounting the peculiar dangers and difficulties attending such critical work, could not fail to be regarded with the greatest interest by modern geographers and naturalists, especially as it was announced that the surgeons of the vessels had been chosen for their special acquirements as naturalists and had been supplied by the leading scientific societies with instruments and appliances suitable for the investigations which they had consented to undertake. It was therefore hoped that the voyage, notwithstanding its purely commercial object, would yield results of very great interest from a scientific point of view also.

The origin of the expedition was entirely due to the energy of the brothers David and John Gray of Peterhead (11), themselves descendants of a line of ice-kings. They, seeing that the Arctic whale-fishery is rapidly becoming a thing of the past, have for the last twenty years been urging an expedition to the Antarctic seas, chiefly influenced by the reports of the numerous whales identical with, or of a species closely resembling, the Northern Right Whale (*Balæna mysticetus*), which are recorded to have been met with in these seas by Capt. Sir James C. Ross. Neither of the Grays was destined to take a part in the venture. Capt. John Gray is dead, and David Gray could not succeed in fitting out vessels from Peterhead. It therefore fell to the Port of Dundee to make the trial trip; and the "Active," "Balaena," "Diana," and "Polar Star" were fitted out, and sailed from that port about the 6th September, 1892. The Norwegians, ever alert and our keenest competitors in the Arctic fisheries, also sent out the "Jason" on the same venture.

The narrative of this voyage (1) has recently been written by Mr. W. G. Burn Murdoch, an artist who, at his own urgent request, accompanied the "Balaena," by the kindness of Mr. Kennis, the owner, and of Capt. Fairweather. Although rated in the ship's books as assistant surgeon, he was really a passenger. Had Mr. Murdoch's book been descriptive of any other portion of the world save a virtually unknown region, it would have been looked upon in the light of a very amusing production of no scientific value. But any contribution to the knowledge of the Antarctic seas, however slight, is possessed of interest, and the artistic way in which the scenery of this wonderful region is depicted is really of value. Mr. Murdoch is often flippant and too frequently subordinates fact to fancy even beyond the limits of poetic license, but there are occasional little bits of descriptive writing which are both truthful and poetic.

The "Balaena," a barque-rigged vessel of 417 tons gross burden, and 65 horse-power auxiliary screw, commanded by Capt. Fairweather, an experienced and successful whaler, left Dundee on the 6th of September, 1892, with a crew of 45 all told. Her passage south was much delayed by bad weather, and the 21st of September found her barely clear of the Irish Coast.

On the 24th of November, in Lat. 40° 39′ S. and Long. 48° 57′ W., "many hundreds of small whales or porpoises" were seen travelling south; "they resembled the American drawings of the pigmy sperm, but had a larger dorsal fin" (p. 141). Mr. Murdoch remarks in a footnote that on the return voyage in the following March a similar abundance of apparently the same species was met with nearly in the same position, and adds that "Almost all the whales and porpoises we saw south of the line on our voyage out were travelling south or south-east, and those we saw on the voyage home were travelling north with their young. . . I conclude they have a grand nursery down in the ice, where they bring forth their young in the Antarctic summer, and come north when the winter sets in." The whales seen in such numbers were probably a species of Ziphioid, which would appear to be as strictly migratory in these regions as its relative in the Northern Hemisphere.

Many birds were seen about the 1st of December as the ship approached the Falkland Islands; but unfortunately, owing to the author's hurried exodus from Edinburgh, and we presume also that of his companion, the "Naturalist" of the "Balaena," he was unable to bring books on bird life in the South Seas, and therefore was only able to give them names by which they would scarcely be recognised by "scientists" at home. Stanley Harbour in the Falkland Islands was reached on the 8th of December, and here bird life seems to have been varied and abundant: "hundreds of divers and ducks scurried over the dull green water, splashing and diving—waiting at times till we were nearly on the top of them before they moved away. Gulls and petrels flew from the shores and circled round our masts—strange, unfamiliar, silent birds, with a quaint, old-world look and odd colours, as if they had been designed for a pantomime, or had just flown out of a Noah's Ark. Some of them were the gigantic petrel, I think—big, clumsy birds, nearly as large as an albatross, with coarse feathers of a raw chocolate colour, and big, yellowish beaks; some of these birds were almost entirely white. Some of the gulls were like our black-backed gulls, with a band of red on their yellow beaks. There were also molly mauks, and a pretty gull of a French-grey colour, with black wing-covers with white edges, and brilliant red beak and legs. Besides the petrel and gulls there were many kinds of divers and ducks, white-breasted shags, and several varieties [*sic*] of penguins. The last only showed their heads above water, as our cormorants sometimes do at home. Sometimes schools of them leapt clean out of the water, making black-and-white half circles in the air, popping in again with hardly a splash. Such an island is a naturalist's paradise"—to reach which who would not brave the stormy seas about the Horn?

The "Balaena" remained at Port Stanley until the 12th of

December, during which time several excursions inland were made and some good birds obtained, including the curious Steamer Duck *Tachyeres cinereus*. The author speaks of the ground being covered with "heather," but corrects himself: "it was not heather on which we reposed, but *Empetrum rubrum*, which is much the same at a distance, and is a sort of crowberry . . . Diddle Dee is its local name. I have a list of other plants of the islands—splendid names—*Gidmardia australis*, *Bostkovia grandiflora*, and the like, and I feel tempted to throw in a number here, but refrain. Neither does my companion [the Naturalist of the "Balaena"] approve of such inexpressive, unpopular names. Science is meant for all, not the few, he says, and we should call a spade a spade and not a bally shovel as the Bishop remarked"; and this notwithstanding the author's expressed difficulty in enabling the "scientist" to recognise the birds met with on a recent occasion, through ignorance of any but their popular names.

The Falkland Islands[1] were left on the 11th of December, on the 15th the "Balaena" met the first sheath-bills, and the next day in the afternoon the lifting of the mist disclosed to view a huge island of ice estimated to be half a mile long and 200 feet high, "the top as level as a billiard table." These typical Antarctic bergs soon became common enough, and were sometimes many miles in extent; their beauty and delicate tints of colour are described as passing conception. On the 17th the first seal was shot, the "crow's nest" was sent up, and on the 23rd the first outlier of the Antarctic continent in that longitude, one of the group of Danger Islands, was made. The whale lines were now coiled into the boats and a lookout set for the Right Whales that never came; any number of Fin Whales of various species were seen, but the main object of the long voyage was never discovered.

The numerous penguins were a constant source of interest and amusement. They were stupid in the extreme; instead of seeking safety in the water, they invariably jumped upon the ice, and even if forcibly thrust into the water returned to the charge totally unaccustomed to the new danger, fearing man probably less than the aquatic enemies which hitherto had been their only foes. Three or four species of penguin were seen, including the giant "Emperor" of which several were killed. Unfortunately the smaller penguins were found to be fairly good eating, and great numbers of them were killed. The enormous destruction of these defenceless birds that is the invariable concomitant of all these expeditions, scientific as well as commercial, must awake in the naturalist sad fears for the speedy destruction of the whole order.

[1] The reader will be glad to learn that the Company's coal-ship, about which the Agent was so anxious, was not 101 days "over-due" but 101 days out; it will also be a relief to him to learn that the Glasgow ship, the crew of which were "dying one by one" from scurvy, sailed three days after the "Balaena," having lost only one man.

On the 23rd of December the "Balaena" was joined by the "Active" and a few hours later by the "Diana"; the "Polar Star" put in an appearance subsequently, as also the "Jason." Presently seals were found to be numerous, and Capt. Fairweather, although practically unrestricted in his actions, finding that there was no chance of meeting with Right Whales, cleared the boats of their whaling gear and availed himself of the only opportunity of making his voyage a paying one. For this the Captain of the "Balaena" is severely taken to task by Mr. Murdoch, and doubtless it was disappointing enough to turn from the chances of new discoveries to the pursuit of "blubber";

THE MASSACRE OF THE PENGUINS.

The block kindly lent by Messrs. Longmans, Green & Co.

but Mr. Murdoch was fully aware of the commercial character of the voyage and had no reason for complaint. His question—"Is it not a hideous marvel that Dundonians should show such splendid enterprise as to send four ships out here for whales, and at the same time show total disregard for the scientific possibilities of such a cruise?" is singularly out of place when Mr. Bruce and himself had berths in the "Balaena"; while his severe strictures on Capt. Fairweather's ignorance and cupidity (p. 244) are scarcely borne out by the fact that the only two birds which reached the British

Museum from the expedition were the gift of the commander of the "Balaena."

At last, on the 18th of February, 1893, the vessel bore up for home, and reached Dundee on the 30th of May, having a cargo of 5,226 seals. The rest of the fleet also arrived safely, the "Active" with some 4,000 skins, the "Diana" with 3,572, and the "Polar Star" with 1,908, a total of 14,706. The commercial result of the voyage was not such as to encourage a repetition, and the Scotch vessels did not return to the Antarctic, leaving the field entirely to the Norwegian ships.

Appended to Mr. Murdoch's book is a chapter by Mr. W. S. Bruce, who accompanied the "Balaena" as Naturalist, which contains some remarks on the results of the voyage; but it is to be hoped that a fuller account will eventually be forthcoming. Mr. Bruce and Mr. Donald have also contributed papers to the Royal Physical Society of Edinburgh (4 and 6), which are both interesting, as well as preliminary reports in the *Geographical Journal*, vol ii., pp. 430 and 433.

Although the Scotch owners abandoned the Antarctic sealing, the Norwegian vessels, which are worked on a more economical principle, have by no means done so. In the middle of September, 1893, Commander Svend Föyne of Tönsberg, the proprietor of the noted Finwhale Fishery in Finmarken, despatched one of his sleuth-hounds, the "Cap Nor," re-christened for the occasion the "Antarctic," a barque-rigged vessel of 226 tons register, commanded by Capt. Leonard Christensen, with a crew of 26 all told. She carries no scientific staff, but a Mr. Bull, her manager, has received instruction from the Norwegian Meteorological Institute and the Christiania University, and will make meteorological and other observations, and there is no doubt Professor Collett will give a good account of the results so obtained. This vessel is to make a more extended cruise than the others, and to visit some of the former haunts of the Fur Seals on her way out. She arrived at Melbourne on the 27th of February, 1894, having touched at Les Palmas, Tristan da Cunha, and Kerguelen; at the latter place she is reported to have run into Greenland Harbour and visited Royal Sound, where a colony of 59 persons was found, consisting of Europeans, Chinese, and Indians, but there were no Fur Seals. She was to sail for the south in November and to search for open water between 75° S. and 78° S., in hopes of finding whaling grounds.

The "Jason" returned to the Antarctic Seas last season accompanied by two other Norwegian vessels, the "Castor" and the "Hertha," with the "Orion," which remained at Port Stanley as a store ship. They returned to the Falkland Islands from the south on the 12th of January, 1894, having seen an enormous number of seals, which, owing to the broken condition of the ice, it was impossible to approach. They there discharged their small cargoes into the

"Orion" and again went south. On their return to Port Stanley they will winter there, while the "Orion" will bring home their catch.[2]

Some interesting extracts from the journal of Capt. Larsen are reproduced in the *Geographical Journal* (9), showing that he had succeeded in reaching 68° 10′ S. and Long. 60° W., while one of the other vessels reached 69° S. further to the west, very considerably extending the knowledge of the Antarctic regions in that longitude. Most interesting particulars are given of the animals and birds met with at several places where a landing was effected. Unhappily, Capt. Larsen is not a zoologist, or his observations would have been most valuable. On one occasion in Lat. 64° a hawk was observed, penguin rookeries (species not given) were visited, numbers of *Blaahvaler* (Blue whale), the name by which *Balænoptera sibbaldi*, Gray, is known to the Norwegian whalers, were seen, also a *Knarhval* (of course not *Monodon*, but probably *Megaptera longimana*, the Humpback whale); "small fishes with big eyes and a bright skin" were seen in Lat. 66° 4′ (can they have been a species of *Maurolicus*?), also small whales supposed to have been *Minkehvaler*, by which name *Balænoptera rostrata* is known to the Norwegians. Emperor Penguins were very numerous in the fjords in Lat. 67° S.; and on December the 5th, in Lat. 67° 13′ S., Long. 60° 16′ W., what was believed to be a *Rethval*, Right Whale (*Balæna*), was seen to blow but was soon lost sight of; this was the only instance during the voyage in which a *Balæna* was supposed to have been sighted. A small volcanic island in full activity was observed and landed upon on the 11th of December, 1893, about 65° 5′ S., 58° 40′ W. Capt. Larsen and his first mate proceeded about four English miles on this island, which they called Christensen Island. He says "the seals lay in places so closely packed that we had to make circles in order to advance. It was delightful to see those masses of animals, most of which proved to be youngsters of the *Fiskesæl* [species ?], which already had changed their hair; they were beautifully fed and looked like so many bulls. Here and there an old animal was amidst the youngsters. The seals were not a bit afraid of us; on the contrary, they stretched their flippers towards us as we pelted them." On Middle Island in the bare places "the soil was covered with moss." After a fruitless visit to the coast of Chili, the ships went to Port Stanley to discharge their skins and to take in coals for a fresh journey southward, finally returning to the Falklands on March 15th, 1894.

The chief result of the visits of these vessels is a considerable extension of our knowledge of the geography and configuration of the northern extremity of Graham's Land and of the adjacent islands included between 47° and 60° West Longitude and 63° and 68° South Latitude. These discoveries are laid down in a map published in the

[2] I am told that, on her return voyage, the "Orion" got aground on the Goodwin Sands, but with what final result I have not heard.

Geographical Journal illustrating the voyage of the "Jason" (9), and need not be further referred to here. The object of the vessels was to find whales and seals, and for this purpose they would not leave the ice margin, as open water would be barren for them; the commercial interests of the voyage would therefore prevent them taking advantage of likely openings for purposes of exploration; if the two could have been done together well and good, if not the latter must give way to the exigencies of the former.

Of course the object of this search, the Cetacea, possessed the greatest interest in the eyes of the visitors, but their hopes of finding any member of the family Balænidæ were soon disappointed; various Balænopteridæ (Fin Whales) were very abundant, but on only one occasion was it even surmised that a Right Whale was seen. After Ross's repeated assertions that he had seen great numbers of the largest sized black whales lying about upon the water in all directions, this was a grievous disappointment. The ships were in the same latitude and longitude as Ross, and on the same day of the year, but not a Right Whale was seen. Ross also speaks of the sea being full of marine invertebrata upon which the whales were feeding, amongst them *Clio borealis*, which with "rice food" (*Calanus finmarchicus*) constitutes their principal diet in the Arctic seas. Capt. Gray was told by an old whaler, trained by himself, who was on the "Diana," that no "whale's food" such as is met with in the Greenland seas was on any occasion seen by him. This in itself, if normal, would account for the absence of the whales; but it seems incredible that such a change should have taken place since the year 1840 in the character of the lower forms of life inhabiting the same sea. What are we to think then—was Ross mistaken, and did his characteristic description of the Right Whale, an animal which must have been well known to him, really originate in a mistake as to the species? It is impossible to say, but the facts remain that neither the Right Whale nor its food were met with, though large Fin Whales and the *Euphasia* which forms their proper foods warmed in the water. Unfortunately, those present were not sufficiently acquainted with the characteristics of the various Fin Whales to speak with certainty as to their species, and none were captured; it is therefore useless to add anything to what has already been said on the subject, further than that a species of *Orca* was also met with in abundance.

Failing whales, the commanders of the vessels turned their attention to the seals, which were exceedingly numerous. No new species were found, nor any of the Otariidæ. The four species were the Sea Leopard, *Stenorhynchus leptonyx* (de Blainville), Weddell's False Sea Leopard, *Leptonychotes weddelli* (Lesson), the Crab-eating Seal, *Lobodon carcinophaga* (Gray), and Ross's large-eyed Seal, *Ommatophoca rossi* (Gray). Weddell's Seal appears to have been the least frequent. Mr. Bruce (4) gives some interesting particulars of the habits of these beautiful creatures: they leave the ice about 7 p.m. to feed, returning about 9 a.m. to bask in the sun during the day; in December they were all in bad condition but improved during January, and in February were very fat; their food consisted of fish, small crustaceans, and an occasional penguin. In February the embryo was well developed, and almost every female examined was with young. The seals were perfectly fearless of man, and offered no resistance to the sealer, simply waiting their turn to be killed. If the present indiscriminate slaughter of both sexes goes on, there is not the slightest doubt that at no distant period the true seals (Phocidæ) of the Antarctic seas will be as completely exterminated as are the millions of Fur Seals (Otariidæ) which formerly inhabited the islands of the Southern Seas. It is sad to think that our increased knowledge of the Antarctic seas should be purchased at such a price.

Of the fishes we learn next to nothing. Mr. Murdoch (1) gives a sketch on p. 255 of a small fish, which he describes vaguely enough as "something between a whiting and a gurnard," a large number of which were taken from the stomach of a seal; and Capt. Larsen, who found apparently the same fish under like circumstances, calls it a *Kvitting* (Whiting). Ross (13, vol. ii., p. 160) killed a seal in 95° S., 155° W., from the stomach of which he took 28 lbs. weight of fish of two kinds, "one a *Sphyræna*, the other a *Notothenia*"; they were very much mutilated, but sufficient material could be found to enable Sir John Richardson (14, part ii., page 8) to describe the latter as a new species under the name of *Notothenia phocæ*; it seems not improbable that this was the fish found by Capt. Larsen. The possible examples of a *Maurolicus* have already been alluded to; and this is all the information given with regard to the fishes.

Of the marine invertebrates even less is said, a red shrimp which was also found abundantly by Ross being about the only form mentioned.

Of the birds, fortunately, more information was obtained; but the material brought home was not in a condition to be of the greatest service to science, and very few of the specimens were deposited in the National Collections. A specimen of the Emperor Penguin has come into the possession of the Museum of Science and Art at Edinburgh, and Capt. Fairweather of the "Balaena" presented examples of *Larus scoresbyi* and *L. dominicanus* to the British Museum. Mr. Murdoch (1) laments (p. 244) that "common albatross skins were collected by the score, and rare penguins killed by the hundred, their bodies eaten, and their skins chucked overboard. Emperor Penguins, King Penguins [?], an endless variety of birds, some unheard of [?], all go over the side because they are supposed to be of no commercial value." Surely some of these skins so little valued might have been rescued by Mr. Murdoch and his friend. Professor D'Arcy Thompson, who seems to have received most of the material brought home, and by whom it is hoped that some report will in due time be published, sent thirteen skins, which had been presented to the Museum of the Dundee University, to Dr. Sclater, who has contributed a short paper to the *Ibis* on the subject (7). They consisted of eleven species with "no original dates or localities attached to them, but are nearly all of birds that may well occur in the 'Antarctic Seas,'" none of them are new species. Quoting Dr. Donald's paper in the *Scottish Geographical Magazine* for February, 1894, Dr. Sclater states that 20 species in all were met with, and calls particular attention to two birds mentioned by Dr. Donald. The first was a "Hooded Crow," which appeared to resemble closely the "ordinary Carrion Crow"; this was seen on three occasions, twice picking "at a dead seal on the shore and the third time on the wing." The second bird referred to was a "black and white Duck." Of these Dr. Sclater says, if the former were really a Corvine bird it will probably be a new species, as no such bird is known in Arctic America, and the latter may be a species of *Bernicla*. Of course the most interesting birds, and at the same time by far the most numerous, were the penguins and petrels. Of the habits of the former Dr. Donald gives some very interesting particulars in another paper (6). He mentions the difficulty he experienced in killing these large birds, a difficulty also experienced by Ross, and it may be well to mention for the benefit of future explorers that the latter found a "tablespoonful of hydrocyanic acid accomplished the purpose in less than a minute" (Voyage II., p. 158). Capt. Larsen states that at Cape Seymour he saw "a species of land-bird, belonging to the Rapaces, which resembles our hawk; it occasionally came down and pecked some eggs."

Botany seems to have been totally neglected. Capt. Larsen, who appears to be a very intelligent observer, incidentally remarks that on one of the islands where the snow was blown away "the soil was covered with moss." A fucus I think, is also mentioned somewhere; and the sea and ice were in places stained by diatoms. Doubtless more trained observers would have found other signs of vegetable life; but in this respect as in others the country must have been a complete contrast to the corresponding regions of the North, where the soil left bare by the retreating snow speedily bursts forth into a garden bright with beautiful flowers. Nor is the absence of animal life less remarkable: where are the Musk Ox, the Fox, Arctic Lemming, Reindeer, Bears, and Walrus, White Whales and Narwhals, some of which are found as far north as man has penetrated, and which afford sustenance to a race of hardy native hunters all round the pole? At the antipodes the species are few if the individuals are numerous, and man is conspicuous by his absence.

There is much yet to be learnt with regard to the Antarctic regions, and it will only be accomplished by trained observers, men who know what to look for, what is worth collecting, and what conditions must be observed to render their collections of service to the specialists who will eventually have to deal with them. It is gratifying therefore to learn that, as a result of representations made by the Royal Geographical Society, the Royal Society appointed a committee to report on the subject of Antarctic exploration, and that it has issued a most favourable report showing the great value of Antarctic Expeditions both practically and scientifically, also that a resolution has been passed by the Council of the British Association strongly in favour of the work taken in hand by the Geographical Society. This could not be otherwise after the address delivered in November, 1893, by Dr. John Murray on the Renewal of Antarctic Research (2). Furthermore the Agents-General of the Australian Colonies have been approached on the subject, and in due time a deputation will be formed to represent the matter to H.M. Government with full details as to the cost of an expedition. The President of the Geographical Society speaks hopefully, but adds the caution that we must all put our shoulders to the wheel. Let us hope that the end will be a success.

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III.

The Continuity of the Protoplasm in Plants.

WITHIN comparatively recent years a profound change has come over our conceptions of plant organisation. Previous to the year 1883 we find it stated in the Physiological text-books that: The life of a higher plant is the sum of the separate life-activities of its component cells; or that "To many, the cell is always an independent living being, which sometimes exists for itself alone and sometimes becomes joined with others—millions of its like—in order to form a cell-colony, or, as Häckel has named it for the plant particularly, a cell-republic." (Sachs' Lect. Physiology.)

To-day we can no longer regard these assertions as strictly true.

It has been found that the cell-walls which were supposed to mark the limits of the component individuals of a multicellular plant are not intact, but that, on the contrary, they are covered at definite points with numerous perforations through which fine filaments of protoplasm run, connecting together the protoplasmic bodies of adjacent cells.

Thus the protoplasm of a many-celled plant, as that of a single-celled individual, forms one mass; and the phenomenon of cell-wall formation points, not to the compound nature of the plant, but to an adaption by means of which the protoplasm may attain a large extension without losing its coherency, and through which it obtains, moreover, both a support and a protection from external dangers.

When the doctrine of the continuity of the protoplasm throughout the plant body, or at least throughout large areas of it, is realised, many problems which were difficult to explain find a ready elucidation. In this respect we may particularly mention the transmission of stimuli from cell to cell. For instance, if the terminal leaflets of a compound leaf of the well-known sensitive plant be touched, these will close together; but the response does not end here; on the contrary, the stimulation will be communicated from leaflet to leaflet until it reaches the base of the whole leaf, which will in consequence become depressed. There is little doubt that it is the living protoplasm which is the active agent in this phenomenon of transmission; but, on the supposition of the isolated nature of the protoplasmic bodies of the separate cells, it is not clear how the stimulus is conveyed from cell to cell. With the knowledge of the continuity of the protoplasm before us, the difficulty vanishes, for we then perceive a direct channel of intercommunication in the delicate threads of protoplasm which traverse the wall from cell to cell, and which play a part in some repects roughly comparable to that of the nerve-fibres in our own bodies.

To understand so important an alteration in our ideas as that indicated above, the best plan will be to state the various steps which the movement has taken since its inception.

In 1854 Theodor Hartig (1) studied those peculiar structures, the sieve-tubes, which occur in the phloem. He found here that the protoplasmic bodies contained in the several elements of a tube were not distinct from each other, but that each was connected with its neighbour by a number of small strands of protoplasm which ran through perforations in the transverse septa of the tube. This is practically the first recorded instance of a connection discovered between the protoplasmic bodies of neighbouring elements.

The matter rested here for twenty-five years; it was not until 1879 that Dr. Edward Tangl (2) discovered continuity in other structures than sieve-tubes. He studied the endosperm cells of certain seeds, and was able to demonstrate the existence of a communication between the living contents of the various cells. The material he used for the most part was *Phœnix* and *Strychnos nux-vomica*. Mr. Spencer Moore, a few years later, showed continuity in most of the other species of *Strychnos*.

In 1882, Professor Strasburger published his famous work on the cell-wall (4), and we find recorded here both a confirmation and an extension of Dr. Tangl's observation. It was Professor Strasburger who first, in the above-mentioned monograph, called attention to the curiously perforated, sieve-like membranes closing the channels of the pits in the thickened cell-wall.

In the same year a new worker came to the fore in the person of Mr. Walter Gardiner (5), and it is to him more than to any other single worker that we are indebted for the establishment of the important doctrine of continuity. So far we had only possessed definite knowledge with regard to the passive reserve-cells of seeds; but Walter Gardiner turned his attention to the vitally active portions of the plant, and was able to show here also a distinct continuity between the protoplasmic contents of the different cells. He found it to be the case in the cells of the bast-parenchyma, the pulvini of leaves, the cortical tissues and the stamens of *Berberis*. Not only does he give us an account of the facts observed, but he also points out the significance which they possess. In the case of endosperm cells, he believes we have an adaption which permits of the ready transference of nutritive materials and unorganised ferments from place to place; in functionally active cells we have, he thinks, channels along which stimul can be transmitted from the protoplasmic mass in one part to that of another. "For instance," he says in his memoir, "there can be little doubt that the conduction of a stimulus, which can be readily observed in the leaves of *Mimosa pudica*, is effected by this means."

In the next year, 1883, observations had been brought so far that both Gardiner (6) and, a little later, Professor Schaarschmidt (12) were enabled to venture the opinion that continuity of the protoplasm was not an exceptional phenomenon restricted to the comparatively few cases noticed, but one which was universal in vegetable cells. All subsequent facts tend to confirm and strengthen this view.

The year 1883 was in every way an important one for the establishment and extension of the doctrine; the numerous publications which then appeared show that the idea had taken a firm hold of the leading botanists of the day. Russow (8) had already published a memoir on the subject, in which he expressed his belief in the active part played by the connecting filaments in transmitting dynamical stimuli. Professor Hillhouse (10) published a paper in which he showed continuity to exist in the cells of the bases of many leaves (*Prunus laurocerasus*, *Æsculus Hippo.*, *Ilex*, etc.). Schmitz (9) issued his well-known work on the Florideæ. Here also he could demonstrate the existence of protoplasmic intercommunications. Schmitz, however, was not quite the first to point out continuity in the Florideæ; before him, Mr. Archer had noticed a connection between the protoplasmic bodies of the cells of *Ballia callitricha*, and Professor Percival Wright between those of *Polysiphonia* and *Griffithsia setacea*. Both Messrs. Archer and Wright spoke of the continuity as being direct through an open pore, and they also considered it to be only a temporary connection which became broken at a later stage in the history of the cell. Professor Schmitz and Gardiner are led to believe that the continuity is both permanent and not direct but indirect. That is to say, they think the continuity is effected not by a single thick strand of protoplasm running through an open pit, but by a number of delicate filaments traversing a porous membrane (pit-closing membrane) which stretches across the pit or canal in the cell-wall. It may be mentioned here that it is Gardiner's belief that continuity is in all cases indirect, never direct. Mr. George Massee, who has studied the subject in two species of *Polysiphonia*, thinks that continuity is direct in the early stages, but that a sieve-like diaphragm is subsequently developed across the pit.

In a paper read before the Linnean Society in 1885 (16), Mr. Spencer Moore gave an account of his work on continuity in some Florideæ. He also believes that the connection is direct in the young cells, but that later it sometimes becomes indirect, while at other times it remains direct throughout life. "The young cells," he says, "are placed in communication by means of a fine filament, upon which is, in most cases, placed a small nodule, just as a bead is strung upon a thread." Both thread and nodule grow, the latter assuming the form of a ring; the protoplasm, encompassed by the nodule, gives rise to a delicate closing membrane, which stretches across the ring transversely, and in which there either remains a single central pore, or several of these scattered over its surface; in the latter case we get a sieve-like diaphragm interposed. Mr. Hick, in 1884, published a work on continuity in the Florideæ (14), and in 1885 he issued another paper wherein he showed the existence of protoplasmic communications in several species of *Fucus* (15). In 1884 Terletzki (13) and Schaarschmidt (11 and 12) noticed continuity in certain ferns.

The above is a brief sketch of the chief landmarks in the history of the doctrine of protoplasmic continuity; there are many more published articles and memoirs on the subject, but it is believed that these, excellent as many of them are, do not mark the decisive steps in the establishment of the principle that most of those here mentioned do. The complete literature can be gathered together from references in the works quoted at the end of this note.

It will be perceived that there is very good reason for believing in the universality of the phenomenon, and a little reflection will show the significance which such an intercommunication of the living substance has for the plant.

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IV.

The Structure and Habits of Archæopteryx.

I.—An Explanation.

WHEN in Berlin during the past summer, I learned with deep regret that my words in an article in this Journal (October, 1893) had been interpreted as an accusation of deliberate falsehood against certain distinguished palæontologists, and on reading isolated sentences in that article I am bound to admit that they are open to such a construction if the reader chooses to ignore the whole tenour of the article. The object of the article was to show how errors arise and are multiplied, and I attempted to show that it was possible for the most distinguished men to fall into error when their minds were not entirely free from an unconscious bias. Under the influence of such a bias or dominant idea, alterations are deliberately made in drawings or descriptions in such way as to remove what appears under the influence of such a bias to be misleading. I need not here repeat my argument to show that the figure which I described as "spreading like a plague" had been altered *deliberately* in order to remove an apparent absurdity; or, in other words, to show that the errors in that figure are not such as are made by a professional woodcutter and passed over undetected by an author. That the figure was thereby rendered altogether false and misleading, I asked my readers to prove for themselves by comparing it with the photograph with which the article was illustrated.

I cannot now do less than express to Professors Steinmann and Döderlein my sincere regret that such an unfortunate interpretation has been put upon isolated sentences in my article, and assure them I aimed only at showing that they, *with the most honest intentions*, had so manipulated a drawing as to make it worthless and misleading, and that in this they had only fallen into an error to which we are all alike liable. In spite of what certain English correspondents have written to more than one German professor on the subject, I deny altogether that I have made any charge of dishonesty against any person whatever. The paragraph commencing near the bottom of p. 280 shows my meaning so clearly that I am astonished that anybody should put a wrong interpretation upon it, or at least that any Englishman should.

II.—THE SKELETON OF ARCHÆOPTERYX.

It will be noticed that the present article is not one of my series upon Biological Theories, and it has nothing to do with my contention in a previous article on the origin and multiplication of errors.

Apart from a single feather, only two specimens of *Archæopteryx* are known, and it is possible that these may not be identical in species or even in genus. So far as we know them, the differences between the two appear, to those who are best qualified to judge, to be too small to justify separation into two species. Though both were found in Bavaria, I shall refer to them as the "Berlin specimen" and the "London specimen" respectively.

It is not convenient to begin with a description of the external form of the bird, as is customary with recent species, for that external form can only be guessed at with reasonable chance of guessing accurately after a careful consideration of the structure of such parts as are still preserved. This is even more conspicuously true of the habits of the animal.

Of the skeleton, if we assume the two specimens to be so nearly related that the characters exhibited in either may be taken as true of both, we have quite an extensive knowledge.

The **vertebral column** is readily divisible into four regions: cervical, trunk, sacral, and caudal. Whether the vertebræ are fully ossified or not it is difficult to say. I can find no justification for the statement that they are amphicoelous. Professor Dames tell me that his statement to that effect is a mere slip of the pen, and that he intended only to say that, so far as can be seen in a specimen in which the vertebræ are still in their natural relations with one another, the ends are flat and not, as in most birds, saddle-shaped. The central or internal part of each vertebra in the London specimen is stated by Owen to be represented by a deposit of crystalline "sparry matter" in the caudal region, while the outer "crust" has adhered to the upper slab or "counterpart." Whether this really shows that the vertebræ (of the tail) were mainly cartilage or other soft tissue with only a crust of bone or not, may be open to question. The perfectly-fitting joints, the large transverse processes of the anterior caudal vertebra, and the slenderness and stiffness—as shown by the straightness of the tail in both specimens—of this region of the vertebral column are strong evidence that the bones were *well-ossified*.

Of the nine **cervical vertebræ**, only eight are well-preserved, the first being almost unrecognisable. Measuring the lengths of the centra of these on a large photograph (scale $\frac{132}{137}$), I make the sum of the eight in the Berlin specimen to be about 75 mm.; but Professor Dames gives numbers which together make only 60·5. A glance at the plate will show the position of the neck in this specimen. It is very strongly arched so as to bring the head almost into contact with the back of the animal in the region of the thorax. It is difficult to make these measurements accurately in either the specimen or the photograph, but the discrepancy between the two measurements is too great to be accounted for by this difficulty, and I suspect that Professor Dames' measurements have been made along the inner curve—*i.e.*, through the neural spines—while mine were made near the ventral curve, *i.e.*, through the centra of the vertebræ. I suspect, therefore, that when the animal's neck was straightened out it would be 75 mm. long in addition to the length of the atlas, which may be taken to be a very small quantity as in modern birds. Of the nine cervical vertebræ the middle ones are longer than those nearer the ends of the neck, the fifth being the longest.

Cervical ribs, apparently movably articulated, may be made out, and there appear to be eight pairs of them. The neural arches and spines are well-developed and strong, the spines being 2 to 3 mm. high.

The **trunk vertebræ** being somewhat displaced, and the vertebral column distorted, it is not very easy to make sure of their number. There appear, however, to be ten, measuring together about 70 mm. The vertebræ appear to be almost equal in size, and nine of them bear **ribs.** There are also **ventral ribs,** resembling the "**abdominal**" **ribs** of the geckos and chamæleons, and clearly showing the ventral boundary of the abdominal cavity (*see* 14 in Plate).

The **sacrum** is hidden in the Berlin specimen except at its ends. It measures 26 mm. in length. It is probable that there are about seven sacral vertebræ.

The **vertebræ of the tail,** twenty in number, measure together about 170 mm.—slightly less perhaps. The first few are very short and stout, each measuring about 4 mm. in length and 4 mm. in height. The first four have well developed transverse processes; in the fifth this process is not well preserved, and the vertebræ behind this have no transverse processes, but only a ridge. The vertebræ are longest nearer the middle of the tail, the eleventh measuring nearly 12 mm. The tail as a whole seems to have had little flexibility, for it is almost perfectly straight in both specimens. The tail of the London specimen has apparently only eighteen vertebræ and measures 180 mm.

The **skull** has been much further exposed since the photograph was taken. It is large and fairly massive, the jaws are stout, and teeth are very easily made out in the upper jaw. Those of the lower jaw are, however, hidden by those of the upper, and it is impossible to say at present how many there were (*see* Fig. 1). The sclerotics are ossified. The hinder part of the skull is destroyed in the Berlin specimen, and it is worthy of note that the cranial cavity was not filled with matrix. No part of the skull is recognisable with certainty in the London specimen, though it may be that the supposed cast of the brain (!) shown at *b*, in Fig. 2, is a portion of the skull.

The **ribs**, both vertebral and ventral, are very slender. There are no uncinate processes visible.

Of the **sternum** nothing is known, though much has been written. In the Berlin specimen it probably lies still hidden in the matrix. The position of the ventral ribs shows that it must have been small.

The **scapulæ** in the Berlin specimen were broken in exposing the specimen. The right one is easily recognisable in the plate. They are flat curved bones, not unlike those of a modern bird. Their length is 43 mm. or thereabouts, according to Dames. In the photograph only a portion is seen.

The **coracoids** are in the Berlin specimen largely hidden. I have not specially examined what portion is exposed in the London specimen. The dorsal ends are exposed in the Berlin specimen and possess a furcular tuberosity as in other birds.

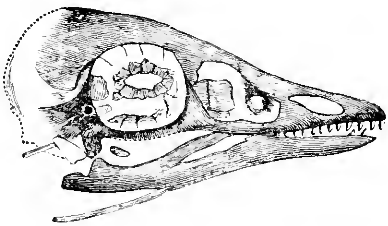

FIG. 1.—The Skull of *Archæopteryx*. After Dames. (*Natural size.*)

Of the **furcula**, a small portion is seen at the left shoulder of the Berlin specimen. It was, however, imperfectly exposed at the time when the photograph was taken. A larger portion is seen in the London specimen. It is a characteristically avian furcula, U-shaped ventrally, and articulating with the furcular tuberosity of the coracoid at each shoulder.

The **humerus** is a well-developed bone in each wing. Its form and dimensions may be seen in the plate of my former paper (vol. iii., p. 275), as also in the one accompanying the present communication. In the London specimen the inner surface of the right humerus (called "left" by Owen) is seen on the right side of the specimen (see *h'* in Fig. 2), while both are seen from the outer surface in the Berlin specimen. It differs from that of other birds in being devoid of the pectoral crest or ridge for the insertion of the great pectoral muscle. As Dames points out, this confirms his view that the sternum must have been small, as must also the great pectoral muscle. In the plate illustrating my previous article the proximal end of the humerus is covered by a portion of the matrix, which has since been removed (at 11 in Plate I. of the present article), and that plate consequently gives an impression of a

FIG. 2.—The specimen of *Archæopteryx* in the British Museum. About ¼ natural size. Explanation of letters:—*1*, portion of second (?) wing-digit, with claw; *cr*, metacarpals IV and V (?); *u*, *u'*, ulna; *r*, *r'*, radius; *h*, *h'*, humerus; *sc*, right scapula; *fu*, furcula; *c*, ribs (partly ventral); *l*, feathers; *i*, right innominate bone; *a*, acetabulum of the same; *f*, *f'*, femur; *t*, *t'*, tibia; *mt*, metatarsus; *p*, phalanges of left foot; *b*, supposed cast of brain. The accented letters refer to bones of the right limbs.

The block kindly lent by Dr. H. Woodward, F.R.S.

humerus which is slightly shorter than the true length. It unfortunately has given rise to the supposition that I "retouched" the photograph before sending it for reproduction—which, of course, is not true.

The bones of the fore-arm seen in the plate facing p. 275 of vol. iii., and on p. 351 of vol. v., and in Plate I. herewith, are a straight **radius** 55 mm. long, and a *curved* **ulna** 56 mm. long.

The **carpus** offers great difficulties. Owen figures two bones, one of which is visible in the London specimen. Why he should ignore the enormous ulnar carpal, which is a conspicuous object in the London specimen, need not here be discussed, as I am not *now* discussing the origin of errors. It is conspicuously shown in fig. 2 and in plate i. of Owen's memoir, where it is numbered 56′ and described (presumably with the radial carpal) as "left carpus" (it being of course a part of the right carpus), and something wholly unlike it is put in its place, in dotted lines, in his second plate in the figure which is reproduced as Fig. 2 (p. 439) of Mr. Pycraft's paper in the last volume of this Journal.

Of these bones I have seen two clearly, one being the radiale (4 in Plate I.), which is visible in both the specimens, the other the "ulnare," visible only in the London specimen. In the Berlin specimen the carpus lies radial side uppermost, and it is not surprising that, like some other parts, the ulnar portion of the carpus lies still embedded in the matrix. This is even admitted by Dames. The little bone called "ulnare" and drawn from imagination by Owen, and also drawn by Dames, may or may not be present. I have tried, and failed, to make it out in the Berlin specimen, and I have also tried, and failed, to make sure that it is not there. One thing only I can say of it, viz., if present it is probably the *intermedium*, and not the ulnare. The "ulnare" is the enormous and conspicuous bone shown at the distal end of the right radius and ulna in Fig. 2. It is, for a carpal bone, of enormous size, and I am not prepared to believe that it played no part in the support of the metacarpals.

Of the distal row of carpals it is only possible to say that they are not yet recognised in either specimen. Whether they have fused with the metacarpals, as they do in modern birds, or were cartilaginous and so not preserved, or were fused with the bones I have referred to as belonging to the proximal row; or whether the two figured by Owen and Dames are the proximal row, and the large bone I have called "ulnare" is really, as the London specimen suggests, a fused mass representing the whole distal row of carpals, can only be decided, so far as I can see, by one of two consummations "devoutly to be wished"—(1) the excavation of the exceedingly thin and fragile Berlin slab *from the back*, or (2) the discovery of fresh specimens. The first of these involves too great a risk to what it is hardly an exaggeration to say is the most valuable palæontological specimen in any museum in the world.

To admit that one does not know what that bone is, is one thing; to ignore its existence is another. Whether it be right or wrong, I shall for the present call it the *ulnare*. Subsequent proof that it is something else, *e.g.*, a crocodilian "lenticulare," will not invalidate my argument.

The **hand** has been so much misrepresented both in words and in drawings that I took it as an "awful example" in my contention as to the sources of error. There are **five digits** and no fewer, and I never suspected that it would be necessary for me to give further proof than that already given in my essay on errors. Great as I represented the power of a "dominant idea" to be, its power over the minds of some persons, to whom I will refer in the sequel, appears to be vastly greater even than I had guessed. To anyone who has understood the evidence already brought forward and whose knowledge of mechanical principles is sufficient to enable him to assess its value, what I have already adduced constitutes a demonstrative proof. I will venture now to prove it over again by three distinct proofs, each of which is in itself conclusive.

(1.) Three *long, slender* fingers on each hand are plainly seen on the Berlin slab. They are made up of two, three, and four phalanges respectively, in addition to a metacarpal each. Each bears a claw, which, though not easily made out in the photographs, especially in the smaller photographs, is perfectly distinct in most cases in the slab itself. There can be no doubt, and nobody does doubt, that these three correspond to the digits I, II, and III respectively of the normal pentadactyle reptilian fore-limb. The lengths of the various metacarpals and phalanges in the Berlin specimen are as follows, beginning at the proximal end, *i.e.*, with the metacarpal, in each case:—

I. $8+20+11=39$ mm.

II. $27{\cdot}5+15+19+13=74{\cdot}5$ mm.

III. $26+5{\cdot}5+4+?+?=44{\cdot}5$ mm. The joint between the third and the ungual phalanx is hidden, but these two together measure 19 mm. Of these bones the second metacarpal is the largest, and at its basal end it is under 4 mm. thick.

Some of the bones corresponding to these are to be seen in the London specimen; but as they are displaced, it is not possible to identify them with certainty. What Owen called the two terminal phalanges of the digit I, closely resemble the two terminal phalanges of digit II of the Berlin specimen, and I take them for these phalanges. They measure respectively 22 mm. and 15 mm., *i.e.*, they exceed the bones of the Berlin specimen in the proportion of rather over 9 to 8. To justify this determination I give the lengths of some other bones in the specimens. The first number in each case is the length of the bone in the Berlin specimen; the second, that in the London specimen. Ulna, 56 mm., 63·5 mm.; Radius, 55 mm., 62 mm.; Femur, 51 mm., 58 mm. (?); Tibia, 71 mm., 81 mm. In each case except that of the femur the ratio is almost exactly 8 : 9, and in the case of the femur it is impossible to measure the exact length in the Berlin specimen. The numbers of vertebræ in the tails differ in the two specimens, so that it will not be safe to take the ratio in length of the two tails as a guide. There is no other bone which can be identified and measured with certainty in both specimens, so we may adopt 8 : 9 as the relative sizes of the Berlin and the London specimens respectively.

But in thickness a different relation holds. In corresponding bones of two similar animals we find that the ratio of thickness to length is always greater in the larger animal. And this is true here: all bones of the London specimen are stouter and more massive than those of the Berlin specimen. Now, in the London specimen two conspicuous bones (badly shown at *cr.* in Fig. 2) were identified by Owen as the "third" and "fourth" metacarpals. They measure 39 and 33·5 mm. respectively in length. In thickness they are much greater than any hand-bone of the Berlin specimen. Others have regarded these bones as the second and third metacarpals. Suppose this were the case, then we get these ratios between the London and Berlin specimens. 39 : 27·5 = more than 11 : 8 and 33·5 : 26 which is 10·3 : 8. Further, the bones are utterly unlike their supposed equivalents in the Berlin slab. They are far stouter, and the longer of the two is exceedingly broad at the base, and therein is well-fitted to resist torsional stress or twist at the joint. In their proximal halves, instead of being slender and almost circular in section, they are stout and have ridges which, when the two were fitted together, would have prevented their movement one on the other. Whatever they are, they are utterly unlike any bones visible in the Berlin specimen. Their position with reference to the feathers of the wing, in spite of the dislocation of other bones, is just that of the large metacarpals in an ordinary bird's wing; and the fact that these feathers are still in their normal position in this wing (the left) justifies the belief that when the animal finally settled down previous to fossilisation those feathers were still bound to those metacarpals by ligament.

This is proof no. 1 that those two bones are the metacarpals of the digits IV and V.

(2.) The second proof is a more formidable one. Some hundreds of experiments extending over hundreds and even thousands of years have shown the effect of "selection" upon dogs, horses, sheep, pigs, pigeons, poultry, vines, roses, plums, apples, pears, strawberries, gooseberries, blackberries, pansies, daisies, dahlias, chrysanthemums, etc., etc., and the result is the same in all cases. Selection occurs in Nature (Naudin, Darwin, Wallace), and its effect is the same as in the case of artificial selection (Naudin, Darwin, Wallace, Bates, and others). I do not think it necessary to repeat the proof of this statement here: the proof is far too long, too well known, and too widely accepted for me to need to say more about it. If anybody should challenge me to do so, I will give a proof of it in a future article. In the meantime, we may take it as proved that the form and dimensions and structure of every bone and feather in *Archæopteryx* is the outcome of long-continued Natural Selection. The form and structure of the bones of the three digits visible in the Berlin specimen, and of the feathers in the same specimen, show what the conditions of selection have been, and what have been the uses of those several parts.

The digits I, II, and III are long, *slender*, and clawed. Each metacarpal and phalanx is concave on the flexor surface. The ends of the bones are curved like pulleys, allowing of free movement at every joint. A distinct tubercle, for the insertion of the flexor tendon, is recognisable at the proximal end of almost every one of the eighteen phalanges, and these, together with the curvature of the bones, show that flexor muscles were well developed and active and useful, and, in view of the forms of the joints, were useful in producing extensive flexion of those digits. One of these joints has been referred to by some who had not seen the specimen as possibly a fracture (the joint between the second and first phalanges of the third digit, marked 16 in Plate I.); but a more perfect joint does not exist in the toe of any existing bird than that joint. It is perfectly preserved, and nobody who has seen it can doubt for a moment that it has been evolved under the influence of Natural Selection, and was exceedingly well-adapted to allow of a very extensive flexion.

The feathers are as perfectly adapted to resisting the passage of air through them as in any modern bird. Those who have studied the mechanism of flight in detail will recognise why those feathers are all so curved that the dorsal surface of the wing when at rest is *convex*; why the anterior division of each vane is narrower than the posterior one, and is strongly curved and overlaps the posterior division of the vane of the next feather in front. They will know that such a wing is useful only if adequately supported by rigid bones capable of resisting very considerable torsional stress.

These two sets of structures—the digits I, II, and III, and the feathers—have been evolved under the direction of Natural Selection. They are both, therefore, fitted to perform the functions they actually did perform. The digits were useful for some purpose involving extensive flexion: they were, in fact, used to grasp parts of trees—for I shall show later that the animal did not habitually walk on the ground on all fours. They could not do this if those large feathers were attached to them. Mr. Pycraft has shown that in *Opisthocomus* the young use the digits for climbing, and that in order to enable them to do so the development of the mid-digital and ad-digital quills is delayed till such time as the young are able to fly or to climb without the help of these digits. I thank him for this excellent illustration (Nat. Sci., vol. v., pp. 355 and 358). It confirms the opinion I have expressed that flexible digits cannot be used for climbing if they bear large quills.

And again, the digits I, II, III of *Archæopteryx* (which the large size and perfectly ossified bones show to be an adult, as also do the well-developed feathers) are, by virtue of their very great slenderness and narrowness at the joints, incapable of resisting a great torsional stress. Unless those feathers exert a great torsional stress on the bones supporting them they are useless. I have shown they were not useless. Therefore they exerted a great torsional stress, and therefore they were supported by bones not yet seen in the Berlin specimen, although those of the left wing are seen in the London specimen. It follows, therefore, that the first three digits were used for climbing, and that one or more others were present to support the feathers. That *two* were needed I hope to prove in another place. It would unduly increase the bulk of the present article to prove it here. It is unnecessary, in view of the fact that anybody may see the two metacarpals for himself in the Museum in Cromwell Road.

(3.) The third proof is incomplete. It shows only that the digits I, II, and III did not support the feathers, and that, therefore, something else must have existed to do so. Its simplicity is unsurpassed. It will appeal even to those who ignore both the principles of mechanics and the action of Natural Selection. The figure 10 is placed on the surface of the right wing in Plate I. In this region the dorsal surface of the wing is convex. A rule or "straight-edge" placed on the wing across this point, parallel to the ulna and resting upon the first and second digits, touches the wing along the whole of its length from number 10 backwards. In front of this the feather-surface curves downwards, so as to be perhaps 2 mm. below the edge of the rule near the digits. The *lower* surface of the metacarpal and of the first and second phalanges of the *second* digit lies fully 1 mm. above that feather-clad surface. The bones of the *third* digit are closely pressed down upon, but not sunk below, that surface. Therefore those digits did not lie *in* but *upon* the feathered wing when that animal finally sank dead upon the mud in which it has been preserved. Therefore, further, other bones (or bone) were present to support those feathers. No argument from the embryology of *Opisthocomus* or anything else will shake that conclusion. Whatever argument be urged in future against this view, all that can be shown thereby is the fallacy of the reasoning—but I will return to this in the sequel.

I must here correct an error made in my previous article. The supposed "shadow" (see 12 on the plate accompanying this article) which I referred to is *not a shadow at all, but a yellow stain on the slab.*

The **pelvis** is seen in the London specimen only, and in this specimen nothing is to be learnt from the left innominate, while even the right one is imperfect (Fig. 2, *a*). This innominate appears to have been about 50 mm. long. The acetabulum is perforate. I believe there is no anti-trochanter, though in absence of the specimen I would not make the statement definite. It is a characteristically avian pelvis so far as concerns the length of the ilium and its prolongation to about an equal extent behind and in front of the acetabulum. If I mistake not, it is conspicuously unlike the pelvis of any existing bird in the matter of width, and the bearing of this will be shown in the sequel.

The **femur**, more slender than in existing birds of the same size, is strongly curved, the flexor surface being concave.

The **tibia** (or tibio-fibula ?) is almost perfectly straight, and has only a small cnemial crest.

The **foot** is a characteristically avian foot. In the Berlin specimen the matrix around the feet is so hard that a complete exposure of them has proved impracticable. The London specimen shows the left foot well. It is more massive and in every way larger than the corresponding parts of the Berlin specimen.

C. HERBERT HURST.

(*To be continued.*)

EXPLANATION OF PLATE I.

A photograph of the Berlin specimen of *Archæopteryx* taken before the skull and certain other parts were as fully exposed as they now are. (Scale 5 : 17.)

1. First digit of left manus. 2. Second ditto. 3. Third ditto. 4. Radiale of left carpus. 5, 6, 7. First, second, and third digits of right manus. 8. Region of right wing which in the specimen lies lower than the visible bones of the hand and lower than the region marked 10. 9. Primary quills of right wing. 11. Small portion of matrix lying upon proximal end of humerus. (See reference to this in the text.) 12. Yellow stain resembling, in the photograph, a shadow. 13. Cnemial crest. 14. "Abdominal" or ventral ribs. 15. Feathers of crural aëroplane. 16. Joint between second and first phalanges of third digit. 17. Left femur.

The specimen was illuminated at the time of photographing by light falling upon it from above and in front and slightly to the left.

ARCHÆOPTERYX.

V.

Earthworms and Oceanic Islands.

IT is impossible in the present state of our knowledge to write anything like an exhaustive article with the above heading; but we are in possession of a certain number of facts which have never, so far as I am aware, been put together in a connected form. It may be of some little interest to do this, as the group of earthworms is one which is in many ways the most suitable for use in attacking the problems of geographical distribution. It will be convenient, perhaps, to commence with an enumeration of the bare facts *seriatim*, and to follow this with a general comparison of them. We shall, therefore, take those truly oceanic islands from which earthworms have been brought one by one.

The Bermudas. *Onychochæta windlei*, Pontodrilus bermudensis, Perichæta bermudensis.

Teneriffe. Microscolex modestus, Allurus tetraëdrus.

The Azores. Allobophora eiseni, A. nordenskioldi, A. trapezoides, A. chlorotica, A. putris, Perichæta indica.

St. Helena. Eudrilus eugeniæ, (Lumbricus helenæ, L. josephinæ, L. hortensiæ, Perichæta sanctæ-helenæ).

Madeira. (Lumbricus vineti.)

Fernando Noronha. Pontoscolex corethrurus.

South Georgia. *Acanthodrilus georgianus*.

Marion Island. *Acanthodrilus kerguelarum*.

Kerguelen. *Acanthodrilus kerguelarum*.

Mauritius. *P. mauritiana*, *Perichæta robusta*, (P. mauritii).

Rodriguez. (Perichæta rodericensis.)

Seychelles. Megascolex armatus.

Marquesas. *Megascolex albidus*.

Upolu. *Perichæta upoluensis*.

Tahiti. *Perichæta grubei*, *P. novaræ*, (Lumbricus tahitanus, Pheretima montana).

Pelew. Fletcherodrilus unicus.

Fiji. *Dichogaster damonis*, *Perichæta vitiensis*, (P. subquadrangularis).

Hawaii. *Perichæta hawayana*, *Pontoscolex hawaiensis*, (P. corticis, Hypogæon havaicus). Limnodrilus sp.

Tonga. (Lumbricus tongaënsis.)

It should be mentioned that in the above list those species which are peculiar to the islands in question are marked in italics, those which are found elsewhere, as well as in those islands, in roman character, while species *incertæ sedis* are enclosed in brackets.

The list is unfortunately by no means imposing, but it sums up the facts at our disposal. I have, of course, not considered New Caledonia as belonging to the category of oceanic islands; and there are some who will find fault with me for placing Mauritius and the Seychelles in this list at all. Otherwise, I think that all the islands which I include are true oceanic islands of either volcanic or coral origin, which have never formed part of a pre-existing continent. It is unfortunate, too, that so large a proportion of the species are *incertæ sedis*; most of these were described by Kinberg in the *Öfversigt* of the Swedish Academy for 1866, at a time when the structure of this group of animals was very little known; his "Lumbricus" is far from being coëxtensive with that genus as at present understood.

The facts, so far as they enable any generalisations to be formed, seem to indicate that earthworms are among those groups which have the greatest difficulty in crossing the sea by the usual means of transit open to such creatures. Very few exact experiments have been made; but what we do know appears to indicate that salt water is fatal to them. This statement cannot be made universally. There are a few—very few—species which habitually live upon the sea shore; this is the case with *Pontodrilus litoralis* of the Mediterranean coast, and of the allied species *P. bermudensis* of the Bermudas, Brazil, and Jamaica. Schmarda described, under the name of *Pontoscolex arenicola*, at least three species from the shores of Jamaica, of which one is the same as that subsequently described by Perrier as *Urochæta hystrix*, a very widely distributed form; *Pontodrilus bermudensis* was also confused under the same specific name, as also was a third species which I have called *Pontoscolex arenicola*, preserving Schmarda's original name. It has been also asserted that certain Ceylonese earthworms are not killed by sea water. It will be observed, however, that the facts contained in the list which I here give argue strongly that it is only exceptionally that earthworms can have crossed the sea to oceanic islands by the help of trees floating along with currents, the only natural method that suggests itself.

In remote oceanic islands, such as the Azores, the species are all widely-distributed forms. *Perichæta indica*, for example, is one of the commonest species to be met with in gatherings of earthworms from foreign parts, and it has also been found in greenhouses in this country and in North America. This fact seems to show some special adaptation on its part for accidental exportation by man. Precisely the same remarks may be made concerning *Pontoscolex corethrurus* and *Eudrilus eugeniæ*. The former species is almost the most widely-distributed earthworm known. Specimens have been recorded from various parts of South America, from the Malay

Peninsula, from Ceylon, and from Australia. One is disposed to argue from its prevalence that it has been accidentally carried about with plants, etc.; there is no doubt that in these various localities it is one of the commonest species, hence the chances of its accidental transference are large. *Eudrilus eugeniæ* has a similarly wide range. It occurs in South America, Ceylon, New Zealand, New Caledonia, etc. Now it will be noted that both these species occur upon true oceanic islands. The semi-marine habit of *Pontoscolex* has been already referred to. This may be considered in relation to the occurrence in Hawaii of a species which really appears to be different from *P. corethrurus*, and also to the existence in the Bermudas of *Onychochæta windlei*, a form which I separate generically, but which Dr. Rosa does not. Here we have apparently two forms which are peculiar to the islands in which they are found; it may be that they have been introduced in the past by some natural means and not by man's interference. It is probable, however, that the two species which occur in Teneriffe are accidental importations; they are both species of wide distribution. *Microscolex modestus* is found in such widely-separated places as Italy and the Argentine. *Allurus tetraëdrus* inhabits Europe and New Zealand.

The facts that are known respecting the earthworm fauna of the islands in the Antarctic area have a greater significance, for, from the present point of view, this region of the world is better known than many others. Here we have a state of affairs which is quite typical of oceanic islands: they are inhabited, that is to say, by species different from but allied to those of the nearest mainland. *Acanthodrilus georgianus* of South Georgia is so near to *Acanthodrilus falclandicus* of the Falkland Islands that I at first confounded the species; they were, however, rightly, as I am now convinced, distinguished by Michaelsen. The small *Acanthodrilus* of Marion Island and of Kerguelen (they appear to be the same so far as my recollection of the characters of the former goes) are near to South American forms. This genus is the prevalent genus of the Antarctic region, being found abundantly in New Zealand as well as Patagonia and Chili, and occurring also, though more sparsely, at the Cape of Good Hope and in Western Australia and New Caledonia. It must be further borne in mind in considering the range of the present genus that it can live in water with greater ease than some species, and that one form at any rate, *Acanthodrilus litoralis* of Kinberg, was discovered by him "Insula freti Magalaënsis juxta litus."

F. E. BEDDARD.

SOME NEW BOOKS.

Trematodes from Japan.

Studies upon the Ectoparasitic Trematodes of Japan. By Seitaro Goto. Pp. 270, with 27 plates. Vol. VIII., Part I., of *The Journal of the College of Science*, Imperial University, Japan. Tokyo, 1894.

We have received from the Imperial University this beautiful memoir, written in English, but concerning work done in Japan, and printed, illustrated, and published in Japan. At the present time the Press of Europe seems to be making up its mind whether or no Japan is to be admitted into the comity of civilised nations. For our own part, we make no nice distinctions in the matter of warfare, holding it all a necessary abomination, and regarding the details as equally savage and degrading whether they be wrought out according to "rules of the game" at Waterloo or against the rules of the game as at Culloden, and, apparently, at Port Arthur. But while the Press disputes, Japan has quietly taken her place. The most casual perusal of the memoir now before us shows that Paris, London, or Berlin might have been proud to issue it.

Thirty new species are described, belonging to the genera *Microcotyle*, *Axine*, *Octocotyle*, *Diclidophora*, *Hexacotyle*, *Onchocotyle*, *Calicotyle*, *Monocotyle*, *Epibdella*, and *Tristomum*. But most readers will be more interested in the anatomical work.

Mr. Goto found that familiar methods of preparation gave the best results. The animals were killed with hot saturated solution of corrosive sublimate, preserved in seventy per cent. alcohol and stained with Kleinenberg's hæmatoxylin. For mounting whole, specimens were killed under the pressure of a cover-slip over the flame of an alcohol lamp, then preserved in seventy per cent. spirit and stained with borax carmine, the excess being washed out with acidulated spirit. By this method "only the internal organs and the nuclei of the mesenchyma remain stained, while the mesenchyma itself is wholly decolourised, so that the result forms altogether a very beautiful object under the microscope."

Among the many anatomical points that will interest specialists, the structure of the mesenchyma will interest all anatomists. This tissue, at least in the Digenea, consists of large vacuolated cells, between which a fibrous network with small nuclei is present. It varies from a truly cellular character to a typical reticulated fibrous connective tissue on the one hand and a true syncytium on the other. It is usually divided by a thin membrane of compact connective tissue into an ectoparenchyma, in which run the diagonal and circular muscular fibres, and an inner more vacuolated endoparenchyma in which the outlines of the cells tend to become obscure. Let those who still hold that there is no such morphological structure as a mesenchyma to be distinguished from a mesoblast note these conditions.

Mr. Goto holds that the term "intracellular" has been applied wrongly to the excretory system of Trematodes and Turbellaria, and that it is phylogenetically of the same order as that of the Nemertines and the flat-worms, thus confirming the opinion of Whitman upon this point.

One of his main arguments for this view is drawn from the presence of cilia within the tubes. We have, as he points out, no reason for supposing that cilia, which are characteristic structures of the outside of cells, ever grow upon the inner sides of the cell-wall.

At the end of the anatomical part of the paper some interesting biological notes are given. Most of the ectoparasitic Trematodes live attached to the gills of fishes; but some live in the mouth-cavity or on the general surface of the body. When two species are found living on the same fish, generally they confine themselves to separate regions. Thus *Tristomum sinuatum* and *T. ovale* live upon a *Histiophorus*, but the latter confine themselves to the mouth-cavity, the former to the inner surface of the branchial plates.

When removed from the host and placed in water many of the Trematodes move about by looping movement, using their suckers like those of the leech. Most of them live upon the slime of their host, but a few are able to extract blood.

Most of the monogenetic Trematodes have a colourless and transparent body in which the vitellarium and the pigment cells of the intestine are the only coloured parts. However, says Mr. Goto, "This must not be regarded as a case of protective colouration; for, in the first place, the nature of the habitat already protects the parasites from being attacked by their enemies, and in the second place, they are but very imperfectly exposed to light and thus the conditions of their existence prevent any effective play of Natural Selection."

A History of the World.

Die Vorwelt und ihre Entwickelungsgeschichte. Von Dr. Ernst Koken, Professor an der Universität Königsberg. 8vo. Pp. viii., 655, with two folding maps and 117 text figures. Leipzig: T. O. Weigel Nachfolger. 1893. Price 14s.

The change that has taken place in the position of geology since the early years of this century nowhere makes itself more felt than in a book such as the present. In former days a Humboldt or a Lyell could appeal with safety to a larger public: vistas were then being opened that stirred the imagination of all; the broad lines of the drama arrested the attention; and details gave no trouble, because details were still unknown. Nowadays we know so much, and we have pushed out into the darkness in so many directions, that a comprehensive survey is a task of far greater difficulty: we are overwhelmed by the extent and by the specialisation of our information; it is the details that interrupt our vision, and "we cannot see the wood for the trees." In these respects, too, the geologist, whose science is a compound of so many other highly-specialised sciences, of physics, of chemistry, of zoology, and the like,—the geologist or the historian of the whole earth stands at a great disadvantage as compared with the historian of but a small portion, or of a small and unremote period. The archæologist and the man in the street soon find a common platform and a common speech, but the geologist of to-day no longer tells his story in the vulgar tongue. These difficulties have been fully understood by Dr. Koken, and his book, written for the most part in a flowing and distinguished German, is an excellent attempt to overcome them. He will not, however, spare his readers all brain-work; he has had to read and toil much to put these things before them; they, for their part, must take some pains to understand him. As he truly says, "A popular superficiality is of service to no one, and by it our science is only injured."

The work is divided into fourteen chapters, of which the first three deal with such general questions as the interior of the earth and the origin of igneous rocks, the upheaval and erosion of the land, and the biological, physical, and astronomical methods of estimating the duration of geological time. Then follow ten chapters which describe the gradual evolution of the world from Cambrian times to the close of the Glacial Period, each chapter dealing with a geological system, describing its living beings and the distribution of its lands and seas so far as knowledge permits. Either the first appearance or the acme of any important forms of life introduces a fuller explanatory description of the group to which they belong. The final chapter sums up the general results from the point of view of changes of climate and geography and the evolution of organic life.

As an example of Dr. Koken's method, let us see how he treats the Silurian System.

This term is used in its older and broader sense for all strata between Cambrian and Devonian. For Europe, the *Dictyonema* Shales are regarded as passage-beds from Cambrian to Silurian, but it is pointed out that the precise boundary might well be drawn at many another level. Broadly speaking, the beginning of the Silurian is marked by increase of trilobites, and the sudden appearance of thick-shelled cephalopods and gastropods. A corresponding lithological change is the development of limestones instead of the sandstones and clays of the Cambrian. This again was connected with a general sinking of the land and extension of the sea in the most characteristic Silurian areas. The enormous thickness ascribed to the passage-beds in N. America gives Dr. Koken "the impression that the American geologists are not yet masters of the faunistic and stratigraphic difficulties." Similar passage-beds are found in China, in Australia, and perhaps also in Argentina.

This account is appropriately followed by a short description of *Dictyonema*, and a comparison of it with other graptolites. The trilobites, already described in the Cambrian chapter, have their chief forms alluded to. An interesting attempt to reconstruct the physical geography of the period leads on to an account of the typical Silurian and its distribution.

The lower beds of the Silurian closely follow those of the Upper Cambrian in their distribution, but in many places overstep the limits of the Cambrian sea. In Upper Silurian times the mainlands were again more elevated, but occasional transgressions of the sea in areas where Lower Silurian is unknown show that there were independent movements of the earth's crust. The close of the system is here and there marked by beds which could only have been deposited in parts of the sea that were passing away, partly brackish, partly very salt from evaporation. While communication between the northern parts of Europe and America remained free, a very distinct marine province ran from Belgium over France and Germany to Bohemia and the Eastern Alps, and also included the Silurian of Spain and Sardinia and such sporadic portions as are known in the Mediterranean. In this peculiar basin the fauna was totally disconnected from that of the preceding Cambrian sea. It is possible that this Silurian Mediterranean extended eastwards into Indian and possibly Chinese waters.

The Russian Silurian, with its regular structure, is taken as a type, and described in detail. A few comparisons are made with that of Sweden; but Englishmen, at least, will find it odd to have the land of Murchison passed over with the merest mention. Neither here, however, nor elsewhere does Dr. Koken confuse that which has been important in the human history of discovery with that which has a more abiding importance in the scheme of world-history.

The Silurian strata of the rest of the earth are briefly discussed. It is held that many North American species are identical with those of Northern Europe. It is thought probable that S. America was then joined to Africa and that no S. Atlantic then existed; but the evidence for this is obviously negative.

Forty-four pages are then devoted to an account of the Silurian fauna, an account both broad in view and remarkably readable. The arachnid affinities of the Eurypterida are somewhat scoffed at, while Simroth's view that the ancestors of these and of *Limulus* were evolved on dry land finds scant favour in Dr. Koken's eyes. The presence of true cirripedes argues a long previous history of the Crustacea in the sea. Considerable space is devoted to the cephalopods, and worthily so, although many may think that the hypothetical Microsiphonulas, Cæcophoras, etc., of Hyatt are a little over-prominent. Dr. Koken is so well known as an earnest student of the gastropods that his account of their Silurian representatives will be read with interest by every palæontologist. The bivalve shells of the period meet with a like capable treatment. Neither here nor in other chapters is so much attention devoted to the crinoids as their numbers and beauty might warrant; there are, however, some suggestive remarks on the cystids. The corals and graptolites, too, are rather crowded out, and we could imagine a more satisfactory treatment of the fish. Considerable attention is paid to the arthropods of the land, and especially to the researches of S. H. Scudder and Erich Haase.

Space precludes our dealing with the interesting final chapter, in which the author points the moral of his tale, and takes his stand on a firm Uniformitarian basis. We could wish to translate the whole of this suggestive summary, but instead we can only recommend all serious students to peruse the book for themselves.

A word of praise is due to the illustrations, many of which are new and, like the text they adorn, well up to date. Could an English translator and publisher be found, the book would make an admirable complement to Professor Bonney's "Story of our Planet" (*see* NATURAL SCIENCE, vol. iv., p. 62). Till then we congratulate readers of German, no less than Dr. Koken and his Leipzig publisher.

F. A. B.

IN THE FORESTS OF GUIANA.

IN THE GUIANA FOREST: STUDIES OF NATURE IN RELATION TO THE STRUGGLE FOR LIFE. By James Rodway, F.L.S., with an introduction by Grant Allen. Pp. xxiii. and 242, with 16 plates. London: T. Fisher Unwin. Price 7s. 6d.

IN reminding Mr. Fisher Unwin of the old proverb that "Good wine needs no bush," we do not mean to cast any slur upon Mr. Grant Allen's preface, which is, as are indeed all his essays, crisp and interesting. Mr. Rodway, however, though it may be that he is less known to the public than many writers with half his abilities, can stand upright without any adventitious props whatsoever. In this volume we are presented with a series of essays upon tropical life as seen in British Guiana, which are partly reprints of papers contributed to this Journal among others. To the present writer, whose sympathies are rather zoological than botanical, the volume appears to be almost too much devoted to plant life; but there is one highly interesting essay upon animal life and another upon "The Man of the Forest," with which we shall chiefly deal in this review; not because they are markedly better than the others, but merely on account of the regrettable fact that the editor's definition of space differs from that of the astronomer.

Mr. Rodway points out that, while elsewhere there is a continual shifting of balance in the organic world, the Indian of the Guiana forest is "in almost perfect harmony with his surroundings." Nor is he to be defined at all after the fashion of Artemus Ward. "One of nature's gentlemen" is among the many complimentary descriptions given of him by Mr. Rodway. The Indian father and mother perform their duties by their offspring in a way which might advantageously be copied by many examples of the so-called civilised man. While the child is young, the father allows himself no free exercise of his natural love for hunting and fishing; he restricts himself to what is the bare necessity, for fear of wearying the "child-spirit" which is believed to be always with him. An education of the kind needed by the young "savage" is most carefully given, and we learn that even infants of a tender age "take life seriously" like their parents; the romping incidental to Aryan childhood is almost unknown to the offspring of the dwellers in Guiana forests. Nevertheless, they have their games, which from their utility should win the approbation of the unintelligent middle classes of this country, who favour a "commercial education." They learn to hunt and to shoot and play games in which excellence in pursuing imaginary prey wins the day. The blow-pipe is, of course, one of the principal implements of sport; we have been told by a friend, who knows the country described by Mr. Rodway, that an experienced Indian will use as many different kinds of blow-pipe as a golfer will clubs. The mystery which is attached to the names of the people is remarkable; a child is given a name, but it is never called by it. It is termed "boy," or "girl," or "friend," or some such general term. Its immediate relatives may be presumed to know what its "Christian" name is, but they cannot be induced to reveal it. The easiest way to annoy this peaceful and kindly race is to be pertinacious in indiscreet enquiries about the names of individuals. Mr. Rodway suspects that the names are those of animals, and that the people have a superstition that to know the name is to have some malign influence over its owner. There appears to be but little "poll-parroting," as Mr. Riderhood would have said, among the ladies of the Guiana communities. Conversation among all classes has a distinctly sporting flavour, and deals with the exploits of the talkers. As the women have naturally but little experience in this way, and as their dress is limited to an apron of beads subject to no fluctuation of fashion, conversation on their part is not discursive. The race is silent enough to have won the approbation of Carlyle. The Indian makes but little impression upon surrounding nature; he leaves no monuments, and when the white man comes he disappears silently. A "deserted village" in Guiana is even more desolate than that pictured by Goldsmith. Not even a parrot remains to hint of the language of the departed tribe.

It has often been pointed out that the forests of tropical America, instead of teeming with visible life, as is the opinion of many of us at home, are apparently almost as void of living creatures as the great depths of the ocean were once supposed to be. The collections of gaudy butterflies and brilliantly painted birds which we see in museums give an idea of wealth and beauty in the manifestations of life which is far from being the truth. "To the stranger," remarks Mr. Rodway, "the forest appears almost deserted. Hardly the sign of an animal is to be seen by any but a skilled huntsman, and by him only after a most careful search." To the new-comer the forest suggests rather an African desert than a home of abundant animal life. Even to the dullest observer, however, the sandflies and ants are plainly sensible ; so too is the moist and steamy atmosphere of this tract of the world, which Mr. Sclater so aptly termed "Dendrogæa." Apart from these insects, it is sounds rather than sights which obtrude themselves upon the wanderer. "The red howling monkey, hidden in the foliage overhead, keeps up his reverberating notes at intervals for hours, and makes the stranger exclaim almost in a fright, 'Whatever can that be?' Then come the tree-frogs which astonish us with their loud whistling or booming, while the buzzing of the cicada or razor-grinder is even more startling."

To Mr. Rodway, however, the forest is all alive, and he gives a most real picture of what he has seen in a fashion which has been but little done for this part of the world, and never done precisely in the author's way. Mr. Grant Allen calls him the "Jeffries of the Tropics." He is not, however, troubled, like the late Richard Jeffries, the "Son of the Marshes," and others of the present numerous race of "scientifico-literary" writers, with the deeply uninteresting yokel and gamekeeper, who taint the freshness of the descriptions with dull remarks phonetically transliterated.

F. E. B.

Spelæology.

Les Abîmes, les Eaux Souterraines, les Cavernes, les Sources, la Spélæologie. Explorations souterraines effectuées de 1888 à 1893 en France, Belgique, Autriche et Grèce avec le concours de MM. G. Gaupillat, N. A. Sidéridès, W. Putick, E. Rupin, Ph. Lalande, R. Pons, L. de Launay, F. Mazauric, P. Arnal, J. Bourguet, etc. By E. A. Martel. 8vo ($12\frac{1}{2}$ by 9 inches). Pp. 580, 320 maps, sections, photogravures, and other illustrations, Paris: Delagrave, 1894. Price 20 frs.

This is an exceedingly interesting book and a valuable addition to our knowledge of caverns and underground watercourses. The volume opens with a chapter on cave-hunting, treating of methods of exploration, classification of subterranean excavations, tools required in working, explosives, dangers to be encountered, photography, etc. The author-editor then proceeds to describe in detail the caverns of the districts of Vaucluse, Ardêche, Le Gard, L'Hérault, Larzac, Bramabiau, Causse Noir, Dourbie, Jonte, Causse Méjean, Florac and Rodez, Tindoul de la Vayssière, Causse de Villefranche, Causse de Gramat, Badirac, Causse de Martel, Miremont, La Dordogne, Puy de Dome, Dourgogne, the Seine basin, and others in France. Then follow three chapters on the caverns of Belgium, Austria, and Greece, and the work concludes with observations on caves in general, springs and underground rivers, theories of the formation of such phenomena, and general notes on objects found in caverns, and their bearings and interests.

The volume is profusely illustrated with valuable and interesting pictures, mostly reproductions of photographs, some of which form full-page plates done by heliogravure; carefully executed ground-plans and sections are given of each cave, most of which are drawn by the author or his companions, and of these plans we may call especial attention to that of the Trou de Grandville in the Dordogne, and that of the subterranean river of Bramabiau (Gard). This latter plan is simply a revelation to the geologist, as about half a square kilometre of the district is a perfect network of tunnels, in which the Bramabiau and Le Bonheur rivers disappear. These plans bring home to the reader the amount of denudation slowly going on from the percolation of water, and the possibilities of minor alterations to the surface features in limestone districts.

We cannot recall any book on the subject which has been so carefully compiled and so beautifully illustrated, and the moderate price of the volume should permit of a large and well-deserved sale.

We learn that this and the other writings of Mr. Martel have aroused such interest in subterranean investigation, that he has succeeded in founding a society, named "La Société de Spélæologie." Those interested should communicate with Mr. Martel, at no. 8 Rue Menars, Paris.

As this notice was going through the press, we saw a second book on the same subject, relating mainly to Austrian caverns. "Höhlenkunde" as the book is called, is written by Franz Kraus, and published by Gerold's Sohn, of Vienna. It has 155 illustrations, and a map showing the Bavarian caves, contributed by Dr. W. Gümbel; but while equally useful, it is by no means so beautifully produced as "Les Abîmes." The price is 10 marks.

THE PERMIAN FISHES OF BOHEMIA.

FAUNA DER GASKOHLE UND DER KALKSTEINE DER PERMFORMATION BÖHMENS. By Anton Fritsch. Vol. iii., part 3. Pp. 81-104, pls. 113-122. Prague: F. Rivnac, 1894.

THE higher fishes of the Palæozoic Epoch are difficult to understand, and further progress in elucidating their relationships can only be made by a detailed study of their osteology such as is now being undertaken by Dr. Anton Fritsch in Bohemia. When reviewing last year's instalment of the Professor's work (NATURAL SCIENCE, vol. ii., p. 435), we alluded to the opinion that these fishes, commonly grouped in the family Palæoniscidæ, are the scaly and normal forerunners of the scaleless and abnormal modern sturgeons. All the facts hitherto published appear to justify such a view; and we thus turn with interest to the new part of the "Fauna der Gaskohle" which now lies before us. In the present instalment, Dr. Fritsch describes two more Palæoniscids with delicate scales, and some species related to *Amblypterus*, while his detailed letterpress, as usual, is illustrated with numerous diagrams and some coloured plates.

The fishes with thin scales which Dr. Fritsch terms *Sceletophorus biserialis*, and the scaleless which he assigns to the genus *Phanerosteon*, are of most interest because they display a few traces of the internal skeleton of the trunk. We observe no remarkably new facts, however, because we consider the author to be entirely mistaken in his determination of the presence of calcified vertebræ. In our opinion the structures described by Dr. Fritsch as such are merely crushed portions of the appended arches. We do not perceive even isolated hypocentra and pleurocentra, and we must thus continue to believe that the Palæoniscidæ are as destitute of rudiments of vertebræ as the modern sturgeons.

The author's numerous diagrammatic sketches and brief detailed descriptions will be very useful for reference, and we advise all interested in Ichthyology to peruse this latest contribution to our knowledge of the Permian fauna. A. S. W.

In the Eastern Seas.

Australasia. Vol. ii., Malaysia and the Pacific Archipelagoes. Stanford's Compendium of Geography and Travel (new issue). Edited and greatly extended from Dr. A. R. Wallace's "Australasia" by F. H. H. Guillemard, M.A., M.D. Cantab. 8vo. Pp. xvi., 574, with maps and illustrations. London: Stanford, 1893. Price 15s.

In Natural Science for December, 1893, we had an opportunity of calling attention to volume i. of the present work, a volume by Dr. A. R. Wallace, dealing with Australia, New Zealand, and Tasmania. We have now the second volume dealing with the numerous islands of the Eastern Indian and Pacific Oceans contained between the 100 E. and 100 W. longitudes of Greenwich, and between the 30 N. and 30 S. latitudes.

The enormous area and the number and variety of lands, peoples, and natural history products therein described, make this volume of equal if not of greater interest than the former, despite the fact that volume i. dealt with lands now mainly peopled by our own kith and kin.

Dr. Guillemard has given us a book of singular value, a book not merely useful for reference, but full of the most varied information and live and general interest, one which can be read and re-read with pleasure and profit.

In a general introduction a sketch is given of the definition and nomenclature of Malaysia, Melanesia, Polynesia, and Micronesia, the extent and distribution of lands and islands, geographical and physical features, ocean depths (and these reach 19,866 feet near the Phœnix Islands, and 17,389 feet near Tonga), the races of mankind (of an interest scarcely surpassed in any other quarter of the globe), the zoology and botany, geological relations, and past history.

Passing to the special subject, Dr. Guillemard treats first of the Malay Archipelago, a geographical term which includes the Philippines, Java, Sumatra, Borneo, Celebes, the Moluccas, and the Sunda Islands. Of these, Java, Sumatra, Borneo, and the Celebes have each a separate chapter, while the numerous islands making up the other two groups are treated in the remaining two chapters. Melanesia includes New Guinea, the Fiji Islands, the Solomon Islands, and the groups of Santa Cruz, New Hebrides, New Caledonia, and the Loyalty Islands. Polynesia includes Tonga, Samoa, Union Group, Ellice, Hervey, Society, Austral, Marquesas, Phœnix, Manahiki, America, Sandwich, Gambier, and Easter Islands; while Micronesia includes the Gilbert, Marshall, Caroline, Pelew, and Ladrone Groups.

Returning to Malaysia, we find the greater part of the land area belongs to Holland, the Spaniard rules exclusively in the Philippines, an English rajah rules over a portion of Northern Borneo, a small portion of Timor is the sole remaining fragment of the former extensive dominion of Portugal, and some few islands are still ruled independently by native sultans.

Malaysia is the seat of one of the most extensive and continuous volcanic belts in the world, and this subject receives adequate treatment by Dr. Guillemard considering the space at his disposal. He has also inserted a map showing the distribution of the active and extinct volcanoes, and this is interesting as showing the almost entire absence of volcanoes in the Borneo and Celebes area, one doubtful volcano only being indicated near the western coast of Borneo, and a few at the extreme north-eastern corner of Celebes, the belt running through Sumatra, Java, Sumbawa, Flores, the Banda Archipelago, the Moluccas, and the Philippines. A sketch of the Malay race and language follows, in which the views of Professor Keane are introduced and general observations on the characteristics of the people given. Speaking of the religions of the Malays, Dr. Guillemard says: "Some of them are Christians, that is to say, they attend the services of the Dutch Church, abstain from shaving their heads or filing their teeth, and drink wine and spirits." It is to be hoped, however, that the Malay Christian has a better reputation than the "Christian" of Africa.

The picture of life in the Philippine Islands is by no means pleasing; after describing the beauty of the scenery, with its mountain ranges and tropical vegetation, the author says, "the populous towns and villages are decimated by frighful epidemics—smallpox and Asiatic cholera, while erratic flights of locusts darkening the heavens like dense clouds, devour the young crops, leaving hunger and famine in their wake. With the changes of the monsoons the swollen streams overflow the land; and when the industrious Tagal fancies he has escaped the devastating floods in his log hut or stone house, he is suddenly buried by an earthquake beneath its ruins, stifled in a burning rain of cinders from some new-born volcano, or hurried to a still swifter death in the overwhelming waters of an earthquake-wave." In a long chapter on the Flora and Fauna of these islands we read that there are 52 peculiar species of ferns, and that the proportion of monocotyledonous to dicotyledonous plants is more than one-half, a peculiar condition in a tropical insular flora. Of animals, only twenty-three terrestrial mammals are known and 303 species of land birds. But as the restrictions to scientific investigation are removed by the Government it is to be hoped that a better knowledge of all groups will shortly be forthcoming. The python in the Philippine Islands is said to reach 40 feet in length. Of the native races, the Negritos and the Malays, Dr. Guillemard has much to say, and an excellent picture of one of the former is given. Blumentrit estimates that there are 20,000 Negritos, and they live chiefly in Luzon, Mindoro, Negros, Panay and Mindanao; of the Malayan stock, the two chief tribes are the Tagal and the Bisayans, the former mustering some 1,500,000 souls, and living in Luzon, Marinduque, and Mindoro, while the Bisayans number 2,000,000 and occupy the islands between Luzon and Mindanao, and some portions of Luzon. There are numerous other races, but we must refer the reader to Dr. Guillemard's volume. After some chapters on the religion, trade, government, population, etc., the author treats the islands composing the group in separate sections, and then passes on to consider the Dutch East Indies.

Taking Java as a starting place, we are reminded that the Dutch did not get a firm hold of the island until 1830, but since then its development has been rapid and successful. A short but excellent account of its volcanoes is given, there being no less than fifty volcanic peaks on the island. The highest of these mountains reaches 12,044 ft., ten exceed 10,000, and ten 9,000, and of the fifty some twenty-five are more or less active. Salak, Galunggung, Guntur, and Papandayang are among the most destructive. The second of these, in five hours, on October 8, 1822, destroyed nearly everything within a radius of 20 miles with a deluge of hot water and mud, while quantities of ejecta fell 40 miles away from the crater. The remarkable antiquities connected with the Hindu religion still remaining buried in the forests of Java are described, and a picture of the wonderful temple of Boro-Bodor, dating from 1344, is given.

But while Java is "throughout its whole extent brought under the influence of civilisation, and covered with a network of roads and railways, Sumatra still remains, to all intents and purposes, a wild and savage land." The Dutch have been settled there since 1824, and have made slow and steady progress in the acquisition of territory; but much remains to be done to bring the island to the state of government so well seen in Java. An equally interesting account of the volcanoes of Sumatra to that of Java is given, and the story of the grand and terrible eruption of Krakatau is repeated. It is almost impossible to realise the magnitude of this volcanic outburst, even when we are reminded that a mass of land 3½ miles by 2 miles was entirely removed and a sea occupies its place giving soundings of 90 to 164 fathoms. One-half of Krakatau Island disappeared, and Verlaten Island was increased to thrice its original size. The singular phenomena of the waves of water and air are also discussed, and the fact of the explosions having been heard at distances of 2,014 and 2,968 miles is also noted. Borneo and Celebes are treated by Dr. Guillemard in the same careful and systematic way, and scarcely less space is given to the Moluccas and the Sunda group; the whole account of Malaysia occupying 375 pages of the book. Interesting notes are given concerning the edible birds'-nests from North Borneo, from which one gathers that no less than 20,000 pounds' worth are annually sent to Singapore and China. The yield from the Gomanton caves is put down at over £5,000 annually, and a description is given of the difficulties of collection from places often more than 400 feet high, and in absolute darkness except for the candle of the Dyak.

Chapter xi. deals with Melanesia. There is a good account of New Guinea, drawn from the latest publications on the island; much more exploration has to be done, however, before a general knowledge even of this vast territory can be obtained. The geology, zoology, and botany of New Guinea are still very imperfectly known, but the island has already yielded the singular *Proechidna*, some tree kangaroos, and a few peculiar rodents, while no less than 400 species of land-birds have been discovered, including 40 species of birds-of-paradise, and some bower birds allied to *Amblyornis*, which construct "playing or coursing-grounds of so remarkable a nature, that were not the facts attested by well-known naturalists, they would be almost incredible. One builds a raised ring around a small tree, this miniature circus being about two feet in height and provided with a parapet," and another, "also selecting a small tree as the centre of its building, forms around its base a bank of moss, which it decorates by inserting flowers. The ring, or circus, is round this bank, and the whole is protected from the sun or rain by a domed construction which completely covers and surrounds it, except for an entrance at one side."

It would not be fair to the author, or to our own space, to say more of this interesting book, which, with the other volumes of the series, puts geography in so pleasing yet instructive a way. We shall look forward to the volumes on Africa and Asia by Mr. Keane, shortly to appear, with considerable pleasure. The maps are numerous, well done, and all-sufficient for the purposes required, and the illustrations seem to be well selected and are a great addition to the text. To many the countries described in this volume are almost mythical, and these will specially welcome this part of Stanford's Geographical Compendium.

New Serials.

In September, 1894, the Para Museum issued no. 1 of the *Boletim do Museu Paraënse de Historia Natural e Ethnographia.* The part contains papers on "Archæology and Ethnography of Brazil," by F. Penna; "Notes on the Spiders of Brazil," by Dr. E. Goeldi; "Notes on some Terrestrial Worms of Brazil," by Dr. Goeldi; and "Observations and Impressions of a Journey from Rio to Para," by the same. A portrait is given of Governor Lauro Sodré, to whose kindly interest the Para Museum has been much indebted.

L'Ami des Sciences Naturelles is the title of a new monthly devoted to zoology, botany, and geology. The new journal is edited by Mr. E. Benderitter, and is published at Rouen.

Dr. Roux is the editor of a new serial devoted to experimental morphology, *Archiv für Entwickelungsmechanik der Organismen,* for which Mr. Engelmann, of Leipzig, is the publisher. The opening paper of the first number gives an interesting account of the methods and objects of this biological study.

The New York *Nation* says that our American contemporary *Science* is to be revived under an editorial committee. Among the names of the committee mentioned are Professors O. C. Marsh, H. P. Bowditch, W. K. Brooks, N. L. Britton, and Drs. Hart Merriam, J. W. Powell, and D. G. Brinton.

The *Footpath* is a new paper that purports to deal with natural history. Having been so fortunate as to obtain a letter by Mr. Gladstone in praise of natural history for the young, it has received some notice from the public Press. We, however, have not as yet been afforded an opportunity of judging as to the merits of its other contents.

Under the direction of the well-known psychologists, Beaunis, Binet, Ribot, Delabarre, Flournoy, and Weeks, an annual record of psychological progress is projected. The first volume will abstract all books on psychology issued during 1894, and will index all papers published during the same period that deal with the histology, anatomy, and psychology of the nervous system in man and animals, also including its pathology. This new review will also serve as the organ of the psychological laboratory at the Sorbonne.

The first yearly volume of *Taschenbuch für Braunkohlen-Interessenten des nordwestlichen Böhmens* has recently appeared at Teplitz. It consists of 80 pages, with a map.

Dr. C. R. Keyes, whose appointment as Assistant State-Geologist of Iowa we duly chronicled, has proved his worth in the newly-started Annual Report. The first volume contains a description of the geological formations of the State, and a Bibliography; the second is devoted to the coal-bearing rocks. The illustrations in both are exceptionally good, for Dr. Keyes is an artist as well as a geologist. One of them is a photograph, taken in winter, of The Cascade, near Burlington; the frozen waterfall hangs in huge icicles from the projecting ledge of "Augusta Limestone" (a term here applied to the Burlington and Keokuk Limestones combined), while behind are seen the soft underlying Kinderhook beds. We explain this obvious illustration, since it was reproduced by a scientific contemporary as an instance of "a curious formation" of limestone giving "the appearance of a cascade." There are curious things in nature, but this is not one of them: the error has been ingeniously (*not* ingenuously) corrected.

OBITUARY.

ALLEN HARKER, F.L.S.

Born 1848. Died December 19, 1894.

SCIENTIFIC circles, particularly those of the West of England, have to deplore the loss of a very able man in the prime of life. Allen Harker was in early life engaged in commercial pursuits in Gloucester; but even then he was far more attracted by scientific research, for which education and inclination fitted him. Consequently, in 1881 he was appointed to the Professorial chair of Natural History at the Royal Agricultural College, Cirencester, in succession to Dr. Fream. He had a sound knowledge, both scientific and practical, of the many subjects he was called upon to teach, and was endeared to his students by his sterling genial character, as well as by his ability to impart information. An active member and Vice-President of the Cotteswold Naturalists' Field Club, he communicated a number of papers to its *Proceedings* on the local Natural History. The cuttings on the new line of railway between Swindon and Cheltenham attracted his attention, and he first described the fine section of Kellaways Beds near South Cerney, which was afterwards visited, under his guidance, by the Geologists' Association. Other sections of Cornbrash, Forest Marble, and Great Oolite were described, and in the Great Oolite he discovered traces of *Solenopora*, identified by Professor H. A. Nicholson, and afterwards described by Dr. Alexander Brown. Among other subjects discussed by Professor Harker were the Habits of some Annelids found in Gloucestershire; the Green Colouring Matter of Animals; the Probable early Extinction of a Cotteswold Butterfly; and the Abstraction of Nitrogen from the Atmosphere by Leguminous Plants. He was a member of the Perthshire Society of Natural Science, and Consulting Botanist and Entomologist to the Newcastle Farmers' Club.

We regret to record the death of the editor of *Knowledge*, Mr. A. Cooper Ranyard, which occurred on December 14. Mr. Ranyard was born in 1845, and was a member of Cambridge University. He was a distinguished mathematician and astronomer, and carried on in an admirable way the late Richard Proctor's labours in the popularisation of his favourite study.

Dr. Frederik Johnstrup, of Copenhagen, the well-known mineralogist, died on December 31, aged 76 years. He had devoted considerable time to he geology of Greenland and Iceland.

Professor Karl von Haushofer, who held the chair of Mineralogy at Munich University, died in that city a few days ago. Professor Haushofer was also the Director of the Technical High School of Munich.

Among other losses which it is our misfortune to record are the following:—Captain Alexander William Maxwell Clark Kennedy, of Knockgray, who died on December 23 at the age of 43. He was best known as a traveller and ornithologist, and published a "Birds of Berkshire and Buckinghamshire" under the pseudonym of "Eton Boy" at the age of 16. Dr. Francis Bisset Hawkins, who died at Bournemouth on Friday, December 7. He was the oldest graduate of Oxford University, having taken his degree at Exeter College in 1818. He was born in 1797, and was one of the men to whom Richard Owen brought a letter of introduction when he came to London from Edinburgh in 1826. Auguste Jaccard, the distinguished Swiss Geologist, who died at Locle, Neuchâtel, early in January. Jaccard began life as a working watchmaker, but his attention was early directed to the rich stores of animal remains in the rocks around his home. Assisted in his researches by friendly hands, Jaccard eventually became Professor of Geology at the University of Neuchâtel. His chief work was undertaken on the Purbeckian beds of the Jura.

Professor Dr. F. A. Flückiger, who held the chair of Pharmacognosy in Strassburg University and was the author of many works dealing with that subject, died at Bern on December 13. He was born in 1828. Dr. Max Kuhn, Professor at the Königstädt Real-Gymnasium, died the same day at Berlin. His chief work was upon the ferns. Professor Dr. J. Schröter, of Breslau University, where he held the chair of Bacteriology, died at Breslau on the same day aged 58. The *British Central Africa Gazette* gives particulars of the death of Surgeon Dr. McKay, of H.M.S. "Pioneer," who was killed by a lion on October 26 in the S.W. corner of Nyasa. He had a hand-to-hand fight with the beast on the 22nd, and though carefully tended in camp, passed away after much suffering. Dr. McKay was buried at Likoma.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Dr. F. Kohlrausch, of Strassburg, has been appointed to succeed the late Professor Helmholtz as Director of the Imperial Physico-Technical Institute at Berlin. Dr. A. C. Oudemans, the director of the Zoological Gardens at the Hague, to be Professor of Natural History to the Gymnasium at Sneek; Dr. d'Arsenval succeeds to the chair of Medicine in the College of France, a position formerly held by the late Dr. Brown-Séquard; Professor Joseph Prestwich has been elected a Vice-President of the Geological Society of France; and Professor F. von Richthofen a corresponding member of the Academy of Sciences, Paris (Mineralogical Section).

The Wayneflete Professor of Physiology, Mr. J. S. Burdon Sanderson, has been appointed to succeed Sir Henry Acland as the Regius Professor of Medicine at Oxford; Dr. S. Nawaschin to be Professor of Botany and Director of the Botanic Gardens at Kiev; F. Oreste Mattirolo, Director of the Royal Botanic Gardens, Bologna, to be Professor; Mr. J. E. Duerden, Associate of the Royal College of Science, London, and for some time Assistant in the Museum of Science and Art, Dublin, to be Curator of the Jamaica Museum, Kingston, Jamaica; Mr. Joseph A. Chubb, B.Sc., to be Assistant in the Liverpool Museum, in succession to the late Mr. R. Paden; and Mr. A. Hutchinson demonstrator of Mineralogy at Cambridge, in succession to Mr. Solly, resigned.

The following appointments have been recently made in America:—Dr. G. M. Dawson to be Director of the Geological Survey of Canada, in succession to Dr. A. R. C. Selwyn, who has been superannuated; A. J. Bigney, to be Professor of Natural Sciences, Moore's Hill College, Indiana; E. G. Conklin, Professor of Biology, North-Western University, Illinois

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As we announced in our November number, the directorship of the Marine Biological Association at Plymouth has been vacated by Mr. E. J. Bles. Mr. E. J. Allen, who has been appointed in his stead, is well-known for his work on the nervous system of the Crustacea. He studied in London under Professor Weldon, and at Berlin under Professor F. E. Schultze.

Professor J. F. Blake, whose appointment by the Gaekwar of Baroda we announced last month, left England on January 9. Mr. W. H. Hudleston goes to India by the same steamer on a pleasure trip. Mr. H. N. Ridley, whose return to England we chronicled in our January number, is, we are glad to learn, resuming his directorship of the Straits Settlements Forest and Gardens Department in June. We hope the financial difficulties of the colony will not be such as to cause the authorities to abolish this important economic post. W. Siehe, of Steglitz, near Berlin, for nine years in the Berlin Botanic Gardens, is starting on a botanical journey to the almost unknown district of Cilicia Trachaea. Professors Haussknecht and Bornmüller have undertaken to work out his collections.

Mr. James William Watts has obtained the Honours diploma for the science and practice of Forestry at the Grand Ducal Forestry College of Eisenach. He comes from Carlisle, and worked for a time at a seed factory in Erfurt. The

University College of North Wales has renewed the research scholarship of Mr. E. T. Jones, who has recently been studying at Berlin. The Berlin Academy of Sciences has awarded Dr. Paul Kuckuck 1,200 marks for the continuance of his investigations on the Algæ of Heligoland.

THE University of Kiev is founding a bacteriological laboratory at a cost of £10,000.

THE awards of the Geological Society have been bestowed as follows:—The Wollaston Medal, Sir A. Geikie; Wollaston Fund, W. W. Watts; Murchison Medal, Professor G. Lindstrom; Murchison Fund, A. C. Seward; Bigsby Medal, C. D. Walcott; Lyell Medal and part of the Fund, Professor J. F. Blake; the remaining part of the Fund is divided between Benjamin Harrison and Percy F. Kendall.

THE Rev Professor Thomas Wiltshire has resigned the Treasurership of the Geological Society. Professor Wiltshire has served the Society in this capacity for thirteen years, and no one will grudge him his well-earned rest. He succeeded Gwyn Jeffreys in 1882.

MR. WILFRID MARK WEBB, of the Technical Laboratory, Essex County Council, has been added to the editorial staff of *The Journal of Malacology*. The journal, originally called *The Conchologist*, was founded in 1891 by Walter Edward Collinge, of whom a biography and portrait appears in no. 4 of volume iii.

MR. HORACE B. WOODWARD has been appointed "Resident Geologist" to the Geological Survey, in the room of the late Mr. William Topley. Mr. Woodward joined in 1867, and is known to our readers as the author of "The Geology of England and Wales." Mr. Clement Reid, who joined the Survey in 1874, has been promoted to the rank of "Geologist," after twenty years' service. Mr. Reid's chief work has been accomplished on the interesting deposits exposed in the Norfolk Coast, and in a re-survey of the Cretaceous and Tertiary deposits of Southern England, and especially of the Isle of Wight.

A NEW second-class assistantship has been created in the British Museum (Natural History), Cromwell Road. This has been filled by Mr. George Francis Hampson, who has been specially appointed by the Trustees and the Treasury to continue his work on the moths, a work on which he has been engaged at the Museum for some years in an unofficial way. We understand that Mr. Hampson, who is "over age," has not been troubled with the customary ordeal of competitive examination. We believe that this appointment has been made in deference to strong pressure brought to bear by entomologists who have long felt that this department of the Museum is considerably undermanned. It is to be hoped that other members of the scientific public will similarly interest themselves in other departments of the British Museum, which are quite unable to cope with the enormous quantities of material that annually pour into that institution.

THE annual meeting of the Geological Society of London will be held on February 15, at 3 o'clock, when the president, Dr. Henry Woodward, will deliver his annual address, which will deal with the Palæozoic Crustacea; that of the Geologists' Association on Friday evening, February 1, at 7.30 p.m., when General McMahon, the president, will read his address, entitled, "The Geological History of the Himalayas"; that of the Quekett Microscopical Club on February 15, at 8 p.m.

THE Entomological Society of France holds its annual meeting on February 27, and the Italian Botanical Society will hold its annual meeting at Palermo towards the end of April.

THE Museums Association will meet this year at Newcastle-on-Tyne, circumstances having necessitated the abandonment of the proposed visit to Edinburgh.

THE details of the sixth International Geographical Congress were issued by the Royal Geographical Society during the last few days of the old year. The headquarters of the Congress, which will be held in London from July 26 to August 3, 1895, will be 1 Savile Row, W. The subscription is £1, and will entitle the holder of the Congress receipt to attend all meetings and receive all publications concerning the Congress. The subjects specially selected for discussion are: Mathematical Geography; Physical Geography, including Oceanography and Geographical Distribution; Cartography; Exploration; Descriptive, Historical, and Applied Geography, including Anthropo-Geography; and Education. Among the special papers promised are the following: "The Distribution of Density over the Surface of the Earth in Relation to the Force of Gravity," "Geodesy in Relation to the Survey of India," by General J. T. Walker; "Photographic Methods in Surveying," by Colonel H. C. B. Tanner; "On the Construction of Globes," by Professor Elisée Reclus; "Arctic Exploration," by Admiral A. H. Markham; "Antarctic Exploration," by Dr. Neumayer; "History of Early Charts and Sailing Directions," by Baron Nordenskiöld and H. Yule Oldham; and "the Influence of Land Forms and Surface Characters on Occupation, Settlements, and Lines of Communications," by H. J. Mackinder and Professor W. M. Davis. Among the special discussions are "International Coöperation for the Study of the Oceans," for which the following constitute a committee: the Prince of Monaco, Professors Krümmel, O. Petterson, Thoulet, A. Agassiz, and Mr. J. Y. Buchanan; "Limnology and Hydrology as a Branch of Geography," for which the following is the committee: Professors Forel and Penck, Dr. Mill, and Messrs. Delebecque and Marinelli; "a Systematic Terminology of Land Forms," Professors Penck, W. M. Davis, and Dr. von Richthofen, and Messrs. Mackinder and Oldham; "To what extent is Tropical Africa suited for Development by the White Races, or under their Superintendence?" Sir John Kirk, Dr. Dove, and Messrs. Decle and Ravenstein. There will also be reports of the Committee on the Map of the World on the scale of 1 : 1,000,000, and on an International Bibliography of Geography, while Professors Levasseur and Lehmann and Mr. Freshfield will consider the subject of "Geography in the School and in the University."

An exhibition will also be held, which the Congress desires should be made as fully representative as possible of the present state and past history of geographical science, and it is proposed that this should include: Instruments used in the construction of maps; for observation by travellers; in oceanographical and limnographical research, etc.; maps of all classes, globes, reliefs, models, and appliances in use in geographical education; photographs and pictures of geographical interest; lantern slides of racial types and other anthropo-geographical matters; gear and equipment of all kinds; personal relics; publications of societies and general geographical literature. The Secretaries of the Congress are Messrs. J. Scott Keltie and H. R. Mill, from whom all particulars can be obtained.

THE Isle of Man Natural History and Archæological Society announces that the quarterly magazine of the Society, *Yn Lioar Manninagh*, will shortly be replaced by an annual volume of *Transactions*. The *Transactions* will be more strictly confined to the immediate proceedings of the Society, and will not include the various notes and news printed in the magazine.

THE library of the late Mr. William Topley has been distributed among those to whom the books will be of most service, by desire of his widow and his son. The Geological Society of London has thus been the recipient of numerous reports and out-of-the-way publications of considerable interest and value.

The amount contributed by the residuary legatees of the late Sir Charles Watken to clear the debts and liabilities of the Bristol Museum and Library Association to enable the Corporation to acquire the various collections for the citizens was £2,871 2s. 7d. Sir Charles had himself undertaken the responsibility; but dying before he was able to realise his intentions, the residuary legatees generously adopted the responsibility. The Trustees of the British Museum also consenting to the release of their interest in the endowment fund, a deed of May 31, 1894, transferred the premises to the Corporation, and vested the Bristol Museum and Library Association in the same body. The matter was formally reported to the Town Council on December 7, 1894.

The recent formation of a Field Club Union by the governing bodies of the four Irish field clubs—Belfast, Dublin, Cork, and Limerick—should lead to increased interest in Natural Science in Ireland by bringing workers in different parts of the country into closer connection with each other. The scheme was started at a very successful joint excursion held last summer at Fermoy by the three southern clubs. We learn that during the coming summer all four societies intend to hold a week's conference and excursions, making Galway their headquarters.

The spring plans for University Extension Lectures at Toynbee Hall for 1895 include the following subjects in Natural Science:—Mr. H. de Haviland will deliver a course of ten lectures on "Darwinism" on Fridays at 8 p.m., and Mr. F. W. Rudler a course on "Our Common Fossils" on Fridays at 8. Botany will be dealt with by Mr. G. May on Thursdays at 7.30; Biology by Miss K. M. Hall on Wednesdays at 7.45; Geology by Miss Raisin on Tuesdays at 7.45; Physiology by Mr. S. Rowland on Mondays at 8 p.m.

With regard to the Museum at Para, in Brazil, Dr. E. A. Goeldi, in an official letter, points out the magnificent field which the basin of the Amazon offers for the study and collection of objects of natural history. Dr. Goeldi intends, if he can find support, to found a biological station on the Amazon. We heartily wish him success.

Apropos of the interesting article on Antarctic exploration by Mr. Southwell in the earlier pages of this number of Natural Science, the following notes on polar matters may be of interest:—An Antarctic expedition to the east of Graham's Land is talked of in Belgium. The route would follow up the recent discoveries of the "Jason," and it is suggested, if the money is forthcoming, that the expedition should leave in September. Should the rigours of the Antarctic prove too much for the party to winter, it is proposed that investigations should be conducted in the lesser known portions of the Indian Ocean. A north polar expedition is projected by Lieutenant Pike, which will leave Tromso in the spring. Lieut. Pike proposes to take with him Sören Krœmer, the well-known hunter and seaman, and proceed to Norse Island *en route* for Franz Josef's Land, where he will build a hut and winter. He hopes to start in the spring of 1896 on sledges for the Pole. There is also projected a Nansen relief expedition, as the "Fram" is thought to have been nipped in the ice.

An expedition under the auspices of the Russian Geographical Society started early in December for Port Said. Its destination is Abyssinia, and among the party are Professors Jelissejev, Sevjagin, and Leontjev. They are accompanied by an ecclesiastic who carries presents to the Metropolitan of Abyssinia.

CORRESPONDENCE.

The "Zoological Record."

The *Zoological Record* for 1893 is now published, and as usual is sold in one bound volume, so that the students of one group of animals are obliged to buy not only the section they themselves need but also all the others. The price of the whole *Record*, however, is necessarily so high that most people end by buying none at all, which profits neither themselves nor the Zoological Society. To meet this difficulty some of us propose to purchase several copies of the *Zoological Record*, and to distribute the separate parts to subscribers. Although it is not intended to make any profit out of the scheme, it will be necessary to charge rather more for the separate parts than their exact proportional value, since some parts are almost certain to remain unsold. The exact price can only be determined when we know what support we are likely to receive. This scheme applies not only to the volume now issued, but also to previous volumes. Will you permit me, through the medium of your valuable journal, to ask all who may need separate sections from any of these volumes to communicate their exact wishes to me, at the Royal College of Science, London, S.W.?

This arrangement is, of course, only a provisional one, as it is hoped that the Zoological Society themselves will soon sell the parts separately. Once more it may be pointed out that this will not damage the Zoological Society, for the present purchasers are mainly Libraries, or such other institutions as need the entire volume.

S. Pace.

Merely for Information.

I have seen "the Rosy Feather Star of our seas" gripping in numbers by their cirrhi to stones below tide marks; I have seen them drop off singly into the sea as I lifted the stone into the air, and swim away with medusa-like contractions of their arms; I have also seen them squirming in the silt of the dredge. But I have never seen the "floating colonies" (Natural Science, Dec., 1894, p. 451) of this interesting species. Does your reviewer of Professor Drummond's book mean that "floating colonies" of the Rosy Feather Star are to be found in our seas, or does he wish us to understand that these colonies have an existence only in Drummond's world of pseudo-biology, or in the reviewer's own domain?

Paddy from Cork.

N.B.—The restoration of Archæopteryx, on p. 443, from Romanes' "Darwin and After Darwin," is a copy of one by Shufeldt which appeared in the *Century* magazine some eight or ten years ago.

"The Study of Existing Glaciers."

Since the appearance of this article in Natural Science, the following names have been sent to me of additional members of the "Commission Internationale des Glaciers." I quote the circular:—

"Denmark .. .. M. le Dr. K. I. V. Steenstrup, Copenhagen.
Norway .. .. M. le Dr. A. Ojen, de l'Institut Mineralogique, Christiania.
Sweden .. .. M. le Dr. Svenonius, Géologue à l'Institut International, Stockholm.

"Les presentations pour les membres de la Commission de l'Italie et de la Russie seront faites au Comité du Congrès par les organes compétants de ces deux pays. La nomination n'est pas encore survenue."

Also, M. le Dr. Léon du Pasquier, Neuchâtel, is a member of the Committee for Switzerland, and is acting as secretary till a formal appointment.

MARSHALL-HALL.

ANLAGEN.

THE meaning of words is determined by long usage and by philological and grammatical considerations, and not by the wishes of a very small section of society whom Mr. Mitchell has called "precise writers," but whom scholars would regard as ignorant blunderers.

For certainly upwards of two thousand years the words "vestige" and "rudiment," in those and slightly different forms, have been in constant use with one fixed and unchanging meaning for each. The two roots of which "rudiment" is compounded are older than European civilisation, and the word itself tells its own meaning. That meaning is *a beginning, a first attempt, or essay; something as yet unfinished, unformed, unpolished; something from which the finished product is to develop.* To apply it to an abortive organ is all very well in poetry, but not in science. Some abortive organs are believed to be lingering traces or remnants of something which has gone. "Vestige" means, and for thousands of years has meant, *a track, or trail, or foot-mark, left behind by something which has gone.* An abortive organ serves as a clue to past structure in some cases, and in these cases the abortive organ is a vestige of previous structure. To apply to either of these two—vestige or abortive organ—the word "rudiment," otherwise than in the sense in which one calls a soldier a "red herring" (that is, in a metaphorical sense), is to go out of one's way in search of confusion.

C. HERBERT HURST.

A CORRECTION.

OUR reviewer of the second edition of Blanford and Medlicott's "Manual of the Geology of India," writes to say that he finds he has misrepresented the difference between the two editions (1879 and 1893) in reference to the age of the Western Ghats. He stated that, in the earlier, the formation of the western scarps by marine denudation was "maintained," whereas it was advanced as a hypothesis only. "The statement also, that Mr. Oldham's evidence was conclusive as to the truth of the alternative theory, is too emphatic; Mr. Oldham admits (p. 495), that until the ground at the western foot of the Ghat-scarp has been examined more in detail, the theory cannot be finally established."

CHANGE OF ADDRESS.

In future the PUBLISHING AND EDITORIAL BUSINESS *of* "NATURAL SCIENCE" *will be carried on at the Offices of* MESSRS. RAIT, HENDERSON & CO., LTD., 22 ST. ANDREW STREET, HOLBORN CIRCUS, LONDON, E.C.

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NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

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NOTES AND COMMENTS.

Argon.

In our attempt to limit the pages of Natural Science to that branch of the sciences long ago called by the French "Natural" we necessarily exclude detailed reference to chemistry and physics; still we are eager to congratulate Lord Rayleigh and his patient collaborators on the discovery of a new element. It may be long before Argon is satisfactorily investigated and assigned its appropriate place or places in the hierarchy of the elements. Indeed, the new substance seems unruly and not to be set in its place without an undue shouldering of its neighbours. What is most impressive to us now is the method of the discovery and its promise for other realms of science. It was found by no restless ranging over the face of the earth seeking out the scanty and the rare. It was gained as the direct result of more careful, exact, and patient investigation of the familiar than had been done before. A few years ago biologists were ill content with any animal not dredged from the remotest ocean, not taken in the furthest land, and many far-reaching theories were founded upon the anatomy and development of "*Outis mirabilis*," some rare organism known only from Cathay. We have no wish that such investigation should be discouraged: the remotest in Nature may yield a clue to the most familiar secret. But, as we have noticed before in these pages, the present fashion in biology is to study the familiar in new ways rather than to seek out the unfamiliar. May we hope that, as Lord Rayleigh, weighing the constituents of the air, came upon an undiscovered element, so some of those who are measuring the lengths of crabs or the variations in the shells of ammonites may come upon unsuspected truths.

But in a directer fashion Argon seems to offer promise to biology. It is a constituent of the atmosphere, and enters the lungs and the air-chambers of animals and the tissues of plants. It is twice as soluble as nitrogen, and, no doubt, actually enters that flux of substances we call the living substance. Does it leave it unchanged? Is it a casual visitor, as impertinent to the stream of life as are straws to the current of a river? Here, as soon as a convenient method of preparing Argon be found, is a field that must be explored by the physiologist. It is certain enough that the most common events of protoplasmic respiration are not known in detail so minute as to exclude the operation of unknown factors. The exact influence of Argon upon vital phenomena must be determined as soon as possible. More than this, its relation to proteids must be investigated. That is an obvious piece of work that already may be in the doing. It is true Professor Ramsay found no Argon in the nitrogen prepared from urea. But as Argon will not combine with oxygen, its absence in urea by no means excludes the possibility of its presence in proteids.

Sir Robert Ball and the Cause of an Ice Age.

The *Geological Magazine* has begun under its new publishers Dulau & Co.) with two very interesting numbers. The most remarkable paper is a criticism on the astronomical theory of the Ice Age, by Mr. E. P. Culverwell, the second part of which appeared in the February number. Mr. Culverwell's criticism of Croll's theory simply confirms the view of its absolutely untrustworthy nature, which has long been held by geologists. The most startling part of the paper is, however, his attack upon Sir Robert Ball's pretensions to have corrected and improved Croll's statement of the case. Culverwell argues that the Cambridge Professor has done nothing for the astronomical theory except misunderstand and misrepresent it. He proves that the supposed fundamental error in Croll's argument, which Sir Robert claims to have discovered "and which it is the chief object of [his] book to expose" is a mere mare's nest. Sir Robert represents Croll as saying that the heat received in summer is equal to that received in winter. This, of course, is not the case, and Culverwell quotes several passages from Croll which show that he said nothing so erroneous. In fact, in the main difference between Croll and Sir Robert, all the advantages are with the former; his statement is more correct and would account for a greater difference in climate than the theory as modified by Sir Robert. All readers of the latter's little book will remember the wearisome reiteration with which the numbers 63 and 37 are used. They are conjured with all through the book. The author gives no justification for their adoption, but declares that it is impossible to discuss any astronomical theory of the Ice Age in which these magic numbers do not form the refrain of every argument. But these numbers, as Culverwell shows, have little or nothing to do with the subject. It would greatly enhance the value of perusal of the work among the class of readers who are influenced by it, if the publishers would print a note on the title page recommending that all passages containing the numbers 37 or 63 should be skipped. The misleading use Sir Robert makes of these numbers may be judged from his sensational description of their influence on climate. Thus he describes how the 63 measures of heat are poured in like a torrent in a hot summer of 166 days, while the 37 measures of heat are dragged out over the whole of the long winter of 199 days. This unfair division of heat, he says, brings about "a climate of appalling severity —an Ice Age in fact." But, as Mr. Culverwell points out, Dublin has a climate with a far more unequal division of heat than this, for there 22 measures of heat, instead of 37, have to do for the 199 days. In fact, to find a climate of the "appalling severity" of that described we have to go as far south as Madrid or Constantinople. Mr. Culverwell concludes his paper with a theory of his own, explaining the cause of Ice Ages as due to an increase in the amount of the earth's atmosphere. Whether this theory be accepted or not, Mr. Culverwell has certainly earned the gratitude of geologists by his complete exposure of the general weakness of the astronomical theory, and its special weakness as set forth in the rhetorical pages of Sir Robert Ball.

The Origin of the Irish Fauna.

A recent short paper on this subject by Dr. R. F. Scharff (*Proc. R. Irish Acdemy* (3), vol. iii., pp. 479-485) will be read with interest by both zoologists and geologists. The author does not explain the peculiarities of the Irish fauna by suggesting a direct ancient land-connection with the Spanish peninsula, nor does he believe that the entire animal population of the sister island has migrated thither from Great Britain by a post-glacial land-connection. His conclusions, based mainly upon the distribution of living and extinct mammals, freshwater fishes, and terrestrial molluscs, are that in Pliocene times Ireland was connected with Wales in the south, and with Scotland in the north, a freshwater lake occupying the central area of the Irish Sea. The southern connection is believed to have broken down in early Pleistocene times, and the northern soon afterwards. Those species which, though found in Great Britain, are absent from Ireland, are supposed to have entered the latter country after these isthmuses had been severed. The distribution of the lake fishes, of the genus *Coregonus*—in Ireland, southern Scotland, Cumberland, and North Wales—is the main evidence adduced in support of the Pliocene lake on the site of Irish Sea.

The most startling conclusion of the paper is that the entire Irish fauna is of pre-glacial origin. Those geologists who believe that the country was buried beneath an ice-sheet, hundreds of feet thick, will be unable to accept Dr. Scharff's views, and may perhaps wonder that he ignores the difficulty which their theory raises. In the detailed memoir on the subject, which he promises at an early date, we may expect this and other questions to be fully discussed. Meanwhile we welcome his contribution, as a warning that theories about the past physical conditions of the earth's surface must not contradict the facts taught us by the distribution of living animals.

IRISH CAVE DWELLERS.

THE February number of the *Irish Naturalist* contains a paper by Mr. G. H. Carpenter, which is of singular interest in connection with the origin of the Irish Fauna. Zoologists remember Mitchelstown because one of its famous caves is the only one in the British Islands known to be inhabited by a member of the peculiar blind, subterranean fauna, which, through explorations of the European and North American caverns, now includes some hundreds of species. A recent visit paid to Mitchelstown by the Dublin, Cork, and Fermoy Field Clubs resulted in the discovery of several additional species, now ably described and discussed by Mr. Carpenter.

The first found was a spider of the darkness-loving genus *Porrhoma*, and not to be distinguished from the species *P. myops*, which was based on a female taken in the cavern of Espezel (Aude), in southern France, but of which another female has since been found in Dorset. A species which, from the description and figures, appears to be identical with *P. myops*, occurs in the Fountain Cave, Virginia, and in the Bat Cave, Kentucky ; it has been named *Linyphia incerta*.

The species formerly known from this cave was a minute insect of the Order Collembola, doubtfully referred to *Lipura stilicidii*. Mr. Carpenter, however, finds that it differs from that species in many points, and therefore names it *L. wrightii*, after its discoverer.

The most interesting animal here described is another blind Collembolan, readily differentiated from the *Lipura* by the possession of a well-developed "spring," two-thirds as long as the body. Mr. Carpenter names it *Sinella cavernicola*, but points out that it differs from the North American spring-tail, *Entomobrya cavernarum* (Packard) only in the absence of clinging hairs from its feet.

A small mite, *Gammasus attenuatus*, and an earthworm, *Allurus flavus*, the latter found near the entrance, complete the list.

Of these animals, the arachnids are bleached and the insects perfectly white. The latter are also blind, and the eyes of the spider are degenerate. The mite is blind, too, but then it belongs to a blind family.

The amount of food forthcoming in such a habitat is not great. Spring-tails live on vegetable refuse as a rule, and those found here extract what nourishment they can from the fine, moist, red clay that carpets the cave, although it cannot contain much organic matter except what may be brought in by water or produced by the disintegration of some of the lower fungi. The spring-tails themselves form the food of the mites, while mites, spring-tails, and all, are apparently a prey to the spiders. These spiders do not seem to spin webs as do all their next-of-kin, but walk about seeking whom they may devour.

The wide geographical distribution of these cave-dwellers is, says Mr. Carpenter, a truly remarkable phenomenon, especially as the caves can be of no great geological age, and there can have been no migration of subterranean animals between southern Europe and Ireland, or between Ireland and North America, within any period during which the fauna can have been specifically identical with that of the present day. Mr. Carpenter, therefore, concludes that "from ancestors, presumably of the same genus, which took to an underground life in such widely-separated localities, the similar conditions of the caves have evolved descendants so similar that, when compared, they cannot, or can hardly be, specifically distinguished from each other. Should the identifications suggested in this paper stand the test of a comparison of types, we shall have proof that the independent development of the same species, under similar conditions, but in widely-distant localities, has taken place. It must be granted, however, that cave conditions are so marked and exceptional, that it might not be safe to argue from them as to what may have occurred in the upper world."

It is to be hoped that this suggestive paper of Mr. Carpenter's may evoke fresh search for cave-dwellers in other parts of the British Isles, especially in the limestone caverns of Ireland.

"The Geologist at the Luncheon-Table."

We cannot close our copy of the *Irish Naturalist* without alluding to a really charming little paper by Mr. Grenville Cole, with the above title. In a writer gifted with a less pleasing style we might resent this imitation of the dearly-loved Autocrat; but Mr. Cole's stylograph is filled with a humour and a geniality not unlike those that flowed from the famous gold pen of Oliver Wendell Holmes.

Those of our readers who have visited Dublin have doubtless received hospitality from their scientific brethren in the delightful refreshment-rooms conducted by the Misses Gardiner. It was a happy idea of the literary and scientific wits who frequent these rooms to make their owners a New Year's present of seven ornamental tables, whose tops of polished Irish marbles were carefully selected by a scientific committee. The little paper now before us shows what lessons may be learned from these table-tops by the geologist who may chance for a few minutes to sit solitary over his midday meal. The paper should be framed and hung up in the rooms that all who eat may read.

THE COURTSHIP OF THE SPIDER.

THE antics of the male spider during courtship are well known, largely owing to the careful observations of two American naturalists, Mr. and Mrs. Peckham. There are, however, the most diverse ideas as to the precise distance at which a spider can see a fly or a lady spider. The extremes are one-eighth of an inch on the one hand, and "three or four yards" on the other. These differences of opinion require, if possible, some reconciliation. And Mr. and Mrs. Peckham attempt this, partly by direct observation and partly by criticism. M. Plateau, to whose experiments upon mimicry and colour-meaning we referred in our last number, seems to have based his theory of limited vision in spiders upon the futile attempts which a female wolf spider makes to recover her egg-sac when it has been snatched away. According to the Peckhams, this does not necessarily go for much in the way of the desired proof. For the egg-sac is manufactured in such a position that the anxious parent has probably never seen it in her life, and only recognises it by touch. Direct experiment tended to prove this assertion. There is, too, as an instance of aggressive clearness of vision, the terrible spider of the pampas, described by Mr. Hudson, which starts in pursuit of anyone passing within three or four yards of its lurking place. Some little spiders kept in captivity darted upon a gnat when it was five inches away. But the sharpness of vision appears to be accentuated by love. A male of *Saitis pulex* was put into a box in which was a female of the same species twelve inches away; we are told "that he perceived her at once, lifting his head with an alert and excited expression, and went bounding towards her." That recognition, in these cases, really is due to sight, and not to any other sense, appears to be shown by the fact that if two spiders are back to back they do not become aware of each other's presence, no matter how close they may be. Moreover, one male, when in the ecstasy of courtship, was interrupted "by taking him out and gently blinding his eyes with paraffin. He was then restored to the box, where he remained quite indifferent to the presence of the females which had excited him so much a few moments before." But one of these males, apparently suspecting some trick, carefully cleansed his eyes from the paraffin by rubbing them with the palpi, and then began "dancing before a female three and one-half inches away." A female of a species named *Astia vittata*, observed to attract the opposite sex, was temporarily removed and painted of a bright blue; the male spiders, who had before been unremitting in their attentions, treated her with the most absolute indifference. After a few moments, however, one of them suddenly leapt upon her, but whether this was the result of hunger or of love does not appear. At any rate, the experiment seemed to argue some colour-sense in the creatures. A further series of experiments put this belief upon a firmer footing. The approach to the lair of a spider was covered with variously coloured paper, which at first proved baffling, but was later recognised. Altogether it would appear that spiders are by no means so deficient in sight and in the power of differentiating colour as has been urged in some quarters. But whatever may be the ultimate value of Mr. and Mrs. Peckham's paper, there is no manner of doubt that it is exceedingly interesting reading. It is to be found in vol. x. of the *Transactions of the Wisconsin Academy of Sciences*.

Comparative Edibility.

With reference to the note in our February number on warning colours and edibility, Mr. O. H. Latter has kindly sent us the following observation. He writes:—"It is well-known, and I have myself observed, that all our 'Cabbage' Butterflies are immune from attacks by birds, presumably because of some unpleasant taste or smell. Wasps, however, have twice been observed by me in the act of devouring these butterflies. Earwigs, too, which undoubtedly possess an unpleasant smell when irritated, fall victims to wasps, in spite of their malodorous attributes."

The Origin of Oolite.

In the last number of Natural Science we referred to a paper by Mr. G. F. Harris on the "Analysis of Oolitic Structure"; curiously enough, a week later, Mr. E. Wethered made a communication to the Geological Society on a somewhat similar subject, only that the last-mentioned author laid emphasis on the origin of the granules. Mr. Wethered has "long entertained the opinion that all oolitic granules are of organic origin, but has not, up till now, been in a position to prove this." The class of evidence laid before the Society mainly consisted of micro-photographs of sections of pisolites and oolites, and we must say, that, like many of the speakers who joined in the discussion, we failed to see the *proofs* referred to. The first few slides shown, appertaining to pisolite, were convincing enough, if a series of vermiform tubules could be taken as evidence of the origin of the granules themselves. That these tubules are the work or the remains of organisms, few would care to dispute. But when the author desires us to see *tubules* in certain of the oolitic granules, we draw the line, and ask for further proofs. We do not suggest, for a single moment, that with the "eye of faith" they could not be detected on two or three of the slides of oolite; but it struck us that much of what we saw could be explained very well indeed as due to ordinary chemical action, while several of the structures shown and referred to as proofs of organic origin appeared to be much more like the results of alteration in the granules since they were formed. For a hypothesis to succeed, it is necessary that all the facts ascertained shall be concordant with it; we do not wish to convey the idea that the origin of oolitic granules, wherever formed, is or has been due to one set of causes, but it is not too much to ask that granules shown in any one micro-slide shall have originated by one method. There should be no selection of isolated granules from a large number of slides, when the majority of other granules, equally clear, do not give that evidence which coincides with one's views. We are far from saying that oolitic granules are never of organic origin. Rothpletz has shown that structures somewhat similar to those found in oolite limestone are produced by fission algæ, and it may be observed that a like structure is seen in many calculi. At the same time, it cannot be ignored that perfect oolitic granules have been made artificially by purely chemical means, and "oolitic granules" are not unknown to be forming under conditions totally unlike those described by Rothpletz and others. If Mr. Wethered had contented himself with drawing attention to the similarity of some of the granules exhibited by him to those of the Great Salt Lake mentioned by Rothpletz and described by certain American authors, we should have nothing to say; but when he asks us to recognise systems of tubules running concentrically round the nuclei of granules, and others radiating from these nuclei, we think he is demanding too much—especially with reference to the "radiating tubules." We are not of those who believe that, in order to show the organic origin of the granules, it is necessary to prove the existence of tubules, though, of course, when these can be satisfactorily demonstrated, they add interest to the problem. We are pleased to see, however, that this difficult subject is receiving the attention it deserves, and have no doubt that the researches of Mr. Wethered and others will do much towards its ultimate satisfactory elucidation.

Peridotites and Phosphates.

That close and painstaking observer, Mr. T. H. Holland, who so recently distinguished himself by his work on the Gohna landslip, has returned to the less exciting details of Indian igneous rocks. In two papers in the *Records of the Geological Survey of India* (vol. xxvii., pp. 129–146) he describes certain peridotites from Bengal. It is quite refreshing to be provided with a summary of the literature poured out upon this timid and retiring group of rocks at a time when they were a prevailing fashion—say, in 1885. We fancy that a somewhat pale biotite is commoner in peridotites than Mr. Holland supposes; but his phosphatic series from the Lower Gondwana rocks is a distinct addition to our knowledge. In some of these apatite is so abundant, in characteristic rod-shaped crystals, as to have been mistaken by a previous observer for felspar, and as to form a near approach to ophitic structure in the larger crystals of biotite. These rocks are intrusive in coals in a manner that suggests the possibility of the occurrence of diamond as a contact-product, since, as Mr.

Holland points out, that attractive mineral seems connected with the junction of peridotites and carbonaceous rocks. We believe, however, that at Kimberley the diamonds are still forthcoming, although the neck of ultra-basic rock has been worked down into the granite that underlies the shales. No diamonds have yet been found in connection with the Indian peridotites, so that their interest is for the present somewhat esoteric. Even the large quantity of phosphoric acid, present in the form of apatite, is not enough to render the rock of economic value. Not only does the apatite withstand decomposition longer than all the other constituents of the rock, with the exception of the biotite, but "even the richest form would be poor compared with the basic Bessemer slags, and the use of these has been attended with indifferent success."

Fortunately, the world has as yet no need for a fresh source of the fertilising phosphates. From the *Revue Scientifique* we learn that a wide band of phosphate of lime, 70 or 80 kilometres in length, runs right through Algeria and extends into Tunis. Its richness is remarkable, and the supply is enough to furnish the whole of Algeria and Europe into the bargain. At Tebessa, not far from the frontier of Tunis, the phosphate is already being worked, and it is estimated that there are in the quarries a hundred million tons of this precious food for cereals. France is to be congratulated. But, alas! even here she is not safe from the perfidious and enterprising Albion. Of the three concessions at Tebessa, two are already in the hands of British companies, who pay a royalty of only 50 centimes per ton. As the market price of the phosphate is 40 francs a ton, it can be sold on the London market at a profit of more than 20 francs per ton. Here is a grand opening for the Socialists to spring another attack on the President of the Republic, and to furnish the papers once more with the familiar headline, "A Scene in the Chamber."

But we must return from the stormy seas of politics and commerce to calm scientific contemplation of phosphates. France has yet other phosphates, and among them some of the most remarkable are found in pockets in the great limestone plateau that stretches from Uzès to the neighbourhood of the Rhone opposite Avignon. The limestone is of Lower Cretaceous age, being that which is called Urgonian, from the town of Orgon, near Arles. The pockets of phosphate of lime are of two kinds. In the first type the Urgonian Limestone is penetrated to a depth sometimes of 60 metres by large pockets, filled by coarse sand, with grains of flint and small phosphatic nodules of chalky consistency. The pockets also contain blocks of Urgonian flint, with the casts of such characteristic fossils as *Requienia* and *Monopleura*; but there is no trace of vertebrate bones. In the second type, narrow and irregular cracks, sometimes 20 to 30 metres deep, furrow the Urgonian plateau in all directions. They are filled by a calcareous clay of reddish colour, more or less ferruginous, and richer in phosphate than the first kind of deposit.

The phosphate is often concretionary and stalagmitic, just like the well-known phosphorites of Quercy. Like them, too, it contains, especially in the upper part of the pockets, an abundance of bones and teeth of vertebrates, belonging to the oldest Quaternary, or perhaps to Pliocene times. These phosphates have recently been studied by Mr. Charles Depéret, who has communicated his results to the Academy of Sciences of Paris.

Geology in France.

The University of France appears to have dismissed geology from its course of higher studies with some abruptness, although it is true that the science is suffered to remain in the fourth class of secondary instruction. This action has caused a vigorous and well-merited protest to be issued by the Geological Society of France. Obviously, it is ridiculous that a science which is, as it were, a summary of all the others, should be taught to beginners and prohibited to advanced students. To put the question on the lowest grounds, it would be a sad day for any country when so eminently practical a science were thrust into the hands of those who can have only a rule-of-thumb knowledge of it. This science has over and over again shown the enormous value of a thoroughly philosophical training to all who would utilise their knowledge for their own or for the public good. We venture to give our hearty support to the French Geological Society, and to hope that its protest may be speedily effectual.

Madagascar Fossils.

The February part of the *Quarterly Journal of the Geological Society* contains a valuable paper by Mr. R. B. Newton, "On a Collection of Fossils from Madagascar," which should interest readers of the article on Madagascar in Natural Science for September, 1893. The chief interest of Mr. Newton's paper lies in the careful and detailed account of previous work on Malagasy geology, which, with its abundant references, cannot fail to be of considerable service to all interested in the faunal riches of Madagascar. The specimens described were collected by the Rev. R. Baron from the Tertiary and Secondary formations in the north and north-west of the island. The figures accompanying the paper show that the specimens are not so perfect as might be wished, and it is to be hoped that future collectors will discriminate when sending home such heavy material. This imperfection is specially unfortunate in the case of the dinosaurian remains sent to England by Mr. Last, and more recently described by Mr. Lydekker before the Geological Society. The remains give evidence of most interesting animals, but are so insufficiently preserved that little can be done with them. More careful search would, no doubt, reveal further materials.

When is a Ruby not a Ruby?

It is recorded that a police constable on duty in a public museum was once overheard making zealous reply to a visitor of an enquiring turn of mind, who had asked the meaning of the word corundum, conspicuous upon a case of minerals; "Oh, that be the place where they put all them stones as they can't guess at." Though inaccurate as an indication of the habits of museum curators, this definition might well refer to the riddle relating to this very mineral which museum curators and others will, unless we are greatly mistaken, be soon called upon to rede.

The artificial rubies made in Paris a few years ago by Messrs. Frémy and Feil were regarded as scientific curiosities. But stones are now being largely sold (it would be very interesting to know how largely) in London and elsewhere which, while closely resembling in all essential respects the rubies of Burma, are undoubtedly of artificial origin. Tried for hardness, specific gravity, lustre, and subjected to all the tests which are usually applied to precious stones, they cannot be distinguished from the natural ruby; this is not surprising, for they are not, like other artificial stones, different from what they profess to be, but are actually crystallised red alumina, only differing from the natural ruby in the process by which they have been produced. Examined with the microscope they betray their origin by the glassy enclosures which they contain and sometimes by a streaky appearance.

Yet it would be difficult to assert that these are not rubies, unless, indeed, the definition of a ruby be understood to include of necessity a natural origin. Considering, however, the enormous prices paid for Burmese rubies, it is certainly not fair that mere imitations should pass as such. If their beauty as jewels be equal to that of the true ruby, let them by all means fetch as high a price as they deserve on their own merits; but we cannot refrain from speculating as to their market value if they were labelled "Made in Paris." According to French law it has been decided, we believe, that a ruby is certainly not a ruby when it is made in a crucible.

Oranges and Frost from Florida.

Walt Whitman brings forward as a proof of civilisation—

> "A bunch of orange buds by mail from Florida.
> To my plain Northern hut, in outside clouds and snow,
> Brought safely for a thousand miles o'er land and tide."

For us the Florida oranges are a similar proof of the smallness of our modern world, with its civilisations made interdependent through commerce, just as its natural phenomena are proved already interdependent by science. A few weeks ago the wholesale price of Florida oranges was some ten shillings a box, but now the price is twice or three times that amount. A cold wave passed through

Florida in the last days of December; frost appeared, an unwonted and unwelcome visitor. The fruits on the trees were frozen, next season's buds were blasted, and many a young orchard was destroyed. Hence the higher price of oranges, and the diversion of the trade to our own sunny South. But even thither, as the *Revue Rose* tells us, this cold wave shortly after found its way, damaging the bananas and the early vegetables of Algeria. Lastly it came to us, and consoled the populace for their lack of oranges, by an abundance of skating.

Flora of Borneo.

Dr. Stapf's exhaustive paper on the Flora of Mt. Kinabalu in North Borneo occupies part 2 of vol. iv. of the Linnean Society's *Transactions.* It is based chiefly on plants collected by Dr. G. D. Haviland in 1892, but the author has also made use of material obtained by Sir Hugh Low more than forty years ago, as well as some more recently acquired by Mr. F. W. Burbidge, who went in search of treasures of horticultural interest for Messrs. Veitch, of Chelsea. The paper includes an interesting account, supplied by Dr. Haviland himself, of his ascent of the mountain, in weather unusually wet even for Kinabalu. The southern spur of the mountain forms, like many Bornean heights, a knife-like ridge a few feet broad at the top. "Along this is a track, probably kept open by deer, on either side of which grow plants. These are almost always in flower, and love the sunshine, which can be here felt even through the mist. Among these plants are notably *Vaccinium*, *Rhododendron*, and sometimes Pitcher-plants. A few feet lower down the vegetation is completely changed." "The sides shelved steeply down, and on them grew the sub-summit shrubs with lanky, bent, and angled stems, densely clothed with long damp moss and lichen, and growing *from* the slope rather than *upwards* in the struggle for the light." This zone was more than 100 feet in height; below it came common jungle of mixed trees, shrubs, and herbs. There were no monkeys, and the birds and squirrels were different from the low-country types. At about 11,000 feet quite a different region was reached at the foot of the granite cap which crowns the whole. "Here were patches of shrubs in flower, patches of bare rock, and patches of mossy swamps, where grew buttercups, potentillas, and a small white gentian." The plants on the top itself "were stunted, only a few inches high, growing only in the crevices of the rock, to which they had a very firm hold, so that it was difficult even with both hands to collect them with their roots." In two hollows shrubs were growing; in one, the blood-red rhododendron, in the other, *Rubus Lowii.*

Dr. Stapf distinguishes four zones of altitude. The first, the Hill Zone, from the foot of the mountain up to 3,000 feet, almost entirely occupied by cultivated land and young jungle, or secondary forest, which springs up rapidly on abandoned clearings. This secondary forest is essentially evergreen, with all the characteristics of a true tropical forest. Here are found palms of the genera *Areca*, *Pinanga*, and *Calamus*, while bamboos play also a prominent part, rising in immense feathery clumps to 50 or 60 feet on the river. In the cultivated land the most important crops are kladi (*Caladium esculentum*) and rice; then sweet potatoes, yams, bananas, tobacco, gourds, melons, cucumbers, and chillies.

Next comes the Lower Mountain Zone (3,000–6,000 feet), occupied, with occasional interruptions or modifications, by old jungle or primary evergreen forest, described as abounding in fine, tall trees, creepers, and epiphytes, commonly also in undershrubs, while the ground is carpeted with ferns and mosses, which increase in quantity and luxuriance with the elevation. A few slender, graceful palms are also to be seen. Bamboos are again a characteristic feature, growing in thick clumps beneath the trees, or skirting open places, or scrambling and creeping and smothering everything. Among the herbaceous plants may be mentioned a violet.

This is followed by the Upper Mountain Zone (6,000–10,500 feet), in which two formations are recognisable from the specimens, namely, primary evergreen dwarf forest and bogs. The forest consists of small trees and tall shrubs from 10 to 20 feet high, sometimes well separated, sometimes forming almost impenetrable thickets. The trees are stunted, twisted, and weather-beaten, with trunks and branches clothed inches deep with dripping moss and festooned with long, beard-like lichens. Only conifers form fine trees in some more favourable places. There is a marked tendency among some of the species to grow gregariously. The foliage is often crowded on short, thick branches; the leaves, which are stalkless or with short, stout petioles, are very coriaceous, dark green in colour, smooth, and glossy. Oval and round forms are frequent. The shrubs blossom nearly all the year round, many of them very freely. No less than nine rhododendrons adorn the ridge with brilliant flowers in clusters or scattered among the dark foliage. Among the climbers, the wonderful pitcher plants are by far the most peculiar feature; at least five species occur here, some climbing in the trees, others scrambling over shrubs or straggling on the ground. Here, too, the ferns attain their most luxuriant development. Two tree ferns are represented, one—a new *Cyathea*—was found at 10,500 feet. The bogs are confined to a few spots, where grow a *Drosera*, a *Utricularia*, some sedges, a dwarf gentian, and some interesting plants of Australian affinity.

Of the Summit Zone (10,500–13,698 feet), the greater part is occupied by bare rock. The forest, dwarfed to a mere shrubbery, ascends to 12,000 feet, while on a flatter area, where many little streams collect and unite before beginning their rapid descent, boggy patches occur, with buttercups, potentillas, and gentians. Here and there a scanty vegetation clings to the rocks.

Dr. Stapf discusses at some length the affinities of the flora of the different zones; among the most interesting is the Australasian, indicated by such plants as a *Patersonia*, two species of *Coprosma*, a species of *Pratia*, and others.

THE TROUBLES OF A MISSIONARY.

As everyone knows, scientific people, from Darwin down to the merest collectors of bugs, have been indebted in many ways to missionaries; for these go to the uttermost parts of the earth, see much of the natives, and have opportunities for collecting, of which they have availed themselves to the great benefit of science. But there is another side to the question. We have received from a correspondent the following extract from the letter of a missionary:—

"Whether it is due to the Board Schools, or to the improvement in the mail service all over the world during recent years, I think modern missionaries ought to be deeply pitied for the great amount of time and patience they have to expend in writing answers to letters which the old missionaries could never have been bothered with. People write from all parts to say they sympathise with us—will we give them an account of the country in which we live. They write to say they pray for us—will we write and tell them what we are doing. They write and say they are interested in moss, ferns, orchids, beetles, snakes—will we collect, preserve, and bottle a few specimens, and in this way further the interests of Science. It takes three minutes to write questions that it would take a day to answer. I think of filing all such letters for the future, and getting a few forms printed off which will serve all purposes. Something after this style:—'I beg to acknowledge with {thanks / gratitude} your letter of ———, assuring you that I value your {sympathy / prayers / requests / interest}, and that the subject you mention shall receive my careful attention when I find time hanging heavily on my hands.'"

We trust that our correspondent's friend will not carry out his intention; but scientific people would do well to be considerate in their requests.

TRINOMIAL NOMENCLATURE.

WE are glad to print "O. T." 's letter on this subject, as it gives us an opportunity of explaining our views with more clearness than we seem to have hitherto attained. "O. T.," we must first point out, writes as a zoologist, while our note dealt with the matter professedly from the standpoint of the botanist. The trinomial system, as "O. T." clearly explains, is that "under which races, especially *geographical* races, thought to be of less than specific rank, receive distinctive Latin names in addition to their specific ones." Since the system is applicable only to such races, and not to aberrations, individual variations, and such like, it is clearly immaterial whether "subsp." is or is not inserted between the second and third terms of the series. The term "races," then, is, in the mind of "O. T.," synonymous with the term "subspecies." This is all very well, and upon certain conditions the system is acceptable enough. The conditions are, first, that the races in question shall be sufficiently definite to be worthy of a Latin name; secondly, that the relation of the subspecies to the species shall be of approximately the same character as the relation of the species to the genus; and finally, that usage shall be invariable and consistent. If zoologists find their discussions facilitated by such a system, and if they will conform to these conditions, we shall not say them nay.

The whole point of the protests that we have raised against certain botanists lies in the fact that these writers, while outwardly employing a trinomial system, have violated its first principles. Let us see how our critic's dicta work out with plant names. Anyone who knows anything about the latter, knows that the usual term for indicating a rank less than that of the species is "var.," but unfortunately "subsp." is used often enough to make it uncertain what term we are to supply when no indication is given. Take, for instance, the first volume of De Candolle's *Monographiæ Phanerogamarum*, the volume containing the names to which we called attention. The book includes three large and important natural orders, elaborated by three well-known botanists all of the first rank. In the first—Smilaceæ—by Alphonse De Candolle, subspecies are frequently admitted; but in the second and third—Restiaceæ, by Dr. Masters, and Meliaceæ, by C. De Candolle—only varieties are found. A repetition of the examples quoted in our former note will show the utter want of comparison between the unmistakable binomial and the very doubtful trinomial. *Smilax Bona-nox Wrightii* stands for *Smilax Bona-nox*, subsp. *Wrightii; S. Bona-nox senticosa* stands for *S. Bona-nox*, subsp. *polyodonta*, var. *senticosa; S. invenusta armata* stands for *S. invenusta*, var. *armata*. So that, on "O. T." 's definition of trinomialism, we are right once in three times, and this is a high average. If we admit *Smilax Bona-nox Wrightii*, why not *Smilax Bona-nox polyodonta senticosa*, and so on? Should we then be any better off than under a pre-Linnæan system, in which the few words by which a plant was known did at least convey some information about it?

Esoteric Science.

Max Nordau, in his well-known work on Degeneration (an English translation of which is on the eve of publication, and which may be expected to become the sensation of the dinner-tables of this season), extends the method of Lombroso to a discussion of most of the prominent writers, musicians, playwrights, and poets of contemporary Europe. We hope to give our readers a detailed account of his book next month. Among the many symptoms of degeneracy that he diagnoses in modern work is a fondness for esoteric terms, for involving a plain idea in an intricate word. Signs of this tendency occasionally are apparent in our own pages; unfortunately they are too familiar in scientific writing generally. We are well aware that frequently the invention of new words is a necessity and a gain; if a new idea has to be expressed, exactness and conciseness may be found only by neology. But what we must insist upon is that the necessity is always a misfortune, and that the neologist, instead of claiming gratitude, should be a suppliant for pardon and should present the fullest excuse. Last month we put the American "letusimulation" in the pillory; this month the English "isocleronomic" and "anisocleronomic" must succeed it.

In his treatise on the germ-plasm, Professor Weismann expanded an idea that he had previously suggested. It is the idea that two kinds of cell-division exist. In the one case the tendencies and characters of the daughter-cells are exactly alike. Each has received an equal legacy from the parent, and, the legacy being supposed to differ only quantitatively and not qualitatively from the stock of characters that were halved, when the daughter-cells grow up to full stature each becomes an exact replica of the parent. In the other case (the existence of which is not admitted by Weismann's critics), the legacy received by the two daughter-cells differs qualitatively, the characters and tendencies of the parent are distributed unequally between the daughters, and, when these grow up, each is unlike the other and the parent. These two kinds of division Weismann designated as "erbgleich," "equal-heired," and "erbungleich," "unequal-heired"; but, throwing a sop to esoteric terminology, he called them also "Homœokinesis" and "Heterokinesis." Professor Hertwig wrote a treatise (of which a full account was given by Mr. Chalmers Mitchell in Natural Science for August, September, and October, 1894) largely devoted to disproving Professor Weismann's hypothesis of the existence of unequal-heired division. Mr. G. C. Bourne, writing in *Nature* last month a criticism of Professor Hertwig's criticism of Weismann, and agreeing that the existence of the two kinds of division has not been established, invents the new names for them *isocleronomic* and *anisocleronomic*, overlooking, first, that we might have the fact before the name; secondly, that the German name and its English equivalents are ample; thirdly, that if Greek be necessary the original inventor has already suggested Greek names.

I.

The Mammals of the Malay Peninsula.

Part III.

UNGULATA :—As might be expected, the larger Ungulates are absent from so small an island as Singapore; but Deer (*Cervus equinus*), Wild Pig (*Sus cristatus*), and the small Mouse-deer (*Tragulus*) still occur, the latter being abundant. In the Peninsula are also the Elephant, Rhinoceros (one or two species), Tapir, Wild Ox (one or two species), the Kijang (*Cervulus muntjac*), and the Wild Goat (*Nemorhœdus sumatranus*).

The Wild Pig (*Sus cristatus*), or "Babi Hutan," is far too abundant. It is most destructive to the pineapple- and tapioca-fields. Coming out of the woods at night and falling on the crops, it contrives to do a great deal of damage in a very short time. It is not entirely nocturnal, for one may often see it moving about in the day-time, and I have seen boars feeding among the buffaloes in the swamps at midday. The young are spotted with yellow, like the young of the tapir.

The habits of the Rhinoceros here are but little known, and it is by no means certain how many species there are. The Malays call it "Badak," and they also talk of a beast by name "Badak Api" (*lit.*, fire-rhinoceros). From the only native I ever met who could tell me about the latter animal, I gathered it had a red appearance, and guess it to be perhaps *R. sumatrensis*. The animal is not often seen, and I never heard of one being trapped. Europeans sometimes shoot them, but never take the trouble to bring any specimens home, so that it is really difficult to decide what species we really have. The common one appears to be *R. sondaicus*. It frequents the hill-jungles, ascending to 4,000 feet altitude, and seems usually to move about at night, though one may come upon it by day. It has a habit of constantly using the same track, and dropping its dung in the same place daily, a habit common also to the tapir. As the jungle gets cleared, it wanders often into the low, open country, apparently losing its way. It is a quiet, inoffensive beast.

The Tapir (*Tapirus malayanus*) is called "Tenok" by the Malays; the name "Kudah ayer" (water-horse) given as a Malay name in some Natural History books is not known to any native I have ever met, and appears to be entirely fictitious. This animal is still tolerably abundant in the further jungles of the interior, and though rarer than the rhinoceros, is oftener to be seen in captivity. It is wonderful how so defenceless an animal should be able to escape the attacks of the bigger carnivora; but it inhabits the deeper hill-woods where the tiger is scarce or does not go, and is very quick at hearing and slipping away from an enemy. It eats grass, and more usually bushes. A tame one was found to prefer the common *Melastoma polyanthum* to any other plant. It also ate fruit and boiled rice, and was very fond of biting and eating bones.

The young one is dark brown, with yellow, creamy spots, a most beautiful adaptation for protection; as it lies during the hot part of the day under the bushes, its coat is so exactly like a patch of ground flecked with sunlight that it is quite invisible. Some of the spots are round and some are elongate, the former resembling the flecks of sunlight falling vertically, the latter those which come slanting through the foliage. The little animal lies in such an attitude that the round and long spots are exactly in the position in which the sun-spots would be. On one occasion my tapir was lying asleep during the hot part of the day among some bushes, and wishing to shut it up, I went to look for it; but on parting the bushes I could not see it at all, though I was absolutely looking down upon it. In the jungle any animal of a single colour is much more conspicuous than a parti-coloured one, the mass of colour striking the eye more clearly. So that an entirely black or entirely white animal is easily seen; but a mixture of the two colours blends with the reflections of light and shadow falling on the ground through the foliage. The adult tapir is black, except for the tips of its ears and from the saddle to the rump, which parts are greyish-white; this would be thought conspicuous enough at first, but it is not at all so. When lying down during the day, it exactly resembles a grey boulder, and as it often lives near the rocky streams of the hill-jungles, it is really nearly as invisible then as it was when it was speckled. It feeds at dawn, and till the sun gets hot, when it retires to sleep, recommencing at dusk.

The tapir when young utters a kind of whistle, which can be heard afar; it is done through the trunk. The adult gives a kind of low coughing bark, as an alarm-note. As in its haunts it has often many obstacles, such as fallen trees and rocks, to scramble over, it is quite skilful at clambering about, and when as big as a fair-sized pig my animal would contrive, if left alone, to get upon a chair and thence upon the table. It can trot, or gallop at a good pace, and goes very fast through the jungle when disturbed. When taken young it becomes very tame and amusing. As the jungles are cleared the tapir becomes rarer and rarer, but it is to be hoped that it may be long ere this most interesting element in our fauna becomes extinct.

On the Elephant it is hardly necessary to give any notes. It is not excessively abundant, but is plentiful in many parts of the Peninsula. It is called "Gajah" by the Malays, who seem rather to dread it. The Aborigines, known as Sakais, sometimes hunt it. There was recently living a man who used to follow up a herd and, selecting an animal with good tusks, creep quietly up; when near enough he would drive a large-bladed spear between the hind legs into the abdomen, which wound was soon after fatal, and tracking the animal, he would secure the ivory.

The black Wild Goat (*Nemorhædus sumatranus*) inhabits the isolated patches of limestone rocks which flank at intervals the main granite chain of the Peninsula. Though apparently not rare in these places, it has never been shot by any sportsman, and the only specimens I have seen are skeletons and a head in the Perak Museum.

The natives often affirm that there are two distinct species of Wild Ox in the Peninsula, viz., the Sāpi and the Sĕladang; but I have never been able to find out what the former is. It is stated to be quite a different animal, with longer horns than the Sĕladang (*Bos gaurus*). The latter is probably the biggest and most powerful ox now living, though even it is inferior in size to the old British ox, *B. primigenius*. The Sĕladang usually inhabits the denser hill-jungles, where its tracks may often be seen; but it is also abundant in more open, grassy spots, such as the banks of the Pahang river. It lives chiefly on leaves of trees, but also eats fruit.

There are two or three species of Mouse-deer in the Peninsula, of which the commonest is the Napu (*Tragulus napu*). It is as big as a hare, and generally inhabits the thicker woods. It can swim well, and I have seen it crossing a small stream, swimming very low in the water. The call is a low bark, like that of a fallow-deer, though by no means as loud; but they often call to each other by beating their feet upon the ground. This is taken advantage of by the natives in shooting them, in the following way. A hunter, placing a dry leaf on the ground in the wood where mouse-deer are supposed to be, taps it with a stick, thus—Tap: tap-tap-tap: Tap: tap-tap-tap: Tap, now and then making a rapid succession of taps like the roll of a drum. Presently the Napu answers by stamping its feet in the same manner, and the tapping is continued. The animal comes nearer and nearer, answering as it comes, till it comes within range of the gun. Sometimes, it is said, a tiger, thinking that the tapping is really that of a Mouse-deer, comes up instead. The Napu is also caught by springes. A small palisade of sticks about a foot high is made across a wood, and at intervals passages are made in the wall big enough to admit the animal. In these is set the springe, made by bending down a small slender shrub, with a noose held in position by a small stick, and the noose is covered by a leaf. The Mouse-deer, on meeting with the palisade, does not jump over it, as it could easily do, being very active at leaping, but goes along it till it can find a passage through, and putting its foot in the noose is immediately caught. It is sold in the markets for food.

In captivity the Mouse-deer require plenty of room to run about or they will stay perfectly still in one corner till the hind legs become callused at the joints, and even paralysed. They become very tame, but sometimes fight together, biting each other's ears and noses. The males can also give sharp cuts with their long, sharp canine teeth. They eat sliced sweet potatoes, and almost any green vegetable. Sometimes they do a good deal of damage to garden crops, and are rather troublesome.

The Napu varies somewhat in colour, and I have had some specimens which were remarkably bright chestnut on the back. These came, I was told, from Rhio, south of Singapore. They appeared so distinct in colouring that it is possible that they may belong to a distinct species.

The Kanchil (*Tragulus kanchil*) is a much smaller, perfectly brown animal. It is supposed to be very cunning and plays the part of Brer Rabbit in Malay folklore, but I never saw anything in its behaviour to justify its reputation. The natives state that there is a third species which is called Pelandok, intermediate in size between the two; but the name is also used vaguely for either species, and if there is a third species here, I have either not seen it or failed to distinguish it.

The Napu breeds in confinement, producing one or two at a birth. It appears to have a long period of gestation, for one that had been for some months in an enclosure without a male unexpectedly produced a young one.

The Kijang (*Cervulus muntjac*) does not occur now in Singapore, if it ever did. It is abundant in many places, such as the slopes of Mount Ophir, and is often shot by planters and others in and about the coffee plantations. Very little is known of its habits.

The Rusa (*Cervus equinus*) is common in the Peninsula, and a few still occur in Singapore; but most have been killed by sportsmen. It inhabits the open country and small woods. I have, however, seen the tracks of a big deer quite at the top of Mount Ophir, at 4,000 ft. elevation. As the animal seems to avoid paths when possible, it is comparatively rare to find its tracks, while those of tiger, pig, mouse-deer, ox, tapir, rhinoceros, and elephant are all conspicuous whenever one is in the district where they abound. It feeds at dusk and dawn, remaining quietly in the woods during the day. It is often kept in captivity, and sometimes breeds; but the buck is rarely safe in the rutting season, and sometimes becomes then most dangerous. A fine example in the Botanic Gardens, though brought up from a fawn, on one occasion attacked the coolie who was giving it water, and tossed him over the palisade of its enclosure, inflicting severe wounds on him, and later succeeded in forcing its way into an adjoining paddock, where a black buck was kept, which it killed by one thrust of its antler. The young are produced singly, and are coloured like the adult, but with much softer hair. There are, however, faint traces of light spots on the rump, which disappear after the first week.

The natives say that there are two species of deer—the Rusa Daun (foliage deer) or Rusa Hijau (green deer), and the Rusa Lalang (grass deer), the former residing in the forests, the latter in open grassy country. They are said to differ in colour, and are probably merely local varieties.

Sirenia :—The Dugong (*Halicore dugong*) is tolerably common in the strait between Johore and Singapore; but one does not often see it. However, the Chinese sometimes catch it in nets when fishing, and sell it in the markets as food. It is said to live on the marine phanerogam Setul (*Enhalus acoroides*), but very little is known about it. I have seen it sleeping on the surface of the sea, when it looked like an old brown trunk of a coco-nut tree floating about. It is called "Duyong" by the Malays. It is remarkable that an animal so defenceless and slow should be able to hold its own against the sharks and crocodiles which abound in its haunts.

Edentata:—Our only representative of this Order is the common *Manis javanica*, the Tengiling of the Malays. It is frequently found in open sandy country making large burrows in the ground. Termites form its principal food, but ants are also acceptable to it. From the difficulty of feeding it, it is impossible to keep it long in captivity; but it is often brought for sale, and I have had a female with a young one for some days. In spite of its appearance it can climb trees well, but slowly, using its prehensile tail to aid it, and one would remain for a long time suspended by its tail to a bough, its head curled in between its paws. The Chinese are fond of eating this animal, but the flesh is tough and not worth eating.

These notes are naturally very incomplete as an account of our mammal fauna, but they may call attention to many points which require settling, and which it is to be hoped intelligent and observing explorers will in time work out. It is only by watching the animals in their native haunts that it is possible to realise the meaning of their special colouring. Much depends on their habits: an animal which is much exposed by day is naturally differently coloured from one which, well concealed by day, exposes itself in the dusk. Animals which change their habits as they develop often change their colouring to suit their environment. The young of the tapir, pig, and kijang illustrate this, lying hid during the day beneath the foliage, where their speckled coats match with the sun-flecked ground. Their parents are weak beasts which are unable to protect them from attacks of enemies, and their only hope of escape is in remaining motionless, so as to be overlooked. When strong enough to seek safety, like their parents, in flight, they adopt the adult colouring. The adult tapir, as has been said, resembles a grey rock. The pig is of the colour of the dark mud of the forests in which it spends the day; but not being quite black, appears at a short distance of a dusky grey in the evening twilight, when it usually leaves the forests to feed in the open country. The russet brown of the kijang, flying squirrel, and the wild dog appears at first sight most conspicuous, but somehow it is not as visible as one would think. This brown-red colour, so common in wild dogs, such as the dingo, is very often to be seen in pariah dogs, *i.e.*, the offspring of domestic dogs which have taken to a jungle life, and I have noticed that these brown ones are much more difficult to see in the dusk than those of any other colour. The invisibility of the tiger when moving quietly through the long grass and fern, or when waiting in ambush for its prey, has been mentioned by others. About our only other striped animal, *Hemigale hardwickii*, little is known. It may be diurnal in habit. The only person whom I have met with who has seen one wild is Mr. H. L. Kelsal, who saw one under a log in thick jungle by the Tahan river in Pahang. It may, perhaps, conceal itself usually among the grasses by the river edge, where it would be very inconspicuous. Of really black animals we have the bear, black panther, *Hylobates*, and some *Semnopitheci* and *Sciurus bicolor*. The first of these not being carnivorous, and, at the same time, being a match for almost any enemy, has no necessity to conceal itself. The black panther is quite nocturnal, and being a powerful beast has no need for special colouring to conceal it during the day. *Hylobates*, *Semnopithecus*, and *Sciurus bicolor*, all strictly diurnal in habits, seem free from any enemies, living high up in the trees, where carnivorous animals never go. The monkeys do not attempt to conceal themselves, but, on the sight of an enemy, dash off with immense leaps, often uttering warning cries. The squirrel, which is slower in habits, usually hides itself among the thick creepers in the nearest tree which it can find when alarmed. It is interesting to note that in both *Hylobates* and the squirrel we have forms of the equally or more conspicuous colour—white, a colour very rare among mammals, and there are also white species of *Semnopithecus*. The smaller monkeys, on the other hand, which often come down on the ground and are liable to attacks from wild cats and other carnivora, are grey or brownish and far less conspicuous, though the young of the Kra are born with black hair, which is replaced by grey as soon as they can go alone.

The colouring of mammals is, then, in this region adapted for concealment. There is no instance of modification for signalling to each other, either by warning colours or by attracting colours, like the white tail of the rabbit. Indeed, such would be absolutely useless in a jungle country, where animals at a few yards' distance from each other could not see each other. Warning and attraction are both effected by the voice, and the latter probably also to a large extent by scent. Still, even in jungles, where, from the fresh tracks, one can see that big animals are abundant, one may remain for days without hearing more than the morning wail of the Wa Wa, the loud cry of the Lotong, and occasionally the distant bark of a tiger.

H. N. Ridley.

II.

The Development of Spiders' Lungs.

I REGRET that, when compiling my "Further Notes upon the Organs of Arachnids" (NATURAL SCIENCE, vol. v., pp. 361–5), I overlooked an important recent memoir by M. Jaworowski, describing the development of the breathing-organs in the large hunting-spider, *Trochosa signoriensis* (*Zeitschr. für Wiss. Zool.*, vol. lviii., 1894, pp. 54–78, plate iii.); especially as the results obtained by this author seem to contradict those of Mr. O. L. Simmons, whose work, as I mentioned, supplied evidence for that view of the relationships of arachnid lungs to tracheal tubes which I myself am disposed to accept. It will be remembered that, according to Mr. Simmons' researches, the lung-plates of spiders arise on the hinder surface of abdominal limbs (comparable to the gill-bearing appendages of *Limulus*) which sink into the abdomen as development proceeds. The tracheal tubes are said to show at an early stage traces of pulmonary plates which afterwards vanish. This, by itself, seems conclusive evidence that external gills preceded lung-books, and that lung-books are more primitive than tracheæ. It is only right that the attention of the readers of NATURAL SCIENCE should be called to M. Jaworowski's paper, which seems to favour the opposite view.

In the early stages of the development of the lung-books of *Trochosa*, according to this observer, a tube runs dorsal-wards from the in-pushing behind the abdominal appendage. This tube ramifies into several branches, forming an embryonic tracheal system. As the lung-book is developed, by the folding of the wall of the invagination, the tubes degenerate, the process beginning with the smaller branches; and, as growth proceeds, they disappear altogether. The existence of similar vestigial tracheæ has already been pointed out by Professor Schimkewitsch in the embryo of *Lycosa* (*Arch. de Biol.*, vol. vi., 1887).

From these considerations, M. Jaworowski concludes that among the arachnids tracheal tubes must certainly have preceded lung-books. He admits, however, the arachnid affinities of *Limulus*, and the homology of the external gill-bearing limbs on the abdomen of that animal with the lung-books, sunk within the abdomen, of a scorpion or a spider. His conclusion, therefore, is in accordance with the view of Dr. Simroth, as expressed in his book "Die Entstehung der Landtiere" (Leipzig, 1891), namely, that *Limulus* has been derived from tracheate ancestors, living on the land. He further brings forward reasons for supposing a similar origin for the Crustaceans.

I need not again dwell at length upon the morphological and palæontological facts which seem to forbid us to accept this theory. The comparative study of living forms shows that the scorpions and scorpion-spiders (Pedipalpi), which breathe by lung-books only, are more primitive than the spiders; and that those spiders which breathe by lung-books only are lower in the scale than those in which the hinder pair of lung-books are replaced by tracheal tubes. These conclusions are confirmed by the study of fossil forms. In phylogenetic speculations, due weight must be accorded to evidence from all quarters; and, while M. Jaworowski's researches deserve careful consideration, the embryological fact which they establish need not lead to the abandonment of the position required by the comparative study of living and fossil arachnids. The admission of the correspondence between the gill of *Limulus* and the lung of the spider leaves us free to accept Mr. Simmons' interpretation of his observations, if some reasonable explanation can be found for the evanescent tracheal tubes which M. Jaworowski describes. It seems possible that, in the passage from an aquatic to a terrestrial life, before the gill-bearing limbs became converted into lung-books, air tubes in connection with these limbs would have been of great service to the primitive arachnids. Such a stage is somewhat similar to what we find at present in the land-isopods. As the limbs sank into the abdomen, and the plates became adapted for breathing air, the necessity for the tubes would pass away, and they would, in course of time, become lost. Their appearance in the embryonic stage of certain spiders is what might be expected upon such a hypothesis.

Geo. H. Carpenter.

III.

The Origin of Species among Flat-Fishes.

THE purpose of this paper is to consider how far, by the help of the various principles which have been suggested or advocated, we can obtain a satisfactory explanation of the evolution of the family of flat-fishes. We may assume that descent with modification is the process of organic evolution. But the question is, how far we can really explain the modification, and especially how we can explain the peculiarities of its results, the relations of structure which form the subject-matter of taxonomy, the relations between the characters which divide organisms into species, genera, families, etc. Thus, by the evolution of flat-fishes I mean, not what is usually meant—how they came to lie on the bottom on one side and have both eyes on the upper side; but how and why they diverged into a multitude of distinct species arranged in a number of genera, which form higher groups called sub-families. This method of studying the problem of evolution, namely, by examining all the problems presented by the morphological relationships of a particular kinship, from the family down to the individuals, or from the individuals to the variety, from varieties to species, species to genera, etc., has very seldom, if ever, been followed.

Zoologists may be divided into three classes:—(1) The investigators of evolution who, with Darwin as their pattern, follow the inductive method, and having arrived at some general principle give a general survey of the biological facts which support it. (2) The embryologists and comparative anatomists, who investigate and describe morphological facts and structural phenomena, and trace homologies: their work consists almost entirely in investigating the results of descent with modification, and its historical course; they have little concern with the origin of modifications, though they usually assume one theory or another. (3) The systematists and naturalists, who investigate the minutiæ of structure and habit which characterise the different kinds of organisms, from main divisions down to the smallest distinctions by which a variety or race can be recognised: these as a rule do not trouble themselves about evolution at all; their method is simply empirical. We may roughly characterise the three classes as the followers of Darwin, the followers of Cuvier, and the followers of Linnæus. The theorists of the first class are too apt to take illustrative facts from various provinces of the animal kingdom without reflecting that these facts may be all of one class, and that there may be facts of quite a different class which they ignore. The question whether the principles of the theorists are in harmony with the details of the empiricists has yet to be thoroughly examined.

The most recent important contribution to the study of evolution in this country is Bateson's "Materials for the Study of Variation, treated with especial regard to Discontinuity in the Origin of Species." In this work, as in its predecessors, the method is to search the animal kingdom for instances of one class of facts, instead of taking a portion of the animal kingdom and ascertaining how many classes of facts it presents. Valuable as the work is, and important as are the aspects of variation to which the author draws attention, one cannot help being astonished at the fact that Mr. Bateson writes as though, apart from adaptation to the environment, no explanation of the discontinuity of specific forms had been offered before he himself took up the problem. He does not mention that, so long ago as 1886, Romanes maintained that Natural Selection was a theory of the origin of adaptations, by no means a theory of the origin of species; or that two years later Gulick published most important evidence of the origin of varieties by mere divergent variation through isolation, without any adaptation at all. In fact the modes in which variations, instead of being individual peculiarities within a species or other taxonomic group, are to become constant and characteristic of a distinct group, are never discussed by Bateson. Except in one or two rare instances, he entirely omits to consider the relation between the variations, whose description makes up the body of his book, and the taxonomic characters of the forms in which they occur. There is some logic in his contention that, since environments are often continuous, their influence cannot always explain the discontinuity of species. But it is clear that, if progressive modification in different directions goes on in two groups of a single species which are isolated so that no interbreeding takes place, then two species will be formed, the discontinuity between which will become greater at every generation. This result will follow, however gradual and continuous may be the modification in each group. Mr. Bateson's argument is defective, therefore, in two respects: first, there is no necessity for discontinuity of variation to explain discontinuity of species; secondly, he has not attempted to show where discontinuity of modification probably did occur historically in the evolution of any particular group.

In reference to the flat-fishes (Pleuronectidæ), Bateson (p. 466) discusses only one class of variations, namely, the partial or complete coloration of the under side, and the abnormality of the head associated with the complete condition. He argues that this kind of variation cannot be explained simply as reversion to an ancestral condition. In this I entirely agree with him. But he does not consider the question how the occurrence of this variation bears upon the specific characters, or on the origin of the actual differences between existing forms of Pleuronectidæ. Bateson mentions cases of the abnormality in question in six species belonging to three genera. In no case is the variation diagnostic of a distinct race, variety, or species. Bateson argues that it is discontinuous, that is to say, normal parents may produce an individual presenting the abnormality fully developed without intermediate stages. There is every reason to believe that this is true. But supposing this condition were to become constant and peculiar in separate races, what would be the result? The difference between these individuals and normal individuals is a difference not in specific or generic, but in family characters. Supposing the condition became constant and were regularly inherited, we could not even say that we had a new family or sub-family, because the quasi-symmetrical turbot would still be a turbot, the quasi-symmetrical flounder still a flounder, and so on. We should simply have dimorphic species, or species which presented two distinct but parallel forms. If, however, the condition in question became the normal condition, and the condition which is now normal became rare or extinct, then the characters of the family would be changed, and changed by a sudden leap from the one condition to the other. Bateson might suggest that the normal characters of the Pleuronectidæ arose by a sudden change from the symmetrical condition, just as the abnormal individuals at the present time arise without gradation from normal parents. But this would be an untenable supposition. In the individual Pleuronectid at the present day the family characters arise by a gradual change in development; and we should have to believe that this change took place in certain individuals completely the first time it occurred. But the occasional variation we have been considering is probably due, as suggested by Bateson and myself, to the occasional manifestation in the organism of a tendency to symmetry, in virtue of which the characters of one side are reproduced in the other. This phenomenon, which Bateson calls homœosis, is exhibited in asymmetrical animals, where the two sides are normally different. But we have no evidence of any converse tendency, of a general tendency to asymmetry, by virtue of which one side may be entirely different from the other. We have therefore no facts which suggest the probability that the first flat-fish arose directly, without any intermediate steps, from parents whose two sides were quite similar. Here, for the present, we may leave the discussion of these abnormalities, merely recurring to the main point insisted upon, that they affect, not specific or generic characters, but family characters, and, moreover, family characters which are adaptive.

Let us turn to the consideration of specific diversity in this family. For the details to be discussed I am largely indebted to a very able paper by David Starr Jordan and David Kopp Goss, entitled "A Review of the Flounders and Soles (Pleuronectidæ) of

America and Europe," published in the Report of the United States Commission of Fish and Fisheries for 1886, although, so far as relates to British species, I can speak from my own observations.

Zeugopterus is a fairly well-marked genus, with a very limited distribution. Its definition is to some extent a matter of opinion, different ichthyologists classifying the species differently. It is, however, agreed that there are three species that all possess the following characters: a large foramen in the septum between the gill-cavities; the posterior extremities of the dorsal and ventral fins prolonged on to the lower right side of the body beneath the base of the tail; the scales on the upper surface ctenoid, having a single row of spines, of which the central one is prolonged; the shape of the body approaching the rectangular. The three species are definitely distinguished from one another by the following single characters: in *Z. unimaculatus*, Risso, the first ray of the dorsal fin is produced into a filament; in *Z. punctatus*, Bloch, the pelvic fins are concresced with the ventral (or anal); in *Z. norvegicus*, Günther, neither of these characters is present.

As to distribution, no species having the foregoing characters has been found anywhere except on the coasts of Europe, and all three species occur on the British coasts. *Z. punctatus* is fairly common near Plymouth, and occurs all along the south and east coasts of England. It has been taken on the east coast of Scotland as far north as the Orkneys, on the west coast in the Firth of Clyde, also on the east coast of Ireland. Northward the species extends to the north coast of Norway, southward to the northern shores of France, but it is absent from the Mediterranean. *Z. norvegicus* is also a northern form absent from the Mediterranean. I have taken several specimens at Plymouth; one specimen has been taken on the west coast of Ireland, and three specimens in the Clyde. It is somewhat rare on the Scandinavian coasts. *Z. unimaculatus*, on the other hand, is a Mediterranean form, only occasionally taken on British and northern coasts. I have never obtained a specimen at Plymouth. On the south-west coast of Scotland it is more abundant than at any other part of the British coast, several specimens having been taken in Loch Fyne and the Firth of Clyde. It has been taken on the coast of Denmark, but not on the coast of Norway.

Steenstrup is the only ichthyologist who has placed these species in the same genus, *Zeugopterus*, and he included with them the whiff or megrim, *Z. megastoma*, because it too has the perforation in the septum of the gill-cavity. The three species have been separated by Günther and others according to the presence or absence of teeth on the vomer, and the union of the pelvic with the ventral fin, or its separation therefrom. But the three species are so similar in their general character, and are united by such salient features, that it is clear they have recently diverged from a common parentage.

We have certain direct observations on the habits of these fishes which enable us to some extent to form a judgment on the utility of the generic and specific characters respectively. *Z. punctatus* and *Z. unimaculatus* have been watched in the living healthy condition by the late Mr. George Brook and by myself, and both of them found to have the habit of adhering to the sides or glass front of the tank in which the fish were confined. *Z. norvegicus* has not been observed in the living condition. Mr. Brook in his first account of the habit in *Z. unimaculatus* stated that when the fish adhered to the glass front of a tank the body was raised up from the surface, and the outer parts of the fins tightly pressed against the glass. A constant current of water passed out from the branchial chamber on the lower side, between the body and the glass, and out beneath the hinder portion of the longitudinal fins. This current was produced by a "vibratory motion of the accessory portions of these fins." Brook's conclusion was that the accessory portions of the unpaired fins were specially constructed to aid in the respiratory function. In a later paper Brook abandoned this view, stating that the motion of the fins by which the current was produced was more vigorous in the rays immediately in front of the tail than in the accessory flaps situated beneath it. In a specimen of *Z. punctatus* observed by me, the anterior parts of the fins were kept quite still and tightly pressed against the glass. The lower surface of the tail and of the accessory flaps of the fins were also in close contact with the glass. The other parts of the surface of the body were separate from the glass. The motion of the broadest part of the fins anterior to the tail was undulating, and aided to cause the current of water which entered the space between the body and the glass, not only by passing through the mouth and gill-clefts, but also behind the operculum between the lower jaw and the pelvic fins. The question arises, what causes the fish to adhere to the glass? Other flat-fishes adhere to smooth surfaces for a time, but none keep in that position at rest for a long time as the topknots do. Is it simply the adhesiveness of the mucus on the epidermis? Or is it that the pumping action of the posterior fins causes a negative pressure beneath the body, and so there is a slight pressure on the outside of the fish? There is no sucker action of the ordinary kind, because the space beneath the body is freely open in front to the outside water. It was suggested by one of us who were watching the fish that the tail fin and the accessory flaps together formed a small complete sucker by themselves. The accessory parts of the unpaired fins were quite motionless. I cut off these accessory portions with two snips of a pair of scissors and returned the fish to the tank. The fish did not alter its behaviour in the least; it stuck to the glass when placed there as easily as before, and seemed in nowise the worse for the operation. The accessory flaps, therefore, do not appear to be essential either to the respiration of the fish or to its adhesion to a vertical surface.

It seemed difficult to believe that the pumping action of the posterior part of the fins could cause a negative pressure beneath the body, because the respiratory movement was going on while the fish adhered to the vertical surface. This respiratory movement forces a current of water into the space below the body through the lower gill cavity, and this would necessarily produce a positive pressure. However, the currents of water were ascertained by watching the course taken by particles of suspended matter in the water, and afterwards by dropping carmine with a pipette at points in the neighbourhood of the head. The carmine was seen to pass in at the mouth and out at both gill-openings, and no difference was observed in the respiratory currents from those of an ordinary fish. The perforation of the gill-septum was therefore not found to play any special part. But the carmine was also drawn into the space below the body through considerable passages above and below the jaws. This proves that more water is pumped by the action of the posterior parts of the fins than passes through the mouth and gill-openings. This, therefore, must cause a pressure on the outside of the fish against the vertical surface on which it rests. In order to test whether a pressure caused in this way would be sufficient to produce the adhesion, I endeavoured to construct a simple apparatus which would reproduce the conditions observed in the fish. I took a rectangular piece of sheet india-rubber, 6 inches long by 4 inches broad, and at each end of its longer axis I fastened, with needle and cotton, a short piece of glass tube. The tubes did not meet in the middle of the piece of india-rubber. A siphon was placed on the edge of the glass front of a tank, and its end in the tank connected by a long india-rubber tube to one of the tubes on the india-rubber flap. When the flap was placed against the glass with the glass tubes towards the latter, a current of water was drawn from beneath the flap by means of the siphon. The longer edges of the flap became pressed against the glass, while the entering current passed through and at the sides of the glass tube which was not attached to the siphon. In this apparatus the drawing of the siphon represented the pumping action of the fins in the fish, while the piece of glass tube in front represented the mouth and gill-opening of the fish, the water being able to pass at the sides of the tube as it passes at the edges of the jaws and head in the fish. The only differences are that the respiratory movement in the fish is absent in the apparatus, and that the tube representing the mouth communicates only with the space beneath the rubber flap, not with the outer side of the flap also, as in the fish. These differences are unimportant, because it is certain that the respiratory movement in the fish does not cause the adhesion, and there is in the fish a current caused by the pumping action of the fins over and above the ordinary respiratory current. It was found that the flap just described adhered to the vertical surface of the glass front of the tank so long as the siphon was running, and fell off as soon as the flow through the siphon was stopped.

An important specific difference between *Z. punctatus* and *Z. unimaculatus* is the concrescence of the pelvic and ventral fins in the former and not in the latter. Yet the observations on the two species show that this feature has nothing to do with the peculiar habit described. No difference of habit has yet been detected corresponding to this difference of structure. Nor have we yet any evidence of the utility of the prolongation of the first dorsal ray in *Z. unimaculatus*.

But these are not the only specific differences. There are others which consist in differences of the degree to which the generic characters are developed. In other words, the specific characters of the three species form a series. Even the vomerine teeth and the union of the pelvic and ventral fins are to be regarded as terms of such a series, for the teeth on the vomer are, in *Z. norvegicus*, very small, in *Z. unimaculatus* absent, in *Z. punctatus* well developed, while in the former two species the pelvic fins extend backwards so as to embrace the anus and commencement of the anal fin, although they are not united with it. The other characters, for example the accessory finlets and the scales, are developed in different degrees in the three species. The finlets are largest and the rays most branched in *Z. punctatus*, in which each contains six rays, while in *Z. unimaculatus* and *Z. norvegicus* there are not more than four. The generic character of the scales is that the exposed portion is short antero-posteriorly; there is a single row of spines of which the centre one is prolonged and extends directly outward from the surface. In *Z. punctatus* this character is developed to the maximum, the whole exposed portion of the scales being bent nearly at right angles to the embedded portion; the projecting spines are not of one uniform length, but those of maximum length occur at scattered points. It is this character of the scales which gives the peculiar hirsute character to the skin of *Z. punctatus*. It is developed also in *Z. unimaculatus*, but in *Z. norvegicus* occurs to a much less degree. In the last species the exposed portions of the scales are broader and lie flat to the body, but here and there occurs one with a projecting central spine; these are more abundant on the head.

If the generic characters were adapted to peculiar generic habits, the specific differences in these characters might be similarly adapted to differences in the habits. I have given evidence that the accessory flaps of the fins beneath the tail are not necessary to the habit of adhesion, which is produced by the movement of the posterior part of the fins, and this is related to the characteristic shape of the body, which depends on the breadth of the body and fins at the posterior end. This, therefore, might have been selected, but on the other hand the enlargement of the posterior part of the body might have been produced by the movement. Young brill have been seen to move the posterior parts of the fins when adhering to glass. The peculiar character of the scales has, so far as the evidence goes, no adaptive meaning at all, either in its generic extent or in the specific differences. Nor, as already shown, is there evidence that any advantage is conferred on the fish by the perforation of the gill-septum.

The utmost that can be admitted then as adaptation in these fish is the shape of the body and breadth of the fins behind. The linear extent of the fins is as great and greater in many other genera. The accessory flaps beneath the tail, and the extent of the pelvic fins, cannot be regarded on present evidence as of adaptive meaning; still less can the specific differences.

The relations of these three species, on the view that their differences are not adaptive, illustrate certain principles which have been elaborated by Romanes and Eimer. They exemplify the class of facts which Romanes intended to explain by his theory of physiological selection, which, reduced to its simplest terms, states that species whose geographical ranges overlap or to a large extent coincide, could not become distinct if they were constantly interbreeding. Thus *Z. punctatus* and *Z. norvegicus* occur together in Norway and Britain, but do not interbreed, and *Z. unimaculatus*, though alone in the Mediterranean, lives with the others from Denmark southward. Whether these species are sterile *inter se*, whether fertilisation of one by the other is physiologically impossible, whether their reproductive seasons are such that opportunities for intercrossing do not occur, or whether the intercrossing is prevented by the mating instinct of the individuals, is not at present known, and does not very much matter. The main point is that so long as intercrossing took place, variations that occurred in one group of individuals would sooner or later become the common property of all the members of the parent form; and that when the three groups were kept apart, a variation that arose in one would be confined to that one. We may take it as settled by observation that when groups of a species are isolated they will diverge by variation. It may be asked, why should two groups of individuals, having originally the same habits and spread over the same area, enter upon divergent lines of modification, simply because they are separated? We do not know, but we can safely say that they will do so. We have first to ascertain what does take place before we can find out why it takes place; and when we find that the differences between species are not differences of adaptation to different modes of life, we have simply to study these differences and their history as structural features. It is clear that in artificial breeding, separation of varieties is one of the most important conditions, and there is abundant evidence of the same separation in nature. The term selection often seems to imply this separation, but inasmuch as selection always means principally the preservation of certain individuals in a given variety or race, by human choice in domestication, by the struggle for existence in nature, and therefore always implies a teleological view of structure, therefore it is better to use a distinct term, and the best seems to be "isolation." We may reasonably hold that the explanation of the similarity of individuals in a species is the constant intercrossing of individuals. In other words, Weismann's condition of panmixia occurs within every homogeneous group, and tends to produce not degeneration, but uniformity. There is no reason why this condition should prevent modification. A species may produce great variations, and these may be gradually extended to all the individuals, so that the later characters of the species may be very different from the original, but unless there is segregation or isolation of one group of individuals from another there will be no divergence, no splitting up of the one species into two or more.

Although the mode in which Eimer has expressed his views on variation is sometimes obscure and involved, there can be no doubt that he has perceived important phenomena which have been more or less neglected by other naturalists. The relations of several of the characters of *Zeugopterus*, the fact, that is, that they differ only in degree of development, form examples of what Eimer calls "genepistasis." But this is not to be considered as implying that, as Eimer believes, the less developed characters represent stages actually passed through in the development of the individuals of the species where these characters are more developed. The development of specific characters is a subject I am investigating, and I have not actually traced the development of those of *Zeugopterus*; but I hold this part of Eimer's view to be exceedingly improbable. It is evident, where the character depends on the number of parts, that, as Bateson points out, the greater number is not usually formed by addition to a smaller number, but by the greater subdivision of the developing blastema when the parts are about to be formed. It seems to me that the facts represented by the term "genepistasis" are simply due to the common descent of closely allied species. The generic characters are either derived from the common ancestral species, or represent the similarity of variation due to the blood-relationship of the species. The specific differences of degree are merely instances of the general fact that the species have varied independently, some developing characters of the ancestral species more strongly, some less, while at the same time variations occur in one species which are not represented at all in the others.

Plymouth. J. T. CUNNINGHAM.

(*To be continued.*)

IV.

Wasps and Weather.

THE last two years have afforded in close proximity diametrically opposite conditions with regard to both weather and abundance of wasps. It may, therefore, be of value to put on record a brief summary of the climatic susceptibility of these insects.

In Natural Science, vol. iii., pp. 273–275, I have already given an account of the phenomena of the "Plague of Wasps" in 1893. It will there be seen that there was an absence of frost after March 23rd, while the total rainfall from March 1st to June 30th, 1893, only amounted to 2·38 inches, with never more than 0·34 inch on any one of the 27 "wet" days of the four months combined.

In 1894 we again had fine warm weather in the early spring, and the female wasps appeared on the wing in great abundance, these individuals being in reality a legacy from last year's plague, and giving further evidence, if such were needed, of the strength and vigour of the nests last season—an abundant supply of food furnished by the thousands of workers having resulted in the feeding up of an unusually large number of perfect females. The daily press was full of warnings to catch these "queens," and nip in the bud any recurrence of the plague! Referring to my own notes I find that I saw the first wasp (sp.?) of the season on the wing as early as February 7th, at Outwell, Cambs. A few were noted during March and more in April, chiefly *Vespa vulgaris* and *V. germanica*, with one or two *V. rufa*. Towards the end of April they became abundant, so much so that a neighbouring hawthorn hedge and oak fence were a source of terror to the nervous. Between May 4th and 7th I captured rather over 50 females: *V. germanica* 40, *V. vulgaris* 8, *V. norvegica* and *V. sylvestris* 2 or 3 each. Towards the end of May the numbers were diminishing. I first saw workers (*V. germanica*) on the wing on June 7th. But the remainder of the year was singularly free from wasps, and as a consequence, in my opinion, aphides and earwigs were present in most destructive abundance.

The weather during the four months above named enables a judgment to be formed on the conditions fatal, or the reverse, to wasp life. Between March 1st and June 30th, 1894, there fell at Godalming 7·76 inches of rain on 56 days—roughly three times as much rain as in the corresponding period of 1893, on only twice as many days—pointing to heavy downpours. As a matter of fact, on six occasions during this time the fall for one day exceeded 0·35 inch. Now it is most remarkable that the heavy fall of rain exactly coincides with the period at which my notes cease to remark abundance of females. It has been observed already that wasps were plentiful up to May 7th. I note in my weather-chart that on May 11th there fell 0·42 inch of rain, while between May 26th and June 6th (both inclusive) my rain-gauge registered 2·23 inches. It was precisely from this time forward that wasps became conspicuous by their absence. During July they were in no better state, and many nests which had survived till then must have perished by drowning. The total rainfall for that month was 4·86 inches on 21 days; on one occasion 1·59 inch fell, on another 1·13 inch, on a third 0·43 inch, while on July 10th 0·48 inch fell in the short space of ten minutes.

With regard to temperature, the conditions were extremely favourable to wasps. My screened thermometer registered no frost after March 19th. This will be surprising to those who remember the disastrous (horticulturally speaking) frosts of May 21st and 22nd. These, however, were strictly "radiation" frosts. The exposed thermometer on grass fell to 25° F., while that in the screen only reached 32·8° F. Such a frost most assuredly could not affect wasps injuriously, for the very slightest covering was a protection against it, *e.g.*, exposed leaves of potato plants and scarlet runners were blackened and killed, but leaves vertically below these were uninjured.

From these observations it may, I think, be fairly concluded that in spite of favourable conditions of temperature, heavy rains are extremely fatal to wasp communities. It would be interesting to note the effect of the converse conditions; viz., a dry spring and summer accompanied by repeated late frosts of some severity, but for this opportunity we must be content to wait.

The present frost has furnished the opportunity of testing the powers of endurance of cold by hibernating wasps. The results show that, at any rate during the period of hibernation, severe cold has little effect upon their vitality.

January 12.—♀. *V. germanica* placed in corked test tube and embedded in a heap of snow, temperature 31·0° F., for four hours. Completely recovered on being brought into a warm room.

January 13.—Same wasp placed out of doors, temperature 34° F., for one hour, then in heap of snow for one hour, and then for three hours in a vessel containing mixture of snow and salt and surrounded by snow; temperature fell to —6° F. Wasp died.

February 5.—♀. *V. germanica* found in corner of thermometer screen, apparently dead. Completely recovered during examination by a class of pupils on February 8, having been in the screen during the intervening days. Minimum temperatures, February 6, 13·8° F.; February 7, 10·4° F.; February 8, 9·9° F. Maximum temperatures, February 6, 22·7° F.; February 7, 22·3° F.; February 8, 24·8° F.

Oswald H. Latter.

Charterhouse, Godalming.

V.

The Structure and Habits of Archæopteryx.

III.—The Feathers.

THE evidence obtainable does not justify the expression of any opinion as to whether the body of *Archæopteryx* was covered with feathers all over or only partially. Three chief kinds of feathers are, however, recognisable in the fossils:—(1) quills, (2) coverts, and (3) contour feathers.

The **quills** are exceedingly well-preserved, especially in the Berlin specimen. The **remiges**, or wing-quills, had the characters of those of many ordinary birds, such as a pigeon. The calamus is not clearly seen, as it is hidden by the coverts; but the narrowing of the vane near the base shows (*e.g.*, in the second and third primary quills of the left wing of the Berlin specimen) that its length was much the same in proportion to the rest of the feather as in the corresponding feathers of a pigeon. The rachis is clearly seen, and is slightly curved so as to render the ventral surface of the wing concave and the dorsal surface convex. It tapers gradually as in feathers of the usual type. The groove seen along the dorsal surface of the rachis is probably due to shrinkage of the medullary substance during fossilisation—but this point is open to dispute. The vane, as in nearly all birds, is curved, the anterior and narrower moiety much more strongly than the posterior, which latter is overlapped dorsally by the anterior portion of the feather next following it. The barbs are easily seen, even in photographs; but I have been unable to make out the barbules with certainty. Their existence, and even that of the hooks which serve to maintain the relative positions of the barbs, is safely to be inferred from the extreme regularity with which the barbs lie side by side in the Berlin specimen.

Of the quills, there are in each wing seven primary and ten secondary. The lengths of the primary quills, *i.e.*, of the quills borne by the metacarpals and phalanges, are as follows (commencing with the first):—65, 90, 120, 125, 135, 130, and 120 mm. The secondary quills, borne by the ulna, are not easy to measure accurately, but they diminish gradually from the carpal region to the elbow. The first is 115 mm. long; the last or tenth, 75 mm. Taken as a whole, these remiges, though less numerous than in most modern birds, are as perfectly fitted, by their form and arrangement, for the purpose of flight as in, say, a pigeon. Their size, though not difficult to determine absolutely, is difficult, if not impossible to determine relatively to the weight of the body; for in our guesses at the weight of the animal a very large margin must be left for possible error.

The **rectrices**, or tail-quills, differ very remarkably from those of any other known bird. Unfortunately, I overlooked the question as to their number when I was in Berlin. Previous authorities regard them as arranged in pairs, one pair to each vertebra of the tail, but Dames is very cautious on this point. My large photographs suggest that they are somewhat more numerous, but the point is one which may be left for the present undetermined. They differ from the corresponding feathers of modern birds of flight chiefly in size, being very much smaller than those of ordinary birds of equal size. Those of the anterior part of the tail are about 50 mm. long, or perhaps a little less (Berlin specimen). Further back they are longer, the maximum length of about 95 mm. being at about the twelfth caudal vertebra. To what extent, if any, they could be spread out and closed together we can only guess, and the fact that they lie in both specimens at an angle of about 30° to the axis of the tail does not help us much; for, if the animal had the power of spreading its tail-feathers, this would perhaps have been effected by means of a muscle arising from some bone in the pelvic region, through the mediation of a slender tendinous band running along each side of the vertebral column of the tail, and opposed by a slender elastic ligament which would, when the muscle was relaxed, bring the feathers into some such position as that which they occupy in the fossils known to us. These tendons and ligaments may well have been very small, and the absence of any trace of them in the fossils is not sufficient justification for our stating that they did not exist. We must, therefore, remain in ignorance as to whether *Archæopteryx* could or could not spread and close its tail.

Though the feathers are small, their great number gives to the tail, looked upon as an aëroplane, a very considerable surface, and this surface is greatly increased by the development of a series of feathers, which may perhaps be classed as rectrices, along the sides of the hinder part of the trunk. So far as I can make out by means of drawings that I have made to scale of the bird in the flying position, these lateral rows of feathers constitute with the tail-feathers a continuous aëroplane, extending forwards as far as the posterior edge of the extended wings. Why those who have made drawings of the animal "restored" should all, so far as I know, ignore the existence of this lateral aëroplane, and represent the lateral feathers of the tail-series as coming to an end in the tail, is a subject which those who are interested in the origin and multiplication of errors may appropriately consider; but this is not the place for the consideration of it. It is sufficient here to point out that the extant "restorations" are as utterly wrong and misleading in this respect as they are in others.

Archæopteryx, unlike any other known bird, bore quills on its tibiæ. These are not either remiges or rectrices—and, indeed, the lateral aëroplane in front of the tail is not strictly made up of rectrices. In the absence of a better name, I will call them **tibial quills.** These *appear* to have lain in a single plane, which is the plane of flexion of the leg, the plane in which femur, tibia, and metatarsals all lie when the limb is bent; and they were apparently arranged in two series along the surfaces corresponding to the anterior and posterior surfaces of the human tibia, *i.e.*, the extensor and flexor surfaces. The number of them cannot be made out with certainty. The longest appear to have measured a little over 30 mm. in length. How they were placed in reference to the muscles I cannot say, and though they appear to have lain in a single plane, they may perhaps—though I do not believe it—have been "breeches," as they have been described. They extended along the whole length of the tibia, and certain appearances in the region between the left leg and the tail in the Berlin specimen suggest that the flexor-series extended also to the region of the femur.

Coverts are recognisable with certainty over the primary and secondary quills of the wings, especially the left wing, but they are not nearly so well preserved as the more robust quills. They appear to have been very slender, and now lie at an angle of about 40° to the quills. This deflection may have been due to the action of a stream of water passing over the dead bird when it first came to rest where it was finally fossilised; and, if so, the animal must have come to rest with its head up-stream, as one would naturally suppose.

The appearances in the fossil do not justify any statement as to the coverts in the tail or over the tibial quills.

Contour feathers may be recognised with certainty only in the cervical region. Three are well preserved between the right hand and the label bearing the number 11 in Plate I. (facing p. 122). I believe I have recognised others ventral to the fourth and fifth cervical vertebræ; but this is far from certain. As to the covering of the rest of the body, we are in the dark.

IV.—Habits.

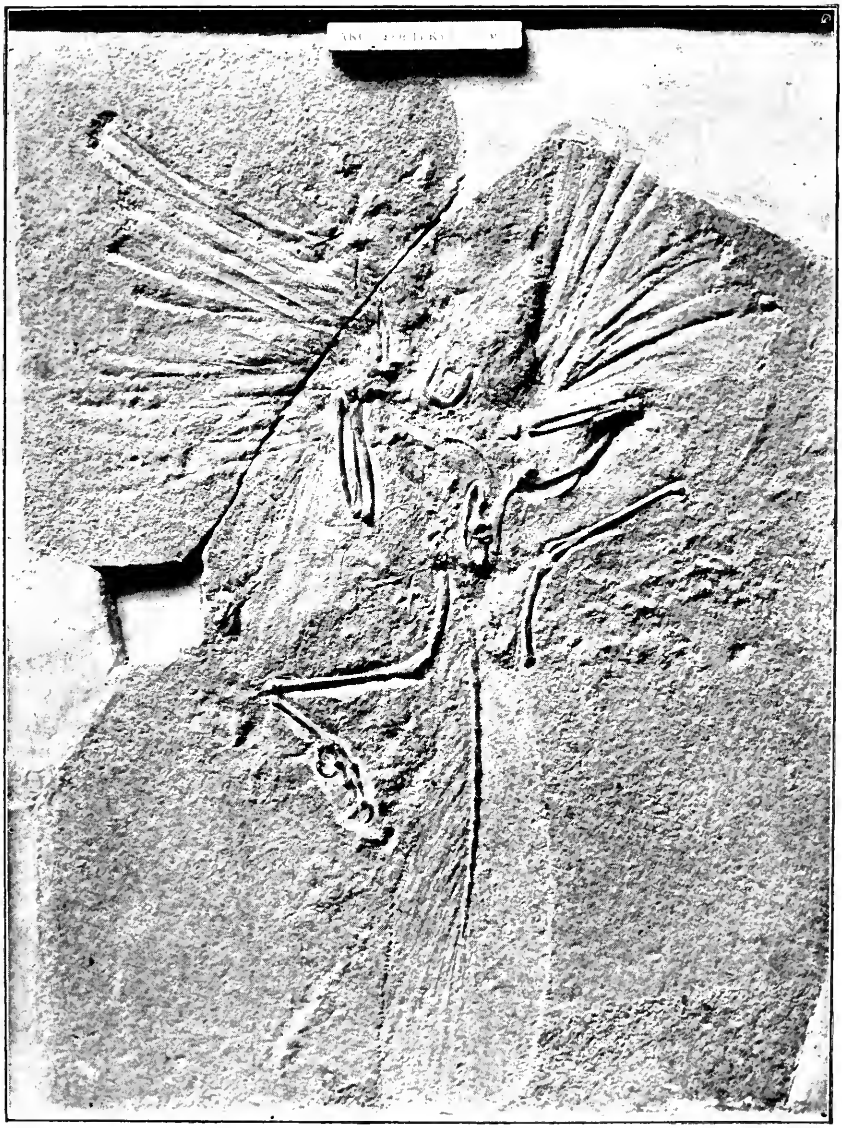
Archæopteryx was an arboreal quadruped fitted for flight, if not for prolonged flight.

First, as to quadrupedalism and attitude.—I have already (p. 120) pointed out that the digits I, II, III of the hand are long and slender and flexible; and that each metacarpal and phalanx of these digits is curved, the concavity being ventral; and that the tubercles for the insertion of the flexor and extensor muscles of some of these phalanges are distinctly recognisable. To this I would now add that their joints are at different levels, showing that each finger could be flexed independently of the rest, and therefore that they were *free* and not bound together. I also showed that they did not support the quills, but were free from the wing except at their carpal ends. If they were bound together in the wing they would be inflexible, inasmuch as the joints of each digit are at different levels from those of the other digits.

I have already referred to the long, lizard-like body of the animal, and to the absence of that shifting backwards of the heavy abdominal viscera which is seen in such bipeds as birds, squirrels, kangaroos, dinosaurs, etc.; but I do not wish now to rely upon mere analogy. The form of the body is so clearly shown that it is obvious that its centre of gravity would be in front, not only of the acetabulum, but of the knee. If the animal walked, or even stood, on two feet at all, it would have had to stand either bolt upright or with its dorsal surface directed slightly downwards. Whether the tail would then be on the ground (as in a kangaroo) or in the air (as in a squirrel) is open to doubt, for we do not know enough about the flexibility of its proximal portion to be able to say with certainty whether it could or could not be bent up over the animal's back. The long, heavy neck and the great weight of the head, which was supported by an elastic ligament, as shown by the curvature of the neck in the fossil (as in *Compsognathus* and the pterodactyles), add greatly to the force of this argument. A duck has to walk with its body tipped up almost on end, in spite of the great shifting of the heavy organs of the abdomen backwards between the legs. There is no room for doubt that in *Archæopteryx* the centre of gravity would be much further forward, not only on account of the heavy head and neck, but also on account of the solidity of the wing-bones, as shown by the absence of pneumatic foramina. Let any who doubt the justice of this argument compare the pelvis as seen in Plate II. (which the authorities of the British Museum have kindly allowed to be prepared in illustration of this article) with the pelvis of, say, a pigeon or a dinosaur, or any other animal whatever capable of walking on two limbs in any but an erect position. The small size of the cnemial crests, moreover, forbids us to believe that the hind-limbs alone were able to bear the weight of the body when the knee was bent.

This bird was not only not a biped, but it did not walk on the ground at all. It would have been as helpless on the ground as a bat or even a sloth. The great length of the hind-limbs and shortness of the fore-limbs, at any rate when these latter were so flexed as to keep the feathers off the ground; the position of the shoulder joint, and especially of the articular surface of the humerus: these render the animal unfit for such a habit as even quadrupedal locomotion on the ground. The perfect state of the wing-quills at their tips shows that they were not brought habitually into contact with the ground, and I know of no rational argument in favour of the view that the animal did live largely upon the ground. For quadrupedal locomotion in trees the animal is admirably adapted. Long, flexible digits, provided with claws on all four limbs, fit it at least as perfectly to arboreal quadrupedalism as *Galeopithecus* or *Petaurus* or any other "flying" mammal; and I think, from the greater length and flexibility of those digits, fit it even more perfectly for such a habit than even these mammals, and incomparably more so than the bats with their backwardly-directed hind limbs, which, while they serve to enable the animal to hold on to the tree or other body, are so modified for the support of the wing as to be of little use for quadrupedal locomotion.

As to flight.—*Archæopteryx*, though less well fitted for prolonged flight than most modern birds, was certainly capable of flight. As some have maintained that it was well fitted for powerful and prolonged flight, I will mention the features pointing to an opposite conclusion. The absence of a pectoral crest on the humerus, the small size of the sternum (*see* p. 115), and consequent relatively small size of the great pectoral muscle, indicate a deficiency of propulsive and sustaining power in flight. The absence of pneumatic foramina and consequent absence of air-cavities in the wing-bones show that rapid to and fro (*i.e.*, up and down) movement of the wing would involve a larger amount of exertion than in a wing with hollow bones as in most modern birds. It is not so much the extra weight that would be impedimental as the extra inertia. The narrowness of the body and smallness of the sternum indicate that the air-cavities of the abdomen, even if present, were smaller and less effective for respiration than in modern birds. I may be permitted here to make a remark as to the use of abdominal air-sacs in birds, or I may be suspected of having fallen into an old error of supposing that they materially diminish the weight of the bird. Flight involves, perhaps, a greater, *i.e.*, more rapid, consumption of energy than any other form of locomotion, and all powerful fliers, whether birds or insects, are provided with respiratory organs of enormous effectiveness. In birds, instead of air being only pumped into the bronchial tubes and the rest being left to diffusion, the air is drawn right through the lungs into the abdominal and other air-sacs, so that the highly vascular lung with its venous blood supply is brought into more direct relation with the "tidal air" than in mammals, while the "residual air," which is not changed at each double respiratory movement, rests, not, as in mammals, in the lung itself, or at least not chiefly so, but in the air-sacs outside the lungs. The respiratory organ proper is thus brought into direct relation with air capable of far more rapid renewal than in the mammalian lung. In other words, birds breathe the tidal air, while mammals breathe the residual air. *Archæopteryx*, however, shows no sign of the possession of large air-sacs, or of that large expanded sternum which, in modern birds of flight, insures the rapid change of the air by the same



ARCHÆOPTERYX.

From a photograph, one-quarter natural size, of the specimen in the British Museum (Natural History), taken by kind permission of Dr. Henry Woodward, F.R.S., Keeper of the Geological Department.

muscular movements as are involved in flight itself. While in all powerful fliers, both birds and insects, every movement of the wing insures a change of the air in the respiratory organ itself (and not merely in the passages leading to that organ), the form and structure of *Archæopteryx* forbid us to believe that such an adaptation existed in it.

Conclusions as to the size and efficiency of the heart might be drawn, but they are not only obvious but also perhaps a little risky; so I will leave them.

Nobody, except the constructors of flying-machines, seems to be ignorant of the fact that all powerful fliers have a wing-area which is very large in proportion to the area of the immoveable aëroplanes. Birds do not nowadays rely upon immoveable aëroplanes at all. The wings serve all the functions of both propulsion and sustentation, and the tail when used at all is used only for steering; while the vastly superior flight of insects is effected in the absence of any special steering apparatus, the wings themselves serving alike for propulsion, sustentation, and steering.

In *Archæopteryx*, then, we find an animal as yet not evolved beyond the aëroplane phase of flight; a phase characterised by the use of large aëroplanes, which, while offering considerable resistance to flight, take no part in the propulsion. It may further be pointed out that among flying things, whether birds, mammals, insects, or flying machines, the most efficient are all far broader from side to side than they are long. Lilienthal, alone among men who seek to fly, seems to have appreciated the real bearing of this fact. The further mechanical consideration of the point, even as applied to *Archæopteryx*, would, however, take us too far. It is sufficient for my present purpose to have pointed out wide differences of structure both as to the immediate organs of flight and also as to the propelling muscles and the respiratory organs on which powerful flight depends, between, on the one hand, *Archæopteryx*, and, on the other hand, all other flying animals. I will only add an argument for the benefit of biologists who may be unable to grasp the significance of mechanical considerations. The oldest known bird—*Archæopteryx*—was constructed, so far as the sustentatory apparatus of flight was concerned, on a principle which has been superseded in all modern birds, and which may therefore be safely pronounced, even on purely biological grounds, to be inferior in effectiveness to the principle of construction which has superseded it.

Archæopteryx was, therefore, not a very good flier. How good it was as a flier, I dare not guess; though the perfect, but all too small, wings strongly suggest that, in spite of the impediments I have mentioned (free digits, heavy head and neck, large aëroplanes offering resistance to rapid movement, small muscles, heavy non-pneumatic bones, and deficient respiratory apparatus), *Archæopteryx* could fly better than the competing pterodactyles.

Since the days of *Archæopteryx*, its descendants, or the descendants of its near relatives, have been evolved into the modern bird, which, in its more perfect forms, is a perfect biped and a powerful flier; and a brief enumeration of some of the chief changes involved in that evolution will throw an additional light upon my contention as to the comparatively small power of flight, and as to the quadrupedal locomotion of *Archæopteryx*. The shoulder has shifted backwards, the trunk has become shorter, bringing both elbow and knee-joints nearer the centre of gravity; thereby rendering balance in walking or standing independent of the aid of the fore-limb. The fore-limb, in accordance with this release from one of its functions, has lost the digits I, II, and III on which that function depended, and has thus been reduced in inertia, while the pelvis has become widened to allow for the backward displacement of the abdominal viscera, and the tibia has acquired a cnemial crest of much greater size, enabling the hind-limb to support the weight of the whole body, with the knee flexed and placed in such position that the centre of gravity is behind it. The aëroplanes of the tibiæ have been abolished, and that of the tail shortened, lightened, and reduced to a steering apparatus pure and simple. The wings have gained in size; their inertia has been reduced by the development of air-cavities in the wing-bones; more powerful muscles have been evolved for the movement of the wings, and this has involved the development of a pectoral crest and of a large sternum. The reduction in weight of the head (especially jaws) has aided in the backward shifting of the centre of gravity. The development of the large sternum has facilitated the evolution of a respiratory apparatus of more efficient type, and the shortening of the whole body and concentration of its weight near one point, aided by the stiffening of the back—often by ankylosis of the vertebræ and always by the increase in length of the functional "sacrum"—has rendered the support of the hinder part of the body by cumbrous aëroplanes unnecessary.

This is by no means all that might properly be urged in favour of the views I have expressed, but I fear to make my discussion too long.

C. Herbert Hurst.

(*To be continued.*)

VI.

An Eelworm Disease of Hops.

IN September, 1894, NATURAL SCIENCE (vol. v., p. 170) was good enough to draw the attention of its readers to my investigation of a new disease, which causes hops to become what is termed "nettle-headed," and which during the last two or three years has been giving a considerable amount of trouble to growers in several of the hop districts of Kent.

The following paper is the more extensive communication which was then promised to this Review. Last July I was asked to examine some of the affected plants, and since that date have been busily engaged in investigating the matter. The results of the work are, I venture to think, of the greatest importance, as the disease, if thoroughly established, promises to become one possessing serious consequences to the hop-grower.

The exact distribution of the disease in the county I have not been able to learn, owing to want of accurate observation and description on the part of the growers, and also to the inherent difficulty of distinguishing the disease in its early stages from obscure pathological conditions induced by unfavourable soil and manures. However, where it has been known for some time it has assumed a more virulent character, with well-marked characteristic symptoms. Affected plants are said to be "nettle-headed," "silly," or "skinkly." Isolated examples seem to have been noticed as long ago as 1880, but these cases did not extend beyond very small areas in the gardens. Lately, however, the disease has spread considerably, and necessitated the complete uprooting and destruction of several acres of plants in various localities. One correspondent informs me that it first appeared in old plantations of "Colegates," and spread to such an extent that this variety has almost gone out of cultivation. Almost all varieties are subject to attack, but there is a general consensus of opinion that the early kinds suffer most, and especially on loamy soils. The worst cases are met with in old gardens, and the disease is rarely noticed on a serious scale until five or six years after planting. I have, however, seen plants attacked in the second year, and this may become the rule rather than the exception.

The parts above ground in a typically diseased specimen show a marked want of vitality, although this stunting of growth is not evident until about the end of June, when the stem or bine has grown some distance up the pole. Then the bine becomes slack, and loses its power of twining. The head of the plant hangs away from the pole, and if the plant is not retied at a higher point the whole mass of stem and leaves slides down into a heap on the ground. When this stage is reached, further growth and development are arrested, the plant becomes worthless, and is generally grubbed. Those specimens which show symptoms of disease one year are almost invariably worse the following summer. What the ultimate result is I have not been able to learn from direct observation, but I am informed that the plant dies away altogether.

Stages between apparent health and complete ruin are always to be met with. It is not infrequent to observe some bines diseased, which are nevertheless sufficiently strong to reach the top of the pole, and even to yield a moderate quantity of hops; but by far the larger number in an infected garden are overcome before reaching this stage. Upon the same "hill" it may often be noticed that some bines topple over, and others remain normal. The diseased and normal stems in such instances generally grow from different "sets," and the cures said to have been produced by cutting off the affected bines and tying up others may be explained by a consideration of this fact. In one or two cases, however, I have noticed two bines springing from opposite sides of the same root stock, one of which showed symptoms of disease, and the other an apparently healthy growth.

In a diseased plant the stem not only loses its twining power but tapers rapidly, becoming very thin; the branches and young shoots also have the same extremely thin character, and the internodes of the plant are shortened, so that the leaves become more crowded than in healthy bines. The growth in length of the fibro-vascular system, or woody parts, all through the plant seems to be arrested.

The most characteristic symptom of the disease is the peculiar leaf development. The earlier leaves differ little from the ordinary type, and die a natural death. Those formed later are, however, very much smaller than usual, often darker in colour, and their edges curl towards the upper surface. (Plate III., Fig. I., *b*). The "veins" on the under surface stand out from the soft tissue in a marked degree (Plate III., Fig. I., *a*), and this, together with a slight puckering and increased serration of the leaf, makes it closely resemble the leaf of the stinging-nettle. On holding a leaf up to the light and viewing the under-side, the chief veins are seen to be drawn together at the angles where they branch from one another, and between them at these points are noticed yellowish transparent patches of thinner tissue. These patches are surrounded by thicker dark-green tissue, which also extends along the sides of the veins for a short distance. These are the first indications of the disease, and arise generally at the edges of the leaf and then extend backward along the sides of the veins to the middle and even to the base of the leaf-blade. Before curling takes place leaves exhibit this peculiarity, and although all which subsequently curl and die do not show it, I have never seen a case of true "nettle-headed" hop without the leaves about half-way up the bine being altered in this manner. Moreover, these changes take place in regular order after their first appearance, being always met with in leaves belonging to successive nodes and never at random on the stem. A cross section of the leaf at these altered points shows an abnormal production of "palisade" parenchyma, extending about half-way round on each side of the fibro-vascular bundle, and giving a darker green appearance (Plate III., Fig. II.). In some cases increased growth of soft tissue goes on until a very minute leaf-blade is produced on the side of the fibro-vascular bundle (Plate III., Fig. III.). In the lighter parts between the veins, the tissue shows no distinct differentiation into "palisade" and "spongy" parenchyma. This peculiarity may be brought about by strains in the older parts, caused by the subsequent growth of new tissue, but I have not had time to follow the development completely.

It was the discovery of the shortening of the fibro-vascular system in the young shoots and leaves, and of the concomitant increased cell-division in the leaf, leading to the abnormal production of parenchyma along the sides of the circulatory system there, that led me at once to suspect that nematodes, or eelworms, were at work in the root. Although such features are not invariably met with in all attacks by nematodes, in a large number of instances I have found such to occur. Complete diseased plants were kindly supplied to me by several growers, and an examination of the roots led to the discovery of large numbers of eelworms in different parts of the cortex and young root-fibrils. The parasites belong to the family of Anguillulidæ, and are representatives of two distinct genera, the respective species being *Tylenchus devastatrix* (Kühn), known as the "stem eelworm," and *Heterodera schachtii*, A. Schm, the much-dreaded "sugar-beet eelworm."

So far as I am aware, no species of eelworm has previously been recorded as attacking the hop in any part of the world; nor has *Tylenchus devastatrix* ever been met with in the roots of any kind of plant. It is, I believe, also the first appearance of the "beet" eelworm in the British Isles, although its ally, the "root-knot" eelworm, *Heterodera radicicola* (Greeff) Müller, is a well-known pest of cucumbers, tomatoes, and various other plants.

The *Tylenchi* occur in the thicker parts of the roots and live within the cortex, destroying it, and making their way close up to the living bast. They are not uniformly distributed all over the root, but only in isolated patches, and contrary to their usual behaviour when met with in stems and leaves of plants, produce no excessive growth of tissue in their immediate neighbourhood. Where they are present the cortex loses its bright orange colour, decay sets in, and the root at that point is generally found to have an eccentric development of the woody rings of growth. The cells round about the parasites die, turn brown, and become disorganised; thus small open spaces are formed in the living cortex (Plate, Fig. V.). Many of the cells become filled with a dark-brown resinous substance.

Knowing the extreme similarity of many of these nematodes, especially in the larval state, and the consequent difficulty of accurate identification of the species, I asked Dr. de Man, of Ierseke, and Dr. J. Ritzema Bos, Wageningen, Netherlands, to examine them. Both gentlemen kindly did so, and pronounced the *Tylenchus* to be *T. devastatrix.* Dr. Ritzema Bos, who has made a special study of this particular species, was at first inclined to believe that the hop-root *Tylenchus*, although very nearly allied to *T. devastatrix*, was not identical with it. On more careful examination, however, he informs me that he can find no constant difference between the two species, except that the hop-root variety is smaller than any he has seen in other plants.

It is worthy of note that males and larvæ are much more abundant than females in all the cases I have examined.

The *Heterodera* females I first discovered loose in the cortex of the thicker roots, along with *Tylenchus*-like larvæ, which no doubt belonged to *H. schachtii*, but which at the time I was unable to identify with certainty. There was, however, no difficulty in recognising the motionless citron-shaped females as of this species. Subsequent examination showed that these had become detached from finer rootlets, which, in a large number of instances, are found to grow and permeate the decaying bark of the root, instead of making their way into the soil.

An examination of the finer rootlets of well-marked "nettle-headed" plants revealed the presence of females of the *Heterodera* in thousands on all the younger fibrils, even among the root-hairs. They are readily seen with a pocket lens or with the naked eye as whitish points about the size of a very small pin's head, seated on the outside of the rootlet (Plate, Fig. VI.), and even distributed in the soil close by. In older stages they are a rich chestnut-brown, considerably darker than the root epidermis. Developing beneath the epidermis, they swell up and ultimately break through it, and become exposed on the outside of the root (Plate, Fig. VII.). The delicate youngest roots are seriously injured in this way, and with an ordinary lens the older fine root-fibrils are seen to be covered with minute holes and slits, the work of former generations of the nematode. With a slightly higher magnifying power all the various stages of growth of the female may be observed on the root. The rootlets are slightly thicker and more stumpy in character than usual, and not so copiously branched as in healthy plants. Nothing, however, like the galls of *H. radicicola* is produced, nor are the parasites so far embedded in the tissues of the plant, as is usual with the latter species. Dr. de Man found dead females in pieces of roots which I forwarded to him; and at a later date he examined fresh specimens and males, and stated them to be *Heterodera schachtii*. Dr. Adolf Strubell, of Bonn University, who has monographed this species, and Dr. A. Voigt, kindly examined examples, and reported to the same effect. Here, however, it may be mentioned that, as with the *Tylenchus*, the specimens on the hop-roots are considerably smaller than those occurring on other plants, and attack rootlets much thinner than usual. The females measured by Dr. de Man measured from 0·5 to 0·6 mm. long (Dr. Strubell's measurements on females from beetroot, etc., are 0·8 to 1·03 mm.). Detached from the rootlets and examined with the microscope, they are observed to be like a lemon in shape, white or brown according to age, and too opaque for the contents to be seen clearly (Plate, Fig. VIII., *a*). On pressing one between a cover-slip and glass side, the eggs, numbering from 200 to 300, can be squeezed out of the dead body-cavity (Plate, Fig. VIII., *b*). The eggs are somewhat bean-shaped (Plate, Figg. VIII. and IX.), about 0·096 to 0·11 mm. long ($\frac{1}{250}$ of an inch), and about half as broad, and go through their development in the body of the mother. The parasite is viviparous, and it is not infrequent to obtain from the same female both free completely formed larvæ, and eggs only partially hatched.

The larvæ, which are eel- or *Tylenchus*-like, leave the body-cavity in which they develop, and, according to the researches of Kühn, Strubell, and others, wander but a short time in the soil, soon finding their way to a fine root close at hand. Here they pierce the epidermis by means of a comparatively large spike which they can extrude from the mouth, and then wriggle their way into the soft parenchyma surrounding the central bundle of the root. They then come to rest after casting their skin, live upon the root-sap, and lose their eelworm shape, becoming plumper in form. At this stage the sexes are differentiated, and the females continue to swell and grow beneath the epidermis, until the pressure is sufficient to lift up and rupture the outer cell-layers of the root. In this manner they become exposed, only the head remaining in the root-substance. The females are never eel-like, nor do they move freely. The males develop from the enlarged resting larvæ by contraction of the contents of the latter and subsequent formation of a new membrane. Although, as larvæ, they swell and lift up the soft tissue by which they are surrounded to a considerable extent, they do not break through the epidermis like the females, but, after assuming a *Tylenchus*- or eel-like shape within the old case or cyst of larval-skin in the rootlet, they pierce their way outward, and after fertilising the female they die. Their life is very short—only four or five days—and thus casual observations of the male are less frequent.

The whole life-cycle of the parasite from the egg to the adult condition occupies about four weeks, so that a large number of generations are possible in a year.

It is a matter of considerable interest to find two distinct species of parasitic nematodes present in the hop roots. The exact part which each plays in the disease it is impossible to decide without carrying out a series of infection experiments, and such are now in progress. Neither species finds its way into the stems or leaves of the hop; and, judging from their position, manner, and rate of development, the peculiar changes observed in the leaves are no doubt to be attributed to the secretions of the worms being carried there in the ascending current of sap.

The *Tylenchus* is one of the causes of clover "stem-sickness" and "tulip-root" of oats, and is also met with in stems of buckwheat, rye, hyacinths, onions, and several other plants. The *Heterodera schachtii* is the cause of "beet-sickness," and is also known to attack and live upon the roots of all cereals, and practically all cruciferous crops, as turnip, cabbage, kohl-rabi, mustard, etc. Lately some leguminous crops have been attacked. Potatoes and all Compositæ (in which Order the artichoke, lettuce, and salsify are the chief food-plants) seem to be free from injury by this parasite.

With regard to remedies, it may be remarked at the outset that there is no known method of exterminating these nematodes from the soil when once they have become established there, and consequently it becomes imperative to call attention to the necessity for dealing summarily with the pest in the earliest manifestations of this new disease.

Thousands of experiments have been made on the Continent with a view of eradicating the *Heterodera* from the sugar-beet fields, but without success. The most satisfactory means of keeping it within bounds was suggested by Kühn after his researches in 1881, and his plan has so far yielded the best results, although it is no cure. He finds that the larvæ, after entering the roots of plants, require at one stage of their development considerable quantities of food. If this supply is cut short, the parasite dies before reaching maturity and before the fertilisation of the females has taken place. "Trap-plants" are grown, the most suitable being summer rape, on account of its extensive root-system, easy infection, and rapidity with which it dies when disturbed. After the larvæ have entered the rootlets and reached a certain stage of growth, the crop is destroyed, and with it all the immature parasites. A repetition of this process three or four times during the summer cleans the ground of most of the parasites, and a remunerative crop of beet can then be taken off in the ordinary course of cultivation for some time before the "sickness" shows itself again to any serious extent. To reduce the expense of idle ground, a rapidly-growing crop of potatoes is taken off in midsummer. It is obvious, of course, that such a process is not applicable to hop-gardens where the plants remain on the ground several years, but some such scheme must be adopted in order to clean the ground after grubbing a badly attacked garden. There is hope in a properly selected rotation of crops for diminishing the number of nematodes in the soil, but it is important that no crop should be grown which is known to be subject to attacks of *Heterodera schachtii*, or *Tylenchus devastatrix*, until the ground has been thoroughly cleaned in this manner. The evil effects of planting hops on the site of old gardens which have been grubbed only a year previously on account of the disease, I have witnessed more than once. Both the theory and practice of cleaning the ground by Kühn's method are good, but much of its success depends upon the estimation of the correct time at which the destruction of the "trap plants" should take place, and this can only be determined by an examination of the root, which requires a certain amount of skill in the use of the microscope.

The mature parasites are not more than a twenty-fifth part of an inch long, and the larvæ and eggs very much smaller. The minute character of the pest, coupled with its great reproductive power, makes it very difficult to deal with by direct application of poisonous chemical substances. Much of the life is passed inside the delicate roots of the plant. Moreover, the eggs are protected during their development by the dead leathery chitinous body of the female, and would not be injured by solutions strong enough to destroy the free larvæ in the soil.

The addition of chemical substances, such as carbon disulphide and gas-water, to the soil, apart from the cost of saturating the latter to any appreciable depth, would not be suitable in the case of growing hops suffering from the disease, since these substances are injurious to plants. Application of alkaline solutions and alkaline salts to the soil as manures has met with some success at the experimental stations in France and Germany, and these are to be recommended. Top-dressing in spring with muriate of potash or sulphate of potash has been found most beneficial; kainite, which is a hydrous magnesium sulphate with potassium chloride, has not been so satisfactory. Liming, in some instances, and working in of salt have diminished the evil effects of the parasites, and the former process should be tried, especially where the soil is full of humus from the indiscriminate use of large quantities of organic manures. As soon as an affected plant is seen in a garden it should be grubbed and burnt on the spot, and quicklime applied to the ground in the immediate neighbourhood. All implements used in cultivating infected gardens should be properly cleaned before being used among healthy crops, and the planting of "sets" taken from infected gardens should be discontinued.

It is difficult to determine the rate at which the parasites would naturally spread if left to themselves; there is no doubt that the parasites are at present largely carried from "hill" to "hill" by the incessant cultivation which goes on in the hop-garden.

Owing to the great extent of the root-system of the hop-plant and its rapidly growing and recuperative powers, it takes a considerable time before a plant is totally disabled and rendered useless;

but there is danger in the fact that many growers have allowed the plants to grow as long as any hops were produced, and thus the roots and ground have become thoroughly permeated with the parasites before steps have been taken to eradicate the evil. This is a bad policy, not only because the plant becomes a centre of infection for the immediate neighbourhood, but also because there is some evidence that by such unconscious rearing of successive generations of these nematodes upon one kind of plant, a new "race," so to speak, is raised, which becomes more virulent in its action and more completely adapted to its surroundings.

It is perhaps necessary to point out that the soil of a hop-garden teems with living organisms on account of the excessively large amount of humus present, due to the use of heterogeneous organic substances as manures (sprats, star-fish, leather, wool, fur waste, etc.), and enormous numbers of free-living non-parasitic nematodes are met with. Such fungi as live on decaying vegetable matter, especially those imperfect conidial forms belonging to the group *Hyphomycetes*, are also very abundant, both in the soil and upon every dying and dead leaf throughout the gardens.

In order that credit may be given to whom credit is due, I take the opportunity of claiming priority for an early English observer whose work on the subject of nematodes in root-galls has escaped the notice of continental writers. I refer to the late Rev. M. J. Berkeley.

Schacht, in 1859, while investigating diseased sugar-beet plants, became acquainted with peculiar white points upon the outside of the finer rootlets, and ultimately found that these were "sacs" filled with eggs and larvæ of an eelworm. In 1862 he discovered free male nematodes in the soil, which from their close similarity to the embryo worms obtained from the "sacs," he concluded belonged to the same species. Although he was informed by Lieberkühn and Wagener that it was a new species, he gave no name to the worm, and little notice was taken of his discovery until 1871, when A. Schmidt worked at the parasite. A complete study of the development was made, and although his observations were faulty in one or two minor zoological points, Schmidt nevertheless established the remarkable dimorphism which exists between male and female. As has previously been pointed out, the sexes differ so much in size and shape that were the development unknown the relationship of the two forms would readily be overlooked. Schmidt gave a name to the parasite, calling it *Heterodera schachtii*—a new species of a new genus. Kühn (1881), Strubell (1886), and others, have since cleared up the obscure points in its life-history.

Between the time of Schacht's last paper in 1862 and Schmidt's work in 1871, another species of eelworm had been seen by Greeff in 1864 in root-galls on certain grasses; and again, in 1870, by Magnus, in excrescences upon the roots of *Dodartia orientalis*. Greeff looked upon these eelworms and Schacht's species as belonging either to the genus *Dorylaimus* or to *Anguillula*. Ultimately he named the species *Anguillula radicicola*, and from the measurements which he gave (female, 2 mm. long and ·095 broad, male somewhat smaller), it is evident that as late as 1872 he either did not notice or did not understand the nature of the "cysts" in which the nematodes are met with in the root-galls under observation. The difficulty of realising the exact nature of the so-called "cysts" in the root-galls, even when they were observed, may readily be appreciated by imagining the free females of *H. schachtii* embedded in the hypertrophied tissue of a plant. The eggs and free larvæ would in such a case be easily observed on cutting a section, but the dead chitinous membrane forming the body of the female might be looked upon as belonging to the altered tissues of the plant. The free nematodes would then be

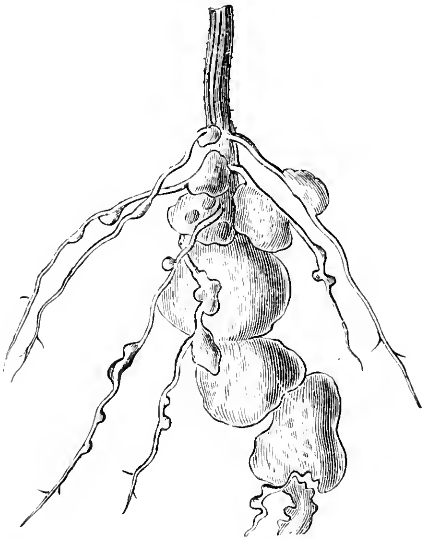


FIG 1.—Root-galls of cucumber plant caused by eelworms (*Heterodera*). From the Rev. M. J. Berkeley's paper in *Gardeners' Chronicle*, April 7, 1855.

classed in the ordinary *Tylenchus*-like groups. This mistake was made by practically all the earlier observers, until the matter was cleared up in 1883 by the work of Frank and Müller; the latter showing that Greeff's *Anguillula* was really a *Heterodera*, which he named *H. radicicola*.

In all the papers upon *Heterodera*, Greeff is credited with the first discovery of eelworms in root-galls of plants; but while examining the literature in connection with this subject, my attention was called by Mr. Carruthers to a paper by the Rev. M. J. Berkeley in the *Gardeners' Chronicle* of April 7, 1855, which clearly shows that this view is an error. Berkeley's observations were made upon the diseased roots of cucumbers nearly ten years before Greeff's discovery, and even four years before Schacht's notice of the beet eelworm. Not only does the English observer note the occurrence of nematodes in root-excrescences for the first time, but gives two figures in his paper, here reprinted by kind permission of the editor of the *Gardeners' Chronicle*. One of them is an enlarged view of what is now recognised as a female of *Heterodera radicicola* embedded in the hypertrophied tissue of the root. Eggs and young larvæ are figured, and Berkeley mentions that these occur in free cyst-like bodies, which appear to be regular membranous sacs.

"The cyst," he says, "was destitute of any evident organic structure, was not affected by iodine and sulphuric acid, but showed some appearance of giving way under caustic potash. It was clear, then, that it did not consist of cellulose, but might possibly be some

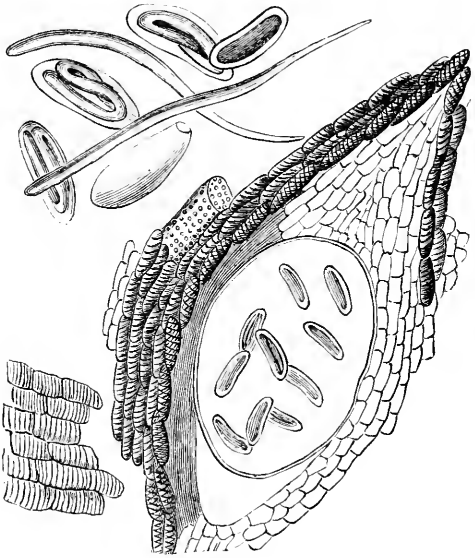


FIG. 2.—"Cyst" female *Heterodera* from root-galls of Fig. 1, with eggs and young larvæ.
From the Rev. M. J. Berkeley's paper in *Gardeners' Chronicle*, April 7, 1855.

modification of xylogen. It is not conceivable that such a cyst could have been deposited by the vibrio itself, and we must therefore consider it as due to the irritation caused by the presence of the eggs, and exactly analogous to the cysts produced by the larvæ of the cestoid worms in animal structures. We are not aware that anything of the kind has ever been observed before in vegetable parasites; for, though the tissues of vegetables are greatly altered by the presence of the larvæ, which produce galls, it does not appear that they ever give rise to a free cyst, as in the present case, differing altogether from the surrounding tissues."

It is obvious that although Berkeley looked upon the body of the female as a cyst, his careful attempts to ascertain its nature by chemical means placed him within measurable distance of clearing


VII
VI
V
IX

up what remained an imperfectly understood phenomenon for more than a quarter of a century.

In conclusion, I wish to express my thanks to the Keeper of the Botanical Department of the Natural History Museum and to Mr. George Murray, for aid and for permission to work in the laboratory of that institution.

South-Eastern Agricultural College,
Wye, Kent. J. PERCIVAL.

EXPLANATION OF PLATE III.

I.—*a*, Under, *b* upper surface of leaf from "nettle-headed hop," showing characteristic curling of edges and puckering of veins. Natural size.

II.—Transverse section of leaf, showing abnormal development of tissue at A and B. Magnified forty times.

III.—As in II., showing further growth of tissue at side of midrib. Magnified about forty times.

IV.—Transverse section of root, showing injured cortex and irregular growth at points where *Tylenchus devastatrix*, Kühn, appears. Natural size.

V.—Radial longitudinal section of IV., showing *Tylenchus devastatrix* in cortex. *a*, cork cambium ; *b*, cortex ; *c*, bast. Magnified.

VI.—Hop rootlet, with attached females of *Heterodera schachtii*. Natural size.

VII.—Hop rootlets, with *Heterodera schachtii* females; (*a*), beneath epidermis; (*b*), exposed after breaking through epidermis. Magnified.

VIII.—*a*, Female of *H. schachtii*; *b*, the same after rupture of female under cover-glass, showing eggs and larvæ. Magnified.

IX.—Eggs in different stages of development, and free young larvæ of *Heterodera schachtii*, showing exsertile mouth-spear. Magnified about 250 times.

SOME NEW BOOKS.

Andrew Crombie Ramsay.

Memoir of Sir Andrew Crombie Ramsay. By Sir Archibald Geikie. 8vo. Pp. xii., 397, with 13 plates. London: Macmillan & Co., 1895. Price 12s. 6d. nett.

This biography of the late Director of the Geological Survey of Great Britain and Ireland, by the distinguished pen of his successor in the office, will not disappoint the geologists who have eagerly awaited it. To all interested in the noble science of geology, and especially to those concerned with British geology, the book will prove first of all a treat to read, and then a valuable, indeed an indispensable, work to place on their shelves for future reference. Our own circle of readers need not be reminded of the scientific labours of Ramsay, his detailed and masterly field-work—chiefly in Wales, his philosophical studies in the stratigraphy of our islands, his restoration of vanished geographies, and his far-reaching theories of denudation by rain, rivers, and above all by ice. Many indeed who know nothing further of geology have read with profound interest his charming volume on the Physical Geology and Geography of Great Britain. But his work as a geologist was far more than ever met the public eye, work in the field, in the office, and in the lecture-room; and the man himself was greater than his work.

But the volume before us does not give merely the details of Ramsay's own life and a sketch of his labours as a geologist; it is a storehouse of information for all who desire to learn something of that great institution, the Geological Survey, and the establishments now or formerly connected therewith, namely, the Museum of Practical Geology and the School of Mines. Among the many figures that are brought vividly before us in these pages, next to Ramsay himself, none stands out more prominently or more lovably than the gifted man who was the instigator, the founder, and the first head of these institutions, Sir Henry Thomas De la Beche, the Father of British Official Geology. It was harder work then than it is now, to fight against an economical Government and "the eternal 'No' of the Treasury"; harder work, too, to enforce the claims of the investigation of earth-structure and earth-history, in days when "earth" and "dirt" were hardly discriminated, and when polite society regarded the geologist in much the same light as the collier or the quarryman. In enthusiastic and humorous sentences, Sir Archibald sketches the life of a "Royal Hammerer." Many are the curious characters for which the itinerant geologist is mistaken, sometimes by an ignorant peasantry, sometimes, with direr consequences, by gamekeepers and policemen. The funniest story is that told of "one of the staff who, poking about to see the rocks exposed on the outskirts of a village in Cumberland, was greeted by an old woman as the 'sanitary 'spector.' He modestly disclaimed the honour, but noticing that the place was very filthy, ventured to hint that such an official would find something to do there. And he thereupon began to enlarge on the evils of accumulating filth, resulting, among other things, in an unhealthy and stunted population. His auditor heard him out, and then, calmly surveying him from head to foot, remarked, 'Well, young man, all I have to tell ye is that the men o' this place are a deal bigger and stronger and handsomer nor you.' She bore no malice, for she offered him a cup of tea, but he was too cowed to face her any longer."

In dealing with a subject so wide as the growth of the Geological Survey, with which growth Ramsay was so intimately bound up, the historian and biographer has naturally to mention a large number of individuals whose names, though familiar enough to the professed geologist, may be no longer known to the public of amateurs. Each of these, as he is first mentioned, is introduced to the reader in a footnote, while a dozen of them have the additional honour of an excellent portrait. From this point of view, the book, provided as it is with an exhaustive index, may serve as a biographical dictionary to the mesozoic period of the history of British geology. The best of the portraits are those of De la Beche, Murchison, Richard Gibbs (the Survey collector), Thomas Oldham, and J. W. Salter.

Not only geologists, but many other celebrities, are met with herein, outlined in Ramsay's own picturesque phrases. Many, too, are the glimpses that we get of social, scientific, and scientifico-social assemblies—the Red Lion Club, the Geological Society, the "Wisdom meeting," now known as the British Ass, the dinners of the Survey and of the Geological Club, the Royal Institution and others. Murchison asks young Ramsay to breakfast, when he first comes to London—"His house is a splendid one. They are quite people of fashion, but, notwithstanding, Mrs. M. is a kindly body, and made me quite at ease at once." At dinner there he meets "Mr. Featherstonhaugh, the American plenipotentiary, . . . a lively man, but takes no wine for his stomach's sake." Dean Buckland condescended to make Ramsay's acquaintance—"So I was introduced, and the Doctor gave me two of his digits to shake." This was at a meeting of the Geological Society, and the discussion continued till eleven; is it for suburban trains that the meetings nowadays are always "closured" at ten, or are geologists more married than they used to be? At the Royal Institution, this day forty-six years ago, Owen lectured on Limbs; Ramsay stood on the steps—"the theatre was quite full. I saw many I knew: Dr. Fitton looking good-humoured, Sir Roderick looking anxious to keep awake, Dr. Mantell looking eager, Dr. Macdonald looking jolly and anxious for a hole in Owen's coat, Sir Henry looking attentive and queer when Owen came to the peroration, Sir Charles and Lady Lyell looking knightly, Lady S—— looking vulgar, Nicol looking Scotch, with a doubt in his eye, and Mrs. F—— looking at her dress." When Murchison lectured at the same place, he "was quite nervous in the early part of his lecture." Forbes, on the other hand, "lectured in first-rate style, coolly and boldly." "The Astronomer Royal lectured to a crowded audience, Prince Albert in the chair. Airy forgot himself, and lectured an hour and three-quarters! The Prince fell asleep." When Ramsay himself first lectured there, "Faraday ran up to him at the close, shook him by both hands, and asked 'Where *did* you learn to lecture?'" Shortly before the Museum in Jermyn Street was opened, it was inspected by many great people. Among others by Lord and Lady John Russell. "He, cold and uninterested; she, most charming and intelligent. When I was introduced, he merely bowed coldly. Ditto to all. Blewitt, the M.P. for Monmouth, he coldly bowed to. 'Who would have thought,' said Blewitt, 'that I've sat beside that man and supported him for fourteen years; he is a nice man to keep a party together!'" About Disraeli we are told this story,—"Vernon Harcourt asked a Conservative friend, 'How can you and your party follow such a man?' 'We look on him as a professional bowler,' was the reply." There is an interesting account of Lyell out with some Survey men in Dorsetshire,—"We all like Lyell much. He is anxious for instruction, and so far from affecting the big-wig, is not afraid to learn anything from anyone. The notes he takes are amazing; many a one has he had from me to-day. He is very helpless in the field without people to point things out to him; quite inexperienced and unable to see his way either physically or geologically. He could not map a mile, but understands all when explained, and speculates thereon well. He wore spectacles half the day, and looked ten years older. Logan says it is vanity that prevents his always doing so. I think it is custom, and perhaps his wife." Of the meetings of the Geological Society accounts are numerous. There have been changes in the procedure of this body, but in other respects things are much as they were when Ramsay, on hearing that there was a lack of papers, wrote as follows,—"They might have more were it not that authors of theoretical papers are afraid to send them in for fear of the fatherly care of the Council. Green's last paper was quashed by —— in particular. You will see it in the *Geological Magazine*. I have a great mind to send in a paper entitled 'The Wonderful, the Councillor,' with illustrations, by Rutley, of living examples."

We must pick out no more plums, not because it is unfair to the book, for that can well spare a few from its abundance, but because space fails us. Two more extracts only really must be inserted. One shows Ramsay, as Local Director, in the field. "Never was there a more delightful field instructor than he. Full of enthusiasm for the work, quick of eye to detect fragments of evidence, and swift to perceive their importance for purposes of mapping, he carried the beginner on with him, and imbued him with some share of his own ardent and buoyant nature. . . . He would take infinite pains to make any method of procedure clear, and was long-suffering and tender where he saw that the difficulties of the learner arose from no want of earnest effort to comprehend. But woe to the luckless wight who showed stupidity, inattention, or carelessness! Ramsay's eye would flash, his hand would whisk the tips of the curls on his head, he would seize the map and rush ahead, calling on the defaulter to come on and look. And he would keep up his offended tone until he felt that his pupil had at last been made to feel his delinquency. Then some snatch of a song or line of an old ballad or fragment from Shakespeare, appropriate to some phase of the incident, would come into his head, and instantly it would be on his lips with probably a hearty laugh, that showed how entirely the cloud had passed away."

Our last extract shall be the concluding verse of Ramsay's famous Survey song, "The Lay of Sir Roderick the Bold and the Emperor of all the Russias," which is sung to the air of "The Auld wife ayont the Fire." The song relates how the Knight, with De Verneuil and Count Keyserling, went at the invitation of the Czar "to map the rocks ayont the sea, that rise upon the Ural," for, as the Czar said, "'Twill be droll but you will find a bed of coal, and I'll sing Tooralooral."

"Then hame he cam, and left his mates,
And wrote a book wi' maps and plates,
And sections o' the Russian states
Frae Baltic Sea to Ural.
The Emperor he scratched his poll,—
''Tis bravely done! but by my soul!!
I wish we had some beds o' coal!!!
Oh! Tooralooralooral!!!!
There's auld rocks ayont the sea,
There's British rocks ayont the sea
Hae lots o' coal, the worse for me,
There's nane beside the Ural.'
(*Weeps.*)"

Monkeys.

A Handbook to the Primates (Allen's Naturalists' Library). By Henry O. Forbes, LL.D. 2 vols. 8vo. Pp. xvi., 286, 22 coloured plates; pp. xvi., 296, 7 coloured plates, with 8 maps, showing distribution of various genera, and other illustrations. London: W. H. Allen & Co., 1894. Price 6s. each vol.

To those familiar with the rich series of living monkeys which pass from time to time through the gardens of various zoological societies, and those which adorn the cases in our museums, it may seem strange to say that comparatively little is known of this group of animals. But it is a fact that a considerable proportion of the monkeys known to science have been described from menagerie specimens, with imperfect ideas as to their exact habitat and with a still less perfect knowledge of the variations of the individual species which has been described. Numerous examples occur in our museums about which the only information is thus recorded: "From the zoological gardens of ————, purchased of Mr. ————; said to come from ————." And all this imperfect knowledge comes from the fact that the sportsman, filled with sentimental yearning for his kinsmen, hesitates to shoot, and so zoology has to grope along as best it can, except in certain cases. Happily, this state of things is passing, and we are now able to understand with a degree of certainty the Bornean monkeys, for instance, thanks to the careful collecting and observation of Mr. Alfred Everett and Mr. Charles Hose. It is well to insist somewhat on this fact, because until better collected series of skins and skeletons of monkeys from all parts of the world are available, the construction of maps of distribution of this interesting order is almost useless and leads us into grave and persistent error.

We are particularly pleased to see, almost for the first time, this effort to collect together a general and particular account of the Primates between four covers. Especially so, as, from the reasons given above, a hopelessly muddled state of things prevails, and the whole of our present knowledge of the group is widely scattered in literature. This undigested mass of literature Dr. Forbes has now arranged, and despite the obvious signs of hasty writing, and the evidence of carelessness in references, the book will form a valuable basis on which some future zoologist may build up a permanent and an enduring structure.

The work begins with a too brief Introduction, containing the description of a Primate, and passes at once to the consideration of the Lemurs, in which group the author has received considerable and important assistance from Dr. Forsyth Major. These include the Aye-Aye of Madagascar, the Tarsiers of Malaysia and the Philippines, and the true Lemurs of Madagascar, Africa, Ceylon, India, and Malaysia. Of the numerous species comprised in the Lemuroidea, by far the larger number are strictly confined to Madagascar; but the fact that species also occur in Southern India and Malaysia has caused some zoologists to view with favour the idea of a former land junction between these distant areas. But when we consider the geological distribution of the group, as evidenced by the fossil remains already brought to light, we see that it formerly extended over the land area of Africa and Southern Europe and Asia, and are more inclined to believe that conditions of climate or other causes have determined the present distribution at either end of the earlier extension. Dr. Forbes makes a feature of his geological evidence, and treats of the majority of the remains described by various authors from the rocks beneath us. So far as we know at present, the Lemuroidea appeared in Lower Eocene times, and obtained a considerable development in the Middle Eocene. Zittel has called attention to the fact that in the Upper Eocene of Europe lemuroid genera formed a very characteristic element of the fauna, and the evidence, though less abundant, is perfectly sufficient to permit a similar though not so strong statement as regards North America. By far the most extraordinary of all these fossil lemuroids is *Megaladapis* from Madagascar, an account of which was given in NATURAL SCIENCE (April, 1894, p. 243).

Following on the Lemuroidea come the Anthropoidea (or true monkeys), containing the Harpalidæ and the Cebidæ of the New World and the Cercopithecidæ and Simiidæ of the Old World, both of which two groups are exclusively confined to their own areas. Moreover, no fossil representative of the two former has ever been found in the Old World, nor have any remains of either of the latter families ever been found in the New World, a fact, as Dr. Forbes remarks, which doubtless indicates a separation of great antiquity between the two groups. The chief genera of the Harpalidæ and Cebidæ are those containing the Marmosets, Capuchins, Woolly Monkeys, Spider Monkeys, Howlers, Sakis, and Squirrel Monkeys; and their chief habitat is the extensive equatorial forests of the Amazons, the Orinoco, and their tributaries. It is interesting to note that Mr. Salvin has recorded troops of the Mexican Spider monkey at an elevation of 7,000 feet on the volcano of Atitlan; but as a rule the basal line of a mountainous tract seems to be preferred by these animals. All these New World monkeys are characterised by a flat nose, the opening of the nostrils directed outwards, and the nostrils widely separated by a broad cartilaginous septum; on this account they are designated Platyrrhini. All the Old World monkeys, on the other hand, have the nose narrow, the nostrils close together and directed downwards, as in man, and separated by a thin septum, or partition, of cartilage; and from this peculiarity they have received the name of Catarrhini.

These Catarrhini include the Baboons, and the Guenons of Africa, the Black Apes of Celebes, and the Asiatic Macaques, the curious Nosed-monkeys of Borneo, the Langurs of India and Malaysia, the Guerezas of Africa, and the Man-like Apes. Among the best known forms of the Old World apes are the Chimpanzee, the Gorilla, and the Orang-Utan. The remaining family of the Primates, the *Homo sapiens* of Linnæus, is somewhat better known than the rest, and Dr. Forbes dismisses it in some six pages.

Each genus and species of monkeys dealt with by Dr. Forbes is described in brief but comprehensive terms, and notes are given on its geographical distribution (as recorded), its general habits and characteristics, while a synopsis of the literature connected with it is quoted. When possible, a brief extract from the personal observations of travellers is also given; these are often of considerable interest, and go far to make the subject inviting to the general reader, as may be seen from Mr. Ridley's paper now appearing in our pages. A somewhat larger proportionate space is devoted to the Simiidæ than to the other groups of monkeys, by reason of their being the most highly-organised and nearest to man in structure; and from the generalities appended to the several species included in the family we recognise, as might be expected, that they are among the best-studied members of the whole group.

Respecting the range in time of the extinct Anthropoidea, the geographical evidence goes to show that they appeared later on the scene than did the Lemuroidea. In South America, the Santa Cruz beds of Patagonia (Upper Eocene or Oligocene) have yielded remains, but in the Old World undoubted monkeys do not appear until the Miocene period. In Pliocene times, existing genera are represented by numerous species. A molar tooth of a form said to be identical with the Orang-Utan (*Simia satyrus*) is recorded from the Pliocene beds of the Sivalik hills, and this is of considerable interest in connection with the flint flake from Burma, of Miocene or early Pliocene age, illustrated in NATURAL SCIENCE for November, 1894 (p. 346); for, if correct, it seems to show that the higher Primates were already differentiated in those early times.

The volumes close with an attempt to show the geographical distribution of the group, limited, of course, by the difficulties to which we have alluded in our opening paragraph. In illustration of this distribution eight rough but useful key-maps are given, and one can obtain at a glance a good general notion of the monkey world.

With regard to the twenty-nine coloured plates, we are tempted at first to rail at the publishers for providing such chalky pictures; but on reflection, and considering the cheapness of the volumes, we come to regard the pictures in a more favourable light. It may be well, nevertheless, to remind Messrs. Allen & Company of the old proverb about the "ha'porth of tar." But in spite of these minor defects we have the message of the monkey people, as Dr. Forbes understands it, and to him, to the publisher, to Dr. Bowdler Sharpe, and to the public, we offer our congratulations on the issue of these volumes.

THE YOUNG COLLECTOR.

BUTTERFLIES AND MOTHS (BRITISH). By W. Furneaux, F.R.G.S. With 12 coloured plates and numerous illustrations in the text. Pp. xiv. and 355. London: Longmans, Green and Co., 1894. Price 10s. 6d. nett.

WE notice with joy, but without understanding, the present publishing "boom" in natural history. Volumes of scientific series lie on every library table, appear in every publisher's catalogue. Many are designed for a scientific public; more for a general public. Of the latter order is the book before us, and we can praise it without reserve. The illustrations, even the coloured ones, are conspicuously good. Nearly every English butterfly, and not a few of the moths and microlepidoptera, are figured.

The first sixty pages are devoted to a general account of the habits, structure, and classification of butterflies and moths. This part is clear, intelligent, and interesting. The only suggestion we should like to make is that a compound microscope is unnecessary for any of the details that Mr. Furneaux mentions. A first-rate platyscopic hand-lens is much more convenient, and the young naturalist should train himself thoroughly in the use of it. There is no more common error than an undue use of the higher powers of a microscope. Except for the intimate details of histology, a low power or a hand-lens is much more easy to use, and its employment gives a much better idea of the structure.

The directions for catching, killing, and preparing specimens are excellent. We note with great pleasure a clear account of the best method for preparing larvæ. The chapter on breeding is most useful.

With the aid of this book young collectors may train themselves, not merely to make a postage-stamp collection of butterflies and moths, but to study these insects in relation to their natural surroundings and to the phases of their life. We commend it heartily.

DARWINISM IN THE LECTURE-ROOM.

LECTURES ON THE DARWINIAN THEORY. Delivered by the late Arthur Milnes Marshall, edited by C. F. Marshall. 8vo. Pp. xx., 236, with two plates and 35 text-illustrations. London: D. Nutt, 1894. Price 7s. 6d.

IT is rather ungracious to complain that a book is too sumptuously turned out, and yet that is what we feel inclined to say with regard to the one before us. We expected a companion volume to that containing the Biological Lectures and Addresses, reviewed in NATURAL SCIENCE for August, 1894; but the present volume is in larger size, on better paper, printed with larger type, profusely illustrated, and furnished, not only with an index, but with an elaborate table of contents occupying ten pages. All these features are praiseworthy, and would doubtless become a book that was "a useful contribution to the literature of Darwinism," and a fitting memorial of the accomplished and lamented Milnes Marshall. But this book remains, in spite of its garniture, neither of those things. Extension Lectures are well enough in their way, but, like the diagrams that illustrate them, they are scarcely intended or fitted for presentation to the public as a permanent possession. For Extension Lectures these chapters and their illustrations were prepared, and on every page they bear the mark of their origin, not to be veiled by beautiful printing or expensive process-engraving.

There is no serious fault to be found with the book. It will serve the turn of the Home Reading Union and such bodies excellent well; and if an occasional lecturer utilises it to supplement his own learning or imagination, as the parsons of an older and a wiser day utilised the homilies of the church, why, no great harm will be done. If, however, we must play the serious critic, as the editor of the book doubtless desires, we will venture an objection to its title. It is an instance of the old and oft-corrected confusion between the fact of evolution and the Darwinian explanation of evolution. We are ashamed to have to repeat that Darwin's contribution to the philosophy of evolution may be summed up in the phrase Natural Selection and its subsidiary Sexual Selection. Darwin of course did more than proclaim these methods of evolution; but what else he may have done is no part of the Darwinian theory. These lectures, however, deal far more largely with the evidence for evolution than they do even with Natural Selection, while Sexual Selection is mentioned only in the two pages that deal with Epigamic Coloration, and then in a cursory manner. What, for instance, is meant by the argument from palæontology or the argument from embryology? These branches of research have never furnished any evidence for Natural Selection, nor can they be expected to do so.

An inappropriate title need not prevent the book from doing good. These lectures are quite as interesting as the fluff that sells by hundreds of thousands, and have the additional merit of sobriety and correctness. They should set right many popular errors. One or two warnings may, however, still be given. It should be more clearly stated that the facts of modern geographical distribution are in themselves no evidence for evolution: on a hypothesis of special creations such facts present no difficulties, and Louis Agassiz was able to use them in defence of his own anti-evolution views; on a hypothesis of evolution, however, the facts do present difficulties, which are to be explained by geology and palæontology on the assumption that evolution has taken place. To bring these facts forward as evidence of evolution is therefore to argue in a circle. Again, one would like to have seen a more cautious treatment of the Recapitulation Theory. Obviously Professor Marshall could not have considered the recent objections raised by Mr. Adam Sedgwick in the *Quarterly Journal of Microscopical Science*, but he might well have discussed the criticism published by Dr. Hurst in our own pages before these lectures were delivered. Possibly he did so, but the notes may not have been preserved. There is little doubt that many of the instances given by Professor Marshall can be explained without the aid of this theory. In fact the author distinctly excludes from consideration those cases which, as some believe, can alone afford conclusive evidence. One of his tests of recapitulation is that "Each stage must be an advance of [*sic*] the preceding one. . . . Intermediate stages, which are not and could not be functional, can form no part of an ancestral series." But there are surely cases in which the descendants are degenerate, not because they are specialised, but because they are actually less able to cope with their surroundings. Why are these cases to be excluded? A decadent which, in its development, shows features characteristic of a more highly-developed ancestry, is a more powerful witness to recapitulation than are all the advanced types put together.

Objections such as the preceding, however, will present themselves only to the serious and professed student, and it is not for him that the book was written, for whomsoever it may have been published.

F. A. B.

Foraminifera.

A Synopsis of the Arctic and Scandinavian recent Marine Foraminifera hitherto Discovered. By Axel Goës. Kongl. Svenska Vet. Akad. Handlingar. Vol. xxv., no. 9, 1894. 4to. Pp. 128, with 25 plates.

Since Rupert Jones and Kitchin Parker published their monograph on the North Atlantic Foraminifera, students of the Rhizopoda have had to piece together the scattered writings on the subject. Now that Dr. Goës has brought out this handsome quarto, taking in, not only the North Atlantic, but the Arctic Ocean and the Baltic Sea as well, we have an up-to-date account of Scandinavian Foraminifera.

Opening his paper with a brief sketch of the various expeditions by which material has been obtained, Dr. Goës makes some remarks on variability among the Foraminifera. He then proceeds with a detailed specific description of all the forms, giving their synonyms, depths, and collective and other details. There are two new genera, one *Crithionina*, a labyrinthine polymorphic sandy form, and the other, *Ceratina*, a *Trochammina*-like form, with a porcellanous test. This latter is from the Azores, 540 metres, and is not a Scandinavian form. The plates are such as we are accustomed to from Stockholm, are drawn by Goës and Hedelin, and lithographed by Schlacter, and are beautiful: an especially fine figure of *Astrorhiza* occupies plate 1.

Among the Californian Mountains.

The Mountains of California. By John Muir. Pp. 381, with 53 illustrations in the text. London: T. Fisher Unwin. Price 7s. 6d.

This book contains some excellent studies in natural history, among which, in our opinion, one of the best is that upon the Dipper. *Cinclus mexicanus* is a frequenter of the coldest waters of the sierra, and is one of the few birds that is equally merry under depressing and exhilarating circumstances. Many birds droop and are dull when external nature does not suggest joviality; not so the Water Ouzel, who "both in winter and summer sings sweetly, cheerily, independent alike of sunshine and of love, requiring no other inspiration than the stream on which he dwells." It is one of those birds that are thoroughly at home under the waves despite the fact that, apart from the plump and rounded form of the body, there is no special adaptation to an aquatic life. The feet are, of course, not webbed, and so progression is effected by flying under water as in the case of the penguin. The Water Ouzel is dignified with a chapter all to himself; so, too, is the Douglas Squirrel; but no other beast is thus treated, though scattered through the book are numerous notes and casual allusions. Like "Orpheus at the Zoo," Mr. Muir whistled to the animals, and especially to the squirrels. He sang or whistled "Bonnie Doon," "Lass o' Gowrie," and many other national airs to which the squirrel listened with more than a polite attention. But when, remembering, perhaps, its effect upon the Scotch professor, our author came to the "Old Hundredth," off went the squirrel screaming "his Indian name, Pillillooeet." We are inclined to praise Mr. Muir's book, not only for what is in it, but also for what is not in it. With praiseworthy fortitude, in these days of free and easy generalisation, the author refrains from relating his experiences, if he had any, with living creatures looking like bits of sticks or, like the dweller in the suburbs, trying to appear to be something else than what they really are. However, we must not speculate further upon the "might have been"; and so, with a final commendation, we bid —not farewell—but *au revoir* to Mr. Muir.

Recent Publications and Periodicals.

Mr. Mellard Reade's paper in the last issued number of the *Proceedings of the Liverpool Geological Society*, on the Dublin and Wicklow Shelly-Drift, will be welcome to geologists who want some general account of these deposits, and it will also be valuable as containing careful mechanical analyses of the gravels; that is to say, of the size and nature of the stones composing them. From a biological point of view it is not so satisfactory, for Mr. Reade, like most writers on the subject, ignores the strange mingling of the contained fossils. We observe also that the same fauna occurs at extremely different levels, and that there is no apparent division into zones of depth, such as one would expect in undisturbed marine stratum. A great deal has yet to be done before we can accept such confused gravels as evidence of a "great submergence." How is it that no one has yet found on the lowlands, anywhere in the British Isles, a marine fauna pointing to a depth of 100, or even of 50, fathoms? Such deposits should be common, if there has really been a submergence of 500 or 1,000 feet.

We have received from Mr. C. A. Barber, the Superintendent of Agriculture in the Leeward Islands, a paper on the grasses of Antigua, and the second number of the *Agricultural Journal of the Leeward Islands*, an octavo of 28 pages, published at St. John's, Antigua, price 6d. The journal contains notes on Durians and Gambier in Dominica, two plants native to the East Indies, as will have been noticed by readers of Mr. Ridley's notes on the mammals of Singapore, and now transplanted to the West through the medium of the botanists at Kew. The Durian is a paradox, revolting to the smell but delicious to the taste; a nutritious food, it never palls upon the appetite or injures the digestion. A good description of it is to be found in Wallace's "Malay Archipelago." The fruit grows on a large forest tree, is about the size and shape of a cocoa-nut, has a thick rind with short, stout spines, and its five carpels are filled with cream-coloured pulp. Its acclimatisation in Dominica will be an additional attraction to tourists. *Urucaria Gambir*, which also has been successfully introduced, possesses very valuable tanning properties, and is said to impart an unrivalled softness to leather. The propagation of the plant presents some difficulties; hitherto its cultivation has been "a wasteful one, in that it has formed a catch crop in the Chinese pepper gardens, until the pepper crop was matured." Its establishment at Dominica is expected to be of advantage to the island. We are glad to see, from these and other papers, that the energetic planters and merchants of the Leeward Islands are fully alive to the necessity for truly scientific work in these days of severe competition.

The new edition of the Molluscan portion of Bronn's "Klassen und Ordnungen des Thier-Reichs" progresses slowly but surely. Lieferung 15–17 has just reached us, containing the conclusion of the Amphineura, and the opening chapters on the Scaphopoda (pp. 337–400). It is accompanied by three plates illustrating the development of *Chiton* that are most beautifully executed. As an instance of the way in which the text of the work is brought well up-to-date may be cited the inclusion of Sowerby's new genus *Schizodentalium* with a reproduction of the figures, which appeared in the *Proceedings of the Malacological Society*, pt. iv., pl. xii.—a reproduction, by-the-bye, that is a decided improvement on the originals.

Brill, of Leiden, has published "A Bibliography of the Japanese Empire," by Fr. von Wenckstern. This professes to be, as its secondary title informs us, "A Classified List of all Books, Essays and Maps in European languages relating to Dai Nihon [Great Japan] published in Europe, America and in the East from 1859–93 A.D. [VIth year of Ansei—XXVIth of Meiji]." The book contains, in addition, a facsimile reprint of Léon Page's well-known "Bibliographie Japonaise depuis le xv^e^ siècle jusqu'a 1859." It is divided into sections, as: Travel, Religion, Philology, Belles Lettres, History, Law, Medicine, Education, Fine Arts, Ethnography, Natural History, etc., and is published at twenty-five shillings.

We learn from *Science Gossip* that the Quekett Microscopical Club has decided in future to issue its journal half-yearly instead of quarterly. The same journal informs us that the *British Naturalist* is dead. *Science Gossip* itself is to be raised in price from fourpence to sixpence a month, and sundry alterations are promised in its pages.

Messrs. Dulau & Co. have sent us a catalogue of works on General Zoology, which they offer for sale.

OBITUARY.

AMONG the deaths which we regret to record this month is that of DR. ROBERT ANSTRUTHER GOODSIR. Although at first trained as a banker, he afterwards followed the medical profession, in which one of his brothers so conspicuously succeeded. Dr. R. A. Goodsir went as surgeon to Penny's ship "Advice" on a search for Franklin, in whose expedition had sailed his brother Harry. His account of the voyage was a successful book of its time. In 1850 he again accompanied a search expedition for Sir John Franklin. He was a great traveller, but returned to Edinburgh about 1885, and settled down quietly. He died, at the age of 71, in the middle of January.

DR. HENRI CLERMOND LOMBARD, of Geneva, the famous climatologist, died at Geneva at the end of January, aged 92. He was born in that city in March, 1803. He studied at Edinburgh University and completed his education in Paris under Andral and Louis, graduating in 1827. His thesis was on Tubercles. In 1856 he published his "Climats de Montagne." His great work, "Traité de Climatologie," appeared between 1877 and 1880. In 1882 he presided over the International Congress of Hygiene at Geneva, and retained up to the time of his death full mental vigour.

DR. LEWIS R. GIBBES died on November 21 at Charleston. He was born in that city August 14, 1810. Educated for the medical profession, he speedily resigned it for that of mathematics, but resumed it again in 1836, when on a visit to Paris. His chief work in natural history was a series of papers on American Crustacea; but he was also well-known to astronomers, chemists, and botanists.

DR. HUGO CHRISTOPH died at St. Petersburg on November 5. He worked for many years on the Lepidoptera of Persia, Caucasia, Armenia, and the Amur Region. Dr. C. v. FELDER, the entomologist, of Vienna, died on November 30, aged 80 years. Mr. E. H. ACTON, M.A., Lecturer in Natural Science at St. John's College, Cambridge, died suddenly on February 15. The death of the MARQUIS OF SAPORTA is announced.

WITH very deep regret we learn, as we go to press, of the death of Mr. J. WHITAKER HULKE, President of the Royal College of Surgeons, which took place on February 19, from pneumonia following influenza.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

THE following appointments have recently been made: Mr. F. E. Allhusen, B.A. Cantab., as science-master at Charterhouse, to give instruction in Physics, Chemistry, and Geology; Mr. H. B. Pollard, of Christ Church, Oxford, known for his valuable researches on the morphology of the fish's head, to be Lecturer in Biology to Charing Cross Hospital Medical School, to take which post he will vacate a Berkeley Research Fellowship at Owens College; Dr. Gabriel von Perlaky to be assistant at the Botanical Institute of Budapest; Dr. Filippo Giovannini, to be Chief Conservator of the Royal Botanic Institute in Bologna; Mr. V. K. Chesnut, to be Assistant in the Botanical Section of the U.S. Department of Agriculture; Mr. W. W. Clendenin, professor of geology and mineralogy in the University of Louisiana, to be Geologist of the State. Mr. S. S. Buckman is lecturing on geology at the Agricultural College, Cirencester, pending the appointment of a permanent successor to Professor Allen Harker. The professor is expected to reside in college and to lecture on geology, botany, and zoology. Applications may be sent to the Principal.

THE Hayden Medal of the Academy of Natural Sciences of Philadelphia has been awarded to Professor G. A. Daubrée, the well-known author of "Géologie Expérimentale"; Professor Daubrée was born in 1814. The University of Cambridge has awarded the Sedgwick Prize in Geology to Mr. Henry Woods. The subject for 1898 is "The Glacial Deposits of East Anglia." Mr. Philip Lake has received a grant of £50 from the Worts Travelling Scholars' Fund; he will investigate the distribution of the trilobites in Russia and Sweden. Mr. Malcolm Laurie has been appointed to the Cambridge University table in the Zoological Station at Naples for three months. The Regia Lyncei Academia has conferred a medal and diploma of foreign membership on Professor James Hall, of Albany, N.Y.

IT is proposed to erect a memorial tablet in St. Ninian's Cathedral to the late Dr. F. Buchanan White. Subscriptions to this end should be sent to Dean Rorison, Perth; or to Henry Coates, Pitcullen House, Perth. Dr. White's "Flora of Perthshire," a work on which he had been engaged for some years, is left in an almost completed condition.

Mr. Brock, R.A., has finished his model of the statue of Sir Richard Owen, which is to be placed in the Natural History Museum. It is said by those familiar with Owen's features to be a perfect likeness. The model will be seen in this year's Academy. The subscription list is not yet full, and Mr. Percy Sladen, of the Linnean Society, Burlington House, will be glad to hear from those interested.

WE understand that there is a vacancy in the Museum at Singapore, for which a curator is required. The Museum has been in existence about eight years, and Mr. William Davison was the first curator. A collection of various specimens of natural history had been made by Mr. James Collins previously, and this formed the nucleus of the exhibitions in the museum. Mr. Davison died in 1893, and was succeeded by Dr. Haviland, who resigned the post in a year, since when a clerk has been employed to look after the buildings and their contents. There is a considerable collection of mammals, birds, reptiles, fishes, and insects, and also a large number of ethnological specimens and minerals. To the curator's duties is added the charge of the lending and reference library, which is in the same building. The salary is 3,600 dollars, with free passage to and from England, and a house.

THE thirteenth annual meeting of the American Society of Naturalists opened at Baltimore on December 27. The first discussion was held upon "Environment in its Influence upon the Successive Stages of Development and as a Cause of Variation," Professor Osborn leading, and being followed by Professors Cope and Hyatt. Professor Brooks read a paper on "An Intrinsic Error in the Theories of Galton and Weismann." Professor Cope was elected President for 1895. The retiring President, Dr. Minot, gave an address entitled "The Work of the Naturalist in the World."

THE Geological Society of America held its seventh annual meeting on December 27, 28, 29, at Baltimore, in the rooms of the Johns Hopkins University. Professor Chamberlin presided, and delivered an address on "Recent Glacial Studies in Greenland."

PROFESSOR EDMUND B. WILSON has been elected President for 1895 of the American Morphological Society. At a meeting recently held in Baltimore he read papers on "The 'Quadrille of the Centrosomes' in the Echinoderm Egg; a second contribution to Biological Morphology," and on the "Polarity of the Egg in *Toxopneustes*." Professor Hyatt, at the same meeting, announced his researches upon the "Parallelisms between the Ontogeny and Phylogeny of *Pecten*."

THE officers of the "Michigan Academy of Sciences," which was founded at Ann Arbor on June 27, 1894, are:—W. J. Beal, President; J. B. Steere, Vice-President; F. C. Newcombe, Secretary; W. B. Barrows, and I. C. Russel. It has been decided that the Academy shall have for its first object the study of the Natural History of the State of Michigan.

AT a meeting of the Zoological Society of London, on February 6, Dr. Holding exhibited a deer horn which had been gnawed by some animal. The general statement of keepers is that deer nibble the horns themselves, especially the hinds; but the appearance of the antler shown seemed to show that the gnawing had been the work of rodents. Dr. Gadow remarked that in Germany it was not unusual for portions of the exterior surface of the antler to be removed by slugs. In the *Journal of the Bombay Natural History Society* (ix., no. 2) the Assistant Conservator of Forests at Nagpur, Mr. P. H. Clutterbuck, writes to say in that district Sambuhr horns are gnawed by porcupines, and he has found such gnawed horns in the "run" of that animal. The natives say the porcupine gnaws such articles for food, but Mr. Clutterbuck thinks the purpose is more for sharpening the teeth.

THE changes in the house list of the Geological Society this year are as follows:—General McMahon and Mr. Hudleston become Vice-Presidents in the room of Dr. Hinde and Professor Judd; Mr. Blanford succeeds Professor Wiltshire as Treasurer. The Geologists' Association have lost Mr. Hudleston and Professor Blake as Vice-Presidents, their places being filled by Dr. Hinde and Mr. T. V. Holmes. The Association rejoices in a balance of £16 on their annual account.

Mr. Bayard, the United States Ambassador, was present at the Geological Society on February 15, and received the Bigsby Medal on behalf of Mr. C. D. Walcott, of Washington.

THE Liverpool Naturalists' Field Club held their Annual Meeting on January 25. Mr. G. H. Morton, the President, read an Address on the "Recent and Fossil Flora and Fauna of the country around Liverpool." This field-club was the first, or one of the first, started in this country, and by the end of its first year totalled 482 members (1861). Five years later the total membership was 725, while now the membership is under 300. The chief work of the Association all along has been done in Botany.

WE learn from the newspapers that the Mitchell Library of Glasgow has received a bequest of £500 from a man who passed as a tramp and who lived in a model lodging house at Vauxhall. The incident is so unusual that it is as well to chronicle it.

ON February 13 the Oxford University Junior Scientific Club held its 150th meeting. This club has now been in existence a little over twelve years, and its popularity shows no signs of decreasing. The editor of its *Journal* regrets, however, that the junior members contribute little to the discussions and less to the papers. The Oxford undergraduate still retains a wise dread of making a fool of himself. Among papers recently reported in the *Journal* are an interesting account of *Opisthocomus* by Mr. Pycraft, and a note on the use of certain Aniline Stains by Mr. R. A. Buddicom, which should prove of service to the histologist. Should this meet the eye of any old members of the Club, we hope it will induce them to subscribe to the *Journal* the small sum of 5s. per annum. Its editor is now A. W. Brown, Christ Church, Oxford.

THE third International Zoological Congress will be held at Leyden in September. Those intending to join should write to Dr. P. P. C. Hoek, Helder, Holland.

THE eleventh German Geographical Congress will be held in Bremen, April 17 to 19. Dr. W. Wolkenhauer, Laagenstrasse 44, Bremen, is the Secretary. The special subjects for consideration are:—"Polar Exploration, especially the state of the South Polar Question"; "Oceanography and Maritime Meteorology, as well as the development of Charts"; "Commercial Geography"; "Geography of the German North Sea Coast"; "School Geography."

THE American Museum of Natural History, New York, has purchased the unique collection of fossil mammalia made by Professor E. D. Cope, of Philadelphia. The Museum thus acquires a valuable series of type specimens, and increases the already important addition of fossil mammalia made to its collections since Professor H. F. Osborn assumed the direction of the Department of Vertebrate Palæontology.

THE Museum at Hastings, originally opened in August, 1892, has recently undergone some alterations, and was re-opened in January by Lord Brassey. The alterations have permitted the symmetrical grouping of the exhibits under two heads, the works of nature and the works of man. The authorities of this Museum are in full accord with the modern spirit, and endeavour to exemplify the history, the industries, the geology, the botany, and the zoology of their own district. Among the natural history exhibits may be noted collections of Wealden fossils, of bryozoa, and of hydroids, made and arranged by Mr. P. Rufford, whose fine collection of Wealden plants was recently acquired by the British Museum; a collection of invertebrata, mostly marine, presented by the Rev. J. W. Tottenham; good collections of birds and British lepidoptera. We learn from the Report that "For educational purposes it is proposed to start a special section in connection with a zoological laboratory, which has now become the usual supplement to a museum. A portion of the museum will be screened off for the use of teachers and pupils, and to be used for the display of specimens stored in chests of drawers." We have so often insisted on the need for greater co-operation between educational and museum authorities, and especially on the advantage to a museum of a properly appointed laboratory, that we are delighted to learn that such has become "the usual supplement to a museum"; hitherto we had been sadly unaware of that fact. At any rate the Hastings people are shining examples. The Secretary to the institution is Mr. W. V. Crake, Highlands, St. Leonards.

IN imitation of the Yorkshire naturalists, those of Lincolnshire have formed themselves into a union, which, if we may judge from the first volume of *Transactions*, kindly sent us by the organiser and late secretary of the union, Mr. Walter F. Baker, seems likely to do good work. It is not, we gladly read, the ambition of the Lincolnshire naturalists, "to record new species, or find new localities for species already recorded"; nor do they sympathise with the mere laboratory microscopist. They wish to unite the best work of the two classes. They want, as Professor Miall says in his admirable article, "*live* Natural History." The presidential address by Mr. John Cordeaux attempts to define the faunal areas of this large and diversified county, and gives useful hints for future work. Mr. F. M. Burton lists the chief writings on the geology of Lincolnshire, and urges the formation of a Boulder Committee to work in connection with that of the British Association. A vigorous attempt is being made to found a museum in Lincoln, a city which has allowed many famous treasures to elude its grasp for the want of such an institution. And already the union has done yeoman's service to British natural history, for we are told that an application has been made on its behalf to the Lindsey County Council to protect the coast area under the Act of 1894 relating to birds' eggs, and the Council have agreed to it unanimously. The area thus protected extends from the northern end of the Lincolnshire coast down to Boston; and if the County Council of Holland follow suit, the whole of the coast area of the county will be protected.

We congratulate the union on its start. *Macte virtute!*

ON Saturday, February 2, the Duke of Westminster opened the Chester Museum extension. In the course of his address he referred to the opening of museums on Sunday, and stated that he "had always considered Sunday as the only day on which many of his fellow-citizens could see such collections." Chester has always kept up its reputation as a centre for local scientific research, and the energies of Mr. Newstead are admirably directed towards the continuance of that useful work.

SPEAKING at the Birkbeck Institution, on the occasion of the distribution of prizes to successful students by the Duke and Duchess of Devonshire, on Wednesday, the 13th ult., Mr. Justice Bruce dealt at some length on the great utility of the British Museum as a means of spreading education of the highest possible order among the inhabitants, not only of the metropolis, but of the United Kingdom. He paid a splendid tribute to the officers of the Museum, who, he said, were always willing to impart the great stores of knowledge they possessed to those seeking it.

WE regret to learn that that valuable publication, the *Index Medicus*, is likely to come to an end for want of funds. To enable it to continue, it requires 500 new subscribers at £2 a year. Surely among medical men enough have attained to such prosperity as will enable them to support a work of such practical service to their profession.

Apropos, we learn from the *Revue Scientifique* that, in France, Mr. Marcel Baudouin, general secretary of the *Association de la Presse medicale française*, has founded a circulating library of medical literature, from which the local practitioner can borrow the works of which he may be in need in return for an annual subscription of 20 francs and a small sum of "caution money," varying with the value of the books. Mr. Baudouin has also organised a bibliographic service similar to those for the benefit of zoologists and botanists to which we have recently referred in our columns. References are grouped according to the various specialities of medical art. In addition to the mere references, Mr. Baudouin hopes to add soon a special service of abstracts and translations. We do not know if foreign subscribers are admitted; but if so, we should recommend English students and practitioners to avail themselves of this praiseworthy organisation.

We learn from the *Eastern Evening News* of January 26 that the old tower of Eccles Church on the Norfolk coast is no more. Famous to geologists from the writings of Lyell, it has borne witness to the encroachments of the sea along our eastern shores. Originally placed on the marsh inside the dunes, it was gradually overwhelmed by the advancing sand, so that when sketched by Lyell in 1839 it was in the midst of the sand-hills. As the sand drifted inland the tower was left standing on the foreshore, with the basement portions of the nave showing amid the sand and shingle of the beach. Year by year, notwithstanding the efforts to protect the coast by means of groynes, the tides have beaten more and more often against the church, so that nothing but the substantial architecture and the foundation of tough boulder-clay have enabled it to survive so long. During the storm of January 23 the sea dashed furiously against the tower, the spray of the breakers going at times over its summit. Eventually, the fabric was overthrown, splitting in two in its fall, and now all that is left of it are huge masses of masonry lying about in strange confusion.

Further details have been received of Lieutenant von Götzen's expedition across Africa from Pangani, opposite Zanzibar, to Banana at the mouth of the Congo. The journey occupied only a little over thirteen months, viz., from October, 1893, to December, 1894. The main new results were in the exploration of the group of mountains named Mfumbiro, to the south of Ruwenzori. This was first seen by Speke, but most of our knowledge of it is due to Stanley. Lieutenant von Götzen finds it to consist of a group of five peaks, one of which, Virungo, is an active volcano. He discovered a new lake which is of interest as the source of Ruzizi, a river well known in the old controversies as to the connection of the Tanganyika and the Nile. This river was claimed by Livingstone as an outlet from the lake, but Stanley demonstrated that it is really an inlet. Lieutenant von Götzen reached this district by ascending the Kagera river and then following the Lowa through the forest region to the Congo.

Globus informs us that an expedition has been sent by King Menelik of Abyssinia to the lakes Suai, Hogga, and Orrorecha. Lake Suai lies in a volcanic tract, and supports on its islands a population of about 1,000 Christians who had fled the persecutions of Mohammed Granye. Two streams feed the lake, the Maki from Guraghi, and the Katara from the Albaso plateau. The three lakes are connected together, but Orrorecha is salt; all three are said to drain into the Indian Ocean by the Wesa (Webi-Sidama).

Johannes, a German, succeeded in visiting Balball or Dalubi lake, at the western foot of Mt. Meru. It was found to be 1,260 yards N.N.E. S.S.W. long by 875 wide, is situate on a hill 200 feet high, and is enclosed by steep cliffs.

Mr. Donaldson Smith has been turned back by the Abyssinians in Gallaland and has had to partially retrace his steps. He hopes, however, finally to reach Lake Rudolph, though it is slow travelling with the camels in the hilly regions.

Dr. Sven Hedin writes to the Geographical Society from Kashgar, November 9, respecting his explorations between Mustagh-ata and Kashgar. He has constructed a topographical map, collected rock specimens, and made geological observations which will complete the researches of Bogdanovich. Hedin intended to start for Lob Nor in December; he will cross the Tarim Desert. He has decided not to attempt to get to Lhassa, but will deal especially with the geology of the Kuen-Lun range. Mrs. Bishop reached Pekin, after a journey from Seoul through Moukden, in October; a postscript dated November 24 shows that the traveller was at Vladivostock. Mrs. Bishop hopes to return to England in April.

CORRESPONDENCE.

"Trinomial Nomenclature of Plants."

(This volume, p. 6.)

As is so often the case with people who are anti-anything, your anti-trinomialist does not appear to have learnt what the system he attacks really is, but to have confused the shadow with the substance. Trinomialism is not, as he and other opponents seem to think, simply the putting of three names in a row, but is the system under which races, especially *geographical* races, thought to be of less than specific rank (not aberrations, individual variations, and such like), receive distinctive Latin names in addition to their specific ones. Whether "subsp." is or is not inserted between the second and third terms of the series is exactly of the same importance—*i.e.*, *nil*—as whether we write "*Homo sapiens*" or "Gen. *Homo* sp. *sapiens*" on the binomial system. The latter way of writing the name is just as much trinomialism as the former, and "*H. sapiens* subsp. *mongolicus*" is as much trinomialism as "*H. s. mongolicus*."

That when left out, the word understood should be "subspecies" goes without saying, and the vagaries of individuals who think of, and then leave out, any other word, or don't think at all, no more condemn trinomialism itself than those of wild and careless "species" makers do the binomialism under which we all work.

O. T.

The Rosy Feather-Star.

Strictly speaking, it was, no doubt, inaccurate to use the phrase "floating colonies" with reference to this animal. That phrase was used to bring out the facts of its gregarious habit and its power of flotation or movement from place to place. These facts are presumably unquestioned by "Paddy from Cork," so that the creature still seems an adequate disproof of Mr. Drummond's dictum, to wit, "The mere fact that animals cling to one another, live together, move about together, proves that they communicate." Jesting "Paddy" may now answer his own question, if he think it worth while; but, an he will stay, he may learn of another mode of occurrence in addition to the three that he has noted himself, and apparently imagines to exhaust the possibilities. Dr. J. Gwyn Jeffreys, a dredger of some experience, wrote in the *Geological and Natural History Repertory* for 1866 (p. 306), "I have myself seen a number of *Antedon Celticus* clinging to the rope several feet from the dredge when it was taken up from about 60 fathoms . . . no part of the rope lay on the ground." Is it not probable that these numerous Feather-stars were floating at a few feet from the bottom, and that they grasped hold of the rope as it came in contact with them?

F. A. Bather.

The Bird's Foot.—A Correction.

In Natural Science for September, 1894 (vol. v., p. 208), I had the misfortune to state that the deep plantars of the Trochilidæ were schizopelmous, as in Passeres. This is incorrect, for there is a short branch connecting the *flexor longus hallucis* with that branch of the *flexor perforans digitorum* which runs to the second digit. No one regrets this blunder more than myself; it probably arose from my severing the minute, anastomosing branch, in clearing away the connective tissue where the deep plantars cross.

Washington, D.C., U.S.A. Frederic A. Lucas.

January 15, 1895.

Anlagen, Rudiments, and Blasts.

Dr. Herbert Hurst, even though he adduces learnedly the Latin derivation, does nothing to clear the issues. To include Darwin among "ignorant blunderers" may be a triumph of scholarship; it does not affect the fact that for all future time he has impressed upon the English word "rudiment" the general idea of incompleteness to the exclusion of distinction between incompleteness that is vestigial and incompleteness that is incipient. *Litera scripta manet*, at least when Darwin is the writer. Personally I think it a convenience that we should now have a word expressing incompleteness without prejudice to the nature of the incompleteness. The word "vestige" admirably expresses reduced or degenerate completeness. There is wanted a word to express incipient completeness, especially in the familiar case of embryonic incompleteness. Whether or no my suggestion "blast," with its obvious reference to the mesoblasts and the epiblasts, will serve, remains to be seen. I wrote my article "Anlagen" to invite comment on the point; and I am indebted to Dr. Hurst for his criticism, although it seems to me barren.

P. Chalmers Mitchell.

The Use and Abuse of Names.

The question which I raised in your January number is too important to be buried under a rhetorical blanket. The point which I made was a very simple one. In the year of grace 1894 two distinguished naturalists, to both of whom science is under deep obligations, describe *the same skins preserved in the same museum*, and give more than sixty per cent. of them different names.

My contention is that, inasmuch as names are meant to assist men in arranging and finding their knowledge, this can only be justified on the ground that it is wise to have as many indices to knowledge and as many plans of naming and arranging facts as there are naturalists.

I am not so unreasonable as to suggest that when forms increase, especially when the increase is rapid, we must not modify our nomenclature; but in the case I mention there was no increase of forms. The very same skins in the very same museum are given different names in the very same year. If a roll call of the lizards is made next year in the Limbo to which lizards go, no lizard will know what his name is, and there may be grave confusion in that tropical region where salamanders ought to be happy. It will be much better to read out "a mere list of numbers"—a solution which startles you—than having to search for the personality of "the old serpent" in a dozen scientific lists. The cause of our trouble is very largely due to the factitious and quite absurd credit which was supposed to attach, until a short time ago, to the man who was not merely lucky enough to give us some new facts, but wicked enough to introduce a new name, especially a new generic name, without a real necessity, and from some wanton craving for notoriety.

This absurdity is now largely obsolete, and, instead of a wreath of olive, a scorpion is the prize which most sensible people would award to the man who coins an unnecessary name, and loads our groaning memories with additional burdens.

In your remarks you distinguish between a privately-issued book and a book published by a museum. I can make no such distinction. When a book is published it is no longer a private matter, and the man who, either from deliberate motives, from ignorance, or from carelessness, adds books to the world which distort or confuse our knowledge instead of illuminating it, is a scientific criminal.

You add that it would be intolerable if in a catalogue published in a museum or in a museum collection such discrepancies of nomenclature as I have mentioned were to occur. Don't they occur? I know one very large museum (the best and the best-arranged museum altogether in the world) in which there are some very curious examples of labelling the same creatures with different names. This is the case both among fossil and among recent specimens, but it becomes rather epidemic when fossil and recent specimens are examined together. This, however, involves another issue to which I should like to call attention in your pages some time, namely, the mischief of separating fossil and recent forms into two entirely different categories under different care and custody, and named and arranged by men differently trained and having a different scheme of nomenclature. This issue I will, however, reserve, merely stating here what is a perfectly well-known fact, namely, that a number of fossil so-called species are as much entitled to the distinction as are the mummified Pharaohs of ancient Egypt. A mummy, no doubt, looks very different to a "nice fresh corpse," as a humorist once remarked, and a periwinkle in Clare Market looks different to one from the Crag, but it does not follow that in either case we should have a different name for the two forms. When a sweep is washed he may still be a sweep, and it is best to call him so.

HENRY H. HOWORTH.

P.S.—I am pleased to have just received a letter from Señor Herrera, of the National Museum in Mexico, ardently supporting the position I have taken.

H. H. H.

ERRATA.

THIS volume, p. 142, line 4: for "Watken," read "Wathen."

Page 139: Mr. J. E. Duerden was never Assistant in the Museum of Science and Art at Dublin, but Demonstrator in Biology and Palæontology at the Royal College of Science there.

Page 76: Mr. G. F. Harris desires us to state that the comparison of a silicious sphere from the Yellowstone to *Parkeria* was not made by him, as might be inferred from our editorial note.

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NOTES AND COMMENTS.

Discussions at the Royal Society.

THE experiment, now in progress at the Royal Society, of having occasional meetings for the discussion of questions of great interest naturally is receiving a considerable amount of attention from all scientific men. Whatever be the value of the new method, it is certain that the invitations addressed to those who are not fellows, but are interested in the subjects of the meetings, have been fully appreciated. No doubt it has always been possible for a stranger to obtain admission to a meeting by the invitation of a fellow, but it is not always possible to come across a fellow at the convenient moment, and the more general invitation brings more together. But, so far as the utility of the method of discussion goes, many who were present at the discussion on variation must have agreed with what Professor Ray Lankester said. Unless some very definite point is submitted, unless, so to say, some definite resolution is put to the meeting, little practical advantage seems to be gained. A discussion in a periodical allows clear issues to be raised; those who join in it have opportunity to think over what their predecessors have said, and to express their deliberate opinions. Moreover, there is the great advantage that when the discussion is at an end one may read it over from the beginning, and form a definite and coherent judgment on the whole question. In the spoken discussion there is always a complete want of continuity, each speaker raising practically new issues.

Variation and Probability.

Professor Weldon's report, given on behalf of the Royal Society's Committee "for conducting statistical inquiries into the measurable characteristics of plants and animals," has not yet been published; but, as it formed the subject of the discussion which he introduced at the meeting referred to in our last paragraph, it is already a subject for comment. It may be taken for granted that most biologists agree that variations of a nature similar to those now occurring among plants and animals have been the source of the differences between the varieties, species, genera, and so forth in which existing animals and plants are classified. Now there are two great classes of variations. There are large and striking variations, sometimes called sports, or monstrosities when they are more extreme and possibly detrimental to the organisms in question. Then there are numerous, almost innumerable, small variations that occur in every part of the structure and in every physiological quality of the animal. Have species come from the large or from the small variations?

Mr. Bateson, in his "Materials for the Study of Variation," called the former set instances of discontinuous variation, the latter set instances of continuous variation. He, as many others before him, suggested that, as species generally are discontinuous, it is more than possible that discontinuous variations have been the chief source of varieties and of species. The chief difficulty in the other view is that it is impossible to see in many cases, perhaps in any case, how small variations could have a value in selection, how, in fact, the gradual increments in a continuous change could each have had a determining value in the selection of the animal or plant.

The Royal Society's committee, or rather Professor Weldon for it, made a statistical inquiry into the variation of certain dimensions in the shore-crab. In the case of one of the dimensions investigated, the "frontal breadth" of the crab, it was found that among seven thousand females practically all possible variations occurred, in the proportions suggested by probability. Precisely the same was the case when the variations in the dimension of the "right dentary margin" were appropriately reduced to standard and tabulated. In both cases, as Professor Weldon's curves showed to a remarkable and convincing degree, the number of individuals in which each variation occurred was such a number as mathematical probability would suggest. In other words, the occurrence of each variation is as much the result of "chance" as the particular combination that is made when a dozen dice are thrown on the table. Any single throw is "chance"; but if a large enough number of throws be made, each combination turns up the number of times that the mathematical possibility determines. Although Professor Weldon did not say so, it must have occurred to many listeners that this first result of statistical inquiry on variation was in direct contradiction to those who assert that variation is not a matter of "chance," but that it occurs in determined directions.

ARE SMALL VARIATIONS ELIMINATED?

WE have explained that Professor Weldon found that the variations in the dimensions of the frontal length and of the right dentary margin in the case of over 7,000 specimens of female shore-crabs followed probability. His next step was to sort his specimens into groups of approximately the same age, and to investigate the occurrence of the variations at these ages. In both dimensions measured, he found that there was a period of growth at which the frequency of deviations increased. In fact, he had practical illustration of Darwin's statement that variations frequently appear late in life. But there was an important difference between the two cases. The preliminary increase in deviations was followed, in the case of the frontal breadths, by a decrease in deviations of a particular magnitude. In the case of the right dentary margins, no decrease followed in later life. Assuming that abnormal young crabs tend to grow up into abnormal older crabs, that, in fact, abnormalities do not tend to correct themselves during growth, it is clear from the data that there must be a special death-rate among crabs which have a certain deviation from the normal in the case of the frontal breadth, and that there is no special death-rate among crabs with deviations of the right dentary margin. Experiments are in progress to test whether or no the assumption that these abnormalities do not correct themselves during growth be true. But, upon the assumption, it appears that there is a selective destruction among crabs with particular deviations from the normal frontal breadth. Professor Weldon estimates this death-rate as amounting to seven per cent.

It is to be noticed that the results obtained by this statistical method recall other facts in very important fashion. They were obtained from specimens of the shore-crab taken in Plymouth Sound. In a communication made by Professor Weldon to the Royal Society more than a year ago, he showed that among crabs collected at Naples a marked dimorphism existed in this matter of frontal breadth, and that variations of certain other organs were associated with dimorphism of the frontal breadth. Moreover, frontal breadth is a character which for long has been taken by zoologists as of systematic value. The importance of these results seems to us more than a reward of the enormous labour by which they were obtained.

THE STATISTICAL METHOD IN VARIATION.

PROFESSOR WELDON'S results have already established the importance of his method, and we cannot doubt but that wherever the method be applied with equal diligence and discrimination, equally important results will be attained. But we cannot agree that the questions of species and of changes in them are to be dealt with without taking function into account. It is clear that statistical inquiries should be conducted without hypotheses as to the function of the organs involved. But when a result like the present result has been obtained, it seems to us that the inquiry has only been begun; that Professor Weldon's method concerns itself merely with the fact that in certain cases there is a *relation* between selective destruction and certain specified dimensions. The question as to whether such small dimensions have, or do not have, a selection-value for the animal remains to be answered. Having got so far, it remains for the statistical method to investigate all the correlations that exist between variations of frontal breadth and variations of other organs and parts. When the correlations are known, then it will be the part of another method, an experimental method dealing with function, to determine upon which, if upon any, of the correlated variations selection acts. The minute deviations of frontal breadth may be associated with functional differences of a much larger order, as the minute movements of the index of a pressure-gauge are associated with enormous and potent changes of pressure. Professor Agassiz suggested that deviations in the adult might only be the record of selective destruction operating upon earlier larval stages. In this case there seems little probability of such a view being applicable, for the destructive selection did not set in until late in life, and only after an increase in deviations had been recorded. But, to our mind, nothing is more certain than that the corollary of Professor Weldon's method is a detailed study of the correlations that exist between this deviation that he has shown to have a relation to the death-rate, and an experimental investigation into each associated change, until there has been found the functional variation upon which the selective destruction actually operates. Pending such inquiry, Professor Weldon may be taken to have shown that there is a relation between selection and minute variations, not that selection operates upon minute variations.

Variations and Mutations.

In a valuable paper, contributed to the *American Journal of Science* for November, 1894, W. B. Scott discusses the question of variation. Like many others, he considers that Mr. Bateson's book on variation would have been much more useful, and its suggestion very different, had Mr. Bateson made use of the facts accumulated by palæontologists. The great point made by Professor Scott is the clear distinction between individual and phylogenetic variation. "Remembering that the significant fact in the history of a group is not so much the character of its variations at any one stage, as the gradually shifting positions successively occupied by the normal or centre of stability, we find that any mammalian series at all complete, such as that of the horses, is remarkably continuous, and that the progress of discovery is steadily filling up what few gaps remain. So closely do the successive stages follow upon one another that it is sometimes extremely difficult to arrange them all in order and to distinguish clearly those members which belong in the main line of descent, and those which represent incipient side branches. Some phylogenies actually suffer from an embarrassment of riches."

Continuity in a series of descent is not peculiar to mammals. Waagen, years ago, found it in Ammonites; other observers in other groups, and we agree with Professor Scott when he says that it occurs "wherever the phyla can be worked out in detail."

Similarly, Professor Scott finds a great contrast between the results obtained by Mr. Bateson from his study of variations in meristic series, and the changes in meristic series as recorded in palæontology. For the details of the evidence we must refer readers to Professor Scott's paper, as indeed the whole object of this note is to call attention to the paper. It is shown from a consideration of teeth and of digits that there is a marked discrepancy between individual and phyletic variation. In the latter there is a marked individuality in the elements of the series, an individuality retained all through the successive changes. However a single member of the series may be modified, it can still be identified with certainty. Moreover, new members are not introduced irregularly and casually. The series show a gradual and steady reduction in number, and there is a definite correspondence, though not always identity, in the reductions of parallel series.

Professor Scott suggests that the difficulties of such discordance in the evidence drawn from phylogeny and from individual abnormalities may lie in the assumption that individual variations are, or may be, incipient species. Following Waagen and Neumayr, he distinguishes these individual abnormalities from the slow modifications recorded in palæontology, and calls the former *variations*, the latter, *mutations*. "It may," he says, "be the outcome of future investigation that, while variations are due to the union of changing hereditary tendencies, mutations are the effect of dynamical agencies acting long in a uniform way and the results controlled by Natural Selection."

The Classification of Birds.

Perhaps no group in the animal kingdom offers more difficulties and more attractions to the biologist who is interested in the problem of species. The structure of all birds is very closely alike. Those accustomed to the anatomy of most other groups would be astonished at the apparently small differences in structure upon which the taxonomist is compelled to rely. The number of species and genera is enormous, the amount of work done upon the group by field naturalists, museum naturalists, and by anatomists is monumental. All classifications are profoundly unsatisfactory even to their inventors.

Mr. Henry Seebohm has recently issued a pamphlet dealing with the classification of birds and supplementary to his previously published work on that subject. In this place we cannot follow him into the details of his scheme. The interesting nature of his work will certainly insure attention from many naturalists, and those who know about birds will object and criticise abundantly.

For, as Mr. Seebohm admits, all the classifications of birds are as yet largely matters of opinion. "Every attempt," he writes, "to discover an ideal classification has hitherto been an absolute failure; and it can almost be proved that it involves a mathematical impossibility, unless we admit that a primitive character may be independently modified in a special direction by two distantly related families under the influence of similar causes, and also that the influence of other causes may afterwards produce a reversion to the primitive character in some of the descendants. This admission is almost tantamount to a confession that the attempt to arrive at finality in the classification of birds is hopeless."

It appears as if there were here some opening for the doctrine of positions of organic stability recently expounded by Mr. Bateson. Upon this theory, the characters of an animal, or some of them, are bound together in some kind of inevitable correlation. If a change occur, it is a sudden change to an adjacent position of organic stability, and in the new position there is a kaleidoscopic change in all the associated characters. Mr. Bateson apparently believes that these adjacent positions of stability are so fixed that the same species in two localities might give rise to the same other species; that is to say, at two places separated in space from each other some of the members of a species might shift to the same adjacent position of organic stability under the stimulus of the same external agencies. In a group of animals so closely allied as birds it would not be surprising to find, if the theory be true, that the same set of characters appeared and reappeared more than once. But to prove or disprove this, or indeed to proceed further with the unsatisfactory classification of birds, still more anatomical knowledge is wanted.

Our own opinion is that in this case, as in the case of Professor Weldon's crabs, the study of correlations must be proceeded with before any great advance in biological theory will be gained. The animal body is in the first place a physiological unit, and investigations into the variations of parts of it, whether these be the variations that occur among individuals or the variations that occur among species, cannot explain themselves until we know the associated correlative changes. No doubt, if we consider the enormous complexity that there is in any of the higher organisms, the study of all the correlations may seem almost a hopeless task. But if it be hopeless, we believe that the problem of species will prove hopeless with it; and until the problem of correlation has been studied more fully, it were idle to talk of hope or of the want of it.

Artificial Production of Varieties of Bacteria.

The power of reproduction by forming spores is one of the first qualities of bacteria to rise in the mind. The habit of the filaments, to break up into spores when conditions are unfavourable to growth, the resisting powers of spores and the ease with which they spread from place to place, would seem properties inevitable in bacteria. The power of spore-formation, however fundamental we may have thought it, apparently can be altered by environment at the will of the experimenter.

In the December number of the *Annales de l'Institut Pasteur*, MM. Surmont and Arnould publish the results of some very striking investigations upon the bacillus of anthrax. They set out from the remarkable but somewhat neglected experiments of Chamberland and Roux, who, in 1883, succeeded in creating a race of bacteria that had definitely lost the power of spore-formation. A number of independent investigators from time to time have obtained similar results, but their methods were so varied and their results so uncertain that the present authors resolved definitely to investigate the whole question. They tried a number of different methods, but came to the conclusion that the use of carbolic acid was the most successful. The bacilli of anthrax cultivated in veal-broth which contained a trace of carbolic, rapidly turned into a variety devoid of the power of spore-formation. The filaments remained unbroken, or, brought into conditions usually favourable to spore-formation, simply decayed. But there is a remarkable difference between colonies of the bacilli from different sources. Some from a very virulent source continued sporogenic after the carbolic treatment; other and less virulent colonies rapidly lost the power of spore-formation. Extended experiments showed, however, that there was no direct connection between the degree of attenuation and the susceptibility to the influence of the carbolic acid. Their conclusion was no more definite than that "certain races of *Bacillus anthracis* are very difficult to transform into asporogenic forms." But the interesting result was obtained that such obdurate races yielded after they had been cultivated for some time at a temperature of 42°. Taking an obdurate race, they grew from it a long series of cultures at that temperature, and the race produced at the end of the series rapidly became asporogenic under the carbolic treatment.

To the specialist in Bacteriology these experiments will have peculiar value, and as the spore-producing power is a chief danger in the bacteria of disease, as it is the chief agency by which the diseases survive and spread, the transformation of bacteria into sterile races may have an important bearing on public health. But to the biologist the experiments come with a wider meaning. What we want just now is exact investigation into the limits and the extent of plasticity in living things. How far do surrounding things mould a living organism? Here is a case where the method of reproduction is at the mercy of accidental chemicals. Here is a case where a change in the environment suppresses what nine out of ten biologists would have called a fundamental character of the organism.

It will occur, however, to most botanists that, striking as it is, this tampering with the reproductive habit of an organism is by no means novel. The yeast plant, for instance, and many of the higher fungi, reproduce by spores only under definite conditions within the power of an experimenter. It is familiar to all that the sexual ripeness of flowering plants can be produced or suppressed by appropriate environment. Even when the organs have been produced and fertilisation has taken place, other definite conditions are necessary for the production of seed. Moreover, in the special case to which we have been referring, it would require more proof than Messrs. Surmont and Arnould have supplied to satisfy us that a permanent, and not merely a temporary, condition of asporogeny had been produced. In its present form, however, their experiment is a most valuable addition to our knowledge of the plasticity of organisms.

Animals in Sterilised Air.

In these days it is chiefly the evil effects of bacteria upon organisms that are studied. Every week some new microbe of disease is discovered, or some old enemies are described as lurking in some unsuspected place. But there is another side to the microbe question. It has been known for long that many kinds of bacteria, normally present in the intestine, aid in the digestion of food, chiefly acting as ferments, altering food-material into substances that can be absorbed by the cells of the intestine. Dr. J. Kijanizin, of the University of Kieff, gives, in a recent number of the *Archives de Biologie* (vol. xiii., p. 339), the remarkable results of a series of investigations he has made upon the influence of sterilised air. He devised an apparatus in which small animals could be kept for a number of days, while the air that they breathed and the food that they ate were supplied, so far as possible, in an absolutely sterilised condition. Although it was not possible to be certain that the food contained no bacteria, it was certain that the air supplied them had been quite freed from microbes; for a gelatine plate, placed in the current, remained without colonies all through the experiments. The animals were weighed before and after the experiments, and their excreta during the experiment were analysed. Parallel experiments, in which all the conditions but the sterilisation were identical, were made.

The experiments seemed to show first that there was a remarkable decrease in the assimilation of nitrogenous matter when the air and the food were deprived of micro-organisms. No doubt, the reason of the decrease was that these micro-organisms aid in the decomposition and peptonising of the nitrogenous matter in the intestine. Were it possible to remove all the micro-organisms from the intestine before the beginning of the experiment, the author thinks that the decrease in the assimilation of nitrogen would be still greater.

A second result was that the animals lost weight more quickly under the sterilised conditions than under normal conditions, while, at the same time, the excretion of nitrogen and of carbonic acid was more than usual.

A third result was still more remarkable. In a large number of the experiments the animals died, sometimes a few minutes, more often a few hours or a few days, after the beginning of the experiment. No cause could be assigned for this. The possible causes were all excluded, and the inexplicable fact remained. The novelty of the idea that sterilisation of the air is fatal to life no doubt is attractive; but we agree with the writer of the paper, that even his careful and laborious experiments are not sufficient to justify the belief that microbes in the air are necessary to the life of air-breathing animals. Physiologists will remember the experiments of Dr. Haldane at Oxford, which showed that animals were not poisoned by their own organic exhalations. It is clear enough that a very large amount of work must be done before respiration is understood.

Geographical Distribution and Temperature.

In an address published in the *American Nat. Geog. Mag.* (vol. vi., 1894), Dr. Hart Merriam discusses temperature as a barrier to distribution of animals and plants. In the northern hemisphere, animals and plants are distributed in circumpolar zones which follow isothermal lines rather than parallels of latitude. The main divisions are known as the boreal, austral, and tropical belts; and each of these is broken up into minor zones. Thus, in North America, the boreal belt is divided into the Arctic, Hudsonian, and Canadian zones; the austral belt into the transition, upper austral, and lower austral zones. But the isotherms are computed for arbitrary periods, for months, seasons, and years, and so cannot be applied directly to fix the boundaries of the regions.

Dr. Merriam suggests that it is not the temperature of the whole year, but the temperature of the period of growth and reproductive activity, that determines the distribution of terrestrial animals and plants. In the tropics this period extends nearly throughout the year, and hence, in the tropics, there is a close agreement between the mean annual temperatures and the zones of life. Within the Arctic circle, and on the summits of high mountains, the reproductive and growing period lasts only about two months: hence in these regions there is a wide discrepancy between mean annual temperatures and the zones of life.

Starting with the generally-accepted idea that a temperature of 6 degrees centigrade is the minimum that marks the inception of physiological activity in plants and of reproductive activity in animals, Dr. Merriam added together the effective temperatures or degrees of normal mean daily heat in excess of the minimum at each station, beginning from the time in spring when the normal mean daily temperature rises above the minimum, and ending at the time in autumn or winter when the normal daily mean falls again to the minimum. By means of such data he plotted out zones on the map bounded by isotherms, and found that they corresponded in a "most gratifying manner to the northern boundaries of the several life-zones." "While the available data are not so numerous as might be desired," he says, "the stations in many instances being too far apart, still enough are at hand to justify the belief that *animals and plants are restricted in northward distribution by the total quantity of heat during the season of growth and reproduction*." Dr. Merriam's paper is illustrated by maps, and is well worthy of attention from all biologists.

Mr. Scott Elliot's African Journey.

A special interest attached to the Linnean Society's meeting on the evening of March 7 in the presence of Mr. G. F. Scott Elliot, who had returned only the day before from a two years' journey of exploration in tropical Africa. Mr. Elliot has suffered severely from fever during the last five months of his trip, but the voyage home seemed to have restored him, as he appeared in excellent health. From the few words in which the traveller described his journey and some of its most striking impressions, it is evident that the work of the past two years will yield valuable results, not only to science, but also in the way of opening up the country to civilisation and commerce. The route followed was from the east coast at Mombasa to Uganda and the Nyanza, whence, owing to political disturbances, a much desired trip to Mt. Elgon proved impracticable. Turning south, Mr. Elliot followed the Kagera river some distance up from the lake, and found it to be navigable. On Mt. Ruwenzori four months were spent in exploration and collecting. The working-out of the collections promises much information, not only on the flora and fauna of the mountain in particular, but on African problems of geographical distribution in general. Only one specimen was before the Society, a new giant Lobelia rivalling those already known from Kenya, Kilima-njaro, and the Cameroons. Leaving Ruwenzori last August, Mr. Elliot made his way southwards, after a perilous journey, to the head of Lake Tanganyika, down this lake, and then down Lake Nyasa to Blantyre. A trip to Mt. Milanji was rendered futile by a serious attack of fever, which suggested to the explorer a speedy return to England, *viâ* Chinde and Zanzibar.

West Indian Liverworts and African Plants.

The recent publications of the Linnean Society contain contributions of some importance in the field of Systematic Botany.

Those interested in liverworts will find a valuable addition to the literature of the group in an account of the Hepaticæ collected by Mr. W. R. Elliott in the British West Indies in 1891 and 1892. A sad interest attaches to this paper from its being the last piece of work on which the veteran botanist and collector, Richard Spruce, was engaged. As stated in the short introduction, the work was interrupted by the death of the worker, and the task of arranging, amplifying, and generally editing the mass of notes fell to Mr. Gepp of the British Museum. There is ample internal evidence that not a little of the credit is due to the editor, by whom also, if we remember rightly, the paper was communicated to the Society. A number of new species are described, many of which are illustrated in the eleven clearly-drawn plates.

The remainder of this issue of the *Journal* (vol. xxx., pp. 331–435) is devoted to a contribution to the Flora of Eastern Tropical Africa, in which Mr. A. B. Rendle deals with some of the plants recently brought home by Dr. J. W. Gregory, and also a collection made by the Rev. W. E. Taylor of the Church Missionary Society. Dr. Gregory, as our readers are already aware, explored the district between the coast in the latitude of Mombasa and Mount Kenya, including the Laikipia plateau, the country south of Kenya, the valley in which lie Lakes Naivasha and Baringo, and the Taita and East Ongalea Mountains. From the terminal moraine of sheet glaciation on Mount Kenya he has brought a new orchid belonging to the genus *Disa*, which finds its greatest development at the Cape, but frequents also the mountains of Tropical Africa as far north as Abyssinia, From the same locality comes a *Gladiolus* hitherto known only from the other great eastern equatorial mountain, Kilima-njaro. Associated with these, or rather on somewhat lower slopes, it is interesting to find one of our native rushes (*Juncus effusus*), and a variety of a British Woodrush (*Luzula spicata*).

Another homely plant, the water plantain (*Alisma Plantago*) was gathered at a height of 6,720 feet on the Laikipia plateau, and Dr. Gregory need not have gone to Losuguta, in the Baringo Valley, to find *Typha angustifolia*. With these temperate types are several new species of a purely tropical character, including orchids, liliaceous plants, and other petaloid monocotyledons, as well as two little aquatic plants, both novelties, and belonging to the tropical genus *Lagarosiphon*. Unfortunately, water-plants are generally neglected by travellers, though there is no doubt that an examination of the lakes and ponds would bring to light many treasures of value, either as additions to science or from the point of view of geographical distribution. To Mr. Taylor we are indebted for some novelties from Mount Kilima-njaro, which he explored up to 10,000 feet. On the higher slopes he found several new orchids, including another *Disa*, and a tiny *Disperis* only three inches high, its thread-like stem ending in a single flower. Another gem from a similar elevation is a little iridaceous plant, a *Romulea*, five inches or more in height, which finds its nearest ally far south in the mountains of Namaqualand. A fine *Gladiolus* (rightly named *splendidus*) nearly four feet high, with a fifteen-inch spike of eight splendid crimson flowers, is another good find in these high altitudes. Mr. Taylor has also sent specimens from the low coast ranges, the Rabai and Giryama and Shimba Hills, near Mombasa and from the district between the coast at Zanzibar and Uyui, near Tabora, in Unyamwezi Country. Of special interest is a new screw-pine from the Rabai Hills, the fruits of which resemble those of a well-known Madagascar species, while in its male flowers it is totally different from any of its neighbours, and resembles most nearly a species from New Caledonia. We hope that Mr. Taylor's duties as a missionary will not prevent him from making further botanical explorations with results as valuable as those to which we have just referred. We remember with gratitude what is owed to another missionary, the Rev. R. Baron, in connection with the botanical and geological exploration of Madagascar.

The Function of Stomata.

Everyone knows that the leaves of plants are furnished with a number of little mouths with moveable lips, the so-called stomata. In some plants these occur on both surfaces of the leaves, usually being more abundant on the under surfaces, but occasionally being more abundant on the upper surface. In some cases they are altogether absent on one surface. There is dispute as to their exact function. It would seem natural to suppose that they are the chief means by which the gases of the air enter the plant, and it is certain that through them water-vapour leaves the plant. Recently, the most usual opinion among botanists has been that carbonic acid is taken in through the cuticle all over the surface of the leaf, not by the stomata. Mr. F. F. Blackman has communicated to the Royal Society (*Proceedings*, no. 342, Feb., 1895) the results of some experimental investigations he has made upon these points. By means of a new process for estimating small quantities of carbonic acid—a process which he describes briefly—he has succeeded in making more exact observations than hitherto have been possible. He finds that under normal conditions the stomata afford the sole pathway for carbonic acid into or out of the leaf. Under abnormal conditions, when the stomata or the intercellular spaces with which they communicate are blocked, passage of the gas may occur through the cuticle by osmosis. He also found that Garreau's well-known experiment, in which there occurs exhalation of carbonic acid from a leafy shoot in bright light, is due only to the imperfection of the conditions, to the presence of immature parts or of tissues not fully illuminated. "Mature isolated green leaves fully illuminated assimilate the whole of their respiratory carbonic acid, and allow none to escape from them."

Formol as a Preserving Fluid.

Every naturalist and every museum curator must necessarily be interested in every new preserving fluid that is suggested. In the January number of the *American Naturalist*, Mr. F. C. Kenyon describes Professor T. Blum's employment of formol, the usual name of a forty per cent. solution of formaldehyde in water. It is a clear opalescent fluid with a sharp odour. It has the advantage of being cheaper than alcohol, of having a more penetrating action, and, in many cases, of preserving colours and microscopical details. Whole mammals, reptiles, and fishes brought into a ten per cent. solution of the fluid were hardened, and after three-quarters of a year were entirely unchanged, although the fluid had never been changed. They became very transparent, and the mucous matter, instead of becoming stringy and opaque as with alcohol, remained quite transparent. For histological work Mr. Kenyon recommends very strongly a five per cent. solution of formaldehyde in fifty per cent. alcohol. The tissues so treated showed no trace of shrinking, required a very short time for hardening, and took the common histological stains very readily. He makes the following general conclusion:—"In conclusion it may be said that for general purposes solutions of at least more than two per cent. must be used in order to avoid the swelling and de-colouration of specimens, and that from four to eight per cent. will give the best result. For histological purposes formalin combined with alcohol will give better results than either used alone."

Do Adders Swallow their Young?

This perennial question has turned up again. It has been raging in the *Field* for weeks, and the *Saturday Review* has a scholarly article showing that in Spenser's time this curious maternal solicitude was believed in. It is very unfortunate that, if the habit exist, no adder which has swallowed its young has fallen into the hands of a naturalist enterprising enough to hand over the specimen to a central place of exhibition, like, for instance, the British Museum. Dr. George Harley, F.R.S., seems to have made the nearest approach to proving the supposed fact. Fishing in Scotland, he came on some children in the state of horror and excitement familiar in all the tales. If all countrymen know that the adder swallows its young, why are they so invariably astonished when, in the narrations, they have come upon an adder performing so harmless a feat? But to return to Dr. George Harley. Like a wise man, he impounded the adder, and placed it in his fishing-basket. But with a perverse curiosity, he opened the adder with a pocket-knife on his homeward road, and, having satisfied himself that the young were inside, not in the oviduct or in the stomach, but in an outgrowth from the lung, apparently he had no more use for the adder. The specimen, no doubt, was left to rot in a Scotch lane like any London rat thrown into the gutter. We could have spared a rarer serpent.

We freely admit that Dr. Harley's letter and the well-informed article in the *Saturday Review* have shaken our scepticism. None the less, the guile of the serpent is known to us all; it has deceived persons even more known to history than Dr. George Harley, F.R.S. You will observe that he preferred his trout to his adder; the serpent did not tempt him and he ate. However, it was in good company that he fell, and before his fall he was able to satisfy himself that if the young are swallowed they get into a plausible part of the adder's anatomy. We hope that before long, if the tale be true, someone will establish it for all time. If anyone comes across an adder that has performed this feat let him bottle it at once; let him resist all temptation, in whatever form the temptation comes, and forthwith send the adder in a bottle of spirit to Sir William Flower at the British Museum, to Professor Stewart at the College of Surgeons' Museum, or, an he will, to the Editor of NATURAL SCIENCE; he shall have due credit and the thanks of everyone.

THE NEW GIRAFFE.

THE Zoological Society and the public are to be congratulated upon the fine young giraffe which has just been housed in the so long empty giraffe shed. It is an awkward time of the year for a South African animal of delicate constitution to arrive; but at present, at any rate, the beast shows no symptoms of discomfort. This new giraffe, the first that has been on view since 1892, when the last survivor of the old gang departed this life, is an example of the southern form; this variety is more seldom to be met with in menageries than the familiar Soudanese animal. It is not spotted, but marked out into areas of dark colour by white lines. In the enclosure outside the house is to be seen the ingenious box in which it performed the journey from Southampton, which reflects credit upon the management of the Society for its minimum of restriction to the animal considering the exigencies of railway travel where bridges abound.

JOHN HANCOCK MEMORIAL MEDAL.

WITH the object of creating a more general interest in the study of natural history, it has been proposed to establish a fund for a medal or other prize to be given annually by the Natural History Society of Northumberland, Durham, and Newcastle-upon-Tyne, for the encouragement of field-work and original observations in any branch of the subject, whether botany, geology, or zoology. The object is not to foster a passion for mere collecting, but to stimulate observation and consequent research. It is therefore proposed that the prize shall be offered for the best essay evincing intelligent study of any of the common objects noticed in the fields or woods, on the moors, or by the sea-shore.

Competitors are to be residents in the counties of Northumberland or Durham, or in Newcastle-upon-Tyne. The prize is to be awarded by examiners nominated by the Committee of the Natural History Society, and the Trustees of that Society will be Trustees of the fund.

It has been determined to associate the medal with the name of the late Mr. John Hancock, who was a typical field naturalist, and who more than any man for half a century imparted to Tyneside a share of his own enthusiasm.

This excellent scheme originated with Canon Tristram. We understand that a good beginning has been made towards collecting the necessary capital fund, and we hope the secretaries of the Natural History Society at the Hancock Museum, Newcastle-onTyne, speedily will receive the necessary subscriptions to complete their scheme.

Geology in the "Court Circular."

We do not often read the *Court Circular*, but when we do we learn a good deal. The money article in the number for February 23 begins by assuring us "there is not a single money article written in this great London of ours that is not in some way influenced by sordid motives." Near the end the writer urges his readers not to "forget to put your hands on a few African alluvials." He predicts a rise. "The fact is, I have a friend out there, and he is a bit [apparently a *very small* bit.—Ed. Nat. Sci.] of a geologist, and he tells me that much of the land where the property is situated is calcareous tufa. This, Crœsus, may convey nothing to you, but it tells me a good deal. *Diamonds are always found where calcareous tufa exists.*" (The italics are in the *Court Circular*.) If this is all that is conveyed to the mind of the financial correspondent of the *Court Circular* by the information of the bit of a geologist, we think Crœsus is a much safer guide than the "ever-watchful Lynx." We expect, after this, to find the latter recommending his readers to drop their money down the shaft of the St. Augustine's. But he goes on, "I should not be at all surprised if the De Beers people don't offer a huge sum to buy up the property." We should not be at all surprised to hear in the next number that Queen Anne is dead. But to find a guarantee from the editor that the "ever-watchful Lynx" is never again to be allowed to mislead the readers of that journal were too much to hope for. Such is financial journalism!

Eccentric Glacial Chronology.

We all know that every geologist has his own opinion as to the date of the Glacial Period, and it is no doubt the charming uncertainty of the subject that attracts so many of our keener youth to its study. But why should the glacialists, as they delight to call themselves, introduce a similar uncertainty of date into the publication of their own magazine? Has not the cold of the past winter satiated them, that they must try to make us believe it is yet December, though March is drawing to its close? We can scarcely trust the chronology of writers who bring out a November number, we believe, in February, and at any rate containing a letter dated "27th December, 1894." The October number, too, did not reach us from the publisher till January 23, and was found to contain the report of a meeting of the Belfast Naturalists' Field-Club, which, we have since learned, was held on November 10. Fortunately, the *Glacialists' Magazine* has not yet published any descriptions of new genera or species, so that we can hardly attribute this misdating to any evil intent. Nevertheless, such a practice is confusing to the worker, and is quite easily remedied by the insertion of the actual date of publication on the wrapper. See to it, Mr. Percy F. Kendall, before your glaciers all melt beneath the heat of a righteous indignation.

Children and Museums.

We have so often urged the importance of museums for educational purposes that we are bound to welcome an addition just made to the Educational Code. This permits the time spent in visits to museums under proper guidance to be counted to the children as time spent in school-work. This may possibly have the effect of further improving the educational exhibits in various provincial museums, and so will do good to both sides. It is, however, to be hoped that the children will not come to regard museums with that horror which naturally clings to everything in the form of lessons. The visits should be made as attractive as possible. Some day we may see this wise license extended to the benefit of those who live too far from museums to avail themselves of it in its present form. If a collection of fossils or a herbarium are thought to be of value to the growing mind, surely the quarry and the flowering meadow have even greater claims on the consideration of our educational legislators. Mr. Acland has made so many changes in the direction of humanising education, that we confidently expect to see in the near future the children of board schools marching with their masters into the country as they do in Germany.

I.

The Origin of Species among Flat-Fishes.

(*Continued from p.* 177.)

IT is stated by Romanes that specific distinctions are useless or non-adaptive, while structures of generic, family, or higher taxonomic value are usually adaptive. He explains this by a very curious supposition: that the specific characters which have arisen by spontaneous variation, and been allowed to perpetuate themselves by heredity, are afterwards eliminated as so much surplusage in the struggle for existence by natural selection. A greater or less time would be required to effect this reduction, and thus we are to understand how it is that useless structures, which do not impose much tax upon the organism, occasionally persist even into genera, but rarely into families or higher divisions. There is much ambiguity in considering this argument, from the fact that the definition of a genus is so much a matter of individual opinion. We can nevertheless inquire if there is really this difference of kind between useful and useless characters, and if so, whether the useful or adaptive are generally distinctive of the larger taxonomic divisions. We have seen that, if we regard *Zeugopterus* as a genus, many of the generic characters are not adaptive. According to the analysis of Jordan and Goss the next higher division to which *Zeugopterus* belongs is the sub-family Rhombinæ (called by them Pleuronectinæ), the principal characteristics of which are, eyes on the left side, mouth large and symmetrical, pelvic fins somewhat asymmetrical and extended at the base. The large mouth and symmetrical teeth are certainly adaptive in relation to the predaceous habits of most of these fish, which rise from the ground to seize moving prey, and do not feed entirely on small animals living on the ground. But this character is shared by the Hippoglossinæ, and the extension of the pelvic fins, the anterior extension of the dorsal, and the sinistral position of the eyes are not obviously adaptive at all.

We are thus driven to the whole family Pleuronectidæ, which are certainly distinguished by their adaptation to a horizontal position on the sea-bottom. But before considering the family characters I wish to direct attention to another group of species whose relations are somewhat different to those of the three species of *Zeugopterus*. As Jordan and Goss point out, the species allied to the plaice, flounder, and dab form a distinct subdivision of the family, the

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Platessinæ, which is characterised by the small terminal, somewha asymmetrical mouth, large eyes, dextral asymmetry of the body, well-developed pectoral fins, vertical fins well separated, symmetry and narrow bases of the pelvic fins, and usually strong anal spine.

All the Platessinæ are arctic or antarctic in distribution. Consequently, all the known forms are completely reviewed in the paper of Jordan and Goss, which makes the study of their characters for the purpose of the present inquiry much easier.

In this sub-family Jordan and Goss make our common dab the type of a genus, *Limanda*, with the following characters:—There is no accessory branch to the lateral line; the lateral line has a distinct arch in front; the scales are rough, ctenoid, and imbricated; the vertebræ are forty in number.

The authors recognise four species. One of these, called *Limanda beani*, is doubtful. It is characterised by the shortness of the head, which is contained $5\frac{1}{2}$ times in the total length, instead of $3\frac{1}{2}$ to $4\frac{1}{2}$ as in the other species. It was defined by Dr. Brown Goode, from certain specimens taken in deep water off the southern coast of New England, which were not seen by Jordan and Goss. Perhaps the characters were individual variations or deformities.

Limanda limanda, the European form, has no rugose prominences above the operculum behind the interocular ridge. The fin-rays are Dorsal, 65 to 78; Anal, 50 to 62; the scales along the lateral line, 86 to 96; the teeth in an irregular series. This species extends from the Atlantic coasts of France along all the coasts of Northern Europe, and on the coast of Iceland: it is absent from the Mediterranean.

Limanda ferruginea is the dab of the American side of the Atlantic, extending from New York to Labrador. It differs in having the teeth more numerous, and in a more regular close-set series, in having a more projecting snout, and rugose prominences above the operculum. The fin-rays are a little more numerous, namely, Dorsal, 85; Anal, 62. The scales are smaller and more numerous, namely, 100 along the lateral line. The colour is brownish-olive, with numerous irregular reddish spots.

Limanda aspera is the dab of the North Pacific. It is distinguished by somewhat marked characters, of which the principal are that there is no angle between the snout and the profile of the head, and the scales of the blind side are more or less rough, the scales of the upper side rougher than in the other species. Specimens have been taken on the coast of both Alaska and Kamtschatka.

It seems, therefore, that while the species on opposite sides of the Atlantic are different, those on opposite sides of the Pacific are the same. But the localities Alaska and Kamtschatka are nearer than Europe and Labrador. On the other hand, since the European dab is found on the Iceland coast, there is no obvious reason why it should not extend to the Greenland coast and thence to Labrador.

This case offers a good contrast to that of *Zeugopterus*. In the latter we had three distinct species not geographically separated: here we have three species whose geographical ranges are, according to present knowledge, exclusive. We have no evidence that the ranges are not continuous, and perhaps if specimens were collected at successive stations along the arctic coasts of Europe, Asia, and America, it would be found that the three species of *Limanda* were connected continuously by intermediate forms. In that case we should have a species with a very wide range exhibiting geographically distinct races. We have no evidence that these forms would not interbreed if brought together. They are geographically isolated, not physiologically. We have, therefore, another illustration of the principle that isolation results in divergence. Even if the forms pass gradually into one another, it is certain that the American individuals are prevented by distance of habitat from breeding with European or with those of the Pacific.

Can we suppose in this case that there are differences in the environments which make the specific peculiarities advantageous to each species? Are rugose prominences on the head required in the life of the dab on the east coast of America and not in Europe? or is the angle between the snout and head useful to the European dab and not to the dab of Alaska? We have at present no facts which support such an assumption. There may be some differences in the conditions of life which produce these differences of structure as a result, the differences of structure themselves playing no part whatever in the struggle for existence. This also we do not know. What we do know is, that forms so closely similar as to be almost one widely-ranging species, present these constant peculiarities in places distant enough from one another to prevent interbreeding. On the selection theory, the variations in the different habitats are indefinite and similar, the selection is different; on the other view, which I prefer, the selection, so far as these structures are concerned, is absent, the variations are definite and different in the isolated groups.

Closely allied to *Limanda* is a species in the Pacific, called by Jordan and Goss *Lepidopsetta bilineata*. Only one species is placed in the genus, the establishment of which seems superfluous. This form is distinguished from *Limanda* only by the presence of an accessory branch of the lateral line, which starts from the anterior part of the lateral line, extending in all members of the family above the eyes, and runs backward along the base of the dorsal fin. This species is very common on the Pacific coast of America from California to Alaska, and its scales become rougher in the north. The evidence that this species and *Limanda aspera* are distinct seems sufficient, and as they are not geographically isolated, we have a case of divergence in the same area. We have no evidence that the accessory branch of the lateral line is required by *L. bilineata* and not by *L. aspera*; it is probably a definite but non-adaptive variation. It is a very interesting fact that this variation is known as a constant character only in the Pacific, and that there it occurs in a large number of species.

Of the thirteen species of the sub-family in the Pacific distinguished by Jordan and Goss, it occurs in eight, and it also occurs in one genus in the Pacific in the quite distinct sub-family, Hippoglossinæ. These species differ considerably in habits and stations, and there is no indication that they possess one common habit or peculiarity of life to which this branching of the lateral line is adapted or related. There may be something in the Pacific Ocean which tends to produce this branching of the lateral line. It seems to have been independently produced more than once in separate lines of descent. We have no reason whatever to suppose that it is useful to the species that have it, and would not be useful to those that are without it. As systematists, Jordan and Goss use this character in uniting those species of Platessinæ that possess it; but examination of the other characters rather favours the view that it is a subordinate character, some of the species that have it being more closely allied to Atlantic species without it than to other Pacific species that possess it. It is difficult among the many cross-resemblances that occur to trace with any confidence the lines of divergent descent among these species, in other words to found the systematic arrangement on genealogical ideas. I prefer to leave these species and take up the consideration of the species allied to our plaice and flounder.

Leaving other characters aside, the plaice (*Pleuronectes platessa*) is distinguished from *L. limanda* by wanting the arch to the lateral line, by having reduced cycloid scales, and tubercles on the post-ocular ridge. The flounder has differentiated scales, most of them being reduced and cycloid as in the plaice, but those along the bases of the longitudinal fins, along the lateral line, and on the head being enlarged to form rough spiny tubercles. We have seen that the dab has a relative on the west side of the Atlantic, and another in the North Pacific. *Pseudopleuronectes americanus*, the representative of the plaice on the east coast of America, approaches the dab in having imbricated ctenoid scales. In *Liopsetta putnami* the spinulation of the scales is a secondary sexual character, the scales in the male being rough and strongly ctenoid, in the female smooth and almost completely cycloid.

Now, of *Liopsetta* only one species is adequately known; it ranges from Cape Cod to Labrador, and is rather common. It is perfectly distinct from the plaice, and yet Mr. Holt has just described, in the *Journal of the Marine Biological Association* (vol. iii., no. 3), a local race of the plaice which distinctly approaches to the condition of *Liopsetta putnami*; the latter is therefore a more perfect American representative of the plaice than is *Pseudopleuronectes*. Mr. Holt's specimens came from the Baltic. They were much smaller than the North Sea plaice, the female being mature at 9½ to 13½ inches in length, and some of them were more or less 'ciliated,' that is to say, the scales were more or less ctenoid. This character of the scales was not uniformly developed, but most marked on the head and interspinous regions, and almost absent on the blind side. It was found that the ciliation, though varying considerably in different individuals, was always much more strongly developed in the mature males than in the females. These specimens seem to be identical with the variety *pseudoflesus*, described by Gottsche, but the sexual difference has not been noticed before. Möbius mentions these ciliated plaice, and observes that they form a transition to unusually smooth specimens of the flounder. The flounder (*P. flesus*) is specifically distinguished by the rough tuberculated scales which occur in a single series at the bases of the longitudinal fins, along the lateral line, and on the upper side of the head. Although I know from observation that the degree of tuberculation varies in individual flounders from the same locality, it is also a fact that the character is most reduced in the Mediterranean, where its reduction distinguishes a geographical variety called *glabra*, in which the head and lateral line are nearly or quite destitute of tubercles. According to Moreau, this variety extends into the Atlantic, but is rare to the north of the Gironde. On the American side of the Atlantic no flat-fish is known presenting the tuberculation characteristic of *P. flesus*, but in the North Pacific there is a species, *Platichthys stellatus*, which has it much more developed than in our *P. flesus*, all the scales being tubercular and rough, but the largest ones, as in *P. flesus*, being placed along the bases of the fins and the lateral line.

It appears, therefore, that in the plaice there is a Baltic ciliated variety, and in the flounder a southern smooth variety. Whether there is any direct relation between northern habitat and spinulation of the scales seems doubtful: at least it does not apply to other species. We can see no indication of utility or selection in these cases, but merely instances of variation, corresponding to geographical position. The geographical isolation is not complete, and the characters within the two several forms are not perfectly distinct, but connected by intermediate forms. The differences between *P. platessa* and *P. flesus* are, however, discontinuous, and the geographical ranges to a great extent common. Here the isolation is physiological and the occurrence of hybrids is not established.

We have at present no evidence whatever that these diagnostic differences in the character of the scales are adaptive, and the same might be said of several other characters distinguishing the forms just mentioned, such as the specific coloration and the number of fin-rays. In fact, although certain differences are known in the habits and distribution of plaice, flounder, and dab, of which the most salient is that the flounder lives in estuaries except when spawning, none of the structural characters are known to correspond and be adapted to these differences. Evidence, therefore, that generic and specific characters are adaptive in the sub-family is wanting. On the other hand, we must admit adaptation in many of the characters of the sub-family, namely, in the characters of the mouth, which is asymmetrical, the bones and teeth being much more strongly developed on the blind side, in accordance with the general habit of feeding on small invertebrate forms which live on the ground, and which the fish seize from above with the lower side of the mouth. But many of the characters are neither adaptive, nor known to be correlated with the adaptive: such as the narrow symmetrical pelvic fins differing from those of the Rhombinæ; the free edge of the preoperculum differing from the adnate edge in Soleniæ; the slight anterior extension of the dorsal fin.

We return again to the family characters: are they all adaptive? Certainly the Pleuronectidæ appear to offer strong support to the view that families are distinguished by different adaptations. But among the adaptive characters there is evidence that one at least, the absence of pigment from the lower side, is simply a consequence of the condition of life, not an advantage, and therefore not due to selection. There are other family characters which are not known to be adaptive at all, namely, the well-developed condition of the pseudobranchia, and the character of the tail, the Pleuronectidæ differing strongly in these respects from the Gadidæ, supposed to be their nearest allies. In the Gadidæ the tail very largely consists of dorsal and ventral rays symmetrically corresponding to one another, while the terminal portion of the tail, composed of ventral rays which have become terminal, is very small. In the Pleuronectidæ very nearly the whole of the tail is formed of the ventral rays corresponding to the upturned end of the notochord.

Moreover, if we examine the diagnostic characters of other families of fishes, we find that the most salient among them are by no means obviously adaptive. We have no evidence that the mode of swimming of the Gadidæ is such that three dorsal fins and two anal are necessary to its due performance. The adipose fin characteristic of the Salmonidæ appears to have no function at all. The Scombridæ are distinguished by minute posterior dorsal and ventral fins, which are not known to have any function. When we ascend to still higher divisions, the dominion of adaptation is by no means complete. For instance, we cannot say that there are differences in mode of life between Teleostei and Elasmobranchii which exactly correspond to their striking differences of structure. It may be said that the lungs of the amphibian are adapted to breathe air, the gills of the fish to aquatic respiration, but who shall say that the branchial apparatus of the Teleostei is adapted to a different kind of respiration from that which the branchial apparatus of the Elasmobranchii performs?

I cannot now discuss the limits of adaptation in the main divisions of the animal kingdom. I will content myself with suggesting that, although it is difficult to find anything in the characteristics of birds which is not a modification of the reptilian organisation specially adapting it to aërial life, still it is generally held that the birds correspond only to a subordinate division of the

Reptilia, and that the other divisions of that class are not separated from one another by adaptive characters only. The facts I have given do in my opinion tend to confirm most strongly the conclusion that an explanation of adaptation is not an explanation of the structural relations of species, genera, or even higher divisions. I hold that Romanes' view, that indifferent or non-adaptive characters have been eliminated by natural selection in the differentiation of the higher divisions, is not a full expression of the truth. I think, however, that the discontinuity of species is sufficiently explained by the view of Romanes and others concerning the effects of the isolation of strains or races, whether by geographical, environmental, or physiological barriers to interbreeding. In my opinion, therefore, discontinuity in variation is not necessary, as Bateson maintains, to explain the discontinuity of species. However gradual may have been the process of modification by which two allied species have been derived from a common ancestral species, the existing discontinuity is a necessary consequence of the divergence of the two lines of modification, provided that no intermixture of the variations in the two lines has taken place. At the same time, I consider that the investigation of the modes in which variations actually do take place is of the highest importance, and that Bateson's contribution to this investigation is of very great value. The only general view, as it seems to me, which can be held concerning the structural diversity of the animal kingdom is to regard it as the resultant of two more or less opposing general tendencies. On the one hand, there is universal evidence of a tendency to definite variation, or growth in different directions, leading to manifold variety of regular definite symmetrical forms. This tendency can only be regarded as internal to the organism, as connected with the tendency to growth and multiplication inherent in organic units. On the other hand, there is the moulding, limiting, constructing action of the external forces of the environment resulting in more or less complete adaptation. Whatever be the process of adaptation, whether Darwinian selection or Lamarckian modification, adaptive structural combinations are mechanisms, each working with a particular result which is important to the feeding, living, and breeding of the organism. Whatever the causes of non-adaptive variation, the resulting structural features are the regular "geometrical" forms and characters which the multitude of different organic forms present in such marvellous diversity. No one who, like Weismann, ignores everything except adaptation, or who, like Bateson, regards the study of adaptation as barren and profitless, can hope to produce a consistent and comprehensive theory of organic evolution.

Plymouth. J. T. CUNNINGHAM.

II.

Forms of Mountains.

THE origin of mountains has been so fully discussed by different writers that a paper treating of mountain-forms might be considered unnecessary; but in these days, when an intelligent appreciation of the leading features of our earth's crust is an essential part of a liberal education, a few notes upon the outlines of mountains and of our British hills may be welcome to readers of NATURAL SCIENCE.

Taking from the shelves of a library Jukes's "Manual of Geology," in that battered, dog's-eared, and dirty condition which is the highest tribute to the value of a book, we find hills classified as (i.) hills of accumulation, (ii.) hills of upheaval, and (iii.) hills of circum-denudation, though it is explained that the second and third classes cannot exist alone, as upheaval is necessary for the elevation of the block from which the hills are subsequently carved, and all hills have undergone some denudation. Consequently we may confine ourselves to a consideration of (i.) and (iii.), and of these (i.) may be dismissed in a few words.

The principal hills of accumulation are volcanoes, and an interesting account of their forms will be found in a paper by Professor J. Milne.[1] He brings forward evidence to show that many regular volcanoes have "a form mainly due to the simple piling up of material," and that they are not to be regarded "as cones which have been subsequently modified by a number of secondary causes." Consequently the surface presents a curvature similar to one exhibited by a pile of loose dirt "which would be produced by a simple logarithmic curve revolving about an axis." Of course, such a curve might be subsequently modified by denudation, as Professor Milne remarks, and since all our ancient British volcanoes have had their ancient surfaces entirely destroyed by the action of denudation, we need not dwell further upon this class of mountain, but proceed at once to consider the peculiar features of mountains of circum-denudation.

Taking a broad view of the action of denuding agents, we may divide them into two classes, those which act in the dry way and in the wet way respectively. The first class is responsible for the main outlines of mountains of frost-bound and desert regions; the second, for

[1] *Geological Magazine*, 1878, p. 337.

those of temperate regions, and accordingly for most of the mountain-forms of the globe; but under exceptional circumstances, and on a small scale, we find mountain-slopes existing in ordinary temperate regions which have been produced in the dry way. In arctic and high alpine regions the prevalent sculpturing agent is frost; in deserts, changes in the temperature of the atmosphere working directly, or indirectly through the medium of winds; in temperate regions, rain and rivers.

The familiar forms of mountains in temperate regions are fully accounted for by G. K. Gilbert in his work on "The Geology of the Henry Mountains"; it is there shown that stream-denudation tends to produce curves which become steeper and steeper as one ascends from sea-level towards the highest part of a country, so that if a country were elevated by a symmetrical uplift, which would give the land a convex curve, that curve would be gradually replaced by two concave curves of stream-erosion, having the form indicated above, and meeting at the main watershed. These curves of stream-erosion are the curves which bound hills and mountains in temperate regions, after stream-denudation has been in operation for a sufficiently lengthy period. They may be seen to perfection in any hilly district in Britain. It would be difficult to find a better example than the rocky peak of Bowfell, on the outer edge of the Scawfell group. From every side the two curves may be discerned, though they are best seen when looking southwards from the Borrowdale valley, or northwards from Eskdale. Great Gable, as seen from the foot of Wastwater, also exhibits these curves, and they are repeated below in the slopes of the nearer fells. In Mrs. Lynn Linton's book on the Lake Country, the engravings of W. J. Linton draw forcible attention to them; special mention may be made of the view from the top of Helvellyn looking over Striding Edge (facing p. 149), and the beautiful outline of Gimmer Crag on the Langdale Pikes (p. 170) "as it rises in one unbroken up-springing line from the dale to its height of over 2,000 ft." But it is needless to cite further examples: the curve of stream-erosion is the dominant curve among the hills of our island.

The details of form of hills carved by stream-erosion depend to a large extent on the alternation of soft and hard rocks, and the inclination of the planes of stratification; but in hills carved out of homogeneous rock, the character of their outlines is independent of the nature of the rock, though the slopes of the curves of stream-erosion vary. As an illustration of this, the remarkable hills standing west of the great escarpment of the Pennine Chain in Cumberland may be mentioned. Two of these, Dufton Pike and Murton Pike, show the curves of erosion fairly well, though in both cases they are somewhat masked owing to the existence of soil on the hill-slopes: these hills are quite similar in outline, though Murton Pike is formed of soft clay-slate, and Dufton Pike of hard rhyolite. The similarity of proportion and outline is determined by the trend of the bounding valleys, which, in the case of each hill, coincide with fault-planes. Again and again, among the Lake District fells, hills may be seen which have been carved by stream-erosion out of blocks of rock, whose size is determined by planes of weakness of varying character. Some of these planes are faults, often converted into mineral veins, others master-joints, and others, again, dykes formed of material more easily eroded than the surrounding rocks. Mickledore is an example of the last. Clifton Ward announced some years ago that it owed its existence to a dyke: if that dyke had not been intruded along the line of Mickledore, Scawfell Pike and Scawfell would have formed one hill, and the great cliff of Scawfell, which has become the favourite climbing-ground of an increasing number of cragsmen, would have had no existence. In some cases we see this process of fission commencing. On the north-west side of the Gables the stream which forms the head-waters of the river Liza, occupying Ennerdale, runs along a line of fault; and on the south-east side the stream down Aaron Slack runs into Sty Head Tarn along the same line of fracture. The col along this line, separating Great Gable from Green Gable, is still only a few score feet beneath the lower summit. In course of time it will be lowered until the two Gables form fells separated by a deep gash, like Mickledore.

Turning now to the consideration of mountains formed "in the dry way," we may regard as typical examples the mountains of Greenland, the peculiarities of which have been described and explained by A. Kornerup.[2] Frost is the important agent at work here, and the curve of stream-erosion is consequently absent. The curved outlines of our lovely hills are replaced by monotonous straight contours. (*see* plate vii. of Kornerup's Paper). These contours are due to the frost working along the dominant joint-planes, and consequently the outlines of the mountains depend on the directions of the joints. It is true that among bedded rocks of unequal hardness, dip-slopes and escarpments occur, but even here the lines are straight and not curved, and when prominent planes of stratification are absent, as among the gneissose rocks, the "house-roof" type of mountain results from the action of denudation. cirque-like valleys (Grydedal) do occur, but they are produced by the action of frost on a system of *curved* joints.

In Switzerland, many of the mountain faces show a similar straight outline (it is well displayed on the cliffs of the Matterhorn), indicating the efficacy of frost above the snow-line in determining mountain contours; but one may frequently notice what seems to be the "ghost" of a stream-erosion curve, possibly pointing to the formation of some of the depressions between the higher mountains at a period before the occupation of the area by snow and ice, though subsequent frost-action has nearly destroyed them.

Among the hills of desert regions, so far as one can gather

[2] "Meddelelser om Grönland," part ii., section vi.

from descriptions, the straight outline is also a characteristic feature, as we should expect, for there the blocks of rock would be split off along joints as they are in the domains of the frost-king.

In our own country, though the curve of stream-erosion is the dominant feature, the results of denudation "in the dry way" are easily found, though usually on a small scale. The best illustration is furnished by the terraced hills of the Ingleborough district of Yorkshire, capped with Millstone Grit, beneath which are a few hundred feet of Yoredale rocks, while their bases are composed of Mountain Limestone. The Millstone Grit capping the tops of the highest hills is too porous and too thin to allow important streams to course over its surface, while the drainage of the Limestone region is mainly subterranean. The consequence is that the Millstone Grit and Limestone "scars" have the straight outlines due to the action of frost, which may be well contrasted with the curves of stream-erosion of the slopes of Yoredale rocks, down which numbers of rills flow.

Again, in districts where the stratification is not an important factor in determining the outlines of the hills, excellent examples of the work of frost may be seen on a small scale. Many of the cliff-faces of Cambria and Cumbria owe their straight faces to the effects of frost on the joint-planes, and here and there we find a "pocket-edition" of a frost-formed hill, which differs but in size from its more imposing fellows in Alpine regions. From out the hard, regularly-jointed volcanic rocks of the Borrowdale series of Cumbria has been carved the Napes Needle of Great Gable, of which a good illustration is given in Haskett Smith's "Climbing in the British Isles," part i., p. 95. This may well be compared with the Aiguilles of Chamouni as regards form, though its size is insignificant.

To flowing water, then, we owe the sweeping curves of our beloved hills which, seen afar off on some sunny day, harmonise so well with the quiet grace of a pastoral foreground; but it is to the frost that those forms are due which appear to most advantage when seen fitfully through the changeful mists—

"That marvellous array
Of temple, palace, citadel, and huge
Fantastic pomp of structure without name,
In fleecy folds voluminous enwrapp'd."

J. E. Marr.

III.

The Structure and Habits of Archæopteryx.

V.—Answers to Critics.

MANY and various arguments have been urged against my view as to the homology of the digits of the wing of an ordinary bird, as well as against my interpretation of the *photograph* of *Archæopteryx* and other contentions contained in an article on the sources of error (Nat. Sci., vol. iii., p. 275).

I will take the most frivolous first, and this is undoubtedly contained in a letter signed "A.S.W." Though the text of the German edition of Zittel's "Palæontologie" is perfectly clear and definite, and describes the fore-limb precisely as it figures it, he nevertheless suggests that it was so figured merely through an almost impossible accident to the block. I do not suppose anybody who has looked at that edition will for one moment hesitate to pronounce A. S. W.'s suggestion to be pure nonsense, and I should not have deigned to reply to it had not the letter contained a suggestion of unfairness on my part. It is *not* obvious, as A. S. W. states, that the omission of the digit in the figure "was an accident, and not intentional." It is obvious that the digit was omitted in both text and figure intentionally, and that Professor Zittel fully meant what he said, for he argues as to what those *three* digits were, and draws conclusions as to the relation of *Archæopteryx* to other animals. Let him who doubts it read p. 822 of the third volume of the book in question for himself and his doubt will vanish. The real omission was not so much omission of a digit as the omission of a note to the effect that Owen's figure had been altered. The omission to note such alterations is a very common one, and I attached no blame to it. I merely asked if it was right. Since no one has answered me, I may now say that I think the custom is most objectionable, and leads, or may lead, to serious error and, as in this case, to the attribution of an error to a person (Owen) who was not responsible for it.

Mr. Pycraft has devoted quite a large space to some interesting observations on *Opisthocomus*, but they appear to lend no support to his conclusions as to *Archæopteryx*. He does not even attempt to show that the digits of *Opisthocomus* are any but III, IV, and V. Interesting as his communication is, it has absolutely no bearing on the question of *Archæopteryx*, except in so far as it shows that so long as the digits are useful for climbing, the development of quills upon them is delayed; and that points to a conclusion directly opposite to his views. He throughout "assumes" first one thing and then another which he knows to be in direct antagonism with my views. For instance, he "assumes" (vol. v., p. 352) that the recapitulation theory in its crudest form is true (!), that the ostrich is a suitable species to be taken as type of "existing birds" in the matter of wing-structure (p. 354); that some such arrangement as that he has described from existing birds obtained in *Archæopteryx* (p. 356). This last *assumption* might be all very well under other circumstances, but in an argument raised in refutation of the contrary view it is not easy to see what assumption could be less allowable. He assumes, without offering a jot of evidence for it, that in the matter of the wing *Opisthocomus* is "primitive" — therein begging the whole question (p. 359). On the same page he assumes that the common fowl is a "more recently evolved form," and after stating that it is "probable that the peculiar habits of the nestling [*Opisthocomus*] may be the survival of an order of things handed down from the very dawn of avian development," he regards that as sufficient, and proceeds to argue as if what he had stated, without a scrap of evidence, to be probable, were in reality established fact. As to the powers of flight of *Archæopteryx*, upon which (p. 359) he lays very great stress, he expresses an opinion, but gives no grounds which would justify our acceptance of it. He ignores the fact that the relative size of wing and of body in equally good fliers is very different in the cases of large and of small birds respectively.

Mr. Pycraft's contention as to weak-jointedness of the phalanges of the wing-digits is merely due to his having overlooked the fact that resistance to torsional and not flexional stress was the point I referred to in a previous paper. He says, "The phalanges of living birds seem to be scarcely, if at all, more firmly bound together than were those of *Archæopteryx*" (p. 445). My argument had reference, not to the movement of one phalanx upon another in the same digit (*i.e.*, flexion), but to the movement of the *metacarpal* V on metacarpal IV, or of *metacarpals* III on II, if we adopt his own views. I need hardly urge that since III and II are free in *Archæopteryx*, and what he regards as their equivalents are ankylosed and wholly immoveable relatively to each other in modern birds, his argument fails to affect my conclusion. As to the phalanges, using my own nomenclature, the extraordinary breadth and flatness of the first phalanx of IV in the plane of that of V, while they do not limit flexion, prevent their independent movement and give that torsional rigidity on which I insisted, while leaving unrestricted that flexional mobility upon which the young *Opisthocomus* depends in climbing. The joint, however, is stiffened very considerably in most birds (if not in all), but it is not a point with any direct bearing on the question under consideration.

Mr. Pycraft asks (p. 445), Where did my supposed but unseen digits IV and V articulate in *Archæopteryx*? I have answered this already in the preceding pages of this article; but there is good reason for answering it again. The digits did exist, and their metacarpals are conspicuous in the London specimen, and they articulated to that large rounded carpal bone which is shown at the distal end of the radius and ulna of the right wing in Fig. 2, p. 116.

On p. 446 is a logical error of a type which is very widespread. "A very serious objection" to my theory is that the wing of *Archæopteryx* must have been highly specialised in two directions—"modifications, too, which must have gone on hand in hand, a possibility which I can hardly imagine." Now, though the limits of Mr. Pycraft's powers of imagination may interfere with his comprehension of the works of Nature, they do not in the least interfere with Nature in the carrying out of her work. Bats, Pterodactyles, and Mr. Pycraft's pet *Opisthocomus* are in various ways modified in two like directions. Nay! Even Mr. Pycraft has himself urged (on p. 359) that very double modification which he was unable, when writing p. 446, even to imagine. Space does not permit me to notice other points in his argument; but I believe he will find every objection he has raised fully met and disposed of in the foregoing pages of this article.

I may perhaps be permitted to congratulate Mr. Pycraft on having succeeded in opposing my views, on which I know he feels very strongly, without having once lapsed into that violence of language which I find it almost impossible to avoid. I hope that my frank criticism of his article may cause him no more pain than his own criticism of my views has caused me.

Another critic, Mr. Virgil L. Leighton, in no. iii. of "Tufts College Studies," says (p. 71), "That there is developed a fourth digit in the avian manus is beyond question [!], and the fact that this comes upon the ulnar side of the three permanent fingers is sufficient to invalidate the nomenclature III, IV, and V of Hurst." The "plausibility" of my view is, moreover, founded on ignorance, and "no one without a theory to support would regard it [*i.e.*, what I called *os pisiforme* in NAT. SCI., vol. iii., p. 279] other than a digit." Mr. Leighton, of course, knows that greater men than myself regard the *os pisiforme* itself as the remnant of a digit; but even if he did not, the alleged resemblance of it to a digit in a certain stage in the development of a certain bird (*Sterna*) would not in the least affect my conclusions. It is not difficult to name a few mammals in which things occur much more like digits beyond the normal five than the embryonic rudiment he refers to. (N.B.—Rudiment = beginning, germ, thing as yet undeveloped, "Anlage.")

He further urges "the LAW of digital reduction advocated by Morse, by which in other groups digit I is first to disappear and then V." To me it is astounding to find people who consider that so-called "laws" are all-efficient and irrefragable, and that Natural Selection is a mere whim. Because the middle toe happens to be in the position to be of most use to a horse, we are forbidden by such people to believe that any but the middle toe can be of most use to a bird, and are requested to accept the dogma that the biggest digit in the manus of a horse is the homologue of the biggest in the manus of a bird. The utter worthlessness of such laws is apparent without even looking beyond the hind limb of the bird itself, where Nature has set at defiance this particular law. Mr. Leighton's own paper confutes this absurd notion of the efficiency of these "laws," and he contradicts himself most naïvely in these words: "It is the ulnar phalanges which must bear the stress of the wing; the fingers on the radial side, having but few small feathers, would be most likely to disappear." (*Op. cit.*, p. 73.)

Only one really strong and rational objection to my views has come to my notice. It is a very strong one, and was urged in conversation by Dr. Jaekel, of Berlin, and subsequently by Professor Dames. They urge the principle, well-known among palæontologists, that in all fossils we must expect the evidence of things below the surface to appear on the surface; that all the bones of a single specimen usually lie nearly in a single plane. The bones of *Archæopteryx* in the Berlin specimen certainly do all lie almost in a single plane. There is, however, no evidence in that specimen of the existence of the supposed digits IV and V. What I formerly took for a shadow in the photograph is merely a yellow stain on the slab (but not without significance), and the slender digits I, II, and III are not crushed or distorted as if by underlying bones; the tibial quills, as I have called them, of the right leg do unmistakably show a displacement or distortion where they lie over the left knee; vertebral and ventral ribs lie in juxtaposition, and so even do the ribs of right and left sides.

This is a really strong objection, and it is the only strong one that has come to my notice. Even this, however, not only ceases to be an objection, but actually comes to support my view when we enquire more closely into it. Many fossils are distorted much as they would be by enormous vertical pressure: they are flattened much as wax models of them would be by being put in a hydraulic press and squeezed. But *Archæopteryx* is not so distorted. The skull is not flattened: its cavity was empty, and not either filled with matrix or abolished by the collapse of its walls. The left femur does *not* lie in the same plane as the right, but so inclined that its head is now deeply embedded in the slab, while that of the right lies well above the general surface of the slab. The digits do not lie all in one plane; in the right hand the second lies over the third, crossing it without displacement and without deformation. On the other hand, the first metacarpal lies *lower* than the second, and not above it as it should in the natural position of the parts. The second metacarpal and proximal phalanges lie, not in the same plane as the feathers, but well above that plane. Whatever flattening there might have been it could not have *lifted* the second digit well above the level of the *proximal ends* of the quills it was supposed to have supported. The bones of a dead bird, which has fallen on the bottom of a stream, or lake, or sea, will naturally fall to the level of the underlying deposit as fast as the decay of the soft parts allows them to do so. The weight of overlying deposits may even flatten out ribs and bring them all to one level. It may even crush the bones where they lie one over the other—I am not sure that this has not occurred in the left hand—but it has not, in this case, crushed the skull, or the phalanges of the third digit of the right hand, or the pelvis; and it has not brought the proximal ends of the two femoral bones into juxtaposition. Had it brought the digits IV and V to the same level as I, II, and III, we should have seen them, and these latter might well have been crushed by them; but the perfect preservation of these digits, even where they cross each other, and the fact that they do lie, especially the second, well above the level of the feather-surface, shows that there has been in this case little, if any, deformation by pressure of overlying strata; hence the absence of a trace of digits IV and V on the surface. If any such deformation had occurred it would have brought II and III to the level of the feather-surface. No matter, therefore, what pressure there may have been, the fact that those digits II and III lie now above that surface shows that they did originally lie above it, and not below it as all views except my own demand.

May I ask in conclusion that "Paddy from Cork," who says (p. 143) that the Romanes restoration in "Darwin and After Darwin" "is a *copy* of one by Shufeldt which appeared in the *Century* magazine some eight or ten years ago," will turn to that magazine for January, 1886, p. 355, and then inform us whether that is the sketch by Shufeldt to which he refers? If so, it is rather a libel on Shufeldt to call the Romanes figure a copy, for Shufeldt distinctly shows both primary and secondary quill-feathers, though most of the secondary ones are attached to the humerus (!), while in the Romanes figure the primaries are abolished altogether, and the secondaries are apparently all attached to the ulna. There are also various differences in head, trunk, tail, and feet. If this is not the figure to which "Paddy" refers, if he will simply say so, I will endeavour to find the one he does refer to.

C. Herbert Hurst.

Correction.—On p. 122, line 6, for "tibia," *read* "tibio-tarsus."

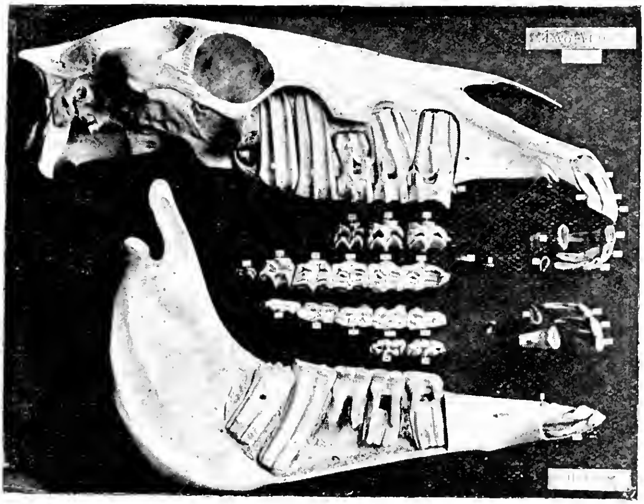
Postscript.—I have just seen an article by Mlle. Norsa in the *Archives Italiennes de Biologie* (1894), on the morphology of birds' wings, in which she states that in all cases (*i.e.*, the Common Fowl, Guinea-fowl, Turkey, Pheasant, Duck, Goose, and Pigeon) which she has investigated, there appears, during development, a transitory digit on the radial side.—C. H. H.

March 6, 1895.

IV.

The Teeth of the Horse.

ONE of the most interesting of the recent additions to Sir William Flower's instructive "Index Collection" in the entrance hall of the Natural History Museum is a series of jaws of the horse prepared to show the salient features of the dentition at different ages. The series consists of seventeen preparations, ranging from jaws of a half-grown fœtus to those of a very old horse of probably not less than thirty-five years. The skulls from which these specimens were prepared were collected and presented to the Museum by



Mr. Thos. Goodall, F.R.C.V.S., an enthusiastic veterinary surgeon, of Christchurch, Hampshire.

The teeth of the right side of the head are exposed so as to show the whole of their outer surfaces; but are left *in sitû* in the jaws, which are widely separated by a space. In this space the teeth of the left side are so placed as to present their grinding surfaces to the observer, the cheek teeth being in most cases shortened by the removal of a part of the root, so as to bring the grinding surfaces down to the level of the external face of the teeth of the right side. In those cases where the milk-molars have not been shed, the underlying premolars of the left side are placed by the side of their corre-

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sponding milk-molars so as to show the form of the functional end before cutting the gum. In these specimens, therefore, there are four rows of upright teeth between the jaws of the right side, the two middle rows being the teeth in use at the time, and the upper and lower rows the premolars which have not yet cut the gum, an arrangement clearly comprehended by a glance at the accompanying photograph of one of the preparations.

In the case of the incisor and canine teeth of the left side another method of exposition has had to be adopted. The teeth, being curved, are set rising obliquely from the board so as to present their biting surfaces. Those permanent incisors which have not yet cut the gum are placed behind, *i.e.*, to the left of, the corresponding deciduous incisors in use at the time.

All the teeth exhibited are lettered in a manner readily intelligible to those possessing but the slightest knowledge of odontology, and the labels, though large enough to be legible to the inquiring student, are so small as to be unobtrusive. The collection is, moreover, fortunate in having the case in which it is exhibited specially constructed for it, most museum exhibits having usually to accommodate themselves to show-cases of stereotyped pattern and size.

"The periodical changes to which the teeth of the horse are subject, afford reliable indications of age, second only in value to positive evidence of the date of the animal's birth. The ages of cattle, sheep and swine are to be judged with accuracy only during the period occupied by the cutting of the temporary teeth and their replacement by those of the permanent set, but the peculiar conformation of the teeth of the horse enables the expert to form an opinion of the animal's age long after the completion of the permanent dentition." (Brown, *Journ. Roy. Agric. Soc.*, 1881.) The specimens exhibited in the Natural History Museum not only show the age at which each tooth is formed and when it cuts the gum, but they also show those changes which the crowns of the permanent teeth undergo with advancing age, upon which the practical horseman relies in his determination of the age, when the date of birth is either not forthcoming or is considered untrustworthy.

The horse possesses that number of teeth which may be considered normal for Mammalia, and these teeth are divided into groups according to their form, position, and other characters; namely, three incisors, one canine, four premolars, and three molars on each side of each jaw—forty-four teeth in all.

The cheek teeth (molars and premolars) are set in close contact, as are also the incisors; but between the canine and the first premolar there is a broad gap (diastema), in which the "bit" of the driving gear of the horse is placed. This diastema, however, appears comparatively late in development; for at five months before birth the first deciduous molar (*dm* 2, the first premolar having no predecessor) is in close proximity to the second deciduous incisor, the third deciduous incisor and canine not yet having appeared. At birth these teeth are separated by an interval of an inch and a half, and the diastema attains its full width of three to four inches in between one and two years.

With this late development of the facial parts of the skull is connected the relation of the last-formed cheek tooth to the orbit. In a half-grown fœtus, a vertical plane through the anterior margin of the orbit passes between the last two deciduous molars; at birth it passes between the last deciduous molar and the first molar, at one year between the first and second molars, at two to three years between the second and third molars, and at fifteen years or so behind the last molar. Each of the last four upper cheek teeth thus begins to develop under the orbit, but, as it increases in size, is gradually brought more and more forward by the lengthening of the maxillary bone.

The first and second deciduous incisors and the three deciduous molars appear to cut the gum together, shortly after birth; but the third deciduous incisor is backward, and only makes its appearance above the gum at eight or nine months. All the milk teeth are thus in use before the end of the first year. At five years all the milk teeth have been shed, the permanent teeth are in position, and the horse is said to have a "full mouth." The dates mentioned in these, and in the subjoined remarks, are naturally only approximate, since much depends on the breed and physique of the horse, the character of the food, and the fact that the upper teeth usually cut the gum a little before the lower.

The *incisor* teeth are known to veterinarians by special names: those nearest the middle line are called the "centrals" or "pincers"; those lying on the outer sides of the centrals are the "intermediates"; while the third incisors of comparative anatomy are called the "corners." The anterior (labial) edges of these teeth are disposed in a curve, which, up to the age of five years, is nearly semicircular, but which later in life becomes gradually flattened out, the lower centrals and intermediates being in a straight line at twelve years. Each incisor tooth is slightly curved, the concavity being directed towards the mouth, and it tapers uniformly from crown to root. That surface of the tooth which is continually wearing away by constant friction with food material and with the opposing tooth, is called the "table." The table of a young incisor bears a central depression or cavity, called the external dental cavity or "infundibulum" (*i.*, Fig. J.); this is usually filled with blackened food-stuff and constitutes the "mark" of the tooth. The enamel of such a tooth is disposed in two layers: one, the "central enamel," lining the infundibulum; and the other, the "peripheral enamel" surrounding the dentine. Before the tooth cuts the gum the two layers of enamel are continuous at the margin of the infundibulum, and it is only when the tooth bites with its opponent in the other jaw that this rim of enamel becomes eroded and the dentine exposed. The external layer of enamel, more especially the embedded portion of it, is invested with cement; and, shortly before cutting the gum, the inner layer of enamel belonging to the infundibulum becomes lined with a layer of cement, the thickness of which is subject to considerable individual variation. The infundibulum is conical in form, the apex being directed towards the root-end of the tooth; and, since the area of the section of the infundibulum is greatest towards the biting end of the tooth, one may roughly conclude that the smaller the size of the mark the greater is the age of the tooth.

The central permanent incisors cut the gum before the second or intermediate, and the second before the third or corner incisors; and, since these teeth wear away at approximately the same rate, the mark disappears in the same order, first on the centrals and last on the corners. The mark is lost in the central lower incisors between five and six years, in the intermediates between six and seven, and in the corner teeth between seven and eight years; but, since the disappearance of the marks depends so much on the nature of the food and on the amount of cement lining the infundibulum, the modern tendency is to place less reliance than formerly on this character when estimating the age of a horse. Owing to the greater depth of the infundibula in the upper incisors than in the lower, the marks are retained for a longer period; they are obliterated between eight and twelve years of age, but with less regularity than in the lower jaw. After the blackened mark has disappeared, the table still exhibits an area of cement surrounded by the central enamel. This usually disappears in the lower incisors at twelve or thirteen years, but the central enamel may sometimes still be seen at twenty or even later. After the central enamel has disappeared, the table exhibits only a ring of primary dentine, bounded externally by the peripheral enamel, and surrounding a central area of secondary dentine of darker and yellower tint. This is known as the dentinal star, and is formed by the filling up of the pulp-cavity by secondary dentine. The dentinal star begins to appear at eight years in the central and intermediate lower incisors, in front of the ring of central enamel.

In a recently cut incisor the exposed crown is broader transversely, but towards the middle and root end the section is longer in the antero-posterior direction, so that the age of the horse may be roughly determined by the position of the long diameter of the table. In the lower central and intermediate teeth the tables are oval, with the long diameter transverse, until fourteen years, when they become triangular; and at nineteen years the longer diameter of the table is parallel to the length of the head.

The lower corner tooth is very often a "shell tooth," the posterior border being entirely absent, so that the infundibulum is widely open behind.

The incisors of the horse, like the cheek teeth, are remarkable for their great length of crown, and as the teeth gradually emerge from the gums in consequence of continual wear, the length is increased, not by development of dentine and enamel, but by the addition to the root of large quantities of cement; the root of a very old tooth, in fact, is composed almost entirely of cement. As age advances the teeth project more and more forwards, so as even to project between the lips, and the upper tend to bite in front of the lower; the teeth also tend to become more horizontal in position, more especially in the lower jaw, and these facts are not lost sight of when judging the age of an old horse.

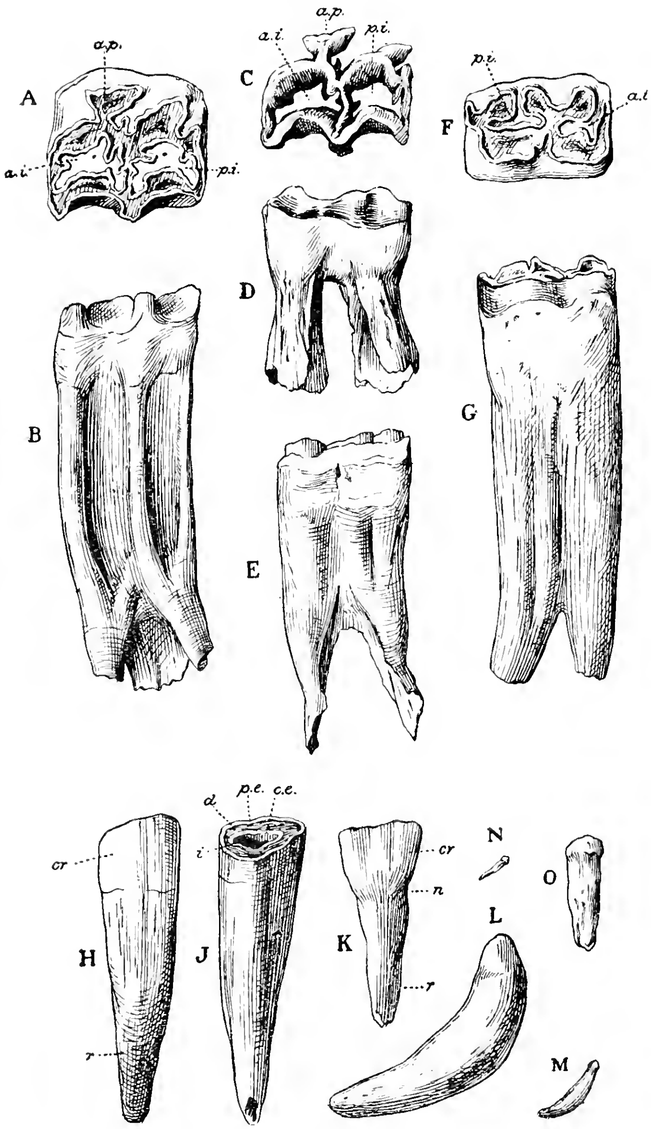
In a young horse the roots of the incisors are very close together and the alveoli are continuous; but as the teeth get shorter with increasing age, the roots separate while the crowns remain in contact; and this divergence of roots may usually be detected in a living horse of considerable age by the presence of interdental spaces at the edge of the gum.

A shallow groove running longitudinally along the outer surface of the upper corner incisor has been employed with satisfactory results by Galvayne for determining the age of the horse after the obliteration of the marks in the lower incisors. This groove is of limited length, and, in a middle-aged horse, does not extend to either extremity of the tooth. It is developed on the embedded portion of the crown, and, as the tooth very gradually emerges from its socket, the lower end of the groove is the first to become exposed. This occurs at about ten years of age. At sixteen years the groove extends halfway down the exposed face of the tooth, and reaches the lower edge at twenty-one years: that is to say, that part of the tooth which is visible at ten years has been completely worn away in eleven years. The upper extremity of the groove now issues from the gum, and the groove gets shorter and shorter, until at thirty years it simply forms a notch on the outer border of the table, and the exposed portion of the external face of the tooth is once more nearly smooth.

The start which the first permanent incisor has over the second, and the second over the third, is maintained throughout life, and since the teeth wear out in the order in which they were developed, the first incisor in an old skull is shorter than the second, and the second than the third, although in youth the order of length is the reverse of this.

The deciduous incisors (Fig. K.) are much smaller than the permanent, and exhibit a marked constriction or neck between the crown and root. The crown is of a milk-white colour and is quite smooth on its anterior surface, whereas that of the permanent tooth is less white in colour and is marked on its front surface by a shallow longitudinal gutter. The infundibulum is shallow and the root is gradually absorbed as the permanent tooth forms behind it. The first and second deciduous incisors begin to calcify[1] at five or six months before birth,

[1] The origin of a tooth is here considered to date from the commencement of calcification, and not from the formation of the dental capsule and other soft parts.



TEETH OF HORSE (two-thirds natural size).

A. Right, upper, fourth premolar of nine-year-old horse. View of table.
B. External view of same tooth.
C. Right, upper, third premolar of colt (twenty months) before cutting the gum. End view.
D. Right, lower, third, deciduous molar of same colt. Side view.
E. Right, lower, third premolar of eighteen-year-old horse.
F. Right, lower, fourth premolar of nine-year-old horse. View of table.
G. External view of same tooth.
H. Upper, central incisor of six-year-old mare. Anterior view.
J. Posterior view of same tooth.
K. Upper, central, deciduous incisor of filly (eight months). Anterior view.
L. Lower right canine of nine-year-old horse.
M. Lower canine of fourteen-year-old mare.
N. Lower deciduous canine of colt at birth.
O. Upper first premolar of fourteen-year-old mare.
a.i., anterior infundibulum; *p.i.*, posterior infundibulum; *a.p.*, accessory, internal pillar.
p.e., peripheral enamel; *c.e.*, central enamel; *d*, dentine; *cr*, crown; *r*, root; *n*, neck;
i., infundibulum.

and the third at one month before birth. The first or central is cut a few days after birth, the second at six weeks, and the third at eight or nine months. The first is shed at two and a half years, the second a year later, and the third at four and a half years. About five months before birth, the first and second deciduous incisors point vertically up and down like the three deciduous molars, but at birth they show marked indications of that forward slope so characteristic of the incisors of the horse. The permanent incisors cut the gum a few weeks after the loss of their respective milk predecessors, and subserve the requirements of the animal till death.

The *canine* teeth in the male, called tusks or tushes, have the form of curved cylinders, tapering off to a blunt biting edge (Fig. L.). The lower are larger than the upper, are less sharply curved, and develop earlier. The crown of the lower canine is situated so far in front of that of the upper that the two teeth do not bite with one another. The canines very rarely cut the gum in the female, but not infrequently they may be felt under the gum, especially in the lower jaw. They are usually of small size (Fig. M.), and are very irregular in shape, as are most teeth that are absorbed without eruption; but the characteristic tooth tissues, enamel, dentine, and cement are present, and are disposed in the same manner as in the canines of the male. Calcification of the permanent canines begins at about two years, and eruption usually takes place, in the male, at four-and-a-half to five years, although in some cases the canines are cut at three years, and in others they only appear at six. The eruption of the canines is thus of less value for the determination of age than was formerly supposed.

The deciduous canines (Fig. N.) are straight, blunt, very slender, and about half an inch in length and are of equal size in the two sexes. They begin to calcify about a fortnight before birth, and are absorbed at three to four years, without ever cutting the gum. They are developed in the lower jaw, not behind the corner milk incisors (*di* 3), but to the inner side of those teeth. In the upper jaw they are lodged in the conspicuous suture between the maxilla and premaxilla. For some inscrutable reason, veterinarians are agreed in insisting on the non-homology of these "splinters of bone," as they term them, with the milk canines; but a microscopic examination demonstrates the presence of dentine and cement, and a thin cap of enamel at the anterior extremity, and the dissection of a large number of specimens cannot fail to convince the comparative anatomist of the true value of these rudiments.

The *first premolar* is not constant in its occurrence, and is almost always small and irregular in shape (Fig. O.). It occurs more frequently in the upper jaw, and rarely in both jaws simultaneously. It is not preceded by a milk tooth, and, although it appears with the deciduous molars of the remaining three premolars, and is in most cases dislodged when the first deciduous molar (*dm* 2) is shed, yet it not infrequently remains to coëxist for many years with the other three premolars, more especially if it is situated so far forward in the jaw as to be unaffected by the eruption of the second premolar. The first premolar is so variable in its occurrence that it is not considered as a normal tooth by veterinarians, but is regarded as a supernumerary premolar, and goes by the name of the "wolf-tooth." Modern investigation tends to show that the alveolus and dental follicle of the first premolar are developed in all cases at some five or six months before birth, and larger in the upper jaw than in the lower; but that in some cases the alveolus closes without ever containing a calcified tooth, while in others the tooth is absorbed without cutting the gum. On account of the widespread belief that these wolf-teeth exert, in some mysterious way, an injurious influence on the eyesight of the horse, they are often forcibly removed at the instigation of the owner; but, as the roots are short and single, and as the teeth are of little service for mastication on account of their small size, no great harm is done by this blind adherence to a superstitious custom.

The *cheek teeth* (second, third, and fourth premolars and the three molars) are massive, and remarkable for their great length of crown, the bulk of which is embedded in the alveolus in the young animal, but which is gradually pushed outwards as the table or grinding surface is worn away. The lower cheek teeth are provided with two roots, and the upper with three. The roots begin to form at four or five years, but indications of their development may be seen shortly after the tooth cuts the gum. They continue to grow in length until they close at twelve or thirteen years, after which no more enamel and dentine are formed, but the roots become coated with a gradually increasing layer of cement (Fig. E.). The parts of the crowns above the gum are in very intimate contact with one another, and appear to form a single solid block; but exposure of the embedded portions of the teeth shows that the roots of the different teeth diverge to a considerable extent. The cheek teeth of the right and left sides are more widely separated in the upper jaw than in the lower, and, instead of biting with their opponents by level surfaces, meet them obliquely, the internal border of the lower teeth being higher than the external, while the reverse is the case in the upper jaw.

The cheek teeth are prismatic in form, and the last molar is distinctly curved. The external face of the second upper premolar is marked by three shallow vertical grooves and two intermediary ridges, while that of the remaining upper cheek teeth is traversed by two grooves separated by a ridge. The internal face of the upper cheek teeth is marked by a very prominent ridge, the accessory column or pillar of the tooth (*a.p.*, Fig. A.). The sides of the lower teeth are also marked by ridges and grooves, but not to the same extent as in the upper teeth. The second premolar tooth is triangular in section, but the remaining teeth are rectangular in section, and are remarkably similar in character, the molars being practically indistinguishable from the premolars.

There are two infundibula, one anterior and one posterior, running vertically through the whole length of the crown of the cheek teeth; but in the lower jaw the infundibula are, until filled with cement, cleft for their whole length down the internal face of the tooth. In the upper teeth, five main divisions of the pulp-cavity can be distinguished, four of these being disposed symmetrically with regard to the two infundibula, while the fifth is an outgrowth from the antero-internal division and belongs to the accessory internal pillar of the tooth. In the lower teeth there are two main divisions of the pulp-cavity, and three (or four) minor divisions belonging to the two duplicated columns on the internal or lingual surface of the teeth. Since the pattern of the worn tooth is determined by the disposition of the dentine and enamel about the cement-filled infundibula and the divisions of the pulp-cavity occupied by secondary dentine, the tables of the teeth are very elaborate; and, by the inequality in the wear of the three constituents, enamel, dentine, and cement, a very efficient grinding surface is preserved. On the table of one of the upper teeth, three closed sinuous bands of enamel stand out in relief by virtue of their superior hardness. One ridge, large and irregular in form, and called the "peripheral enamel," encloses all five divisions of the pulp-cavity (which, on the table, are always closed by secondary dentine); and two, smaller and of crescentic shape, line the cement-filled infundibula, and are known as the "central enamel" of the tooth (Fig. A.). The dentine of the tooth is disposed inside the larger enamel zone and around the enamel crescents, whereas the cement only occurs in the infundibula and on the periphery of the outer enamel band. In the lower teeth, since the infundibula are open on their internal edge, the central and peripheral ridges of enamel on the tables are continuous (Fig. F.), and the cement filling the infundibula is continuous with the peripheral cement. The enamel band is thus even more tortuous than in the upper teeth, but, as in the latter, the dentine is everywhere separated from the cement by enamel. As in the case of the incisor teeth, the infundibula are at first open at the root end of the tooth (Fig. C.), and only close when the tooth cuts the gum, the cavities being by that time nearly filled with the later developed cement.

There is hardly any difference in pattern or size between the last premolar and the first molar in a middle-aged animal; but in advanced life the disparity in the ages of these teeth, visible between four and five years, becomes once more apparent, the molar wearing down more than the premolar, and, consequently, becoming less deeply rooted in the jaw. This applies, however, chiefly to the upper jaw, since the teeth wear more evenly in the lower jaw.

The pattern of the deciduous molars is nearly the same as that of the premolars, but is slightly simpler. The crowns of these teeth do not grow to any great length (Fig. D.), and the roots are formed very early, beginning even three months before birth. Calcification of the deciduous molars commences at about five and a half months before birth, and their eruption occurs a few days after birth. The first two deciduous molars (*dm* 2 and 3) are shed at two years and a half, the third (*dm* 4) a year later. The premolars cut the gum a few weeks after the shedding of their respective milk predecessors, and are retained for life. The first true molar cuts the gum at about ten months after birth, the second between twenty months and two years, and the third, which only begins to calcify at two years, is cut at three years and three-quarters.

The following table, based largely on the classical observations of Girard, Mayhew, and Lecellier, as corrected by the more modern authorities, Brown, Goubaux and Barrier, Chauveau, and others, is here appended, with a view to showing at a glance the average dates at which the calcification, eruption, and shedding of the various teeth take place.

Teeth.	Calcification.	Eruption.	Shedding.
di 1	6 months before birth	A few days after birth	2½ years.
di 2	5 months before birth	6 weeks	3½ years.
di 3	1 month before birth	8 months	4½ years.
i 1	? 1 year	2½ years	—
i 2	1½ years	3½ years	—
i 3	3 years	4½ years	—
dc	2 weeks before birth	Absorbed without eruption	3 to 4 years.
c	? 2 years	5 years (Eruption rarely occurs in the female.)	—
pm 1	4 months before birth	? 3 months (Frequently absorbed without eruption.)	2½ years or later.
dm 2	5½ months before birth	A few days after birth	2½ years.
dm 3	5½ months before birth	A few days after birth	2½ years.
dm 4	5½ months before birth	A few days after birth	3½ years.
pm 2	1 year	2½ years	—
pm 3	1 year	2½ years	—
pm 4	20 months.	3¾ years	—
m 1	2 weeks before birth	10 months	—
m 2	4 months	22 months	—
m 3	2 years	3¾ years	—

W. G. Ridewood.

V.

A Passage-at-Arms over the Amphipoda.[1]

A FEW crustaceans have a gastronomic, and therefore a commercial, value. These are welcome to the eye, because they are familiar to the palate. Their distribution, both horizontal and bathymetric, may engage the thoughts of the purveyor, but the only part of their "range" which appeals to the consumer is that which extends from the market-place to the dining-room. There is little desire evinced for any further information about them, or for any intimate acquaintance with the rest of the class. A cheerful ignorance is not uncommonly cherished in regard to almost everything that concerns crustaceans, as little being known of their size, structure, habits, numbers, and uses, as of the extraordinary variation prevailing within the limits of the class, and of the characters by which the strange diversities are for the most part wonderfully linked together. Even in scientific circles carcinology is often regarded with only a languid interest, a sort of half-discontented surprise that any persons should be found willing to immerse themselves in so uninviting a subject.

With affairs in this posture, it must be matter for astonishment that at this very time two large and costly works should be published, not on crustaceans in general, but on a single subdivision of the class, a subdivision which is more or less coördinate with about a score of others. They treat not of crabs, nor of lobsters, nor yet of prawns and shrimps, nor even of wood-lice or barnacles. They treat of animals which in a general way resemble shrimps more than woodlice, but which, nevertheless, in certain respects, are nearer to woodlice than to shrimps. They treat, in short, of the Amphipoda, a part of the animal world which people in general have never heard of, and of which they will usually be disposed to confess that they never wish to hear. Yet the Amphipoda are extremely numerous in genera, in

[1] The present article is principally concerned with the "Gammarini del Golfo di Napoli. Monografia di Antonio Della Valle. Con un Atlante di 61 Tavole in Litografia. Herausgegeben von den Zoologischen Station zu Neapel. Berlin: Verlag von R. Friedländer & Sohn, 1893. Ladenpreis 150 Mark." 4to volume of text, pp. xi., 948. Volume of plates, sixty-one double plates, with 52 pages of explanation. Quinquennial subscribers to the series of monographs, in the "Fauna und Flora des Golfes von Neapel und der angrenzenden Meeres-Abschnitte, herausgegeben von der Zoologische Station zu Neapel," obtain them at half the published price.

species, in individuals. They are to be found almost everywhere, in sand and seaweed, in well and pond, brook, river, and lake, high up on mountains, deep down in oceans. They serve several useful purposes in nature. They are rarely injurious in any way to man, and never malignant towards his person. They are easy to handle, to examine, to preserve. They can be promptly distinguished from all other crustaceans by their organs of sight and breathing. From lordly crab to lowly shrimp, all the crustaceans which in England we are accustomed to eat have moveable stalked eyes. The eyes of Amphipoda are sessile. They usually have a pair. By exception they may have four eyes, three eyes, one eye, or none; but they never have them mounted on jointed stalks. In this respect they agree with the Cumacea and with the wood-lice and the rest of the Isopoda. But from these again they are completely distinguished by their breathing vessels of simple type attached to the appendages of the trunk. From the higher Crustacea in general and from the Cumacea they are further separated by a point of no little interest in comparative anatomy. There is no difficulty in observing that a lobster, for instance, has six pairs of jaws and five pairs of legs. But an Amphipod has four pairs of jaws and seven pairs of legs. The fact undoubtedly is that the two pairs of mouth-organs known as the second and third maxillipeds in the lobster are equivalent to the two pairs of claw-like feet known as the first and second gnathopods and attached to independent segments of the trunk in the Amphipoda. In the seven pairs of legs which the trunk of an Amphipod carries there is no commonplace uniformity, but each pair exhibits some more or less striking peculiarity of its own. Far from resembling a row of well-disciplined oars, these several pairs point in all sorts of directions, up and down, to front and rear, and to the sides. Hence, upon this hitherto nameless group of animals, some eighty years ago, Latreille imposed the name of Amphipoda, meaning "begirt with legs." As an amphitheatre is surrounded with radiating avenues between its tiers of seats, so have the legs of an Amphipod a manifold divergence, for the various purposes of grasping, crawling, climbing, tube-building, and maintaining the body's equilibrium. Unfortunately, there is as yet no accepted or acceptable English equivalent for the scientific designation of this Order.

Of the works above mentioned, one is being published at Christiania by Professor G. O. Sars. It aims at giving a fully detailed account of the Amphipoda of Norway. These, it may be said, are to a large extent the same as the Amphipoda of Great Britain, and, what is more, the account of them is written in excellent English. Can it be that the acute and learned author with prophetic eye foresees a time when a reasonable number of people in England will have been induced to care about Amphipoda, whether indigenous or exotic? No such faint and distant hope has influenced Professor Della Valle. Accordingly, he has composed his treatise on the

Amphipoda Gammaridea in his native Italian, a language which in this country is greatly admired and seldom read. But each of these works is so lavishly illustrated that its usefulness is in a large measure independent of the language in which it happens to be written. All but the blind, in short, may profit by the multitude of drawings, giving portraits in full, larger than life, and details still more highly magnified, of the various animals described. The coloured figures ought to convince even the most prejudiced that some of these little crustaceans, when alive, are charmingly arrayed. Whether in sober hues or bright ones, in plain or motley costume, they often exhibit a strangely interesting adaptation to the surroundings in which they are to be found. Some please the eye with their pearly delicacy of tinting. Some are resplendently striped or banded. Some are brilliant all over with rose or carmine.

The work by Professor Sars, though far advanced, is still unfinished. It is not included within the scope of the present review, except for the purpose of pointing out a special and singular contrast which exists between it and Professor Della Valle's kindred volumes.

Science is helpless without classification. Classification is helpless without names. There was a time when authorities like Linnæus and Fabricius were content to arrange all the crustaceans they knew under *Cancer*, *Oniscus*, and *Gammarus*. *Gammarus* itself was a daring innovation. But now there are hundreds of genera, and, as for species, they multiply like aphides. On this subject there are naturally two currents of opinion. Those who, by diligence of minute research, distinguish one living form from another wish to have their discovery ticketed with an imperishable label. A new species is accordingly named. But those who follow the footsteps of the discoverer are often disposed to think his distinctions trivial and his names superfluous. They afford but little satisfaction to a naturalist who has specimens to arrange in a cabinet, the fauna of a district to catalogue, or some considerable group to describe in a monograph. Thus, while scientific energy is continually adding to the nomenclature of science, there is a continual longing to simplify and reduce it.

Seeing, then, that from different standpoints writers have a natural bias towards different systems or principles of classification, there is little need for surprise that Sars should be found in favour of one system and Della Valle in favour of the other; but it is a remarkable coincidence that the two professors should at the same period have been treating the same subject, the same branch of it, and often identically the same details of it, from the two opposite points of view; especially as they have both, not without a great expenditure of money and time and labour, endeavoured in the most elaborate manner to introduce to the notice and favour of the public a group of animals about which, according to my own poor experience, the public at large, as I have already gently hinted, knows nothing and cares less. Professor Sars, as might have been expected from his indefatigable industry and keen powers of observation, draws distinction after distinction, and creates a rather formidable number of genera and species. Professor Della Valle, besides particularly treating of the Gammaridea of the Gulf of Naples, undertakes to pass in review those that are recorded from every part of the world. This extensive enterprise inclines him not unnaturally to ignore distinctions that seem to him insignificant, and to simplify matters by cancelling all genera and species of which the claims appear to be at all doubtful and obscure.

In a work belonging to the highly important series of "The Fauna and Flora of the Gulf of Naples," in a work so attractively printed and illustrated, and possessing the merits which are conspicuous in Professor Della Valle's volumes, one is tempted to welcome with open arms his nobly ambitious experiment in classificatory revision. Only, one has to be sure that the revision is based on adequate knowledge, and that it has been carried out with some consistency of principle. For, if not, more harm than good will have been done, and a huge heap of synonyms will have been added to the already towering mass.

Without further preface, then, I propose to discuss, as a characteristic example of Professor Della Valle's methods, the genus *Acanthozone*, to which he has devoted sixteen quarto pages. Under this single name he compresses together, like birds in a game pie, no less than ten previously established genera. *Acanthozone*, meaning "a girdle of spines," is a name highly appropriate to the porcupine-like amphipod for which Boeck, in 1870, invented it. Sir Richard Owen had already, in 1835, chosen for the same genus the name *Acanthosoma*, of equivalent meaning, but preoccupied. The name of Boeck's choice, accordingly, has superseded Owen's. But now Della Valle intervenes, and includes under this name the *Pleustes* of Spence Bate, dating from 1858, the *Paramphithoë*, instituted by Bruzelius in 1859, the *Amphithopsis* of Boeck, which belongs to 1860, and the *Calliopius*, so named by Lilljeborg in 1865. If all these four are synonyms of *Acanthozone*, each one of them has rights of priority over it, and the conglomeration of species which Della Valle has assigned to *Acanthozone* must all be withdrawn from it, and referred to *Pleustes*, while each of them is henceforward for ever saddled with the wrongly-attributed name to swell its list of synonymy. From a bold attempt at simplification such a result is truly deplorable. It is strange that the *Paramphithoë* of Bruzelius should not have sufficed for a beacon of warning, seeing that the nine species attributed to that genus by its author have since been distributed among half-a-dozen genera, amid which the original genus, from its vagueness, is an abiding source of confusion. It is a wandering star, of which the orbit is difficult to calculate. Probably its real rights, when acknowledged, will entitle it to take precedence of Mr. A. O. Walker's recent *Apherusa*.

From what may be thought an accidental error of judgment in attributing to one generic name privileges belonging to the date of another, it is necessary to pass on to matters more essential. A genus, to have any validity, must be defined. It was, therefore, with no little curiosity that I turned to Professor Della Valle's definitions to see how they would justify his amalgamating procedure. Clearly, definitions may be constructed, like fishing nets, with meshes very large or very small. They may be extremely comprehensive or exceedingly restrictive. The characters which one man would think sufficient for a genus might scarcely satisfy another for a family, or, on the other hand, might content his neighbour for defining a species. While we know so little, and are for long likely to know so little, of phylogeny, the various groups remain a question of scientific convenience. But at least a definition ought to define, and it cannot do this without some approach to precision. Now Professor Della Valle's definition opens in this wise:—"Body for the most part compressed (in *Acanthozone incisa* it is depressed), segmented regularly.—Back for the most part armed with spinous processes." The parenthetical exception refers to the "Challenger" *Chosroes incisus*, one of the few Gammarids in which the body is dorso-ventrally flattened, instead of being compressed from side to side, as is usual, not in one genus alone, but in almost every genus of the Gammaridea. Moreover, in this same species the back is perfectly smooth. It would seem, therefore, a most inconvenient perversity to include it in a genus named from the spinous processes with which in the type the back is armed, but which are here only conspicuous by their absence. The next statement in the definition is that the anterior antennæ have the lash very long, but this again is not true of *Chosroes incisus*. Another "Challenger" species, *Stenopleura atlantica*, is reduced to the ranks of *Acanthozone*, although it also is guiltless of any dorsal armature, and although the side-plates, instead of being, as Della Valle's definition requires, "of moderate size," are conspicuously small. Anyone who will be at the pains to compare the detailed figures of this species with those of *Acanthechinus tricarinatus* in the Report on the "Challenger" Amphipoda will, I think, admit that, if Della Valle has succeeded in his attempt to make a happy generic combination of these two, the time has indeed come when the lion may safely lie down with the lamb. To be sure, they both agree with the definition in having the "gnathopods subchelate." There are very few of the Gammaridea in which this is not so. The gnathopods, or two foremost pairs of legs, have in *Stenopleura* the penultimate joint or "hand" almond-shaped, seated in a short, cup-like "wrist," with a long "finger" curving over the margin of the hand to impinge against the "wrist," thus forming a sort of claw as effective perhaps as a lobster's, but of a different pattern. In all this there is nothing unusual or out of the common. It might almost be taken as a typical example of the structure of these limbs in the Gammaridea. Then look at the other genus, in which a very short finger does little more than cap a very elongate hand, and this filiform hand is attached to the apex of a wrist which is little stouter than itself though much exceeding it in length, an eccentric formation, about as remote from the ordinary type as any that is known. A definition which smothers up every hint of such a disparity, in the expression "gnathopods subchelate," rather confuses knowledge than clears its flow.

But, to leave the "Challenger" genera, in regard to which I may be suspected of fighting for my own hand, it will be worth observing that, just when Della Valle is making *Amphithopsis nodifera* of Sars into a species of the omnivorous *Acanthozone*, Sars himself is coining for it his new genus *Stenopleustes*. Nor is this the only new genus established by Sars over which the car of Juggernaut has rolled. For though his *Leptamphopus* and *Haliragoides* were published too late to be noticed in the Italian work, they were absorbed in *Acanthozone* by anticipation, slain, as it were, before they were born. Four other genera also, not mentioned in the generic synonymy, in whole or in part, lie crushed beneath the same destroying wheels. And yet, after all, if the precedents of history may be trusted, a resurrection awaits them. In vain, for instance, did Herbst ignore the genera established by Fabricius. For presently Leach arose to reinstate them and add largely to their number. In vain did Sabine in 1821 sigh over the multiplication of genera, and fancy that in conceding two to the Gammaridea he was conceding one too many. In vain, a little later, did Milne Edwards suppress the *Melita* and *Mæra* of Leach, for these are accepted even by Della Valle. In vain did Kröyer scoff at Owen's *Acanthosoma* as "a structure built upon the sand," for, as I have shown, it is this very genus that Della Valle has exalted to that sort of giddy height where absolute dominion can tolerate no brother near its throne.

In turning to the specific synonymy within this masterful genus, one discovers over and over again that species which Della Valle unites under a single specific name are considered by Sars to belong actually to different genera, even in one case to different families. A species described by Hansen and Vosseler from the Arctic regions is identified with another described by G. M. Thomson from New Zealand. So great a range of distribution is not unexampled, but it is far from common. Difference of habitat, however, counts for nothing, if the descriptions of the specimens agree. In this particular instance they happen to differ. The Italian professor does not take upon himself to say, "so much the worse for the descriptions," but he makes use of a formula almost as elastic in declaring that this, that, or the other portion of an organism is variable. The wonder is that with such a formula he can bring himself to admit any distinctive names at all.

The student will, perhaps, expect that the genus just discussed will be separated from that which follows it by some grand and striking features. He will find, on comparing the two definitions, that there is no single distinguishing character except such as is afforded by a longer or shorter slit in the apical segment of the tail. This is a character about as variable as any that could have been selected. Its worthlessness is proved, not only by its allowing species the most incongruous to be grouped together, but still more by its requiring species the most closely related to be placed in different genera.

It sometimes happens that a spiteful reviewer will pick out a solitary defect in a book and make believe that it is characteristic of the whole. The present criticism can scarcely be open to such a charge, because it is certain that the fault imputed is that which Professor Della Valle regards as one of the special merits of his performance. For this reason I the less regret that want of space prevents me from discussing with any fulness the treatment to which the Orchestidæ have been subjected. These include some of the most generally known Amphipoda. They enjoy the uncommon advantage of having English names. From a difference of habitat, obvious to the ordinary observer, some have been called shorehoppers, and some sandhoppers. Professor Della Valle unites a shorehopper found in England with an English sandhopper of a different genus, and calls them both by the name of a third distinct species found in Chili. To "the common shorehopper," *Orchestia gammarellus*, he attributes a vast synonymy. For disentangling right from wrong in this list, many pages of discussion would be needed. But if anyone will take the trouble to examine the question in regard only to the three New Zealand species, he will wonder on what fantastic principle such a list can have been framed. Variability, indeed, after the usual fashion, is pleaded. One notable point is the expansion of certain joints in the seventh pair of thoracic feet of the adult male. Of the three New Zealand species, one has this expansion, the other two are without it, and, of these two, one has the joints of the sixth pair of feet expanded, which is not the case in either of the other species. There are other differences, and, in fact, the three forms, so far from being one and the same species, belong to three distinct genera.

If human nature were not the paradoxical thing it is, there would be something amazing in the contrast between the lightness of touch with which Professor Della Valle creates and the heaviness of hand with which he demolishes. Among several subordinate instances of this which might be adduced, one assumes a not inconsiderable importance.

There is a strange parasitic or semi-parasitic Amphipod which was first found in the Mediterranean many years ago by the Rev. F. W. Hope. It was named *Guerinia nicæensis* by Achille Costa, and is nearly related to, if not identical with, the species later called *Trischizostoma raschii* by Esmark and Boeck, a mouth-filling generic name which unfortunately supersedes the preoccupied *Guerinia*. This

Gammarid has huge black eyes: the pair of them almost cover its head, and each is composed of a hundred ocelli or more. The organs of its mouth form a sort of tube with lancets for piercing the skin and sucking the juices of the shark or the whiting or other fish on which it may happen to settle. But the queerest thing about it is that in its great claws the clasping finger seems to be set on at the wrong side. The action of the claw is inverted, as the bending of a man's leg would be if the knees were placed at the back of them instead of the front. In the crustacean, the oddity is produced by the curvature of two little elbow-joints, which really reverses the normal position of the "hand" and "finger." Up to a certain age the animals hold their claws in the ordinary direction. Very few specimens have ever been obtained. These have varied in size from a fifth of an inch to an inch and one-fifth, and have ranged in locality from the Mediterranean to the borders of the Arctic circle. The three specimens from the neighbourhood of Naples at the disposal of Professor Della Valle were of medium size. They were all, as far as he could judge, of the male sex. He places them by themselves in a separate species. He goes further. He places them in a separate genus. Yet he admits that in the principal external features they agree with the northern specimens. It does not appear to have entered his mind how extremely improbable it is that there should be two very rare forms, superficially as much alike as twins, but distinguished, according to him, by several subordinate characters of the mouth-organs. On the character of those organs in his own specimens he confesses to having changed his opinion while his book was being printed. He is aware that all the northern specimens hitherto described have been females or young. He well knows, nay, he himself emphatically and repeatedly insists, that in the Amphipoda not only slight but important distinctions are often due only to differences of age or sex. In this particular kind of Gammarid it has been already shown that there is at least one remarkable variation between the adults and the young. Nevertheless, on the strength of trivial measurements arrived at by the dissection of minute organs, upon personal observation of only one sex in specimens of uncertain age, Della Valle goes out of his way to set up a new species and a new genus, such as, judging by his usual practice, he would assuredly have cancelled had any other naturalist been so venturesome. Any other naturalist might have done it without reproach. It is the common fault. Only to Della Valle himself was such a liberty not permissible, since the whole plan of his volume is one long protest against it. Perhaps he was moved by a tender consideration for the dwindling limits of natural history, which he has dispeopled of so many genera and species, and thought it necessary to restore the balance by a few of his own making. When once on the path of creation, he gives play to his inventive genius, and establishes a new suborder of Amphipoda. In this he at first intended to place the form which has just been discussed.

But, on examining a second specimen, he found that the conclusion grounded upon the first must be relinquished. Hereupon, the new suborder would have died of inanition but for a great piece of luck, which needs to be recorded and explained.

Considering how long and generally the scientific world has been contented with three main divisions or suborders of the Amphipoda, the institution of a fourth is a kind of epoch. To the Gammaridea, Caprellidea, and Hyperidea, we are now invited to add the Subhyperidea. The earlier three comprise an immense number of genera and species. In some of these the animals attain a very respectable size. Some, by their astonishing multitudes, are of considerable economic importance. By comparison, it may be a little surprising to find that a single genus, a single species, an animal of a fifth of an inch in length, is the sole representative of the new suborder. The author of it might well imitate the Greek father, who, on seeing a great crowd of people at the funeral of his small infant, could not refrain from expressing his regret that the corpse was not a bigger one. It is true that Nature is able to pack into the tiniest compass the most marvellous and singular characters. It is not to be forgotten that Darwin has established an order of cirripedes for a single genus, a single species, and an animal a tenth of an inch long. But anyone who studies the well-known work in which this performance occurs will see that Darwin ventures upon it with great diffidence, and that in an elaborate discussion he shows himself to be left without an alternative by the singularities of the form with which he is dealing. On the contrary, the form on which Della Valle bases his new suborder has not one single character which is not also found in other Gammaridea. It is a question of the fusion of two little plates in the maxillipeds. In most Gammaridea these inner laminæ are separate. In a few they are coalesced nearly to the tip. In the new suborder they are coalesced quite to the tip. But between the nearly and the quite the difference is nearly or quite inappreciable. Complete coalescence is the rule in the Hyperidea, yet even there it is not without exception, so that the classificatory importance attributed to this character breaks down on both sides. In short, the new suborder must be regarded as a mere vagary, a freak which, on the part of almost any other writer, or in a work of less merit and importance, might have passed without comment. The absurdity of the thing "leaps to the eyes," only because it is committed in the very volume which sets itself stringently to pare away every superfluity, to simplify the whole nomenclature of the subject, and to make an index expurgatorius of all divisions and subdivisions that have been obscurely defined or needlessly established.

It should be clearly understood that the protest here raised is not directed against Professor Della Valle's principle, but against his practice. He would have rendered a very solid service had he with discreet judgment cleared away the rubbish of all badly founded and indeterminable species. But the glaring instances in which he makes ignorance, however unavoidable, the standard of what should be retained and what should be dismissed, must materially detract from our faith in his decisions where he has had the means of speaking with well-grounded authority. To have accomplished successfully all that he boldly attempts would probably have required many more years than he was at liberty to bestow upon the task. In this matter of classification one cannot help regretfully reflecting how much more he might have done had he been content to do much less. In many other respects his volumes are exceedingly valuable. The attribution of the long-time obscure *Acanthonotus nordmannii* of Milne Edwards to the genus *Ampelisca* is a signal instance of Della Valle's acuteness. He gives a particularly important and original chapter on the deposition and fecundation of the eggs of the Amphipoda. He supplies, indeed, a whole library of lucid, and on the whole very accurate, information on the anatomy, development, and general biology of the Gammaridea, as well as a compendious bibliography interspersed with many useful observations. With such a work at his command the student of Amphipoda will find a flood of light thrown upon his researches. Those who have no previous knowledge of the subject can scarcely fail to have some interest in it awakened by the unexpected beauty of the coloured plates. It will be a strange thing if this department of nature can command attention and respect in Italy and yet continue to be slighted and disregarded in England. When the Zoological Station at Naples can afford to publish these noble volumes, it will be a strange thing if the self-respect of English naturalists can afford to ignore them.

T. R. R. Stebbing.

Tunbridge Wells.

SOME NEW BOOKS.

DEGENERATION.

DEGENERATION. By Max Nordau. Translated from the Second Edition of the German work. 8vo. Pp. 560. London: Heinemann, 1895. Price 17s. nett.

MAX NORDAU renders no more than his due to Professor Lombroso when, in his dedication, he acknowledges frankly and fully that his own work would not have been written but for the labours of the Italian. Indeed, it must be stated at once that readers of "Degeneration" will find no scientific facts or reasoning that do not occur in Lombroso's "Man of Genius." Moreover, the Turin professor wrote in a careful and scientific spirit, developing his arguments and drawing his conclusions without passion or prejudice. Max Nordau has the style and the manners of a Hebrew prophet crying out against a faithless generation. He attacks the eccentricities and the depravities of authors and painters, dramatists and musicians, with an envenomed rancour that adds interest to his pages if it tends to leave the reader unconvinced. Professor Lombroso ranged over all modern and ancient times, culling examples from every age and from every people. Nordau deals only with the modern world, and, therefore, has the advantage of treating only of people and subjects in which all of us are interested. Unless a reader has narrow sympathies in literature and art, unless he has been content with the art of *Punch* and the literature of the *Daily Telegraph*, he will find Nordau trying to convict him of having admired a maniac or an idiot. It is easy to predict that his book will have a circulation as wide in its English form as it had in the original, and where so much has been left to lacerate the feelings of those who pin their faith to some modern hero, we need not complain that a few of the violent personal attacks have been softened down so as to avoid the English laws of libel.

Nordau impeaches our age, finding everywhere in it the signs of degeneracy and of neurasthenia. Degeneracy he defines, in the words of Morel, as a "morbid deviation from an original type." He has no doubt but that if the originators of "all the fin-de-siècle movements in art and literature" were to be examined, it would be found that they or their immediate relatives would exhibit what Lombroso has shown to be the physical "stigmata" of degeneration. But the result of such an inquiry could not be made public. "Science, however, has found, together with these physical stigmata, others of a mental order which betoken degeneracy quite as clearly as the former; and they allow of an easy demonstration from all the vital manifestations, and, in particular, from all the works of degenerates, so that it is not necessary to measure the cranium of an author, or to see the lobe of a painter's ear, in order to recognise the fact that he belongs to the class of degenerates."

These modern leaders are not absolutely insane, although in a few notorious cases they have actually developed acute mania. The most of them are "higher degenerates," what Lombroso called "mattoids." The asymmetry of face and cranium finds its counterpart in asymmetry of mental faculties. "Some of the latter are completely stunted, others morbidly exaggerated. That which nearly all degenerates lack is the sense of morality and of right and wrong. For them there exists no law, no decency, no modesty. In order to satisfy any momentary impulse, or inclination, or caprice, they commit crimes and trespasses with the greatest calmness and self-complacency, and do not comprehend that other persons take offence thereat." The psychological roots of these characters are unbounded egoism and impulsiveness. Another mental stigma is emotionalism. The degenerate "is quite proud of being so vibrant a musical instrument, and boasts that where the Philistine remains completely cold, he feels his inner self confounded, the depths of his being broken up, and the bliss of the beautiful possessing him to the tips of his fingers. His excitability appears to him a mark of superiority. He believes himself to be possessed by a peculiar insight lacking in other mortals, and he is fain to despise the vulgar herd for the dulness and narrowness of their minds." Other signs are despondency, which appears as the pessimism of modern authors, apathy, which appears in devotion to ideals like the Buddhist Nirvana, and the predilection for reverie. But the cardinal mark of degeneracy is what Nordau calls mysticism. This is due to the lack of the power of attention in the degenerate. When an idea floats into the brain of one of them, the mental processes are vague,—the thing is not clearly and sharply defined, but is surrounded with a halo of confusing sentiment. What the mystic sees through a glass darkly are not things transcendental, unseen of other men, but merely what the normal person sees clearly, by him seen indistinctly. A still more common feature of the degenerate is abnormal sexuality. In its extremer forms, this is found in the so-called realism of many writers and painters, and in the undisguised obscenity of others. In another guise it mingles with mysticism and symbolism, and appears as a disordered religious emotion.

Seeking for such characters, Nordau ranges over modern literature and art. A few examples of his method will serve our purpose. Nordau regards religion as a typical character of the English race, whence their mental degeneration assumed a religious character. "The first result of the epidemic of degeneration and hysteria" (that had come to England from Europe) "was the Oxford Movement in the thirties and forties. Wiseman turned all the weaker heads. Newman went over to Catholicism. Pusey clothed the entire Established Church in Romish garb. Spiritualism soon followed, and it is worthy of remark that all the mediums adopted theological modes of speech, and that their disclosures were concerned with Heaven and Hell. The 'Revival Meetings' of the seventies, and the Salvation Army of to-day, are the direct sequel of the Oxford stream of thought, but rendered turbid and foul in accordance with the lower intellectual grade of their adherents. In the world of art, however, the religious enthusiasm of degenerate and hysterical Englishmen sought its expression in pre-Raphaelitism." Taking a poet like Rossetti, he analyses some of his verses line by line and finds in them "shadowy ideation, mixture of transcendentalism and sensuality, senseless combinations of mutually incompatible ideas." The refrains "possess the character of an obsession, which the patient cannot suppress, although he recognises that they are in no rational connection with the individual content of his consciousness." Tolstoï presents "the mystical intellect, the intellect without attention of the émotif." This diseased mind conveys to his consciousness "isolated impressions, which can be very distinct if they relate to his emotions; but it is not in the condition to connect these isolated impressions intelligibly just because it is deficient in the attention necessary to this object." His strange religious and sexual views are set down to a "mania of brooding doubt observable in many of the higher degenerates," and to a "pathological alteration in his sexual centres." The description of the state of mind which Gautier and Baudelaire held to require a new language, a language embroidered with wild and bizarre graces, with curious felicities and apparent barbarities culled from every possible source, Nordau declares to be "simply a description of the disposition of the mystically degenerate mind, with its shifting nebulous ideas, its fleeting formless shadowy thought, its perversions and aberrations, its tribulations and impulsions." He describes Huysmans' exaggeration of Gautier's idea as a "delirium and debauch in pathological and nauseous ideas of a deranged mind with gustatory perversion." Huysmans himself "is the classical type of the hysterical type without originality, who is the predestined victim of every suggestion."

These few examples will serve to show the general method of this remarkable book. It must be added that practically no well-known writer of the last forty years escapes inclusion in Nordau's gallery of degenerate fools. There is no question whatever that the book is vastly interesting and vastly amusing. The author is learned and witty; he exposes and lashes many follies and many imbecilities, and probably every reader will agree as cordially with some of the criticisms as he will be annoyed and indignant at others, although no two readers are likely to select the same passages for praise and for reprobation.

But the general thesis of the whole book is another matter. Even Lombroso, taking purely physical characters, pushed his comparison between genius and insanity to the wrong side of absurdity. Every reader of his book will remember that he chose Darwin as an appropriate instance. Darwin suffered from dyspepsia; he could not bear heat and cold; he had curious crotchets, as in the matter of using an old and untrustworthy chemical balance, and in being niggardly with writing paper. He had large ears, a short nose, and a pronounced frontal ridge, while one of his uncles became mad. The extraordinary folly of finding arguments for degeneracy in such foibles or physical peculiarities amounts to an obsession at least as characteristic of the insane as any of the stigmata found by Lombroso and his German follower. It requires the smallest observation to detect that such characters are to be found in all sorts and conditions of men, and that geniuses and the insane enjoy no monopoly of them. But Nordau's argument from mental characters is still more unsatisfactory. At first sight many of his comparisons are interesting and suggestive; notably his account of poetry actually written by the insane. But the characters of the insane and the degenerate are not *sui generis*: they are merely abnormal forms of characters found among all persons. Using precisely the same arguments from analogy as Nordau uses, it would be easy to show that every man, woman, or child exhibits some features, some emotional or mental phenomena, displayed by the insane and the degenerate. It is certainly the case that a few poets, painters, and writers are insane. But it is equally the case that a number of potmen, ploughmen, and artisans become insane. The eccentricities and insanities of the unknown come under the notice of the parish authorities and the county councils; the eccentricities and the insanities of genius are betrayed to all the world. To say that Ruskin, Tolstoï, Ibsen, Zola, and so forth, are degenerates, and to support it by random citations and bitter invectives, is to elaborate an ill-natured and stupid paradox. If Max Nordau wished to convince people, indeed, if he himself believed in his own view, he would have dismissed rancour from his pages, and developed his theory in a calm and scientific spirit.

Physiology in the Garden.

A Popular Treatise on the Physiology of Plants, for the Use of Gardeners, or for Students of Horticulture and of Agriculture. By Dr. Paul Sorauer. Translated by F. E. Weiss, B.Sc. 8vo. Pp. x., 256, with 33 illustrations. London: Longmans, Green & Co., 1895. Price 9s. nett.

Dr. Sorauer's "Popular Plant Physiology for Gardeners" has been in use for four years, and its excellence for its purpose needs no comment. It is an admirable treatise on plant physiology, illustrated by and applied to the common practices of horticulture, and without doubt a book worth translating; but Professor Weiss's translation, though in certain respects a useful piece of work, if only for bringing Sorauer within the reach of a greater number of students, does not always compare favourably with the original. To begin with, it is seriously handicapped by its high price, 9s. It is twice as thick as the German publication, and the binding and general get-up are superior, but we think that gardeners and the like for whom it is intended would have preferred a humbler and cheaper volume. Again, the book is, or professes to be, a translation, not a revised and enlarged edition, and therefore it is unfair to make on the title-page (and in advertisements of the book) an important addition to its professed scope. Sorauer wrote purely for gardeners and horticultural students and throughout keeps this aim in view; it is not a book for agriculturists, and it is unfair to a large class of would-be learners and students to introduce agriculture on the title-page. Of course, the broad botanical principles are the same for both branches, but beyond these the agriculturist will find comparatively little in the way of help or suggestion. Again, a man must be something more than a botanist to reproduce perfectly a work which enters into the technicalities of horticultural operations, and the want of this applied experimental acquaintance with the subject is evident in comparing translation and original. The translator would have done better had he stuck faithfully to his copy. In diverging from this he often obscures the sense or omits important details. Look, for instance, at § 28, which describes the effect of different methods of pruning. The paragraph in which is mentioned the formation of the woody excrescences known as burrs is omitted, and so is the one that follows. In the next we read in the translation that "In the case of cherries and stone-fruited trees . . . the branches which have once borne flowers will not do so again, but remain bare. The formation of flowering buds, therefore, progresses gradually towards the ends of the branches; while in pears and apples fruiting spurs may bear flowers year after year, the spurs increasing continually in thickness." Translated literally it runs thus: "Those parts of the branches of stone-fruit-trees which have once borne fruits do not develop fruit buds again, but remain bare. Here the formation of flower buds progresses gradually higher up the branch and leaves the wood behind bare, while in apples and pears the same parts of the branch (often continually increasing in thickness) develop fresh fruit buds and are thereby always clothed." The next paragraph describes phenomena especially characteristic of pear trees, and in part of only some varieties of the same. Of this, six lines are omitted in the translation, and there is also no mention whatever of the pear. Throughout the book slovenly translations are continually cropping up. Thus in section 18 (p. 105), line 2, bast-system is a correct rendering of "Rinden-Körper" and "tissues meant for conduction" will do for "Leitungswegen" five lines further down; but why is "Leitungswege" in the next line rendered "phloem bundles"? And is "medullary bundles exist, therefore, in plants which withdraw their reserve substances into the roots" a translation of "Markständige Stränge finden sich hier also bei Pflanzen, welche im Winter 'einziehen'"? On p. 42, in speaking of the value of kainit (wrongly stated to be a sulphate and *chlorate* of magnesium), the translation says, "In light soils, in which there is little humus, clay, or other absorptive substances, manuring with kainit would enrich the soil . . . The common salt which is also present acts as a solvent, etc." This means the common salt which is present in the soil. Sorauer says, "The common salt present *in the potassium salt*," *i.e.*, in the kainit. The introduction of terms by the translator does not always induce to increased clearness. In describing heterostyly in the primrose, on p. 239, we read: "Some specimens have a long style, so that the stigma will reach to the top of the corolla-tube (*pin-eyed*)." "In other specimens the reverse will be the case (*heterostyly*)." This unhappy contrast of the special case "pin-eyed" with the general principle "heterostyly" does not occur in the original. On p. 34 the chlorotic condition resulting from absence of iron in the soil is erroneously explained as *etiolation;* this also is an alteration.

Seeing that such departures from the original are allowed, it is a pity that a few useful alterations have not been made. Thus, the employment of the word "lime" to indicate sometimes the carbonate, sometimes the hydrate of calcium, is confusing. Still more so are the two statements relating to the fixation of the free nitrogen of the air. On p. 32 we are told that leguminous plants absorb nitrogen from the air by their green leaves, while on pp. 49-50, in an excellent review of the subject, the root tubercles are correctly described as centres of assimilation of atmospheric nitrogen. The account, on p. 122, of formation of starch as a result of loss of water by the cell from increased transpiration in sunlight, when "we may imagine the starchy substance to be forced out of the thickened (*concentrated*) cell-sap," is scarcely in accord with modern views.

The English itself is not always beyond reproach. We find, for instance, *will* for *shall*, and on p. 44, "Besides these cryptogamic or flowerless plants, many more highly-organised plants afford us examples of plants, etc.," with nine other "plants" in three-quarters of a page.

In our opinion an opportunity has been missed. It was possible with but very little departure from the original copy to have produced a cheap, reliable, and scientific handbook for gardeners and others interested in gardens or horticulture generally. Such a book would have had a large sale, for it would have better supplied a greater want than did Fream's "Agriculture," the sale of which, a few years ago, was enormous.

Newton's Birds.

A Dictionary of Birds. By Alfred Newton, assisted by Hans Gadow. Part iii. London: Adam & Charles Black, 1894. 7s. 6d. nett.

Subscribers to this invaluable work will have made themselves by this time fairly well acquainted with the contents of part iii.; for the sake of those who are not already subscribers, we indicate briefly the nature of some of what we deem to be the principal articles.

Dr. Gadow's contribution on the muscular system will certainly be welcomed. Those of us who are inclined to place faith in the muscular system, as a key with which to unlock the vexed question of Taxonomy, will probably "have doubts" as to the stability of that same faith when we read that "the taxonomic value of muscles is theoretically great, but very limited when put to a practical test. . . . To pick out a few of the most variable muscles of the leg, and to arrange birds according to their presence or absence . . . is an easy, but scarcely serious, mode of investigation. . . It is certain that similar muscular combinations in two or more birds do not necessarily mean relationship . . ."

By these statements, however, we are not, of course, to imagine that Dr. Gadow would entirely veto the use of the muscles as a factor in classification, but that he would rather urge caution and discrimination. Several figures help to make clear what is, at best, a difficult matter to describe; not the least acceptable will be those representing the now celebrated flexor tendons of the foot—the flexor perforans digitorum and flexor longus hallucis—which, it will be remembered, a few years ago were brought into such considerable prominence by that enthusiastic ornithotomist, the late Professor Garrod.

The articles Owl, Parrot, Partridge, and Pheasant are delightfully readable, the last two especially so, carrying us back, as they do, to the days before the use of firearms; and, later, contrasting the conditions of sport obtaining up to the middle of this century with those now in vogue. We have, indeed, besides a mass of valuable historical information relative to the introduction of these birds, a sort of supplementary essay on game-preserving, shooting, etc.

A unique feature of this dictionary is the footnotes. We feel almost tempted to regard these as more important than the main part of the book itself. These footnotes, though never more than a few lines in length, are yet essays in themselves: now we have knotty points in etymology, now an anecdote, or some gem rescued from the classics, and, again, something drawn from the Professor's vast resources which have accumulated during his long life of active work. They contain, in short, a kind of information which *cannot* be appreciated until experience has taught what laborious research and wide knowledge have been brought to bear upon their compilation.

The present number includes articles from "Moa" to "Sheathbill," hence it will be seen that but the merest outline has been given of its contents. If we mistake not, the good wine is yet to come in Professor Newton's "Introduction," the appearance of which we are looking forward to on the very tip-toe of expectation.

Probably no book which has ever seen the light has been allowed to be entirely perfect—especially by the reviewer—and we have already intimated that the "Dictionary of Birds" must not be excepted. We here again reiterate our objections to Swainson's figures, which, we feel, have been unduly honoured in finding a place in the pages of this work. Next, as regards the figures illustrating Dr. Gadow's contributions. These, although helpful, are not always, we venture to think, sufficiently well labelled; for instance, those showing the tendons of the foot and the diagram of the peripheral nervous system of the bird. More especially do these remarks apply to the nerves of the sacral plexus. From the point of view of avian anatomy, this plexus contains the most interesting nerves of the body, and yet there is nothing to aid the uninitiated to differentiate one nerve from another. The more important of the visceral nerves are, however, duly identified, yet these have comparatively only a secondary interest. Points like these, however, are but minor blemishes; few but specialists interest themselves much in anatomical questions, and such have Dr. Gadow's larger works to consult.

This is a work unique of its kind, embracing, as it does, the whole field of Ornithology, and is based upon the latest research and discovery both in England and abroad. To those who live out of reach of large libraries, and who do not read any language but their own, it will prove a boon indeed.

W. P. Pycraft.

The Stuff that Dreams are made of.

Imagination in Dreams and their Study. By Frederick Greenwood. London: John Lane, 1894.

We are in full agreement with Mr. Frederick Greenwood, even though he quote Dr. Ward Richardson, as a man of science, against himself, that a reference of dreams to the action of the sleepless sympathetic system is not so much as the beginning of an explanation. In whatever way they be roused, whether by an uneasy qualm of the full stomach, or by a cold breath playing on the naked foot, the vivid and elusory inhabitants of our dreaming brain are citizens of the mind itself. And we welcome his book as a delicate and graceful contribution to the study of mind.

The little book is divided into two chapters, of some hundred short pages a piece. Of the second, dealing with the study of dreams, we have little to say. It disclaims narrow materialistic and sceptical "explanation" of them as being shallow and self-satisfied conceit; it discards supernatural explanation but with a lesser zeal. It calls on us to study them; with our reasons, not clothed with an assumed and conventional garb of humorous tolerance, but naked and unashamed.

The other chapter, on imagination in dreams, sets forth the matter and the suggestion, in what we take to be a deliberate mingling. Waking imagination, although the most compelling of faculties, seems to piece together only the memories of things seen or heard or read. Even under the powerful influence of religion, the imagination, trying to picture heaven, has invented nothing unknown on earth. But the dreaming imagination seems creative. Sometimes it peoples the visions of the night with known faces; but sometimes, too, the faces that come are arresting, insistent. They impress our consciousness by strange significance, or, and this is even more striking to the reflective mind, by trivial detail, and the mind searches in vain for them among its memories. Are such things in reality memories, and are they unrecognised because the memory is asleep? "Is it that amongst the drowsy faculties is the one which has custody of the dim population of the brain called remembrances?" Mr. Greenwood doubts this; holding that the memory when we are asleep is actively called upon in our dreams; and that the memory when we awake, although active because it is telling us our dream, still fails to recall the intruding face. The other suggestion is that the faces were unconscious memories, that they had slipped into the brain in some forgotten time, eluding the gatekeeper, consciousness. Against this view, he adduces the extraordinary vividness of the impressions: it were unlikely that, as they impress us in dreams by their intrinsic force, they would not have impressed the waking consciousnesss when they first appeared.

These two sides of the dreaming imagination, intense vividness and apparently objective unexpectedness, Mr. Greenwood supports by many examples. In some of these, dialogues in a dream leading to surprising and unlooked-for ends, the impression of objectivity becomes so strong that Mr. Greenwood is led almost into a conception of the actual objectivity of dream-impressions. He gropes vaguely, on the one hand at the transference of thought from mind to mind without the mediations of the known senses, on the other, at suggestions of a dual personality, with their crude references to the bilateral symmetry of the brain. But he stays himself from a full acceptance of either notion.

In his assured conclusion that the imagination is more active, more creative if you will, in dreams than during waking hours, we are in agreement with Mr. Greenwood. But we see no reason to suppose that it invents materials. The treasures of the memory are unrolled before it, rapidly, and without the distraction given in waking hours through the open channels of the senses. To the seer in trances, to the genius by conscious or unconscious abstraction, is given the power of vividly working up accumulated materials; the same power, when the senses are partly or wholly drugged by sleep, is the common lot of humanity. We are all geniuses and seers in our dreams. In dreams the reason appears to lag behind the consciousness of memories, and things appear external to us because of this dislocation.

Mr. Lydekker's Royal Natural History, published by Frederick Warne and Company, has now completed the history of the Mammalia and commenced the birds. We trust the high standard attained by the Mammalia will be kept up, and only wish that Messrs. Warne & Co. would have the pluck to give us a general natural history of the Invertebrata in as solid a form as they have the Mammalia.

"A New Measure for Old Time," by An Amateur, is said to be an easy method of finding the age of the earth's sedimentary crust, etc., etc. This is how it is done. A period of 10,465 years being half the time occupied by the earth in revolving about the line of the Apsides, was obviously required for the deposition of each layer of limestone, shale, or what-not in the crust of the earth. Multiply the number of layers by this sum and you have the age of the earth, and the problem that baffles physicists and geologists is solved. "Easy" is not the word for it.

The Glastonbury Antiquarian Society has reprinted as a shilling pamphlet, published by Barnicott & Pearce, Taunton, various letters and papers by Messrs. R. Munro, A. J. Evans, A. Bulleid, and Professor Boyd Dawkins, on the British Lake-village near Glastonbury. The paper by Professor Boyd Dawkins appeared in our own pages, in 1893.

From Mr. Fisher Unwin there has come a small shilling pamphlet, by Mr. Henry Larkin, on Elliptical Orbits, their distinctive mechanical characteristics and their possible origin. It is out of our line, but some of our readers may be glad to learn of its existence.

OBITUARY.

JOHN WHITAKER HULKE.

BORN 1830. DIED FEBRUARY 19, 1895.

THIS eminent surgeon and oculist, and equally distinguished palæozoologist, was born at Deal, and was the elder son of the successful surgeon, the medical attendant of the great Duke of Wellington. His ancestors, the Hulchers, left the Low Countries during the Alva persecutions of the 16th century. Hulke was educated at King's College School, spent a couple of years in Germany, and then entered the King's College Medical School in 1849. 1855 found him at the Crimea, where he was attached to the general hospital before Sebastopol. He became a Fellow of the Royal College of Surgeons, and assistant surgeon to the Royal Ophthalmic Hospital, Moorfields, in 1857, and in 1862 commenced his long career as surgeon to the Middlesex Hospital, of which place he was senior surgeon when he died. It is supposed that his death resulted from a chill taken while attending to a patient at the latter Hospital, his strong sense of duty taking him there at an early hour in the morning to attend a case of strangulated hernia. Hulke became connected with the Royal College of Surgeons in 1880, when he was attached to the Board of Examiners. In 1881 he became a member of Council; in 1888, Vice-President; and in 1893, President. He was President of the Pathological Society in 1883, and at the same time held the same office at the Geological Society. In 1893 he became President of the Clinical Society. He was to have delivered the Hunterian Oration at the College of Surgeons on February 14 last; this oration, which was never delivered, and which was probably the last manuscript Mr. Hulke ever penned, is to be found in the *British Medical Journal* for February 23, the journal to which we are indebted for the above particulars, extracted from a sympathetic and appreciative obituary notice.

Of Mr. Hulke's services to palæozoology it is impossible to speak too highly. Exact and cautious, his work was sound and reliable; he was never in a hurry to publish his papers, and did not indulge in the fanciful reproductions on insufficient evidence so common with others working in the same field. His first palæozoological paper was published in 1869, and dealt with a saurian humerus from the Kimeridge Clay of Dorset. In the same year followed an important communication on the Kimeridge *Steneosaurus*, and in 1870 three more papers descriptive of specimens from the same district. All these Dorsetshire fossils were in the collection of Mr. Mansel-Pleydell, and are now in the British Museum. In 1870 he began to study the *Iguanodon* and other remains from the Isle of Wight, preserved in the Fox collection, now at Cromwell Road; and between 1873 and 1883 some twelve papers proceeded from his pen on the subject, remarkable alike for their penetration and clearness. Of these perhaps the most important were those on *Hypsilophodon* (*Phil. Trans.*, 1883), *Polacanthus* (*Phil. Trans.*, 1882), *Ornithopsis* (*Quart. Journ. Geol. Soc.*, 1879 and 1880). Besides these Wealden forms, Hulke found time in his busy life to study and describe a new dinosaur, *Iguanodon prestwichi*, from the Kimeridge Clay of Cumnor; he also made important contributions to our knowledge of *Megalosaurus* and *Poikilopleuron*. His addresses to the Geological Society, when President, dealt with the shoulder girdle of the Ichthyosauria and Plesiosauria and with the *Iguanodon* discoveries made in Belgium by Dollo and others. But before studying palæozoology, Mr. Hulke wrote several valuable papers on the eye of the reptilia and mammalia, a subject in sympathy with his special medical researches. His last contribution to our science was made on the shoulder girdle of the Ichthyosauria and Plesiosauria, and was supplementary to his address on the same subject before the Geological Society. This paper was read in 1892 before the Royal Society. Mr. Hulke received the Wollaston medal from the Geological Society in 1887.

Under a somewhat abrupt and heavy exterior there was an abundance of warm and generous feeling. More than one of his younger scientific colleagues are grateful to the eminent oculist for professional advice freely and kindly given. John Whitaker Hulke had no bitterness for those that opposed him: he was scrupulously exact in his punctuality and attention to business, and his demand for that in others made him appear more brusque than he really was. That he was a genial companion when in the field, those who have had the rare privilege of accompanying him on his rambles can testify. Medical science has lost a devoted servant, the hospital poor a sympathetic friend, and zoology a master whom it could ill afford to spare.

GASTON, MARQUIS DE SAPORTA.

Born July 28, 1823. Died January 26, 1895.

By the death of the Marquis de Saporta palæontological science has suffered a severe loss. Pre-eminent among those palæobotanists whose special study has been in the domain of Mesozoic and Cainozoic botany, Saporta has left a lasting and worthy memorial of a long life of scientific work. It is impossible to do justice to his cnotributions to the study of fossil plants within the limits of a short notice; a perusal of a list of his papers in the Royal Society catalogue affords some idea of the main lines of his work. In addition to numerous and exhaustive memoirs on the Tertiary floras of France, there are larger treatises in connection with which Saporta's name has long been widely known beyond the circle of fellow workers in his chosen study. His semi-popular "Le monde des plantes avant l'apparition de l'homme" affords a good example, not only of his wide knowledge of fossil botany, but of the finished literary style which always characterised his writings. In addition to the descriptive details in his voluminous monographs on Tertiary and Mesozoic floras, we always find that he has paid special attention to those wider problems, which his thorough and extensive knowledge enabled him to deal with; such questions as the distribution of fossil floras, their comparison with recent vegetation, and the interesting subject of climatic conditions are constantly kept in view as the great aim of his palæobotanical investigations. Of particular interest to English readers are the volumes on the Jurassic flora of France. In this excellent series of monographs, only recently completed, Saporta did not confine himself within the limits of a single country, but by the comprehensive scope of his work raised it to the level of an invaluable epitome of European botany in Jurassic times. Some few years ago Saporta played a prominent part in the much-controverted question as to the nature of certain so-called fossil algæ; in strenuously upholding the algal nature of the numerous and varied markings or half-relief patterns on the surface of rocks, it would seem that his botanical enthusiasm had been allowed to get the better of scientific reasoning. The work which he published in conjunction with Professor Marion on "The Evolution of the Plant Kingdom" suffers very materially from the questionable nature of some of the evidence, which is drawn from ill-preserved plant fragments or purely inorganic markings. A few weeks ago Saporta completed an important monograph on the Mesozoic flora of Portugal, in which extremely valuable additions were made to our knowledge of the younger floras of that period.

One of Saporta's distinguished fellow-workers in palæobotanical science, in a letter to the writer, bears eloquent testimony to his genial and kindly character; those of us who knew him only as a correspondent, must have been struck by the generous spirit in which he was always ready to place his extensive and detailed knowledge at the disposal of younger students. The Marquis de Saporta passed away at the age of seventy-two, on January 26, at his residence in Aix-en-Provence; he retained to the last "un jeunesse d'esprit, un enthousiasme, vraiment exceptionnel même chez les plus jeunes."

The death of Thomas Twining on February 16 removes from our midst one of the earliest advocates of technical education.

Mr. Twining was born at St. Faith's, near Norwich, in 1806, and had been an invalid and cripple since 1825. He superintended the collection forming the Economic Museum of the Society of Arts, since distributed to the South Kensington Museum and the Polytechnic Institution, some of which was destroyed by fire in 1871. He lectured on the science of common life, and published a smart volume on the subject, entitled "Science made Easy."

Emile Bayle, who died last January, was born at La Rochelle in 1819, and was educated as a mining engineer. Between 1845 and 1881 he was Professor of Geology and Palæontology at the École des Mines in Paris, and also held the Professorship of Geology at the École des Ponts et Chaussées. His best known works are "Cours de Mineralogie et de Géologie," 1869, and "Sur les Fossiles recueillis dans le Chili." He also collaborated in "l'explication de la Carte Géologique de la France." To his energy the École des Mines owes its splendid collection of fossils.

The death of Dr. Charles Girard, at Levallois, at the age of 73 years, destroys a link with the past. Born at Mulhouse, he left Europe in 1855 for Cambridge, Massachusetts, but afterwards went to Washington, where he reorganised the zoological collections of the Smithsonian Institution, returning to France in 1866. He was a friend of Louis Agassiz, and was the author of numerous papers on the fishes and reptiles of America.

Deputy Inspector-General David Lyall, M.D., whose death was recorded on March 1, will be remembered as assistant-surgeon of the "Terror" in Sir James Ross's Antarctic expedition. He also went in the "Acheron" as naturalist and surgeon in the New Zealand surveying expedition, in 1847; and was senior medical officer in Belcher's expedition in search of Sir John Franklin.

Dr. Jollet, the well-known anthropologist, has died at Grand-Bassam. He made large collections, and studied the crania of criminals, of the natives of New Caledonia and the New Hebrides; but death has taken him in the flower of his age, before he could give to the world his observations on his valuable collections. Those collections are now in the Natural History Museum at Paris.

A portrait and memoir of Dr. Beaven Neave Rake, whose death we recorded in our December number, appear in *Trinidad Field Naturalists' Club*, vol. ii., no. 5.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

SIR JOSEPH LISTER, F.R.S , has accepted the presidency of the British Association for the meeting at Liverpool in 1896.

MR. FRANCIS GOTCH, F.R.S., Professor of Physiology at the University College, Liverpool, has been elected Waynflete Professor of Physiology in the room of Professor Burdon Sanderson ; as the former assistant of Professor Sanderson, Mr. Gotch is well known in Oxford, where his return will be warmly welcomed.

Mr. T. H. Kearney, who only recently became curator of the Columbia College Herbarium, has become assistant in the Botanical Section of the U S. Department of Agriculture. Dr. F. von Tavel has been appointed curator of the Botanical Museum of the associated Polytechnics in Zürich. Dr. Alfred Koch becomes extraordinary Professor of Plant Physiology at Göttingen. Professor L. Guignard, President of the Botanical Society of France, succeeds the late Professor Duchartre in the Botanical Section of the Academy of Sciences at Paris. Mr A. B. Badger, of New College, Oxford, has been appointed Organising Secretary for Technical Instruction, Carnarvonshire, with special charge of scientific education. Mr. H. C. Chadwick, who has done some work on the Echinoderms of the Liverpool area, is now Chief Assistant in the Museum at Bootle, the natural history collections of which have long been in great need of attention. Mr. R. T. Günther has been elected to the Geographical Studentship at Oxford University.

DR. W. BRANCO has, according to the *Rivista Italiana di Paleontologia*, resigned his Professorship at Tübingen. Dr. Branco is best known for his researches on the embryology of extinct Cephalopoda, but has recently published an important work on the embryology of extinct volcanoes, " Schwabens 125 Vulkan-Embryonen."

THE awards of the Russian Geographical Society have been bestowed as follows :—Constantine medal to Professor S. Nikitin ; Lütke medal to P. K. Zalesski for his work in Turkestan ; the gold medal to Dr. N. A. Karyshev for Economics ; the Prjevalski award of £60 to Dr. V. A. Obruchev for his travels in Turkestan and Central Asia. The Prjevalski silver medal was awarded to Baron Toll and Lieutenant Shiliko for their recent journey in Siberia.

IN recognition of the distinguished services of Professor Guido Cora to geographical science, a committee has been formed to consider and report upon a suitable testimonial to him. Professor Cora has devoted his life and his fortune to the advancement of geography, and already in 1873, when he was a young man of twenty-two, the Royal Geographical Society of London elected him an honorary member, awarding him their gold medal in 1886. The present testimonial is promoted by his old students of the Royal University of Turin, and they invite the coöperation of all who desire to honour the founder of " Cosmos." Those desirous of expressing their consideration for the professor should communicate with Professor Dr. U. Menicoff or Professor Dr. P. Revelli, at Turin.

Mr. Robert Etheridge, jun., whose appointment as Curator of the Australian Museum was announced in our last October number, has been awarded the W. B. Clarke Medal of the Royal Society of New South Wales for his researches into the geology of Australia.

Cambridge University has conferred the degree of Doctor in Science (*honoris causâ*) on Sir William Macgregor, Administrator of British New Guinea.

The friends and admirers of Sir Henry Acland are taking the opportunity of his retirement to solicit subscriptions towards the endowment of the Sarah Acland Home for Nurses. His scientific colleagues intend to raise some memorial to him in connection with the Oxford Museum, which he did so much to found.

An important draft ordinance has just been issued in Edinburgh under the Universities (Scotland) Act, 1889, 52 and 53 Vict., c. 55, relating to the Swiney Lectureship at the British Museum (Natural History). Dr. Swiney, on making the foundation, stipulated that the lecturer should be a doctor of medicine of the University of Edinburgh. This has frequently placed the Trustees of the Museum in an awkward position in their choice of lecturer, the number of M.D.'s of Edinburgh who take up geological pursuits being very limited. The present ordinance ordains as follows: "It shall be competent to the Trustees of the British Museum to appoint to the said Lectureship any graduate in any Faculty in the University of Edinburgh who has obtained his degree after examination." The ordinance can be obtained of Menzies & Co., Glasgow, price one penny.

We understand that the authorities of the Biritish Museum (Natural History) have acquired by exchange a plaster cast of one of the skeletons of *Iguanodon* from the Brussels Museum. The skeleton from which the cast has been made is complete, and the public will shortly have an opportunity of examining this interesting animal, for it is to be set up in the Gallery of Fossil Reptiles in the Museum.

A further important addition to the collections will shortly be made; the executors of the late Mr. Hulke have decided to hand over the whole series of fossil Reptilia collected by him in the Isle of Wight and elsewhere. These will include the type-specimens of *Iguanodon seelyi*, and some important remains of *Hypsilophodon*.

The professors and assistants of the Natural History Museum at Paris held a meeting on January 29 under the presidency of Professor A. Milne-Edwards, and decided to hold periodical meetings, and at the same time to found a journal to be called *Bulletin du Muséum d'histoire naturelle*. The editorial secretary will be M. Oustalet; the secretaries for Zoology and Anatomy, M. Bouvier; for Botany, M. Poisson; for Palæontology, Geology, and Mineralogy, M. Marcelin Boule; for Physiology and Pathology, M. Phisalix; for Physics and Chemistry, M. Verneuil. The *Revue Scientifique*, in making the announcement, gives the text of M. Milne-Edwards' speech, from which we extract the following sentences:—"The professors of the Museum have thought that it would be an advantage for our establishments to draw together, by closer ties, those men who, each in their own department, assist in the advancement of science. We wish our Museum to become one large family, where the elder direct the way of the younger, give them useful advice, and encourage them in labour often dry at first, where the young hasten to overtake their predecessors, at last to become masters. We wish an intimate union to exist between the professors, the assistants, the preparers, laboratory students, probationers, exhibitioners, correspondents, and travellers who aid us with so much devotion to form our fine collections. By making the efforts of all to converge towards one end, we shall obtain a considerable result, and the work accomplished in common will turn out more easy and more fruitful by reason of the emulation which such union will provoke. The best method for the realisation of this ideal is to call together all the members of the Museum at regular monthly intervals, where each bringing his share, will lay before the meeting the researches he pursues, the facts that he observes, and the novelties which may prove interesting to the establishment."

Such is the excellent and liberal view of the officers of the Paris Museum, a view which cannot fail to bring about the end desired by the distinguished head. Let us hope that Professor Milne-Edwards' example will be followed in other institutions of a like nature.

From the Report of the Manchester Museum from 1890–1894, just issued, we learn that the opening of the Museum on every weekday has resulted in a marked increase in the number of visitors. A more intimate relation with local societies has been established, and permission has been given to the Manchester Conchological Society to hold its monthly meetings in the Museum buildings. The labelling of the exhibited specimens has been improved in that the Museum now employs a regular printer, who turns out from 100 to 200 labels each week. Distribution maps have been adopted and specially coloured so as to show range both in time and space of the object exhibited. The petrological collections have been examined, and the rock-sections catalogued and arranged; the Forbes collection of meteorites has been catalogued; the minerals have been re-arranged by Dr. Burghardt on a chemical basis. Mr. Ogilvie Grant, of the British Museum, is naming the bird-skins, which are being arranged in systematic order. The Insecta have been thoroughly overhauled by Mr. J. Ray Hardy, and several important collections have been incorporated. Mr. Pearcey has arranged a table case of foraminifera, and the collection as now exhibited is one of the best in any public museum. The botanical collections have been improved by the exhibition of flowers, leaves and fruits in spirit, a judicious arrangement possessing considerable advantages over the dried specimens exhibited in most museums. A long list of donations shows that the Manchester Museum has many and generous friends. We congratulate Mr. W. E. Hoyle on his excellent and favourable Report, and shall look with interest to the catalogue of the library, which he promises shortly.

Professor Sir William Flower opened the course of lectures at the Whitechapel Free Museum on Thursday, March 14, by an address on "Museums." Under the curatorship of Miss Hall, who was at first associated with A. Vaughan Jennings, this Museum is rapidly assuming an important place in the education of East London; its collections are exhibited on approved modern principles, and it is not as yet overburdened with the lumber of centuries that is usually to be found in provincial museums.

The annual meeting of the German Zoological Society will be held at Strassburg, in Elsass, from June 4 to 6. Professor Ehlers will open the meeting. Among the papers promised are "On the Origin of the Vertebrata," by Professor Goette; "Nemertines," by Dr. Bürger; "The Origin of the Vagus Nerves in the Bony Fishes," by Dr. B. Haller; and "Researches on the Cell," by Professor Dr. H. E. Ziegler.

The excursion list of the Geologists' Association was published on February 21. There is a larger list than usual, no less than fourteen separate outings being arranged. Of these nine are half-day and three are whole-day, while four days are set aside for Easter, four for Whitsun, and five for the summer excursion. At Easter the Association will visit the Tertiary deposits on the north and south of the Isle of Wight; at Whitsun the Jurassic deposits of the Banbury district; while the long excursion will be devoted to the study of that remarkably interesting district, County Antrim. This summer excursion will be the second visit to Ireland made by the Association.

In his presidential address to the Geological Society, Dr. Henry Woodward referred to the desirability of extending the library and the inutility of any longer maintaining a museum, the space occupied by which was needed for books. Dr. Woodward has struck the right note, for the museum was for many years neglected by the Society, and has become a white elephant. The president also referred to the desirability of admitting women to the meeting—we presume he means geologists, not mere visitors as one may see at the Geographical Society—a concession we hope to see carried out at an early date. But the museum question is by far the most important one at present, and is, on the whole, likely to be well received, whereas the woman question is bound to raise considerable difference of opinion.

The Royal Academy of Barcelona has just published a history of the Academy under the title of "Historia de la Real Academia de Ciencias y Artes." This formed the inaugural address, for the academic year 1893-94, of Don José Balari y Jovany, Professor of Greek in that Academy. Professor Balari traces the general conditions leading up to the foundation of the Academy, its history, its members, its privileges, and professors, publications, library, collections and natural history museum, and, finally, its reforms.

The *Journal of Geology* informs us that the Wisconsin Academy of Science is making a systematic and vigorous effort to establish a geological and natural history survey of the State. The last survey was closed about 1880. A Bill has been drawn carrying an annual appropriation of 15,000 dollars, to be expended under the direction of a commission.

A movement is also being made to secure the establishment of a geological survey of the State of Washington, and an appropriation will be sought from the Legislature for the purpose. A proposal has been made to connect the survey with a mining department of the State University.

In a letter to *Nature*, Professor W. H. Hale states that the reason the American Association is to meet at Springfield, Mass., this year, and not in San Francisco, is due to the shortsightedness of the railway authorities, who refused to give any concessions in the matter of fares. The meeting will take place from August 29 to September 4. The Association met at Springfield once before, in 1859.

Sir Charles Tupper's anniversary address to the Royal Scottish Geographical Society appeared in the January number of their magazine. It deals with "The Economic Development of Canada," and is illustrated by six maps showing the land elevation, geology, agriculture, rainfall, and temperature in January and July. There are other two maps, one showing the routes between Canada and the mother country, and the other the political areas.

The lecture delivered by Professor Sir William Flower on "Whales, and British and Colonial Whale Fisheries," before the Royal Colonial Institute, on January 5, has already been published. A perusal of Sir W. Flower's lecture leads one to the conclusion that the hunting of whales will shortly be so unprofitable that it will cease altogether, and in view of the threatened extinction of these huge animals that is a prospect much to be desired. The only real profit now derived seems to be from the "whalebone," for the competition of mineral oils, and the growth of other methods of lighting, have reduced the value of both sperm and train oils.

The *Standard and Digger's News*, for February 9, gives an account of the formation of a new society, to be called "The Geological Society of South Africa."

Dr. Exton was elected the first President, and Mr. David Draper the first Secretary. The Vice-Presidents are Mr. A. R. Sawyer and Mr. John Ballot; the Honorary President, Mr. Lionel Phillips; and the honorary Vice-Presidents, Professor T. Rupert Jones and Dr. Guybon Atherstone. The formation of the Society is almost entirely due to the energy of Mr. Draper, whose work on the geology of South Africa was made known during his recent visit to England. The head-quarters of the new Society will be Johannesburg, and the subscription two guineas a year.

La Feuille des jeunes Naturalistes announces the foundation of a new Society in France, La Société Grayloise d'histoire naturelle et d'archéologie, at Gray, in Haute-Saône. No. 1 of the Bulletin of the Society will shortly appear, and will deal with the cryptogams, the coal-flora of Ronchamp, and the prehistoric antiquities of the district.

THE Kiev branch of the south-west section of the Imperial Russian Geographical Society, which was dissolved by Imperial ukase in 1876, will probably be re-established, thanks to the more liberal spirit of the new *régime* in Russia. The Society has done excellent work in the ethnography of Ukraine. A new section of the same Society has been formed at Troitzkossavsk, near Kiakhta, on the Russo-Chinese frontier, under the presidency of Dr. Saburov, director of the local lyceum. It will occupy itself mainly with anthropology and ethnology. The Society already possesses the foundations of a museum of archæology and ethnology. The first meeting was held on September 4–16, 1894.

The Academy of Sciences at St. Petersburg has founded a new section, that of geography and anthropology. Dr. Anutschin has been elected head of the section.

THE *Botanical Gazette* gives publicity to the fact that the students of the American Brewing Academy of Chicago have among themselves a society which numbers 200 members and is known as "*Saccharomyces Cerevisiæ*."

IN 1891, Dr. G. Baur and the late Mr. C. F. Adams made extensive collections of the land-fauna on the Galapagos Islands. All of the sixteen islands, with exception of Narborough, Wenman, and Culpepper, were visited. We learn that a series of these important collections, containing many new species, described by Allen, Baur, Dall, Garman, Ridgway, and Scudder, is now offered for sale as a whole. Besides single specimens of the gigantic land tortoises (two species from South Albemarle and Duncan), forms of *Amblyrhynchus* and *Tropidurus* from the different islands are available, as well as bird-skins representing eleven species. More detailed information will be gladly given by Dr. G. Baur, University of Chicago, Chicago, Ill.

A FEW weeks ago, says *L'Anthropologie*, a well-known professor arrived at the Russian town of Vitebsk for the purpose of making anthropometric studies of the local inhabitants. The measurement of the heads gave rise to the conviction that he was the devil in person affixing his seal to their foreheads, and the more courageous among them resolved to attack him and, if possible, to destroy him. Fortunately, the ispravnik of the district prevented the infuriated peasants from carrying out their intentions, and advised the professor to leave the district with all speed.

WE learn from the *Scottish Geographical Magazine* that Herr Julius Payer, who was leader of the Austrian expedition on the Tegethoff, intends to lead another expedition to the Arctic regions in 1896. Herr Payer goes mainly for artistic purposes, but a naturalist will accompany the party. M. Andrée, the chief engineer to the Swedish Patent Office, proposes, in July, to start for the North Pole in a balloon from Cape Thorsden. The balloon will be inflated with compressed hydrogen. A balloon has been sighted as travelling in a south-westerly direction over the mountains near Labesby, Norway. This is thought to have contained messages from Nansen, but is more likely to have been despatched by the Jackson party.

Mr. Rowland Ward kindly informs us that another specimen of the so-called White Rhinoceros (*R. simus*) has just arrived in London from South-East Africa, where it was shot by Mr. Charles R. Varndell. It is now being modelled at Mr. Ward's establishment in Piccadilly. It is so short a time since we gave an account of this animal that we need not remind our readers of its importance and rarity. We may, however, correct the impression still prevailing in certain quarters, that till recently there was no specimen in England. A young specimen has been for years in the British Museum, although its presence there seems, somewhat curiously, to have escaped observation.

In the March number of the *Irish Naturalist* Dr. R. F. Scharff takes up Mr. G. H. Carpenter's parable with a short paper on Irish Caves, illustrated by a map, together with a bibliography of the subject. Dr. Scharff appeals for a thorough exploration of these caves as a means for the better understanding of the extinct Irish fauna.

The Museum at Ipswich has during the past year been open to the public on Sundays; the results achieved have not, however, seemed to its authorities to warrant a continuance of this enlightened practice.

The Archæological Museum at Colchester has now been closed for some time, but hopes are entertained that its present directors, the Council of the Essex Field Club, will shortly open it again with the added attraction of a Natural History collection arranged according to approved modern ideas.

The American Museum of Natural History at New York has recently obtained some valuable collections of Mammalia. The Price collection from south-eastern Arizona and the Granger collection from South Dakota include a number of new forms which have been described by Dr. J. A. Allen in the *Bulletin* of the Museum. The same collectors have added to our knowledge of the range of many species. Another collection of about 250 specimens from New Brunswick includes many noteworthy forms, especially two specimens of *Synaptomys cooperi*. This genus has not previously been known from that locality.

Not long ago we advocated a closer union between the Laboratory of the Marine Biological Association and educational centres, and suggested that greater facilities might be permitted to scientific workers. It is therefore with peculiar pleasure that we make the following extract from the recently-issued report of the Director :—

"A general scheme will be set on foot to map out the fauna and flora of the neighbourhood, and to arrange types for the Museum ; and the Council has directed that tables be placed at the disposal of naturalists who will be willing to assist in this work. I shall be glad to hear from any workers, either zoologists or botanists, who would render help in such faunistic work. An exceptionally good opportunity is thus offered to young men who have recently finished their University course, and are anxious to gain experience in the outdoor work of Marine Natural History. At the same time increased advantages in the supply of material will be afforded to all naturalists who visit the Laboratory, and it is hoped that an exceptional effort will be made to do so by all who are interested in the prosperity of Marine Biology in this country."

CORRESPONDENCE.

"ANTARCTIC EXPLORATION."

WITH regard to the above article by Mr. Southwell, which appeared in the February number of your valuable journal, may I be allowed to make the following remarks?

(*a*.) Mr. Southwell says that my friend, Mr. Burn Murdoch, "accompanied the 'Balaena' by the kindness of Mr. Kinnis (*sic*), the owner (*sic*), and of Captain Fairweather" (p. 98). I must say I absolutely fail to see where the kindness came in, seeing that Mr. Burn Murdoch paid his passage as any ordinary passenger would have done on a systematic passenger boat.

(*b*.) Surely it is obvious that Mr. Burn Murdoch does not use the word "varieties" (p. 99) as a scientific term.

(*c*.) I still agree with Mr. Burn Murdoch when he says that science is meant for all, not the few, and that we should call "a spade a spade, and not a bally shovel" (p. 100). At the same time I fully appreciate the value of generic, specific, and varietal nomenclature when it is used in the right place, time, and manner. I stand aghast when I find that, for sixteen species of earless seals, there are no less than one hundred and three distinct specific and varietal names, eighty-seven being synonyms, and fourteen species wholly indeterminable. Does specific nomenclature in this case have any distinct advantage over popular naming?

(*d*.) Mr. Burn Murdoch's question, "Is it not a hideous marvel that Dundonians should show such splendid enterprise as to send four ships out here for whales, and at the same time show a total disregard for the scientific possibilities of such a cruise?" does not appear to me to be "singularly out of place," although Mr. Burn Murdoch and myself had berths in the "Balaena" (p. 101). For it was on the distinct supposition that opportunities for scientific research would be given that I joined the "Balaena," and that the ship was supplied by the Royal Geographical Society and Meteorological Office with a very valuable set of instruments.

(*e*.) With regard to Captain Fairweather's "ignorance" (p. 101), I shall say nothing. Captain Fairweather is an experienced seaman, and it would be rather out of place for a novice like myself to offer criticism; but with regard to [the other matters mentioned] I believe that I am competent to judge . . . . . I am glad to learn that Captain Fairweather presented, and did not sell, to the British Museum, specimens which he obtained through no exertions of his own. . . . . .

(*f*.) Mr. Southwell himself would, I fear, have experienced difficulty in rescuing specimens, if serving under articles which made obedience compulsory, on board the "Balaena" (p. 105).

Gatehouse, Kirkcudbrightshire. WILLIAM S. BRUCE.

February 21, 1895.

[Though we are very glad to print Mr. Bruce's letter, we have thought it advisable to omit a few sentences from paragraph (*e*.). From enquiries that we have made, we gather that there is a very pretty quarrel between the naturalist and the commander of the "Balaena." It is not for us to say who is in the right, but it appears to be undeniable that the scientific work was prosecuted under greater difficulties than usually accompany even a whaling expedition to polar seas. "Kinnis" was, of course, a misprint for "Kinnes," the name of the managing owner of the "Balaena."—EDITOR.]

Eozoön Canadense.

I desire respectfully to take exception to two statements respecting *Eozoön* in your issue for February (p. 75).

The Tudor specimen of *Eozoön* is in no respect the "original" or type-specimen of the fossil, but an exceptional form, from a distinct formation, and showing only imperfectly the microscopic characters. That it is organic I have no doubt, and in this matter I think my long experience as a collector of fossils of the older rocks should give some weight to my judgment; but its precise nature is, in any case, a distinct question from that of the organic origin of the original Laurentian specimens. The same remark applies to *Archæozoön Acadiense*, recently discovered in the Pre-Cambrian rocks near St. John, New Brunswick, which appears to resemble the Cambrian *Cryptozoön* of Hall rather than *Eozoön*; and similar forms have been found by Walcott below the Cambrian in Colorado. These, it may be hoped, will ultimately throw light on the peculiarities of the probably Huronian specimens from Tudor and Madoc.

If, as you state, the specimens described by Drs. Johnston Lavis and Gregory, are merely examples of "zonal alteration of blocks of limestone which have been enclosed in an igneous magma," it is certain that they cannot be similar in origin to *Eozoön*, which is not known to occur in such relations, and is evidently an original structure in the limestone in which it is embedded, and from which small specimens can sometimes be entirely isolated by treatment with an acid. I have made considerable collections of banded contact rocks for comparison with *Eozoön*, but have not found any to exhibit its characters.

I have not yet seen the paper in the *Dublin Transactions*, and an application to Dr. Johnston Lavis for a specimen of his material has as yet elicited no reply. In the meantime I have no evidence that the conclusions of Dr. Gregory are any nearer the truth than those of Rowney or Möbius.

Montreal, *February* 5, 1895. J. Wm. Dawson.

[We are afraid Sir William Dawson has misunderstood our remark about the Tudor specimen of *Eozoön*. We did not say that this was the original or type-specimen of *Eozoön*, but that the "original Tudor specimen" was sent to England and there re-examined and shown to be inorganic. It was shown at the same time that the Tudor specimen was in no way connected with the original specimens of *Eozoön*, and that the claim that *Eozoön* had been found in carbonate of lime alone, free from magnesian silicates, could not be substantiated. A section of the paper by Professor Johnston Lavis and Dr. Gregory discusses the mode of occurrence of the normal original specimens, and shows that the true *Eozoön* has probably originated by extensive alteration of limestone blocks and their absorption of ferro-magnesian silicates.

We regret that we are unconvinced by Sir Wm. Dawson's arguments, but we are always pleased to hear what he has to say.—Editor.]

"Anlagen, Rudiments, and Blasts."

We have received a letter from Dr. Herbert Hurst maintaining that Darwin does *not* use the term rudiment "in the sense of an organ or structure which has been at one time more complicated than it is at present," and that rudiment is an exact translation of the German "Anlage" so far as this term is used in embryology.

An Omission.

We regret that no mention was made of the fact that the excellent photograph of the British Museum *Archæopteryx*, which we were permitted to reproduce in our last number, was taken by Mr. A. Gepp, to whose skill we and our readers are greatly indebted.

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NOTES AND COMMENTS.

The Bibliography of Zoology.

WE publish in this number two items of considerable interest to zoologists. We have of late drawn attention to the efforts of various people to organise and improve the existing bibliographic service, and we have urged the need for unselfish co-operation and sinking of existing rivalries. We are, therefore, exceptionally pleased to be the means of announcing the fact that the zoologists of the University of Minnesota intend to devote their remarkable energy to the service of the proposed Central Bureau. The attempt which they were making was deserving of all praise; and had it met with support and success, such praise would have been unstintingly bestowed. When, therefore, Professor Nachtrieb and Mr. Clarke Barrows not merely withdraw from competition, but subordinate their services to an organisation which, though undoubtedly international in character, has been established by other individuals than themselves, they are, we consider, even more praiseworthy than they were before, and have given a better earnest of their good faith than even a million printed cards. Let us hope that other corporations and individuals will follow the admirable example of the vigorous newly-started venture of Minneapolis and of the old-established and solidly-based *Archiv für Naturgeschichte* in Berlin.

The proposed Central Bureau is also warmly supported by the Zoological Society of France, by the Naples Zoological Station, by almost all the zoologists of Belgium and Holland, including that eminent worker in the cause of scientific bibliography, Mr. Mourlon, the Director of the Belgian Academy of Science, as well as by national committees in Russia, Hungary, and many other countries. It is, therefore, unlikely that England will stand aloof. In the article that Dr. Field has written at our request, he has stated exactly what it is that is required of England, or let us say of the Zoological Society, which in this matter worthily represents England. But on reading Dr. Field's statement, we are astonished to find that all the giving seems to be the other way. We are to have the *Zoological Record* completed and improved (and if anyone denies that it needs this, we hope he will no longer keep silence); we are to have its publication guaranteed and continued on the same lines as hitherto; our recorders and their editor are to be saved nine-tenths of their drudgery, and yet to be paid no less; and, in return for all this, the Zoological Society will actually be asked to spend less money than it loses at present. We have had opportunities of cross-examining Dr. Field, we are convinced of his good faith, and we see that he is in a position to bring his promises to performance.

A Palæontologist on Zoological System.

We are pleased to be able to present our readers in this number, through the kindness of Professor Karl von Zittel, with a translation of the address which he delivered to the International Congress of Geologists last year. It is always interesting to hear the opinion of an acknowledged leader of his science on such matters of general importance, and Professor von Zittel's warning is not the least of the benefits arising from the recent assembly.

We are struck by the passage in which he compares the scrupulous anxiety that formerly accompanied the establishment of a new genus with the reckless abandonment in which genera are scattered abroad to-day. It is true that, when we regard the history of scientific nomenclature, we are unable to acquit even the older writers of too great indulgence in an irresponsible liberty; and it must be confessed, even by Professor von Zittel, that many of the difficulties under which we now labour are due to authors, who may, indeed, have been very careful to see that their genera were really new, but who did not take such pains as would ensure those new genera being understood by their successors. Nevertheless, it is just as well that our younger writers should have brought home to them a due sense of the extreme reponsibility that attaches to the publication of a new generic or specific name. A name once published is irrevocable; it may be right or wrong, but it can never be withdrawn; it remains a permanent addition to the labour of future investigators, and if wrong, then an irremovable stumbling-block.

The general question, phylogenetic research and the reconstruction of our classifications, though one of enormous importance, is so vast that we dare not enter upon it in a casual note. This only we would venture to say in extension and elucidation of the Professor's remarks. Darwinism and the Doctrine of Descent came upon the majority of scientific workers, especially on the describers and the systematists, almost too suddenly, and the momentum of its impact seems to have carried many of them off their feet. It was at once supposed that traces of descent were to be found everywhere by simple inspection. Hence a rashness in suggesting relationships on the evidence of resemblances which, though striking at first sight, have been shown by more careful research to be due to convergence, homoplasy, or some other cause than affinity or descent. For instance, the stalked larva of *Antedon*, to which Professor von Zittel somewhat sarcastically refers, has actually been brought forward by writers of no mean reputation in confirmation of their peculiar views about certain Carboniferous crinoids, although slight reflection might have convinced them that no connection was possible. *Spirula*, as readers of Dr. Pelseneer's beautiful monograph in the "Challenger" Report may gather, has been another notorious instance. Just because it happens to be coiled, and to have a spherical initial chamber, it has been considered a connecting link with the ammonites, although, as Dr. Pelseneer shows, it is really descended from some ancestor of belemnites or cuttle-fish. It is against this foolhardiness of speculation, based on ignorance and an insufficient appreciation of the needs of the genealogist, that Professor von Zittel's protest should be raised. So far we are in full agreement with our learned leader; but we would not carry his caution too far. We believe in the reliability of palæontology and embryology, so long as they are kept within their proper limits, checked by one another and by the evidence of comparative anatomy and general ontogeny. We believe that palæontology especially is emerging from the mists of its first madness, and arriving, through sober study and minute comparisons, at results of validity and importance. Though every result may not at once be accepted, yet we cannot doubt that the methods employed by Hyatt, Cope, Jackson, and Beecher in America; by Branco, Würtenberger, Buckman, and a few others in Europe, are correct in principle, and we are glad to find that they meet with the approval of so eminent an authority as Von Zittel. The time may not have arrived for the reform of the entire zoological system; but that is no reason why all reform should be refused. Zoology is a growing science, and it must have its growing pains. It is only by the proposal of new classifications, and by the balancing of one against the other, that we shall be enabled to select the best in the end. Change is inevitable; and it is better that it should take place gradually, than that all the spars and tackle by which our voyage has been so far accomplished, should be kept till they are rotten, and then, on a sudden, go by the board.

Sir William Flower on the Principles of Nomenclature.

The subject discussed by Sir Henry Howorth in recent issues of Natural Science, and our comments thereon, have aroused no little interest. A correspondent calls our attention to an extract from part 2 of Sir William Flower's Catalogue of Osteology of Vertebrated Animals in the Museum of the Royal College of Surgeons. This was published in 1884, and as it seems to us of considerable importance, we print it in full:—

"In selecting the name chosen I have been mainly guided by the views which have been gradually gaining general currency among conscientious naturalists of all nations, and which were formulated in what is commonly called the 'Stricklandian Code,' adopted by a committee of the British Association for the Advancement of Science in 1842, and revised and reprinted by the Association in 1865 and again in 1878. These are nearly the same in principle as those concisely and clearly laid down by Isidore Geoffroy Saint-Hilaire in the introduction to his unfinished Catalogue of the Mammalia in the 'Muséum d'Histoire Naturelle de Paris' (1851), and those so copiously elaborated in Mr. Dall's Report of the Committee on Zoological Nomenclature to the American Association for the Advancement of Science at the Nashville meeting in 1877. The regulations laid down in these codes for the formation of new names are unimpeachable; and although some of the rules for the selection of names already in existence have given rise to criticism, and are occasionally difficult of practical application when an endeavour is made to enforce them rigidly, they do in the main, when interpreted with discretion and common sense, lead to satisfactory results. As what we are aiming at is simply convenience and general accord, and not abstract justice or truth, there are cases in which the rigid law of priority, even if it can be ascertained, requires qualification, as it is certainly not advisable to revive an obsolete or almost unknown name at the expense of one which, if not strictly legitimate, has been universally accepted and become thoroughly incorporated in zoological and anatomical literature; and it is often better to put up with a small error or inconvenience in an existing name than to incur the much larger confusion caused by the introduction of a new one.

"Of all the various groups into which animals are conventionally divided by zoologists, such as classes, orders, families, genera, species, etc., the last two are of greater importance than any of the others, as upon the limits assigned to them the name of the animal depends. It matters comparatively little how we arrange and rearrange orders and suborders, families and subfamilies in our endeavours to express our views of the affinities of their members to each other; but directly we apply the same process to genera and species we begin to introduce that greatest source of trouble and perplexity to students, and most fertile source of impediment to the progress of zoological knowledge, the multiplication of names of the same object. All zoologists seem to be agreed as to the value of the system introduced by Linnæus, by which the name of the animal is determined by the genus and species under which it is placed, and all attempts to improve and modify this method of nomenclature have ended in failure. It might have been supposed that this general agreement would have preserved these groups, especially the former, from the inconsiderate, hasty, and useless alterations to which they have been incessantly subjected by zoologists who have often contributed nothing else to the development of their science. I do not mean that with the advancement of knowledge improvements cannot be continually made in the current arrangement of genera. The older groups become so unwieldy by the discovery of new species belonging to them that they must be broken up, if only for the sake of convenience; newly discovered forms which cannot be placed in any of the established genera must have new genera constituted for them, and fuller knowledge of the structure of an animal may necessitate its removal from one genus into another: all these are incidents in the legitimate progress of science. Such alterations, however, should never be made lightly and without a full sense of responsibility for the difficulties which may be occasioned by them, and which often can never be removed. Complete agreement upon this subject can never be expected, as the idea of a *genus*, of an assemblage of animals to which a common generic name may be attached, cannot be defined in words, and only exists in the imagination of the different persons making use of the expression; but there might be no difficulty in coming to some general agreement, if individual zoologists would look at the idea as held by the majority, and would not give way to the impulse to bestow a name wherever there is the slightest opening for doing so. In the following Catalogue not a single new division has been proposed, or a new name introduced; but, on the contrary, very many of the generic divisions of modern zoological writers, founded upon most trifling characters, often artificial or even erroneous, have been ignored, as it is thought that the sooner such names are discarded the better. Others have certainly been admitted which, according to my judgment, it would have been better never to have invented; but as they exist and are generally recognised, less confusion and alteration of existing nomenclature is caused by retaining than by abolishing them.

"Subgenera with names attached to them have always been avoided, as they cause confusion of nomenclature, and nearly always end sooner or later in being promoted to the rank of true genera. It seems preferable in the case of large genera, showing much diversity of characters among their members, to group together those which resemble each other most into sections, but avoiding the use of any distinctive name that would clash with the binomial principle."

American Plant Nomenclature.

In the March number of the *Botanical Gazette* Dr. Robinson severely criticises the "List of Pteridophyta and Spermatophyta of North-Eastern America," which has just been prepared by the Nomenclature-Committee of the Botanical Club. As the expression of the latest phase of nomenclature-reform in the New World, the list affords ample material for a discussion of the merits or demerits of a system, and, by the way, useful object-lessons for the systematic botanist as well as warning to the "nomenclaturist." The chief point raised by the writer is the stability of the new system. Does it possess the elements of permanency? Is it really a rigid code, allowing no exceptions and leaving nothing to individual judgment? It has none of these merits. On the contrary, as Dr. Robinson states, every working botanist knows that the selection of the first specific name, after the still more difficult choice of the generic, often involves most critical judgment both as regards the exact application of brief and unsatisfactory descriptions and the often doubtful priority of publications. A striking illustration that even the form of the name may be the subject of individual judgment or arbitrary modification is adduced from the "List," in which the name adopted by Professor Britton for the pretty little garden plant, Sweet Alyssum, is shown to be founded on a supposition entirely unwarranted by fact. Strangely enough, the name which Professor Britton has evolved and claimed as his own turns out after all to be identical with that given for a different reason to the same plant by no less a person than Robert Brown. Was Robert Brown's combination overlooked? or is also the motive for the name to be taken in consideration when adjudging the right of authorship?

Again, the system of renaming *both* plants when species of the same specific name are brought together from the readjustment or union of genera, instead of conducing to stability, gives perpetual opportunity for change. One shudders at the contemplation of the results of the union of *Aster* and *Erigeron*, *Panicum* and *Paspalum*, and the consequent displacement of names which, on the principle of "once a synonym always a synonym," can never be revived. The misuse of the trinomial system, lately condemned in NATURAL SCIENCE, is also criticised by Dr. Robinson, with the remark that "a style of nomenclature in which there is no distinction between subspecific, varietal, and formal differences is likely to appear to future botanists a rather clumsy tool." Once again we are reminded that ready intelligibility is a more important quality than stability and consistency, or, at all events, the degree of stability and consistency attainable.

There is another argument, to which Dr. Robinson does not allude, against the replacement of well-known names by strange combinations. Many of the former have their origin in a valuable monograph of a group on which some painstaking worker has spent much time and thought, and his duly accredited specific names inspire a certain respect and confidence which the strange combinations of the exponent of a system of nomenclature can never claim.

THE PUBLICATION OF PAPERS BY SOCIETIES.

THE point raised by Mr. Cockerell, in the letter that we print in this number, is one of considerable importance to scientific writers, and has a wider bearing than the writer seems to be aware of. We are authorised to state, if need be, that Mr. Cockerell is in no way biassed against the Zoological Society, and that he would not be thought to be making an attack on its learned and courteous secretary. This, however, is an almost unnecessary statement, for the complaint is one that may be brought against nearly every learned society that is in the habit of publishing the contributions of its members. It makes but little difference whether a society has a publication committee, or a single responsible editor, or whether it calls in the aid of a referee. In almost every case, as affairs are at present conducted, an author's paper is first sent in to the secretary, then read in open meeting and discussed, then (it is probable) made still more public by some printed abstract, which is theoretically rather than practically issued for the private convenience of the fellows alone. After the author's hopes have thus been raised; after his results have been proclaimed to contemporary, perhaps to competing, workers; after a lapse of time, which in the case of some work may be fatal to the author's claim of priority: it may be that, after all this, his paper is refused ultimate publication. The refusal to publish may be based on valid grounds; we have sufficient confidence in the common sense and in the integrity of scientific men to suppose that such is usually the case. But there are cases notorious in the history of science, and there are cases that one hears of every year, if not every day, in which a paper is rejected by a society, either because its views do not jump with the personal views of the referee, because the author is not altogether a *persona grata*, or because some merely technical condition has not been observed. Mr. Cockerell's case is none of these: the paper appears to have been declined simply because the Zoological Society did not think it its business to descend to the publication of the description of so insignificant a creature as a new Coccid; and the Society has a perfect right to limit its own sphere of action. But in this case, as in all the others, the delay and the semi-publication are a hardship, though not necessarily an injustice to the author.

It may be replied to this complaint that an author sends his paper to a society, just as he sends it to the editor of a magazine, under certain conditions that are (or that ought to be) well known and clearly defined. Some societies very properly send a notice to every author, immediately on receipt of his paper, acquainting him with those conditions, and the author can then withdraw his paper if he choose. All this is very true, but the case is not thereby improved. There is a very considerable difference between the action of one of these learned societies and that of a competent magazine-editor. A society, we are told, keeps a paper more than two-and-a-half months; an editor, if he is going to decline an article, usually does so within as many weeks: some editors have been known to return articles within as many hours. Would it not, then, be possible for the procedure of societies to be approximated to editorial methods? These lengthy delays have a quaint, old-world, respectability about them, but they are scarcely suited to modern needs. Even the society must be a loser sometimes, quite as much as the author, especially when, as we know very well to be no rare occurrence, an author's paper gets into type before it is rejected. Might not a committee consider papers before they are read in public instead of after? It is no hard matter to decide whether a paper is or is not simple rubbish, or whether it comes or does not come within the scope of any particular society. As for any fear of unwarranted boycotting, or of illegitimate suppression, or of stealing an author's results, we know, to speak quite candidly, that these things have occurred quite often enough under the present system; and even if there were any greater temptations under the proposed alternative, at all events the author would no longer be handicapped by a two or three months' delay. The acceptance of a paper need not, of course, prevent a little fatherly care at a later stage; some editorial pruning is generally beneficial to even the best authors. But as for the author's opinions, they should be left to him alone; for does not every society plainly advertise on its title-page that it is in no way responsible for the opinions of the authors?

Toxins and Anti-toxins.

A topic of the day is the treatment and prevention of diseases due to organisms, by what are known as anti-toxins. So much confusion exists in the popular writings on these subjects, that a brief statement of their nature may interest our readers.

In the early days of bacteriology it was thought that the damage caused by microbes was a direct, almost a mechanical damage. The microbes of various diseases (men said) lived and multiplied in the tissues, as external parasites may live and multiply on the skin or in the hair. Sometimes their presence caused irritation, and this, when it became severe, was held to give rise to inflammatory symptoms. Sometimes they caused mechanical lesions, actually destroying tissues, as the moulds of many skin-diseases destroy the epidermis. Most often they were held to play their disagreeable part by occluding blood-vessels or lymphatics, by blocking up natural passages or by producing mechanical pressure on nerves. No doubt all these processes are accountable for many of the symptoms of diseases; but it is becoming more and more clear that they are not the most serious factors.

Many years ago Dr. Burdon Sanderson insisted upon the living nature of these organisms, and on the damage they caused as being, in a sense, the result of the conflicting vitalities of the microbes and the tissues. The Toxin theory of diseases really is an amplification of this point of view. A microbe living in a tissue exhibits vital phenomena peculiar to its nature, just as a larger organism differs from its fellow-organisms of other species and genera. As necessary bye-products of their metabolism, many micro-organisms discharge chemical substances into the surrounding media. These, for the most part, are complicated proteid substances, akin to the globulins and paraglobulins. Some of them, passing into the blood and being carried from tissue to tissue, go their round harmlessly, and are extruded by the skin or by the kidneys as useless or waste material. Others, and such are the discharges of the microbes of dangerous diseases, are known as toxins. Being carried through the body by the blood, they exhibit an unholy affinity for some of the cells of the body—generally for some of the nerve-centres. Entering these, they disarrange or even destroy the normal procedure of the cells, with the result that these poisoned cells are unable to discharge their peculiar functions. And so, from a nidus of bacteria growing, say in a toe, there may be discharged a poison that is carried by the blood all over the body, doing no damage save to the nerve-centre controlling, say, the mechanism of respiration.

In some cases the toxins have been prepared from cultures of the microbe on gelatine or agar-agar, and these toxins, carefully separated from the parent-bacteria, cause the dangerous symptoms of the disease, when they are introduced directly into the blood.

The anti-toxins are more difficult to understand. It seems that, as the toxins are discharged into the blood, the blood "makes an effort" to neutralise them by producing a counteracting proteid substance. In some animals it appears as if the anti-toxin for a particular toxin was present naturally, and such animals would naturally be immune to the ravages of the microbe in question. But in all animals, if the toxin be supplied slowly, there is an effort made to manufacture the counteracting agent. The secret of anti-toxin treatment is to select an animal constitutionally able to manufacture the anti-toxin of any particular toxin; to stimulate the production by gradually increasing doses of the toxin, and then when the blood is filled by the anti-toxin to remove and isolate the latter. When the anti-toxin, so prepared, is injected into the blood of a patient suffering from the disease, it neutralises the toxins in process of being discharged; and when the most dangerous symptoms have been mitigated, no doubt the tissues are enabled to manufacture the necessary further supply of anti-toxin themselves.

Such is the general theory of toxins and anti-toxins; in each particular case there is still needed experience and experiment.

The Sexual Organs of Ferns.

Mr. D. H. Campbell communicates to the *Botanical Gazette* (of February) a note on the origin of the sexual organs in Ferns. A study of the development of *Marattia* led the author to conclude that this genus and its nearest allies had arisen from forms resembling *Anthoceros*, a somewhat peculiar genus of Liverwort. The sexual organs in *Anthoceros* differ from those generally characteristic of Mosses and Liverworts, in that they are buried in the tissue of the plant-body and are not protruded and borne on stalks. The female organ, in particular, is of more simple structure, recalling that of *Marattia*, and, to a less extent, of the more typical ferns. The author is convinced that, in tracing the pedigree of the Fern group as a whole, we must work back through forms like *Marattia* to a moss-ancestor resembling *Anthoceros*.

Botanical Education in America.

In the same number, the editor strikes a note of warning *à propos* of methods of botanical instruction prevalent in American colleges, not, he says, those "in which botany receives but little attention," but "those in which there is an attempt to develop it in a full and modern way." "In the recoil from the old-time methods, the college has gone to the other extreme and seeks to become a place of research." "Raw young men and women, after a year or two of the study of 'types,' are assigned original problems, and their uncertain results, with more or less revision on the part of the instructor, are published in some periodical or bulletin." These remarks are worth the attention of all teachers of the science, whose first aim should be to give their students a thorough groundwork in general principles, suppressing, if need be, undue haste towards detailed specialising in any one direction. A man or woman with a good general knowledge will make, for instance, a better systematist than one who has acquired, at the expense of the former, detailed information in one group of plants.

The science of botany is too vast for a year or two's work to be sufficient preparation for creditable research in any one branch. "The different departments of it are so interdependent that it needs long training to bring the perspective and the grasp that make any independent investigation profitable."

The warning, of course, applies to other Sciences besides Botany, and may not be wholly unneeded in our own country.

Mr. Wallace on Organic Evolution.

In the *Fortnightly Review* for February and March, Mr. A. R. Wallace deals with the views of Bateson and Galton. His essays form a closely-reasoned and interesting statement of the case for Natural Selection as against positions of organic stability. He urges that Mr. Bateson has not been directing attention to a class of facts hitherto neglected by Darwinians. Darwin himself considered with great care the kind of variations that Bateson calls discontinuous, and came to the conclusion that they had little or no effect on the origin of species. For the most part they are sports or abnormalities, and grade into monstrosities. Mr. Wallace points out that Mr. Bateson is quite wrong in saying that discontinuous variations are similar to the characters used to define species and genera. Systematists know well that "among the higher animals, at all events, it is not so." As a matter of fact, specific characters, and, indeed, generic characters, in the vast majority of cases, both among higher and lower animals, are of a kind similar to individual and continuous variations.

Galton and Bateson have "devoted themselves too exclusively to one set of factors, while overlooking others which are both more general and more fundamental. These are the enormously rapid multiplication of all organisms during more favourable periods, and the consequent weeding out of all but the fittest in what must be on the whole stationary populations. And acting in combination with this *annual* destruction of the less fit is the *periodical* elimination under recurrent unfavourable conditions of such a large proportion of each species as to leave only a small fraction—the very elect of the elect—to continue the race."

Specific Stability.

At a recent meeting of the Entomological Society (see *Nature*, April 11, 1895) Mr. F. Galton appealed to those who had made experiments in breeding, for information on three points connected with his theory of positions of Specific Stability. First, he wishes to be made acquainted with "instances of such strongly-marked peculiarities, whether in form, in colour, or in habit, as have occasionally appeared in a single or in a few individuals among a brood; but no record is wanted of monstrosities, or of such other characteristics as are clearly inconsistent with health and vigour." With due deference to the eminent inquirer, may we suggest that the distinction between monstrosities and discontinuous variations is one demanding more interpretation than is congruous with the mere collecting of facts. How is it to be decided whether or no a particular variation be a monstrosity? Moreover, if positions of organic stability exist, it would seem to us natural that monstrosities should exhibit signs of them as much as what Mr. Galton might consider not to be monstrosities. If a monstrosity be capable of breeding, the record of its descendants would afford most valuable indications of the range and character of heredity. Whether or no a particular variation were inconsistent with health and vigour, in many cases, must depend on the environment. On the protection theory of coloration, a caterpillar, varying from the protective pattern of its kind, would be a monster doomed to perish: under the conditions of the breeder, or in the natural absence of enemies, would it propagate its unnatural variation?

Secondly, Mr. Galton asks for "instances in which any of the above peculiarities has appeared in the broods of different parents." This point is the root of the whole matter. If such positions of organic stability do occur, they must occur repeatedly. In fact, in addition to each species, there must be a number of possible "para-species" (Mr. Bateson, who is a purist, will object to the term).

To take a theoretical example of "para-species." A species of butterflies with a particular coloration may range over a plain on which occur isolated high mountains. On each of these high mountains may occur the same "arctic variety" of the species. On the theory of positions of organic stability these arctic-forms may be identical and yet of independent origin on each mountain. They are the outcome of a "position of organic stability" near to the plain-form of the species. They (on the theory) have been called into existence, or have been selected by the conditions on the mountain-tops; they are "para-species" of the plain-species. Such a condition of things would simplify one's conception of the process of organic evolution. At present it is pure theory, but if Mr. Galton is successful in getting instances of the natural occurrence of such "para-species" born of different parents, it will go far towards establishing the point.

Mr. Galton's third request is for "instances in which any of these peculiarly characterised individuals have transmitted their peculiarities, hereditarily, to one or more generations," and he asks special attention to be directed to the degree of intensity with which the peculiarity has been transmitted, to the occurrence of any dilution of the peculiarity when transmitted, or to the absence of its transmission. The point of the last inquiry is clear enough. If positions of organic stability are real things, frequently enough they should be transmitted as readily and as fully as that position of organic stability known as the normal species.

A Burlesque Explanation.

Many things have been written about Natural Selection that will last not even our own day; but a Mr. S. E. Peal seems to us to have reached a climax of illusory ingenuity. We regret that Mr. Alfred R. Wallace has laid himself open to the enemy by formally supporting Mr. Peal in *Nature*. It seems that certain tan-spots occur over the eyes of semi-domesticated dogs. These do not exist in wild animals allied to the dog, or in the modern breeds of fully-domesticated dogs. The spots are most conspicuous when the eyes are closed, appearing then like opened eyes. The ingenious suggestion is that they "may have been protective to the animals during sleep, causing them to look as if awake. The reason that they do not occur in wild dogs may be that the latter conceal themselves when sleeping, which the half-domesticated animals were not able to do."

To Mr. Wallace, whose words these are, we must point out with the greatest deference that animals in a state of semi-domestication would hardly need so special a method of protection. No doubt the ways of the young savage might not be agreeable to the dog, and it would be to the advantage of the dog to appear wide-awake; but even the young savage would sleep sometimes, and probably the semi-domesticated dog might snatch enough sleep o'nights to maintain his existence. But it is not against the particular case, but against the general application of "Natural Selection" to individual features that we wish to protest. To argue about this individual feature we should have to know all about the dog and his structure and habits. With what other changes are these particular spots associated? The mere proposition of the question is enough to dispel at once any idea of accepting Mr. Peal's suggestion. For each individual feature a dozen ingenious theories might be formed; but one and all the theories must be illusory in the absence of any knowledge as to the correlations that exist in the bodies of the organisms in question.

Curiosities of Mollusca.

We hope to give a detailed review of the "Cambridge Natural History" in our next issue; but there are so many interesting tit-bits of information in the volume which has just appeared that we feel justified in anticipating a little. Everybody had heard of the use of the cowry shell as money. But, possibly, the extent to which this is legal tender is not so well-known. Mr. Cooke relates how a gentleman in India paid for a house, which he had erected, entirely in these shells. The house cost him £400; the equivalent of this sum in shells was 16,000,000, which were paid as such. Not only is this particular shell used as specie; in other parts of the world other shells are put to the same use. The wampum of the American-Indian is simply an elaborate ornament formed of shells of the common clam, *Venus mercenaria*; an Indian, therefore, like a Belgian maiden with her gold ornaments, carried his fortune upon his person.

But shells have a value also among civilised races. They are apt to be dear to the collector; and rare species have fetched large prices. Mr. Cooke has brought together some excerpts from auctioneers' catalogues, from which it appears that £100 is the record price asked for a shell. The specimen which was supposed to have this extraordinary value is one of *Pleurotomaria adansoniana;* but it has been sold to the British Museum for about half that sum.

Mr. Cooke has brought together some interesting cases of supposed mimicry among the Mollusca, of which the following is perhaps the most striking. "Certain species of *Strombus* (*mauritianus*, L., *luhuanus*, L.) show a remarkable similarity in the shape of the shell to that of *Conus*, so much so, that a tyro would be sure to mistake them, at first sight, for Cones. In the case of *S. luhuanus* at least, this similarity is increased by the possession of a remarkably stout brown epidermis. Now *Conus* is a flesh-eating genus, armed with very powerful teeth which are capable of inflicting even on man a poisonous and sometimes fatal wound. *Strombus*, on the other hand, is probably frugivorous (*sic*), and is furnished with weak and inoffensive teeth. It is possible that this resemblance is a case of 'mimicry.' It is quite conceivable that powerful fishes which would swallow a *Strombus* whole and not suffer for it, might acquire a distaste for a Cone which was capable of lacerating their insides after being swallowed. And, therefore, the more like a Cone the *Strombus* became, the better chance it would have of being passed over as an ineligible article of food."

"Protective coloration" is not unknown among molluscs. The *Scyllæa* of the gulf-weed, with its olive-brown hues mixed with white spots, is one of the most familiar instances. Mr. Cooke draws attention to the fact that the "Blue Limpet," *Helcion pellucidum*, which, when young, has beautiful iridescent tints, lives at that period of its life exclusively upon the fronds of the great *Laminaria*, which has also iridescent hues. Later in life, when the iridescence fades, it transfers its abode to the lower part of the "stalk," and eventually to the "roots" of the Sea-weed, which are not iridescent.

THE QUEEN'S OSTRICH.

THERE has been a death-rate of forty per cent. among the ostriches living in the gardens of the Zoological Society during the last few weeks. To leave the delusions of statistics, that means that two out of five ostriches have died. One of the deaths was that of a very curious piebald male ostrich, the property of Mr. Rothschild. The feathers of that fine bird were all black and white, so that the general aspect of the creature was most curiously mottled. It is being prepared for the Tring Museum, so that naturalists may still have an opportunity of studying this most remarkable variation.

The other death was that of the large male, presented to the Society by the Queen some time ago. It was an unusually large and fine bird, the length of the bones of the leg from the head of the femur to the toe being over seven feet. A few months ago it was in magnificent condition, but during its illness the feathers became so broken and draggled that the skin was useless. The bird was ill for several weeks, gradually becoming weaker. For a number of days before its death it refused to eat.

A correspondent who happened to be present while the examination of the body was being made, informs us that the only organs obviously diseased were the lungs. These showed signs of severe broncho-pneumonia. At the *post-mortem* on an ostrich, the chief excitement is the examination of the contents of the gizzard. Usually there is a remarkable assortment of curiosities—glass bottles, coins, and all manner of foreign bodies. The Queen's ostrich apparently was a fastidious bird. Its stomach contained nothing but grass and pebbles.

CARICATURES IN TEXT-BOOKS.

THE cheapness of electrotypes is not an altogether unmixed blessing. When a book-illustration becomes popular, it is reproduced over and over again; and even if it happens to be proved erroneous, an incalculable period is required to eradicate it from the most approved treatises. Figures of vertebrate fossils are particularly unfortunate in this respect. The Nestor of American geologists, the sad news of whose death arrives as these pages go to press, had just issued a fourth edition of his classic "Manual of Geology," and even here there are some woeful relics of antiquity. Professor Dana's book is admirably up-to-date in most matters, and it is refreshing to see the multitude of new illustrations introduced; but we may be pardoned for exclaiming when we find among the figures of fossil vertebrata so many defunct friends which we had hoped were already decently buried. The Devonian *Coccosteus* restored as if it were a modern bony fish; *Dipterus* still borrowing the head from a codfish; *Dapedius* and *Aspidorhynchus* bearing even less likeness to the originals than the once-popular silhouettes—all these greet us again. "*Pterodactylus crassirostris*," with four free fingers on the hand, appears as if it would never die. It would at least have been possible to cut off the superfluous digit, as is now done by those who still use the block in Europe. Cope's original guess at the form of the body in the Chalk fish *Portheus* (reproduced on p. 844) was long ago corrected; and of this, as of the other animals mentioned, adequate illustrations could have been found in the literature of the last two decades.

The Land and Water Areas of the Globe.

Professor Hermann Wagner has an interesting paper on this subject in the *Scottish Geographical Magazine* for April. He has worked out a new calculation by zones of ten degrees of latitude. The figures are so divergent from Dr. Murray's areas (*Scott. Geogr. Mag.*, ii., 1886, p. 553) that Professor Wagner has sought for some confirmation of the new results. This he has found in the data of a new calculation of the mean depths of the oceans which has been published by Dr. Karl Karstens ("Neue Berechnung der mittleren Tiefe der Ozeane," Kiel, 1894), who divides the twenty-seven seas of the globe into trapezes of one, two, or three degrees in breadth. Adding these areas together, the surface of the land can be indirectly deduced. The agreement obtained is astonishing, for while Professor Wagner by direct calculation arrives at 45,417,000 square miles as his estimate of land area, Dr. Karstens' data give him by indirect calculation 45,445,000 square miles. Professor Wagner considers his figures the more reliable. Only the zones between 60° N. and 60° S. allow of exact comparison, for Dr. Karstens takes the Polar limit at 66½°.

Professor Wagner is of the opinion that some 250,000 square miles will include all the land in the Arctic regions, while he agrees with Dr. Murray that 3,500,000 square miles is a likely figure for the Antarctic lands. The proportion of land to water works out at 1 : 2·54. Professor Wagner promises an exhaustive paper in the *Beiträge zur Geophysik*, vol. ii., no. 3, shortly to appear at Strasbourg.

Wild Fowl in Norfolk.

Under the Wild Birds' Protection Act, 1894, the Home Secretary has recently issued an order for the special preservation of the birds of Norfolk. Frequenters of the Broads and of the coast of Norfolk, or readers of the attractive book by Mr. P. H. Emerson, "Birds, Beasts, and Fishes of the Norfolk Broad-land," which has just been sent us from Mr. D. Nutt in the Strand, will be aware that many birds, rare elsewhere in our islands, are still to be found in some of these secluded haunts, though the extinction of several is threatened.

The order forbids the taking or destroying of the eggs of any kind of wild birds within specified areas for one year, and makes a general prohibition for the entire county in favour of the following birds:—The Bearded Titmouse or Reed Pheasant; the Crossbill; the White or Barn Owl; Wild Ducks and Teal of all species; the Norfolk Plover, Stone Curlew or Thick-knee; Ruff or Reeve; the Ring Dotterel, Ring Plover or Stone Runner; Oyster Catcher or Sea-pie; the Terns, Sea Swallows, Pearls or Dip-ears, all species; the Great Crested Grebe or Loon. Other birds there are whose names might well be added to this list; we may mention the Bittern or "Buttle," as he is called, the Spotted Rail, the Water Rail, and the stately Avocet.

The Anatomy of Ornithorynchus.

In the newly-issued number of the *Proceedings* of the Zoological Society of London (part iv., 1894), there are two important contributions to our knowledge of the duck-billed Platypus. Mr. Manners-Smith has gone over the muscular system carefully, correcting his homologies by examination of the nervous supply. The details are too technical to be entered upon here, but they raise the very interesting question of the value of muscles from the point of view of systematic zoology. It is not too much to say that there are only two opinions on this matter. Those who have worked at the muscles of fishes, reptiles, birds, or mammals believe them to be almost as valuable as bones or nerves, though much more difficult to interpret. Those who have not worked at them tend to despise their value. No doubt while muscles were identified in a rather cavalier fashion, and when the degeneration of muscles into ligaments was not understood, there was little hope of getting valuable results. In spite of the vast amount of work already published, we take muscular anatomy to be one of the subjects of the future.

Mr. F. E. Beddard deals with several interesting points in the viscera. The first is the presence of a ventral mesentery. It is well known that in the Dipnoi and the Amphibia the gut is suspended to the ventral wall of the abdomen by a mesentery. In the frog, this is conspicuous by reason of the anterior abdominal vein, which the elementary student cuts through with a monotonous regularity. In mammals, a short but similarly-placed ventral mesentery exists in the region of the liver, and is known as the falciform ligament. Balfour, in his embryology, cast doubt on the obvious comparison between these two structures; but recent investigations of Professor Howes on the Australian *Torpedo*, and of Mr. Beddard, on *Ornithorhynchus* and *Echidna*, make it seem probable that the falciform ligament is a remnant of the primitive ventral mesentery. Some time ago, Mr. Beddard found that there was an anterior abdominal vein in *Echidna*; he has now found that there is a well-marked ventral mesentery, although it has no vein, in *Ornithorhynchus*.

I.

Palæontology and the Biogenetic Law.[1]

PALÆONTOLOGY has long ceased to place itself exclusively at the service of geology as the study of characteristic fossils. It has gradually grown into an independent branch of the biological sciences, and claims a share in all their movements and tendencies. The conclusive establishment of the Doctrine of Descent has evoked the most powerful revolution in descriptive natural history, influencing and transforming its whole method of research. No large palæontological work of to-day contents itself with the description of new forms, the comparison of them with those already known, and the arrangement of them in systematic order. To determine the genetic relationships, the ancestry, the modification, and the further development, in short, the race-history or phylogeny of the organisms under consideration, is now regarded as essential, by many indeed as the chief aim of palæontology.

As Darwin forcibly insisted, the doctrine of descent depends in no small degree on palæontological facts. Thus, the great similarity of the fossils occurring in strata immediately superposed one on another, *e.g.*, the brachiopods, ammonites, and other molluscs, has made it difficult for geologists to determine the age of sedimentary rocks. In recent years a great number of closely-allied species have been traced through several superposed beds, stages, or divisions of formations, their exact morphological relationships have been studied in the most careful manner, and thus the probability at least has been established, that we are here dealing with a genealogical sequence of blood-relations. To be sure these do not as a rule form complete chains, wherein mutation is linked with mutation and species with species. They are rather discontinuous series, of which all the members change in a definite direction, and obviously form steps in a line of development, which culminates in the last extinct or still-existing representatives. Among the better-known series of forms I will only refer to a few. The succession of genera, which leads from *Hyracotherium*—perhaps, indeed, from the five-toed *Phenacodus*—through *Paloplotherium*, *Anchilophus*, *Anchitherium*, and *Hipparion*, to the single-toed horse, forms one of the most quoted and most beautiful examples

[1] A paper read before the International Congress of Geologists, 1894.

of phylogenetic development. No less complete a series is presented by the genealogical tree of the camels, which already appear during the Eocene in North America, spread there in the Miocene and Pliocene, and then first emigrate to the Old World. The pigs also, and the Oreodontidæ, Anoplotheriidæ, Tragulidæ, and the ruminants studied in so masterly a manner by Rütimeyer, afford us more or less complete genealogical series, beside which may be ranged the crocodiles among reptiles, the Amioidei and Physostomi among fishes. If we glance over these phylogenetic series, we observe that the final terms are almost always distinguished from their predecessors by a more pronounced and distinctive differentiation; and since we are accustomed to assign a higher rank to a specialised organism in which every function is performed by a special arrangement, than to a creature which performs its functions with few and less complicated parts, phylogenetic development, as a rule, implies also progression and perfection. For the existence of the chains of forms already mentioned, to which many others may be added, there is only one rational explanation, namely, that the various links in them have arisen one from another, and are connected by blood-relationship. Moreover, the similarity of the faunas and floras which are nearest to one another in geological age, as well as the geographical distribution of extinct and still existing plants and animals, can only be satisfactorily explained on the assumption of descent.

But, although an abundance of palæontological facts can be cited in the most convincing manner in favour of the theory of descent, on the other hand we must not forget that we still know no point of origin for numerous independently arising creatures, and that the connection between the larger divisions of the animal and vegetable kingdoms is by no means so intimate as the theory specially postulates. The jubilation with which the discovery of *Archæopteryx* was greeted at the time shows at best that links had previously been wanting between two classes which among vertebrates undoubtedly exhibited the closest relationships. Further, *Archæopteryx* fills the gap between birds and reptiles only in a very imperfect manner, and affords no indication of the point at which the former have branched from the latter. It may, indeed, be maintained that we find ourselves to-day in greater uncertainty as to the origin of birds than we were twenty-five years ago, when Huxley's brilliant researches on the pelvis of the Dinosauria seemed to have found the bridge between the two classes. Links between the Amphibia and Reptilia are also still wanting. Perhaps they are to be found among the varied Theromorpha, but as yet palæontology cannot determine the phylogenetic modification of the amphibian into the reptilian type. No zoologist will deny that the Mammalia hold an entirely isolated position, separated by a wide gulf from birds, reptiles, amphibians, and fishes; while among all known mammals it is not some old fossil genus, but the duck-bill, still living in Tasmania, which most reminds one of the more lowly organised vertebrates. Certainly we still know too little about the skeleton of the Mesozoic mammals, and especially the Allotheria, for us to form a final opinion on this point. But the warmest adherents of the theory of descent must at all events admit that extinct links between the different Classes and Orders of the vegetable and animal kingdoms are forthcoming only in a small and ever-diminishing number.

Nevertheless, in the larger groups we know numerous series of forms, which not only bear witness to the great plasticity and adaptibility of their members, but also in their chronological order indicate the line along which modification has taken place in course of time. To be sure, much uncertainty and the personal equation of the authors attach to those genealogical trees that are based entirely on the morphological comparison and determination of the chronological sequence of the forms met with. "It is easy to accumulate probabilities, hard to make out some particular case in such a way that it will stand rigorous criticism," was Huxley's caution so long ago as the year 1870, in his classic address to the Geological Society of London; and one of the most spirited veterans in the field of mammalian palæontology decides in his last exhaustive monograph on the fauna of Egerkingen, that the creaking and crackling of leaves and branches already decayed does not encourage one to set foot in the hastily-explored forests of phylogenetic trees. None the less does the tracing of the hidden bonds of relationship exercise a fascinating charm over every investigator. All of us, indeed, are convinced that the mutual relationships of the extinct and still-living members of any large group of organisms may be represented, not in the form of an entangled network, but in that of a much-branched tree.

In addition to the above facts there is still another series of phenomena which confirms the genetic connection of the palæontological chains of forms, and this was first observed, strangely enough, by one of the most distinguished opponents of the theory of descent. Louis Agassiz certainly regarded the fossil embryonic types as creative attempts which prophetically foreshadowed genera that appeared later with more mature characters. Fossil creatures with persistent youthful and even embryonic characters could not fail also to be noticed by the adherents of the theory of descent, but were regarded as favouring a view which recurred in very different forms in the philosophical literature of the first decades of this century, and which has lately been precisely formulated by our great German zoologist, Ernst Haeckel, under the name of the "Biogenetic Law." According to this, the development or ontogeny of each individual is only a short recapitulation of the long course of ancestral history (phylogeny) of the species or family in question. There must, therefore, also be chronological series of fossil embryonic types which would correspond with the different stages in the development of a subsequently existing form; indeed, the separate divisions of a genealogical tree must correspond essentially with the ontogenetic stages of a determined course of development. If the biogenetic law were correct, embryology would thus be in a position to reconstruct, at least approximately, the primitive forerunners of each group of plants and animals; and these types should, if they were capable of preservation, also lie buried in the rocks.

If we consult palæontology, it shows that these surmises are by no means confirmed. There are, indeed, a great number of fossil genera which retain throughout life the embryonic, or, rather, the youthful characters of their existing allies, but it is only among the mammals, and to some extent among the reptiles, that I could name a complete series of forms following one another in time and belonging to the same line of development. The Eocene, Oligocene, and, in part also, even the Miocene Mammalia, stand to their now existing allies, for the most part, in the relation of youthful forms, while they, almost without exception, exhibit at least some characters which are quickly passed through by their geologically younger successors in the embryonic or youthful stage. On the other hand, they are, as a rule, destitute of the most striking peculiarities, such as antlers, bony processes, fusion of certain bones, reduction of the teeth or of separate parts of the skeleton; and it is not till we study more closely a series of related genera of different geological ages that we see how the differentiations and peculiarities of the existing representatives of any particular group have been gradually formed in course of time. But thus it is also possible to discover, in most of the mammalian orders, a number of primitive characters, which, while they frequently occur united in the oldest representatives of the group in question, also usually correspond to an embryonic stage of one of its living members.

The ontogeny of organisms now living would, for the rest, afford but an exceedingly unsafe basis for the reconstruction of ancient faunas and floras, since experience teaches that the biogenetic law is frequently veiled or completely obscured owing to various causes. Not seldom does it happen that, of two nearly allied living forms, the one passes through a series of continuous, successive stages, while development in the other takes place more by jumps. In the latter case the embryo is driven by peculiar influences to an acceleration of its development; it completely jumps over certain stages, and thus renders unintelligible the historical (palingenetic) record preserved in the ontogeny of each individual. This falsification of development—or Cœnogenesis, as Haeckel calls it—chiefly occurs when the adult individual manifests a high degree of differentiation, and when the embryo has to pass through considerable changes to reach its final form. How unsafe and deceptive palæontological results would be if attained by embryological paths may be gathered from some random instances. What wonderful ancestors would be constructed for the crinoids by a zoologist who only knew the life-history of *Antedon!*

The lowest portion of the family tree would have to present armless crowns, composed only of five basal and five oral plates, set on a stalk; then would follow genera with five large basals, five tiny radials, and five stout massive orals; then forms with five arms, at first short and later on simply branched; and so on. But I will not further elaborate the picture. All know that it does not in the remotest manner agree with the facts of palæontology. What zoologist would conclude from the developmental history of the recent sea-urchins, that the regular forms preceded the irregular, or again that the former had fossil ancestors of the type of the Perischoëchinidæ and Bothriocidaridæ? In the ontogeny of the cœlenterates there is no certain indication of the former existence of Cyathophyllidæ or Cystiphyllidæ. No observations of embryology would warrant our imagining the former existence of graptolites or stromatopores. No stage in the development of any living brachiopod informs us that numerous spire-bearing genera lived in Palæozoic and Mesozoic times. These few instance might easily be multiplied; but they may suffice to show how trivial are the discoveries concerning existence in earlier periods of earth-history that can follow from ontogenetic researches alone.

A further, indeed the practical, reason why ontogeny bears so slight a relation to geology and palæontology, lies in the fact that the earlier stages of development, with which modern embryology almost exclusively occupies itself, are not capable of preservation in the rocks, and that we can, therefore, never expect to find their fossil archetypes. The changes that occur between the embryonic and the completely adult stages have, at least among invertebrates, not yet received the attention they deserve, and it is these very changes that are of special interest to the palæontologist.

In spite of these hindrances, fossil embryonic types are not entirely wanting, even among invertebrates. The palæozoic Belinuridæ are bewilderingly like the larvæ of the living *Limulus;* the Pentacrinoid-larva of *Antedon* is nearer many fossil crinoids than is the full-grown animal; certain fossil sea-urchins permanently retain such features as linear ambulacra and a pentagonal peristome, which characterise the young of their living allies; among Pelecypoda, the stages of early youth of oysters and Pectinidæ may be compared with palæozoic Aviculidæ. Among brachiopods, according to Beecher, the stages which living Terebratulidæ pass through in the development of their arm-skeleton correspond with a number of fossil genera. Among completely distinct groups also, ontogenetic characters have been successfully traced. The beautiful researches of Hyatt, Würtenberger, and Branco have shown that all ammonites and ceratites pass through a goniatite-stage, and that the inner whorls of an ammonite constantly resemble, in form, ornament, and suture-line, the adult condition of some previously existing genus or other.

Series of forms whose successive links correspond with successive stages of development in their youngest, still existing representatives are the only ones that furnish us with an incontestable picture of the path along which any given assemblage has evolved. These are the kind of genealogical trees that form the worthy goal of palæontology. From them a natural system will arise of its own accord. But from this goal we are unhappily still far removed. As a rule our palæontological trees lack an ontogenetic foundation, and that the foundation itself may be constructed in an arbitrary manner is best shown by the unsatisfactory condition of our ammonite-literature.

The time, it seems to me, has not yet arrived for the thorough reform of zoological classification on a phylogenetic basis. Among Protozoa and Cœlenterata there are absolutely no satisfactory fixed points for the phyletic arrangement of the various groups. Among Echinodermata it is proved that the correspondence in embryonic development between Asterozoa and Echinozoa is evidence of a common origin; but the classification of the various Classes is as yet affected only to the smallest extent by the facts of ontogeny and phylogeny. The union of Bryozoa and Brachiopoda into a special phylum—the Molluscoidea, and their connection with the worms, depend entirely on embryological comparison: in their later development the two Classes go so far apart that we can find no further parallel between them; and although the beautiful researches of Beecher, Clarke, and Schuchert on the phylogeny and ontogeny of the Brachiopoda will furnish a solid foundation for a new and better classification of the Class, the first adumbration thereof is still somewhat doubtful. On the other hand, researches on the development of the shell in Mollusca are, without doubt, full of promise. What results we have to expect in this field is shown by the labours of Jackson, Hyatt, and Branco, though it must be confessed they still afford no sufficient basis for a classification of the pelecypods and cephalopods.

Palæontology has made its deepest mark in the classification of the Vertebrata. Here we frequently come across firmly-rooted genealogical trees. Phylogenetic and ontogenetic facts have effected the removal of the order Solipedia and the natural grouping of the ungulates. The discovery of the fossil Condylarthra and Creodontia has brought to light unlooked-for relationships between ungulates and carnivores. The remarkable fauna of the Puerco Beds contains, according to Cope, almost completely indifferent mixed types, which cannot be considered either as true ungulates, beasts of prey, or rodents, nor can they even be regarded as typical Condylarthra, Creodontia, or Lemuria: scarcely can they be dovetailed into the framework even of a geological classification, since they show relationships in the most varied directions.

And here we approach an important question of principle. The larger categories in botany and zoology are almost exclusively based on the investigation of forms that still exist; and it is only in those divisions where the fossil forms surpass the recent ones, in number or in variety of organisation, that they, too, have been taken at all into account. As a rule it has been thought good enough to wedge in the extinct Orders or Families between the groups erected by the botanists and zoologists. Thus the foundations of the system remain intact. It is only recently that attempts have been made to reconstruct individual divisions of the zoological system, to a certain extent from below, on a palæontological basis. Thus, Scudder has established a Sub-Class, Palæodictyoptera, for all palæozoic insects, because they possess a series of common, indifferent characters, and show as much morphological correspondence one with another as they do with the later Orthoptera, Neuroptera, and Hemiptera, whose predecessors can already be clearly recognised among the palæozoic forms, although they have not yet attained the complete differentiation of their later descendants. Could we resurrect the numerous genera of the Puerco Beds and place them among our fauna of to-day, we should doubtless arrange them in one common Order, more or less corresponding to the marsupials; for, like the marsupials, they possess characters that, at all events, point in the direction of Orders more clearly differentiated later on, in which Orders we are at present accustomed to enrol them.

If the zoological and botanical systems were now to be created for the first time, they would in many respects probably assume a different appearance. They would have to represent clearly the natural relationship and the derivation of the organisms. The geologically oldest representatives of any of the larger assemblages, which are as a rule also the most generalised and most primitive, would have to be united under a special name, and would be regarded as the common root of the Orders, Families, Genera, etc., proceeding from them. But it is only in a few cases that palæontology could furnish the materials required for a reform of this kind. As a rule, and especially among the invertebrates, the primitive generalised types are missing, and we should be obliged to begin with those branches and twigs from our stems which are already more clearly differentiated, and of which the majority stretch down as far as the creation of our own day. Here again, then, we should be led to ground our classifications on those organisms of which we were in a position to investigate not merely certain fossilisable elements, but the whole anatomy, physiology, and embryology.

The function of classification, however, is not only to arrange organised beings according to their relationship, but also to facilitate our survey of life's infinite variety of form. It was to this intent that the older systematists constructed their various categories. They it is that have historic rights; and just as little as we geologists are inclined, without urgent need, to alter the historic conceptions and the divisions that have been handed down to us, just so little is it advisable to be incessantly remodelling the systems of botany and zoology. The doctrine of descent has, of course, violently shaken the solid framework of the older classification. The ideas—variety, mutation, species, genus, family, order, etc.—have become indefinite and unstable; the boundaries between the systematic groups are constantly being displaced, and bonds are burst that were once tightly bound. An important part is played to-day by subjective opinions, and when I think of the anxiety with which we elders—we who received our scientific education before the Darwinian era—proceeded to found a new species or genus, and compare it with the light-hearted manner in which to-day species, genera, families, and orders are set up and again put down, I am herein most forcibly impressed by the difference between then and now. The domination of the Linnæan and Cuvierian principles threatened systematic biology with soulless paralysis: the unbridled subjectivity of recent times may easily lead to anarchy. When, after investigating a certain number of forms, every author feels called upon to reform the classification, and where possible to introduce a new terminology, then arises the danger that we shall lose our comprehensive survey of the richly varied organic world, and that we shall use a language intelligible only to the most narrow specialists and repellent to every layman.

With this warning let me conclude. The Theory of Descent has penetrated the descriptive branches of natural science with new ideas, and set before them a nobler goal; but we should never forget that it remains only a theory, and one that has to be proved. I have tried to make plain how greatly it is indebted for its establishment to palæontological research; only I dare not conceal how many gaps are constantly brought to light in the very process of our argument. Science strives in the first place for truth. And the more clearly we keep ourselves conscious of the insecurity of the foundation on which our scientific theories rest, the more actively shall we bestir ourselves to strengthen it by new observations and new facts.

KARL V. ZITTEL.

II.

Field-Club Work in Ireland.

ONE of the most cheerful features accompanying the spread of educational influences throughout Ireland is the support given to societies whose aim is the study of things pertaining to the island. The ancient and still living language, the pre-Christian tombs and dwellings, the early churches, the Norman fastnesses, and, above all, that art of ornamentation which is recognised throughout Europe as essentially Irish, all exercise their local fascination, and all thus tend to bring about that collaboration of intelligence which forms the only true foundation of national life. Such researches, moreover, cannot fail to encourage the study of history on a scientific basis, a fact which may in time atone for the omission of the subject from the curriculum of the National schools. As an example of how scientific enquiry, with its painstaking observational methods, may go to the root of the conditions of life in any district, I may cite the remarkable paper on "The Ethnography of the Aran Islands,"[1] by Professor Haddon and Dr. C. R. Browne. This is, we may hope, only the first of a series of dispassionate enquiries into the racial and tribal characteristics of the people of Ireland; and it will serve as a model for similar studies in other countries, however "civilised" the community may be. Dr. Browne has already followed the matter up by a study of "The Ethnography of Inishbofin and Inishshark, County Galway"[2]; and the two papers, covering 116 octavo pages, reveal to us what a charming field lies open in "the proper study of mankind." The observation of màn, amid his varied surroundings, especially where he has long been subject to the same controlling influences, is, as far as our islands go, a much neglected branch of natural history, and we know more of many Pacific aborigines than of ourselves. The Dublin Anthropometric Committee, in connection with Trinity College and the Royal Irish Academy, has already made a brilliant start towards remedying this defect.

But probably a quietly spreading movement, mainly guided by those who modestly call themselves "amateurs," is doing more towards a complete knowledge of Ireland than even any of the older and more professional societies could effect. I refer to the growth in

[1] *Proc. Royal Irish Acad.*, ser. 3, vol. ii. (1893), p. 786.
[2] *Ibid.*, vol. iii. (1894), p. 317.

Ireland of Naturalists' Field Clubs, which foster the observation of local details and of those seemingly trivial points of difference, the sum total of which forms the natural history of the island. The specialist must be called in to correlate the observations and to decide upon the true nature of the objects collected by the members, but the existence of the field-clubs vastly widens the area over which his material can be procured.

Mr. R. Lloyd Praeger[3] has recently retraced the history of the four associations that are already in existence. The Belfast Naturalists' Field Club, which now numbers some 500 members, admits archæology as well as natural history within its scope; but in its published work it is clear that the more popular study has never been allowed to overshadow the purely scientific branches. The Belfast Club arose in 1863 among the students who had been attracted by Professor R. Tate's courses of lectures in connection with the Science and Art Department. The appendices to its annual reports have now been published as a separate volume, and form a work of constant reference for Irish geologists, botanists, and zoologists. Mr. Stewart, one of the original members, is still ready, in his official position at the Natural History Society's Museum, to give help to the large body of younger workers, field botany having always been a favourite subject in the north. Messrs. C. Lapworth and Wm. Swanston published their famous researches on the graptolite beds of Co. Down in the *Reports* of the Belfast Field Club; and Mr. Joseph Wright contributed, and still contributes, to the same publication his observations on fossil and recent Foraminifera. The presence of serious workers like these in the industrial centre of the north has given a distinct stimulus towards scientific investigation; and the foundations of their published work have been laid, in the manner of true naturalists, during long excursions by field or flood, in the open air. The indoor meetings of the club during the winter provide for the thorough discussion of results; and the excursions during the fairer season at least serve to introduce the members to the beauties of the country near their homes. The artistic work of that keen observer and photographer, Mr. Robert Welch, has already set permanently on record hundreds of antiquarian and scientific objects which have been thus examined. Latterly, one excursion, lasting several days, has been made to some point not accessible within the limits of a single day. The members participating in this return with abundant material collected from shore and heath and mountain, and with photographs illustrating the details of the geology of the district studied. The days chosen for this excursion include those set apart in the north of Ireland for reviving the political animosities of the Boyne. And thus the Naturalists' Field Club is sustaining the best traditions of science in ignoring completely all traditional differences of class or creed or race.

[3] "The Irish Field Clubs," *Irish Naturalist*, vol. iii. (1894), pp. 141, 211, and 247.

For many years Belfast had to remain content with having set forth an example; but in 1885 the exertions of Professor A. C. Haddon in Dublin resulted in the development of a second field-club. From this, considering the other societies existing in the capital, archæological subjects are wisely excluded; and the tenth year of the club's existence sees it in a sound and flourishing condition. While in Belfast the professional element has of late remained almost too much in the background, in Dublin the danger has lain in the opposite direction, and the cordial coöperation of workers already known in the Royal Dublin Society and the Royal Irish Academy no doubt tended at one time to restrain the ardour of the amateur. But at present the Dublin Naturalists' Field Club has a vigorous and independent life, and has been fortunate in securing as its secretary Mr. R. Lloyd Praeger, who acted for some years in the same capacity for the older society in Belfast. The general plan of work in the two clubs is similar, the winter meetings of the Dublin one being held in the handsome and classic rooms of the Royal Irish Academy.

In 1892, Mr. J. L. Copeman suggested the formation of a similar club in Cork; and Mr. F. Neale also succeeded, later in the same year, in founding a fourth field-club in Limerick.

The publication of that bright and well-printed journal, the *Irish Naturalist*, which is now in its fourth annual volume, has provided a means of communication between all the clubs, and between them and many older natural history societies. The Belfast Club still issues independent *Proceedings* of its own, but its meetings are chronicled in the pages of the *Irish Naturalist*. The February number of this journal for the present year bears striking evidence of the work that is quietly going on, even during the pressure of ordinary field-excursions. Mr. G. H. Carpenter, President of the Dublin Field Club, there describes the "Animals found in the Mitchelstown Caves,"[4] on the occasion of a visit lasting two hours, during a joint excursion of the three southern field-clubs. In the same number, Mr. J. N. Halbert discusses the insects collected above ground during the three days of this joint excursion to Fermoy and Mitchelstown; and these two papers show how scientific knowledge may be distinctly promoted on such occasions, despite those serious persons who are apt to look on a field-day as a sort of sanctified picnic.

After all, and especially in a country where scientific training is not easily obtained, the educational side of field-club work is at the outset of paramount importance. The training of the eye in the open country is the training of the whole individual, body and soul. And the feeling that in every county there are observers with the same aims, the same enjoyments, ready to assist, to correlate, to confer, leaves no beginner to cool his enthusiasm in discouragement. Already a Field-Club Union has been formed, through the agency of

[4] *Irish Naturalist*, vol. iv. (1895), p. 25. A full account of this paper was given in NATURAL SCIENCE for March, vol. vi., p. 148.

which the prominent workers of one club may be sent on missions to the members of others; and the Conference of the four existing clubs, which is to take place at Galway, and to occupy eight days in the present summer, will probably result in the foundation of still further associations. The Belfast Naturalists' Field Club, always in the van, has recently organised courses of lectures and practical work for its members, to provide them with a sound basis for their observations in the field; and this scheme promises to be a most healthy factor in the educational system of the north. Already the naturalist in Ireland would feel lost without the warm and ready fellowship of the field-clubs; while every year these organisations are adding solidly to the natural history of the least known and perhaps the most fascinating of our British Isles.

Grenville A. J. Cole.

III.

The Last of the "Challenger."

THERE is a little picture on p. 1,608 of this last volume[1] of the "Challenger" Reports, full of pathetic meaning to all who love the sea, to every naturalist who has gone down to the sea in ships. It represents H.M.S. "Challenger" in 1895, a dismantled hulk, the

"last scene of all,
Sans teeth, sans eyes, sans taste, sans everything.'

It is hard to realise that we have reached the last of the noble volumes that chronicle the doings of the "Challenger." Twenty-three years ago she sailed on the great voyage that has made discovery of regions of our planet comparable only with those of Columbus, Gama, and Magellan. Little more than a quarter of a century ago three-fifths of the globe, covered by the ocean, was barely known to us, and the "Challenger" and other expeditions have now broken up this great deep of ignorance, with results "measureless to man," but set forth in a series of volumes, the mere bulk of which conveys an adequate impression of their gravity. There are two classes of men who ought to inspect these volumes: the grumbler, belonging to the *demi-monde* of science, who wails that the British "Government does nothing for science," who points to this foreign undertaking and that foreign state-aided system of education in science; and the "pure and blameless taxpayer" who wants to see his money's worth, something not bricks and mortar, that makes for national progress in discovery. The intelligent foreigner could probably help us all to a better appreciation of the magnitude of our great scientific institutions, not the least of which has been the "Challenger" voyage and the recording of its oceanographical results.

The first portion of this volume is devoted by Dr. John Murray to a history of oceanography, dealing successively with an account of the aims and limits of oceanography, a historical treatment of the

[1] "Report on the Scientific Results of the Voyage of H.M.S. 'Challenger.'"
"A Summary of the Scientific Results," by John Murray.
"Report on the Specimen of the Genus Spirula collected by H.M.S. Challenger," by the Right Hon. T. H. Huxley, F.R.S., and Professor Paul Pelseneer.
"Report on Oceanic Circulation," by Alexander Buchan, M.A., LL.D., F.R.S.E. 2 vols. Printed for H.M. Stationery Office, 1895.

views of the ancients, beginning with the Phœnicians whose mercantile expeditions were of such astonishing range, and the Greeks who first directed attention to the scientific problems of the ocean, and of the views entertained during the Dark Ages, the Middle Ages, and the Renaissance; next, with the progress of knowledge from Magellan to Cook—from Cook to the "Challenger" and the subsequent expeditions. This account, filling over a hundred of the familiar pages of the "Challenger" Reports, is most clearly and attractively written. It is illustrated with a series of coloured maps showing the progress of our knowledge, the world-maps of the ancients being of particular interest. I should greatly like to see this account made accessible to many readers as a separate publication. From its simple and fascinating style it would even make a school-book of the kind that compels study. A school-boy, to my knowledge, broke off a bloodthirsty tale for the attractions of this chapter accidentally discovered, and he read it all. It is a triumph of the literary exposition of a subject that might have been a dreary piece of pedantry. It is to be hoped that Dr. Murray may be prevailed upon to adapt this wonderful narrative to a wider circle than the readers of "Challenger" Reports. If there were added to it a more extended account of the modern voyages of research, it would make a book of the sea of the highest educational effect on a maritime people.

These great voyages of discovery did nothing for the deep-sea until the middle of this century, and both biologists and geologists have to thank telegraphic enterprise for the first impulse towards a systematic exploration of the floor of the ocean. The desire to effect telegraphic communication between Europe and America led to the birth of the modern science of oceanography, which until then reckoned only with the surface and imperfectly with circulation. The apparatus of deep-sea work had to be invented, and with steady progress in this invention very exact observations can be made, though in all cases such observations are, from the nature of the case, indirect. No better example is likely to be found of the influence of progress in one science on progress in another than this exploration of the ocean resulting from discoveries in telegraphy. Let it not be forgotten, however, that telegraphy was only the opportunity. The first self-registering thermometer was made about 1757 by Cavendish, who suggested that it might be applied to ascertaining "the temper of the sea at great depths." It was so applied by Irvine in 1773. De Saussure and others continued such observations. Again, specimens from the great depths were obtained by Sir John Ross in his voyage to Baffin's Bay 1817–18, and by his famous nephew, Sir James Ross and Dr. (now Sir Joseph) Hooker on the Antarctic voyage, who carried out remarkable deep-sea soundings.

There follows next a general summary of the scientific observations and results at each of the "Challenger" observing-stations, making, in fact, a scientific log of the voyage. This log, as we may call it, occupies, with its details, the greater portion of both parts of this volume, and it is copiously illustrated with charts of the route beautifully engraved. At the first station the first attempts were made at sounding and dredging, the object being chiefly to exercise the seamen in the use of the apparatus. We read: "At 8.10 a.m. took first sounding, and made first attempt at dredging. In hauling in, the sounding line carried away, and a thermometer was lost. At 1 p.m. the dredge came up capsized," etc. Such was the beginning! However, a subsequent attempt at this station rewarded the voyagers with two new species, one being a new generic type; and these were never obtained elsewhere. It would be impossible to exaggerate the value of this great record of the voyage, enriched with notes from the author's own journal and the manuscript journals of Moseley and Willemoes-Suhm, and, of course, based on the official log and the published reports. It forms the companion volume to the Narrative.

At the end of this great summary it is analysed in a series of tables illustrating the bathymetrical and geographical distribution of the organisms captured. The interest culminates in Dr. Murray's general observations on the distribution of marine organisms, where his extraordinary grasp of the bewildering mass of facts and observations is focussed and his views lucidly expounded. It would be vain to attempt here even an outline of this colossal labour, but I cannot resist making an extract dealing with Dr. Murray's speculative views on the determining influence of the origin of climate on distribution. Commenting on the strong resemblance between the north and south polar marine faunas, and the absence of similar forms in a high proportion of cases from the intervening tropical belt—a resemblance extending to the pelagic flora, and even, as I have shown elsewhere,[2] to the more variable coast marine flora—the author submits the following daring hypothesis. "In early mesozoic times cooling at the poles and differentiation into zones of climate appear to have commenced, and temperature conditions did not afterwards admit of coral reefs in the polar area. But the colder and hence denser water that in consequence descended to the greater depths of the ocean carried with it a large supply of oxygen, and life in the deep seas became possible for the first time. There have been many speculations as to how a nearly uniform temperature could have been brought about in sea-water over the whole surface of the earth in early geological ages, as well as to how sufficient light could have been present at the poles to permit of the luxuriant vegetation that once flourished in these regions. The explanation that appears to me the most satisfactory is the one which attributes these conditions to the very much greater size of the sun in the early stages of the earth's history—an idea first introduced into geological speculations by Blandet, who likewise discussed the relations of Arctic and Antarctic faunas—together with the greater amount of aqueous

[2] *Phycological Memoirs*, part iii., 1895.

vapour in the atmosphere and the greater mass of the atmosphere." He proceeds: "The pelagic algæ, radiolaria, and foraminifera above referred to are probably the but slightly modified descendants of a very ancient, universal, pelagic fauna and flora. Life, in its simplest form, most likely first appeared in pre-Cambrian times in the detrital matters laid down about the mud-line, when the mud-line generally was not so deep, and the land surfaces were more extensive than at the present time. From these simple forms, which would occasionally be carried into the superincumbent waters, the pelagic protophyta and protozoa, which peopled the surface-waters of the pre-Cambrian ocean, were most probably derived." It is impossible here, as I have said, to give even the merest glance at the facts and arguments cited in support of these hypotheses. I believe I am right in saying that Dr. Murray arrived independently at the view ascribed by him to Blandet, and that his discovery of Blandet's priority was made after the idea had shaped itself in his mind, though he puts forward no claim to it here. It is, however, one that will meet with criticism and discussion—a discussion that cannot fail to help on the process of digesting farther the great granary of facts preserved in this volume.

There follows a gigantic index of the genera, sub-genera, species, and varieties, which serves also as an index to the species procured by the "Challenger," described and referred to in the Reports, since on looking up the pages in the station lists the reader will find references to the parts in which the species are fully described. There is also a general index to the summary.

As appendices there are printed the Report on *Spirula*, by Mr. Huxley and Professor Pelseneer, with Mr. Huxley's beautiful drawings, and the report by Mr. Buchan on "Oceanic Circulation," illustrated with sixteen double-page maps. Perhaps Mr. H. N. Dickson, or some other competent authority, will give us a brief account of this splendid monument of Mr. Buchan's labour and research. No student of the present distribution of organisms can dispense with this great memoir, but all should learn in brief its main teaching.

It is difficult to refrain from extending to Dr. John Murray personal congratulations on the completion of this great labour of editing and of writing the "Challenger" Reports. From his brief editorial notes prefixed to the present volume the reader will learn the kind of obstacles he has had to surmount. All naturalists now know of the perseverance and brilliant success of his great achievement. This has been rewarded mostly by the recognition of foreign academies. The fountains of honour in this country will, it is to be hoped for their own credit, now begin to flow towards the editor (after the lamented death of Sir Wyville Thomson) of the "Challenger" Reports, the author of some of the finest work embodied in them, one of the most indefatigable of the naturalists who sailed on the memorable voyage.

GEORGE MURRAY.

IV.

Some Definitions of Instinct.

THE phenomena of instinct are of interest both to biologists and to psychologists; who respectively approach them, however, from different standpoints. Whether the divergences of opinion concerning these phenomena, and the diversities of definition of the terms "instinct" and "instinctive," are mainly due to this cause, it is perhaps difficult to decide. That marked differences do exist is only too obvious.

1. **Relation of Instinct to Consciousness.**—"Instinct," says Professor Claus,[1] "may be rightly defined as a mechanism which works unconsciously, and is inherited with the organisation, and which, when set in motion by external or internal stimuli, leads to the performance of appropriate actions, which apparently are directed by conscious purpose." Here, then, we have instinct defined as essentially unconscious. Mr. Herbert Spencer[2] regards instinct in its higher forms as probably accompanied by a rudimentary consciousness; but he does not consider the presence of consciousness essential. Professor Baldwin speaks[3] of a "low form of consciousness which has not character enough to be impulsive"; while Professor Calderwood[4] holds that instinctive activities cannot be attributed to mental power. "The entire chapter on Instinct in Darwin's 'Origin of Species' must," he says, "be read in an altered form, consequent on the deletion of the references to 'mental faculties.'"

On the other hand, Romanes commences his definition of instinct with the words[5]: "Instinct is reflex action into which there is imported the element of consciousness." "The term comprises," he says, "all those faculties of mind which are concerned with conscious and adaptive action, antecedent to individual experience." "The stimulus," he adds, "which evokes an instinctive action is a perception." Professor Wundt also emphasises the conscious accompaniments of instinctive activities, which, he says,[6] "differ from

[1] "Text-book of Zoology." Eng. Trans., vol. i., p. 94.

[2] "Principles of Psychology," ch. xii.

[3] "Text-book of Psychology." *Feelings and Will*, p. 308. He also speaks of instincts as "inherited motor intuitions," p. 311.

[4] "Evolution, and Man's Place in Nature," p. 190.

[5] "Mental Evolution in Animals," p. 159.

[6] "Lectures on Human and Animal Psychology," Eng. Trans., p. 401.

the reflexes proper in this, that they are accompanied by emotions in the mind, and that their performance is regulated by these emotions."

Thus, even if we exclude the extreme views of those who hold that instinctive activity is due to connate ideas, and inherited knowledge,[7] there is a wide range of opinion on this head.

2. **Relation of Instinct to Impulse.**—Professor Wm. James speaks[8] of "instinctive or impulsive performances." "Every instinct," he says, "is an impulse," and he implies that every impulse is instinctive. Professor Wundt[9] and Herr Schneider[10] also regard instinctive activities as prompted by impulse; the last-named author distinguishing between sensation-impulses, perception-impulses, and idea-impulses. But other writers use the term in a more restricted sense. Professor Höffding, though he holds[11] that "instinct is distinguished from mere reflex-movement by the fact that it includes an obscure impulse of feeling," also tells us[12] that "impulse [here used in the narrower sense] involves a contrast between the actual and a possible or future. This," he adds, "is what distinguishes it from reflex-movement and instinct, where the excitation may perhaps cause a sensation, but where no idea asserts itself of what must follow." Professor Baldwin distinguishes[13] between those stimuli and the reactive consciousness which, as originating mainly from within, may be called in general *impulsive*, and those which, as originating mainly from without, may be termed *instinctive*; but he admits that the distinction is inexact.

In introducing therefore into a description of instinctive activities any reference to impulse, the exact sense in which this word is employed itself needs definition.

3. **Relation of Instinct to Intelligence and Volition.**—Mr. H. Spencer describes[14] instinct as compound reflex-action. Although he states clearly[15] that "the actions we call rational are, by long-continued repetition, rendered automatic and instinctive"; yet his main thesis is[16] that instincts are developed on the path of upward development from reflex-action toward volitional activity. Others, who are not prepared to follow Mr. Spencer in his main contention, still regard instinctive actions as essentially involuntary. Such views may be contrasted with the opinions of G. H. Lewes[17] and Herr Schneider,[18]

[7] "Instinct and Acquisition." *Nature*, vol. xii., p. 507. Oct. 7, 1875. The passage is quoted *infra*, p. 325.

[8] "Principles of Psychology," vol. ii., p. 382. See also the passage quoted *infra*, p. 325.

[9] *Op. cit.*

[10] "Der Thierische Wille."

[11] "Outlines of Psychology," p. 91.

[12] *Op. cit.*, p. 322. *Cf.* also H. R. Marshall's "Pain, Pleasure, and Æsthetics," pp. 275–277.

[13] *Feelings and Will*, p. 304.

[14] "Principles of Psychology," ch. xii., § 194.

[15] *Op. cit.*, § 204.

[16] *Op. cit.*, § 211.

[17] "Problems of Life and Mind." *Instinct.*

[18] "Der Thierische Wille."

who regard instinct as due to lapsed intelligence; habits formed under intelligent guidance being inherited in the form of instincts. Professor Wundt seems to go yet further when he says[19]: "Instinctive action is impulsive, that is voluntary action; and, however far back we may go, we shall never find anything to derive it from except similar, if simpler, acts of will. The development of any sort of animal instinct, that is to say, is altogether impossible unless there exists from the first that inter-action of external stimulus with affective and voluntary response which constitutes the real nature of instinct at all stages of organic evolution." Thus, while Mr. Herbert Spencer regards instinct as primarily not yet voluntary; and while many writers regard it as no longer voluntary; Professor Wundt asserts that it is at no time involuntary.

4. **Relation of Instinct to Habit.**—The word "habit," like so many others in this connection, is used in different senses. Many writers describe all the activities of animals as their habits. In this sense we speak of habit as correlated with structure. But the term is generally used in psychology in a more restricted sense, and is applied to those activities which have become stereotyped under the guidance of individual control. A habit is, in this acceptation of the term, an acquired activity, the constancy of which is due to frequent repetition by the individual, in adaptation to special circumstances; and a distinction is drawn between such habits, as individually acquired, and instincts as connate.[20] Those who accept the Lamarckian hypothesis of the origin of instincts through "lapsed intelligence" regard them as the connate effects of the inheritance of acquired habit. Darwin[21] and Romanes[22] believed that instincts were in part due to this mode of origin. Professor Wundt, however, gives to the term a wider meaning, and so defines instinct as to include acquired habit. "Movements," he says,[23] "which originally followed upon simple or compound voluntary acts, but which have become wholly or partly mechanised in the course of individual life, or of generic evolution, we term *instinctive* actions." In accordance with this definition, instincts fall into two groups. Those, "which, so far as we can tell, have been developed during the life of the individual, and in the absence of definite individual influences might have remained wholly undeveloped, may be called *acquired* instincts."[24] They have become instinctive through repetition. "To be distinguished from these acquired human instincts are others, which are *connate*."[25] "The laws of practice suffice for the explanation of the acquired instincts. The occurrence of connate instincts renders a

19 "Lectures on Human and Animal Psychology," p. 409.

20 *See*, for example, Professor Sully, "The Human Mind," vol. ii., p. 184.

21 "Origin of Species," p. 206; "Descent of Man," vol. i., p. 102, quoted in "Mental Evolution in Animals," p. 264.

22 "Mental Evolution in Animals," p. 200.

23 "Lectures on Human and Animal Psychology," p. 388.

24 *Op. cit.*, p. 397.

25 *Op. cit.*, p. 399.

subsidiary hypothesis necessary. We must suppose that the physical changes which the nervous elements undergo can be transmitted from father to son . . . The assumption of the inheritance of acquired dispositions or tendencies is inevitable if there is to be any continuity of evolution at all. We may be in doubt as to the extent of this inheritance: we cannot question the fact itself."[26] "Darwin's explanation of the development of instinct as being mainly the result of passive adaptation seems," says Professor Wundt, "to contradict the facts."[27] Now the majority of writers on instinct distinguish it, as we have seen, from individually-acquired habit. And it is hardly necessary to state that Professor Wundt's explanation of the origin of connate instincts on Lamarckian principles, is not accepted by Professor Weismann and his school. "I believe," says Professor Weismann,[28] "that this is an entirely erroneous view, and I hold that all instinct is entirely due to the operation of Natural Selection, and has its foundation, not upon inherited experiences, but upon variation of the germ." In view of the biological controversy as to the inheritance of acquired characters, it would seem advisable so to define instinct as not in any way to prejudge the question of origin.

5. **The Instincts of Man.**—"The fewness and the comparative simplicity of the instincts of the higher animals," said Darwin,[29] "are remarkable in contrast with those of the lower animals." Romanes[30] held that "instinct plays a larger part in the psychology of many animals than it does in the psychology of man." "Recent research," says Professor Sully,[31] "goes to show that though instinctive movement plays a smaller part in the life of the child than in that of the young animal, it is larger than has been generally supposed." Professor Preyer[32] tells us that "the instinctive movements of human beings are not numerous, and are difficult to recognise (with the exception of the sexual ones) when once the earliest youth is past."

On the other hand, Professor Wundt[33] regards human life as "permeated through and through with instinctive action, determined in part, however, by intelligence and volition." And Professor James tells us[34] that "man possesses all the impulses that they (the lower creatures) have, and a great many more besides." The higher animals have a number of impulses, such as greediness and suspicion, curiosity and timidity, all of them "congenital, blind at first, and productive of motor reactions of a rigorously determinate sort. *Each of them, then, is an instinct*, as instincts are commonly defined. *But they contradict each other*—'experience' in each particular opportunity of application usually deciding the issue. *The animal that exhibits them*

[26] *Op. cit.*, p. 405.
[27] *Op. cit.*, p. 409.
[28] "Essays" (1889), p. 91.
[29] "Descent of Man," vol. i., p. 101.
[30] "Mental Evolution in Man," p. 8.
[31] "The Human Mind," vol. ii., p. 186.
[32] "The Mind of the Child": The Senses and the Will, p. 235.
[33] "Lectures on Human and Animal Psychology," p. 397.
[34] "Principles of Psychology," vol. ii., pp. 392, 3. Italics the author's.

loses the 'instinctive' demeanour, and appears to lead a life of hesitation and choice, an intellectual life; *not, however, because he has no instincts—rather because he has so many that they block each other's path.*" This is in tolerably marked contrast with the statement of Darwin's which stands at the head of this section!

6. **The Plasticity and Variability of Instinct.**—"Though the instincts of animals," said Douglas Spalding,[35] "appear and disappear in such seasonable correspondence with their own wants and the wants of their offspring as to be a standing subject of wonder, they have by no means the fixed and unalterable character by which some would distinguish them from the higher faculties of the human race. They vary in the individuals as does their physical structure. Animals can learn what they did not know by instinct, and forget the instinctive knowledge which they never learned, while their instincts will often accommodate themselves to considerable changes in the order of external events." It will be noticed that there are here two groups of facts: (1) variations, analogous to variations in physical structure; and (2) accommodations to changes in the external order of events. Professor James[36] says, "the mystical view of an instinct would make it invariable"; and he formulates two principles of non-uniformity of instincts, (1) that of the inhibition of instinct by habits; and (2) that of the transitoriness of instincts. The variation analogous to that of physical structure is not here explicitly recognised. Romanes, who defines[37] instinct as a generic term comprising "all those faculties of mind which are concerned with conscious and adaptive action, antecedent to individual experience . . . and similarly performed under similar and frequently recurring circumstances by all the individuals of the same species," appears to lay stress on their invariability; but his subsequent treatment[38] shows that he fully recognised the connate variability of instinct. Under the head of "plasticity" he also[39] insisted on "the modifiability of instinct under the influence of intelligence." He quotes, with approval, Huber's exclamation: "How ductile is the instinct of bees, and how readily it adapts itself to the place, the circumstances, and the needs of the community." There seems, however, some want of logical consistency in first defining instinct as connate and antecedent to individual experience, and then implying that, as modified under the influence of experience, it still remains instinct. For example, Romanes says[40]: "There is evidence to show that the knowledge which animals display of poisonous herbs is of the nature of a mixed instinct, due to intelligent observation, imitation, natural selection and

[35] *E.g.*, Douglas Spalding. *Instinct and Acquisition. Nature*, vol. xii., p. 507.

[36] "Principles of Psychology," vol. ii., pp. 391–4.

[37] "Mental Evolution in Animals," p. 159.

[38] *Op. cit.*, p. 190. *Cf.* Darwin, in the same work, pp. 372 and 383.

[39] *Op. cit.*, p. 203.

[40] *Op. cit.*, p. 227.

transmission." Other writers render the term "instinct" indefinite by including the effects of individual experience. Mr. A. R. Wallace, for example, says[41]: "Much of the mystery of instinct arises from the persistent refusal to recognise the agency of imitation, memory, observation, and reason as often forming part of it. Yet there is ample evidence that such agency must be taken into account." But would it not be well, one may ask, so to define instinct as to distinguish it from these agencies; and to say that the habits or activities of animals are of mixed origin, the term instinct being reserved for particular types of connate activity?

7. **The Periodicity and Serial Nature of Instinct.**—Little need be said on this head, since most writers recognise the facts as, at any rate in many cases, characteristic of instinct. The sexual instincts, nidification, incubation, and migration, exemplify the periodic nature of instinct; and the fact that this periodicity involves internal as well as external determination suggests the rejection of Professor Baldwin's distinction between impulsive and instinctive; not because it is logically incorrect, but because there is so much overlap, many instincts involving an impulsive factor. That instincts are very often serial in their nature and involve a chain of activities is also commonly admitted, and is well brought out by Herr Schneider.[42]

8. **Suggested Scheme of Terminology.**—From what has gone before, it will be seen that there is a good deal of diversity of opinion and of definition in the matter of instinct. Let us summarise some of these diversities.

Instinctive activities are unconscious (Claus), non-mental (Calderwood), incipiently conscious (Spencer), distinguished by the presence of consciousness (Romanes), accompanied by emotions in the mind (Wundt), involve connate ideas and inherited knowledge (Spalding); synonymous with impulsive activities (James), to be distinguished from those involving impulse proper (Höffding, Marshall); not yet voluntary (Spencer), no longer voluntary (Lewes), never involuntary (Wundt); due to natural selection only (Weismann), to lapsed intelligence (Lewes, Schneider, Wundt), to both (Darwin, Romanes); to be distinguished from individually-acquired habits (Darwin, Romanes, Sully, and others), inclusive thereof (Wundt); at a minimum in man (Darwin, Romanes), at a maximum in man (James); essentially congenital (Romanes), inclusive of individually-acquired modifications through intelligence (Darwin, Romanes, Wallace).

It is scarcely probable that in the face of such divergence of opinion unanimity is yet within the bounds of reasonable expectation, and the following scheme must be regarded as provisional and suggestive. Certain points must be borne in mind in endeavouring to frame satisfactory and acceptable definitions of the terms

41 "Darwinism," p. 442.
42 "Der Thierische Wille" *e.g.*, p. 208.

"instinctive" and "instinct." Since the phenomena are in part biological and in part psychological, any definition should be such as to be of biological value and yet such as to be acceptable to psychologists. Since the question of origin is still *sub judice*, the definition should be purely descriptive, so as not to prejudge this question. And since the phenomena of instinct can only be rightly understood in their relation to automatism, connate and acquired, to impulse, to imitation, and to intelligence, our definition of instinctive activities should find a place in a scheme of terminology. Such a scheme is here set forth.

Connate automatism: the congenital physiological basis of those movements or activities which are antecedent to individual experience.

Physiological rhythms: congenital (or connate) rhythmic movements essential to the continuance of organic life.

Reflex movements: congenital, adaptive, and co-ordinated responses of limbs or parts of the body: evoked by stimuli.

Random movements: congenital, more or less definite, but not specially adaptive movements of limbs or parts of the body: either centrally initiated or evoked by stimuli.

Instinctive activities: congenital, adaptive, and coördinated activities of the organism as a whole; specific in character, but subject to variation analogous to that found in organic structures; similarly performed by all the members of the same more or less restricted group, in adaptation to special circumstances frequently recurring or essential to the continuance of the race; often periodic in development and serial in character.

Mimetic movements and activities: due to individual imitation of similar movements or activities performed by others.

Impulse (Trieb): the affective or emotional condition, connate or acquired, under the influence of which a conscious organism is prompted to movement or activity, without reference to a conceived end or ideal.

Instinct: the connate psychological impulse concerned in instinctive activities.

Control: the conscious inhibition or augmentation of movement or activity.

Intelligent activities: those due to individual control or guidance in the light of experience through association.

Motive: the affective or emotional condition under the influence of which a rational being is guided in the performance of deliberate acts.

Deliberate acts: those performed in distinct reference to a conceived end or ideal.

Habits: organised groups of activities, stereotyped by repetition, and characteristic of a conscious organism at any particular stage of its existence.

Acquired automatism: the individually modified physiological basis of the performance of acquired movements or activities which have been stereotyped by repetition.

There is certainly some overlap in the definitions, and it is difficult to see how such overlap is to be avoided. The physiological rhythms—such as the heart-beat, respiratory movements, and peristaltic action—are in part automatic, in the physiological sense of originating within the organ which manifests the rhythm; but they are also in part reflex. The line between reflex movements and instinctive activities cannot be a very rigid one; instinctive activities are indeed in large degree organised trains or sequences of coördinated reflex movements.

Although the psychological aspect of instinctive activities falls under the general head of impulse, yet impulse is broader than instinct—that is, if we adopt the definitions above suggested. On the one hand, some reflex movements are probably accompanied by impulse. On the other hand, when intelligent activities pass into habits through repetition, the performance of these habits is prompted by impulse. Impulse may, in fact, be either connate or acquired, and may be associated both with automatism and with control. Instinct is a form of connate impulse. As such it may be counteracted or modified by an acquired impulse due to pleasurable or painful experience. A chick, for example, which has run after and seized a cinnabar caterpillar, acquires through experience a counteracting impulse due to the disagreeable effect. The connate impulses, termed instincts, may thus be modified by acquired impulses which result from experience; but there is seldom or never a conflict of instincts, as these are above defined.

Whether the objective activities termed instinctive are *always* accompanied by the subjective connate impulse termed instinct is a question which is open to discussion.

A wider definition of instinct by which it would be synonymous with connate impulse may be suggested as an alternative to that above given. This would, perhaps, be more in accord with the popular use of the word "instinctive," but it appears to be less satisfactory as a definition of the technical term.

It is well to distinguish motives, as the determinants of deliberate acts, from the acquired impulses which are the determinants of intelligent activities as above defined. As the intelligent activity is often the outcome of a conflict of impulses, so is the deliberate act the outcome of a conflict of motives.

Mimetic activities are due to a mimetic impulse. Some of them are probably involuntary and due to connate impulse; but others are certainly due to intelligent imitation. They form a group sufficiently well-defined to warrant the distinct place assigned to them in the suggested scheme.

The habits of animals are in very many cases of complex origin. It is claimed that such a scheme of terminology as is above suggested may serve to aid us in discriminating between the several factors, instinctive, mimetic, and intelligent. The fact that many instinctive activities are subject to modification through imitation and experience, clearly indicates that they at least are accompanied by consciousness. But it is submitted that, when thus modified, they cease to be instinctive, that is, if *congenital* is to take its place as an integral part of the definition of instinct. They should be termed habits.

The distinction between congenital or connate on the one hand and acquired on the other hand is a definite one. Objectively considered, those activities, the performance of which is, so far as they are concerned, antecedent to and irrespective of individual experience and guidance, are connate, no matter at what stage of life they are performed; while those activities, or modifications of activity, which are performed as the result of individual experience, are acquired—any modification of connate organic structure correlated therewith being an acquired character. Subjectively viewed, those impulses which are nowise dependent on antecedent experience of pleasure or pain are connate; while those which are due to individual experience are acquired. In any given case of animal habit it may be difficult to determine how far it is due to connate activity, and how far there is acquired modification. But this difficulty is more likely to be overcome by observation and experiment, if the exact terms of the problem are kept clearly in view.

C. Lloyd Morgan.

V.

The New Oban Cave.

ALTHOUGH the former treasures of the English caves are admired, it is so long since anything new has been disentombed, that cave-hunting is regarded by some almost as a lost art. It is, however, probable that many of the gaps still existing in our geological history will be filled by the study of cave-deposits. The more microscopic methods of examination now coming into vogue have already revealed much that was previously overlooked. Thus, as in the case of the rich finds of the Ightham fissure, we have been able to add to our Pleistocene fauna numerous small, but none the less interesting, species. However large the cave may be, pickaxe and shovel should never be used, except for the removal of fallen rock or the breaking through of stalagmite: every particle of the uncemented material should be removed with a knife, or, at the most, by a small hammer or trowel; it should then be sifted through six or eight sieves fitting into each other, and after having been carefully examined for friable things, washed through the sieves, with a judicious application of acid when such seems necessary. The last sieve should be fine enough to stop an ostracod, the gall of a *Cynips*, or a *Chara* capsule.

Between the discoveries of Dr. Hicks in the Welsh caves and the publication of a list of the contents of the Ightham fissure, a long rest intervened. But the succeeding twelve months have been far more fruitful. Other fissures further south have been worked, and, so far as the investigations have gone, have proved quite as interesting. That part of the fissures as yet examined is not older than the Kitchen Midden period; but the closer examination of the material has revealed whole sets of tools, perhaps the most delicate ever used by man. The British Association made a grant last year for the furtherance of these researches, and the results will shortly be published.

On the top of these comes a most interesting discovery in Scotland, where the later chapters of geological history are somewhat different from our own. There, for instance, the old Palæolithic man, whose remains are entombed in almost every rod of gravel from the Wash to the English Channel, is unknown; while Kitchen Middens, which were practically unknown in the South till those just mentioned were discovered, are quite plentiful in the North. A few years ago a cave of some interest was found at the back of Mr. J. W. Higgin's whisky distillery at Oban. Recently Mr. McArthur, while blasting rock on his property in Oban Bay, broke into another cave of great promise and interest. It is the object of the present note to give some few particulars of this cave, and for them I am mainly indebted to Mr. J. W. Higgin, the secretary of the Exploration Committee.

One of the first features to strike the eye of the casual visitor to this charming bay is the raised beach, some 35 ft. above the present one. It was upon this level that the quarrying operations were proceeding when the observant eye of Mr. McArthur fell upon a shell-bed. He immediately informed Mr. Higgin, who recognised the importance of the discovery, and, although rumour has it that many of the earlier finds were thrown away as having no connection with man, still the numbers in which they appeared soon showed that they could not possibly be mere freaks of nature. Upon exhibiting to the Archæological Society of Scotland one of the bone harpoons discovered, Dr. Anderson induced that body to supply the funds necessary for a thorough investigation. With this the severe winter interfered, but the following general facts may prove of interest.

The cave opens to the N.E., and, at its mouth, is about 20 ft. wide; it extends back about 30 ft. The roof was covered with stalactites, which, together with the overhanging rock, have all been cleared away, leaving the floor for examination. The mouth of the cave was artificially stopped by huge pieces of rock, which were placed right across it in the lower part, and subsequently occupied two-thirds of its width. The explorers have made two deep longitudinal trenches and one transverse one, and have removed some fifty cartloads of material. The excavations have revealed the following section:—

1. Rock-debris and humus	3 ft.
2. Shell-bed	12 to 26 in.
3. Beach gravel	3 ft.
4. Shell-bed	3 to 26 in.
5. Stalagmite, sand, bones, etc...	1 ft. 8 in.
6. Bed-rock .. .. at	12 ft.

1. The accumulation of this extensive deposit indicates a considerable lapse of time since the deposition of the underlying shell-bed. At the close of this period, a stray visitor appears to have crawled in and made the cave his last resting-place, as his skull is there to testify.

2. This layer is extremely interesting. It seems to be the equivalent of the ordinary Kitchen Midden material, since it consists of the shells of the limpet, quantities of the large *Pecten* so plentifully found in many of the Scotch glacial deposits, oysters, mussels, cockles, sea-snails (*Natica?*), winkles, crabs, and other edible species. The limpets are said to bear evidence of having been roasted before being eaten. The various species are often found in heaps, and not irregularly mixed with other shells as on a shore. Dispersed through this bed are also the bones of fish, birds, and mammals. The latter are in the hands of Sir Wm. Furness for determination: some of the deer antlers are of very large dimensions, suggestive of

Megaceros. Bone and flint implements are also found here: the former are very numerous and include some beautifully double-barbed harpoons, one of which, 6½-in. long, with nine barbs and a rounded pierced end, was probably used as a detaching harpoon; another, not pierced, which was evidently hafted, measures 4½-in. in length and has four pairs of barbs, being very similar to a tool found in the Victoria cave. There are numerous fragments of these tools, and a quantity the use of which is not quite clear; but the repetition of the same types determines their human origin however unable we may be to decide what was their use. Some of the bone tools are of chisel form and others are suggestive of arrow tips. So preponderating are these bone tools that one might assign the deposit to a "bone age," were it not probable that the extensive use of this material is to be attributed merely to the absence of flint in that part of the country, for in the Hastings Kitchen Midden reverse conditions have produced reverse results. Flint implements have, however, been found, and this is all the more interesting from the fact that the nearest locality for this form of silica is probably Mull.

3. This bed of beach-gravel is evidently of marine origin, and was deposited by the sea when it rose to this height, or when the land and sea had a relative difference of some 35 ft. as compared with the present configuration of the country.

4. The succeeding shell-bed is another relic of human habitation; and although greatly affected and decomposed by the overlying bed and the conditions under which the latter was accumulated, it contains a large number of precious remains in the form of bone and flint implements, animal bones, and, above all, the bones of the men themselves. The human remains include one skull, two lower jaws, and several limb-bones. Exact details concerning these will be impatiently awaited; it is, however, reported that the cephalic index of the skull from the upper layer is 75, while that of the skull from the lower bed is considerably less. During this tenancy it seems that the land was sinking and the sea rising; so that against the incursions of the tide the troglodytes endeavoured to build a wall right across the mouth of the cave. But their efforts were in vain, and man was driven forth from the cave, unable to return until after the re-elevation of the land and the deposition of three feet of shingle. How long a period this represents we cannot say until a full report has been made on the contents of beds 4 and 2 respectively; and it is to be hoped that every inch of the former will be carefully examined, for it may yield many more human bones.

5. Even this lower stalagmitic layer contains treasures sealed up in it, and these may carry us back to a period of whose remoteness we have as yet no idea. This, however, we can say, that since man made the attempt to stem the incoming tide at Oban, that part of Scotland has been elevated some 35 or 40 feet.

W. J. Lewis Abbott.

VI.

Bibliographical Reform and the "Zoological Record."

IN previous communications which I have made on the subject of Bibliographical Reform, principal emphasis has been laid upon such parts of the proposed system as are essentially new, to the exclusion of certain other features which are already realised, and which consequently seem almost self-evident. The task which we are undertaking is a reform in the truest sense. We seek merely to perfect what already exists; and, if we have placed before ourselves a certain organisation as the ideal towards which we are striving, it must not be supposed that we desire to cut loose from existing bibliographical undertakings, or to rival them. Just the contrary is the case. The large body of disinterested zoologists, of whom I have become the rather unwilling spokesman, seek above all to attain the closest possible harmony of action with all those who are now engaged in this arduous but necessary bibliographical work.

It is in this spirit that we now approach the *Zoological Record*, and offer it the support which an international organisation can give. But before I turn to the nature of this offer, I shall be obliged to say a few words in regard to the imperfections in the present *Record*. This is not an easy task. To one who has followed, as I have done, the history of this enterprise, who knows from numerous friendships among the Recorders the personal sacrifices that have been made for its success, who knows also the financial burden which the Zoological Society has assumed in its behalf, it is difficult to criticise frankly the product of that devotion. In point of fact, during my recent visit to England I shrank more than once from repeating what I had heard from all sides. I should not touch upon it even now, if I were not able to follow the statement with a definite proposition which it is believed will remedy those defects.

In the course of the agitation of this question, which I have carried on continuously for several years, I have naturally been able to collect a large number of opinions in regard to the need for a thorough reform. I speak, therefore, with a certain degree of confidence in stating that the *Record* is found in practice to be sadly inadequate. If anyone doubts the disinterestedness of the opinions thus gathered, I should merely point to the various notes in NATURAL SCIENCE, in *Nature*, in the *American Naturalist*, and elsewhere, written for the most part without my knowledge. They all assume the insufficiency of the existing bibliographical means. Indeed, no one has thought it necessary to demonstrate it. I would also point to the committees nominated, some quite independently, to consider the possibilities of a reform. Looking back upon my recent hasty visit to England, it seems to me that the conviction is almost as strong in England as in other parts of the world. Indeed, were it not for the fear of violating confidences, I am sure I could base my entire plea on the statements made to me by English zoologists personally connected with the *Record*. It is needless to insist upon the entire disinterestedness of such persons, or upon their desire to raise the standard of the work. I sincerely hope it will be regarded needless in my own case as well. Let us then face the matter frankly: the *Record*, in spite of all the efforts that are being made in its behalf, is sadly imperfect, and any serious attempt to raise its standard of excellence deserves the hearty support of every English zoologist.

Before turning to consider the means of improving the *Record*, however, I desire to point out another serious defect. Here, I am sure, no one will contradict me. The maintenance of this journal imposes excessive financial burdens on the Zoological Society. Some one has said: "No publication can succeed that is not a commercial success!" I should be loth to insist upon the exactness of this maxim as universally applied, but I think that the experience of the *Record* is a singular confirmation of its justness *in petto*. What has been the answer to those who complained of the exclusion of extinct forms from most of the chapters of the *Record?* "We cannot spend more than we do. We lose £350 annually. We cannot assume a greater burden." Before this astounding statement it was obviously impossible to press the matter. No larger sacrifice can be asked. The sacrifice is already too large.

The defects of the present *Record* are of various kinds. Some are trifling matters of detail and result from a faulty system. Their elimination would require no essential change, and they may be neglected entirely for the moment. Others, such as the want of an author's index, require additional labour. It is with these more serious faults that I propose to deal, selecting from among them the most difficult of all, viz., incompleteness.

If the *Record* is to meet the needs of the zoological world, there must be the assurance that no really important paper has been omitted. For a purely morphological record, this word "important" implies, doubtless, a considerable restriction, but even then, exclusion must be very sparingly used. It is obviously undesirable that the innumerable notes, published often with no intention of contributing to knowledge, should be dragged forth from their obscurity and placed upon permanent record, where every serious student will encounter them. Such a procedure would be only an impediment, no aid. With systematic zoology, however, the case is different. Our rules of nomenclature are explicit on this point. All descriptions of new species, at least, are "important," and the *Record* fails in its object if it overlooks them. Indeed, it is for the obscure notes that the agency of the *Record* is the most useful. The others we find without its aid.

Leaving out of account then certain purely popular articles, there remain, nevertheless, about 8,000 contributions annually, distributed over at least 800 journals, which must be recorded. Furthermore, a very large number of these papers deal with several groups of animals, and should be treated by several different recorders. With very many the title bears no indication of this diversity. Often, finally, very important incidental observations are inserted in a memoir on a totally different topic. These are commonplaces, known to everyone. If anyone care for an example, let me refer him to the *Biologische Centralblatt* for April 1, 1894, where I have given a partial bibliography, in which the most important observations are all published as incidental items.

Here, then, is the task of the present recorder. He is expected to pass in review all these titles, published in at least a dozen different languages, and to record every observation relating to his special group, whether that observation form the substance of an entire memoir, or whether it be merely an incidental note. After doing that, the recorder will be rewarded, if he is concerned with a small group, with a list of 20, 30, or even 40 titles! Personally, I doubt whether any recorder has ever been so conscientious as that, and I am very sure that having been so once, he never would be again. No! many recorders confess frankly that their own records are incomplete. Indeed, the study of the various records is a delightful exposition of comparative conscientiousness. Some recorders, I know, merely note the literature that comes to their notice, and undertake no search for titles.

What then is the remedy? Instead of each recorder having to search through the entire literature for his titles, there should be a single competent person designated to do this work adequately, and once for all. It may be replied that these are impossible conditions. What is a *competent* bibliographer? In addition to being methodical and painstaking, this person must be a trained zoologist; otherwise his allotment of the titles could never be depended upon. Moreover, he should have a reading knowledge of all the languages in which zoological papers are published. I have never met a person who could fill all these conditions. There must, then, be a staff of zoologists, not a single man. The work must be divided. However, the sum total of the work to be performed must, as I have ascertained by actual tests, be counted as requiring the entire time of one person. Obviously then, such a service organised solely by the *Record*, and with provision made for classing Bohemian, Hungarian, Polish, Russian, and such other languages as might be unfamiliar to the bibliographer, could not be realised in England for less than £300 per annum.

It is here that the value of the Central Bureau becomes evident. Relying upon various other contributions for its principal support, the bibliographical service of the Bureau would be able to do this work for the *Record*, provided the latter could assume merely a small share of the entire expense. Indeed, an annual grant of, say £75, would, we believe, suffice.

This, however, is not yet the entire solution of the difficulty. The *Record* would doubtless be improved and the work of the Recorders made easier. We have, however, thus far only increased the expense, not lowered it. We can not even accept the solution proposed by certain Recorders to tax them all for this aid, as if they were not already underpaid! If the Recorders are relieved from part of their burden, let them rather endeavour to better their records. It is for this that their skill and knowledge as specialists are needed, not for collecting titles.

Far better is it to face the question whether a properly organised Record, which should appeal to the support of the entire zoological world, could not be made to cover in large measure its expenses. It has never been tried. The Naples Record only treats a part of the field, the Berlin Record is behind time and is merely German, the English Record is incomplete and is largely dependent on English subscribers. Now, in spite of the competition, one of these, the Berlin Record, has remained entirely self-supporting, while its English rival has been losing £350 annually.

Here, then, is the opportunity of the *Zoological Record*. The Naples Record has specialized in regard to Morphology, the London Record in regard to Systematics and Faunistics: they have by this fact almost ceased to be rivals. Let them take a slight further step. Let them join forces in order that they may together constitute a single complete Record for the whole field of zoology. Against such a combination I have the assurance that the Berlin Record would not endeavour to compete, but would gladly give up an unequal combat and aid the new undertaking to success.

This federation with the Naples Record would be an important step in introducing the London Record into the Continent; but at the present moment we can offer quite exceptional aid in this same direction. I refer to the federation of the two Records with the *Bibliographical Bulletin*, which it is proposed to publish beginning with January next. By this the entire work of recording would become a continuous process, beginning with the first rapid classification of the *Bulletin* and ending with the more perfect elaboration of the *Record*.

The new Bibliographical Bureau into which it is suggested that the *Record* might enter is being founded upon the broadest possible basis. To cite merely the case of France, where the movement chances to be the most completely organised, there is a large and influential central commission, nominated by the French Zoological Society and pledged to the support of the Bureau in its various needs. Then there are affiliated with this central committee a score of associate members distributed among all the zoological centres of the country, and ready to bring the matter before the notice of all the local societies and to solicit their support. In general the Zoological Society of France "is resolved to aid this movement in every way possible." It is at present printing a report of its vice-president, Mr. E. L. Bouvier, on this subject, and will bring it to the notice of all its members and urge them to support the movement. The French Entomological Society may also be counted upon to aid in finding subscribers for the Bureau's publication, including, of course, the *Record*.

It must not be supposed that this proposition implies abandoning the *Zoological Record*; that would be a pity. In the first place, an injustice would be done to old subscribers to the *Record*, who have purchased it presuming that their volumes would form part of a continuous series. Secondly, it would be unjust to the Zoological Society, whose stock of back volumes would thereby become depreciated. Thirdly, it would have many inconveniences for the new journal, which would have to win these subscribers afresh, which would lose the backing and the control of the Zoological Society, and which could not, therefore, give an adequate guarantee of permanence. The founding of a new journal would, I think, be justified only as a last resource.

A better principle is to change the existing state of things as little as possible—a reform, no revolution. The editorship and the entire control of the *Record* should remain as heretofore with the Zoological Society. The English language might be conceded, and, indeed, nothing need be altered save such things as might be specified in a definite agreement to be drawn up in advance.

The principal change necessitated by the federation would be merely a slight further step in a direction already tacitly accepted. I should surely offend no sensibilities in asserting that, as a morphological review, the existing *Record* is, save a few exceptional chapters, utterly useless. For systematic zoology, on the other hand, the Naples *Jahresbericht* is of absolutely no value, and the systematic notes given with certain chapters, *e.g.*, Crustacea, are, to my mind at least, mere disfigurements. Let, then, each *Record* leave to the other that part of the field which it is the more competent to perform. This is an economy; and we are seeking for economies.

The only further change which I need mention here is a change in the manner of publication, so that the two existing Records shall be united and form merely two parts of one complete Record. The Recorders would prepare the manuscript as heretofore, observing, however, the restriction just noted. This would then be turned over to the common publishing firm which would use it for the systematic part of the Record, and the manuscript coming from Naples for the morphological part. There is no reason why the name *Zoological Record* should not be kept for at least the English purchasers. The form of the old Record might also be retained, even the present form of binding if desired. In short, the continuity would be perfect. The *Zoological Record* would be continued (as a *part*, of course), it would be left in the hands of the Zoological Society, and it would be greatly improved.

There remains then merely the question of expense. Here we have to my mind the most convincing argument of all. By the simple exclusion of Morphology, an economy would be attained; by having the *Record* published together with the *Morphological Record* a further saving of at least £100 would be made; by raising the standard of excellence the circulation ought to be increased; by the selection of a publisher with wide international connections in the zoological world the circulation could be increased; by federation with the Central Bureau, and its extensive organisation, it is believed that the circulation would be greatly increased. In short, I have the conviction that the annual deficit would become entirely insignificant. This fact may be doubted. To this I have a perfect answer. We are willing to assume the entire responsibility of the publication made under the conditions specified. Let the Bureau receive the manuscript ready for press, and for this privilege pay a fixed sum guaranteed by the publisher. I have conducted negotiations with publishers on this point; but for the moment it is only necessary to state that the contribution towards the support of the Bibliographer could be levied upon this sum, and yet that the amount which the Zoological Society would need to add in order to pay the Recorders would be *less* than the present deficit of £350. The entire reform could be carried out with an actual saving of money! I need add no further commentary. The Naples *Jahresbericht* is willing to co-operate. Many people sincerely hope that the *Zoological Record* will do the same.

HERBERT HAVILAND FIELD.

SOME NEW BOOKS.

Pocket Gophers.

Monographic Revision of the Pocket Gophers, Family Geomyidæ (exclusive of the species of *Thomomys*). By Dr. C. Hart Merriam. North American Fauna, no. 8; Published by the U.S. Department of Agriculture, Washington. 8vo. Pp. 258, with 19 plates, 4 maps, and frontispiece. 1895.

This valuable monograph is one of the results of the new methods of collecting and working at Mammalia now universal in America. These methods were originally introduced by the author of the present work, and by his energy and remarkable powers of field- as well as cabinet-work, he has revolutionised the whole study of Mammals in that country, and has shown to what a degree of refinement the science of Mammalogy may be brought. Starting as a private collector and observer of animals' habits, he soon perceived the great ignorance of the exact characters and distribution of small mammals which then prevailed, and set himself to remove it by organising a proper system of collecting. Of this system the essential points were that large numbers of specimens should be collected, that these should all be made up exactly like each other on a regular plan, that each should have its skull attached, and that the measurements of every individual taken in the flesh should be recorded.

Hardly had Dr. Merriam commenced work on these lines than he was appointed Chief of the United States Department of Agriculture, in whose province the study of small mammals (sad to say, mostly noxious) naturally falls, and by the energetic use of his widened powers he has accumulated in the Department, as a supplementary collection to that of the United States National Museum, a series of some 50,000 skins of North American mammals—a number perfectly incredible to naturalists of the old type, by whom one or two examples from each of a few widely-separated localities were generally thought to be sufficient to exemplify the range and characters of a species.

Turning, for example, to the Pouched Gophers included in this monograph, *Geomys* in its old sense, we should be very much surprised if all the European Museums together contain sixty specimens (our own National Museum has thirty-four), while Dr. Merriam in writing his work has had for study "upwards of 800 specimens belonging to the U.S. Department of Agriculture, supplemented by 110 specimens from my private collection, and a number from the U.S. National Museum, making a total of about a thousand specimens, among which are by far the greater number of types known to be extant."

The book itself (for though called a paper it is really a book) consists roughly of two parts, one general and the other systematic. In the first a full account is given of the Geomyidæ in general, their structure and habits, the descriptions of these latter being based partly on field notes and partly on observations made on a living specimen kept in captivity by the author.

Probably many or most readers of NATURAL SCIENCE are aware that the Rodents treated of have deep pouches in their cheeks, opening externally, but no one has hitherto known exactly how these pouches were utilised. Dr. Merriam tells us that they are for carrying food into store, and that the bits of potato, apple, or what-not are pushed into the pouch with the long claws of one fore-paw, while the other holds the pouch open, and that they are emptied again by being compressed by the paws against the cheeks from behind forwards, so that their contents are squeezed out of them on to the ground in front of the animal's mouth.

Another point of interest is the use of the tail. Gophers habitually run backwards along their burrows almost as freely as forwards. In so doing they use the short and nearly naked tail as a tactile organ to guide their footsteps, a use, so far as is recorded, quite unique in the Mammalia, although from its suitable length one might suspect that moles' tails are similarly used.

In the structural portion we are given a very full account of the skull; the forms of its individual bones and their methods of ossification, and its changes of form with age.

But the most interesting point of this part of Dr. Merriam's book is the account of the teeth, and the discovery of the remarkable method in which the molars are strengthened with enamel-plates. In all other rootless-toothed rodents, so far as is known, the outer walls of the molars are completely invested with a continuous coat of enamel, of approximately equal thickness all round, however much it may be grooved or infolded, and continuous on all sides equally with the original enamel-cap of the unworn tooth; but in *Geomys* the enamel investment of the top thins out and disappears on the sides at certain points, so that when the connecting-cap has been worn off the top, which takes place at a very early age, the enamel is merely present in vertical strips or plates unconnected with each other, and the positions of these are treated by Dr. Merriam as of considerable systematic importance. At the same time, it should be noted that out of the small number of skulls available for examination in this country we have found two marked exceptions to Dr. Merriam's enamel-plate characters, and are, therefore, surprised to find no mention of any variability, in this respect, among the enormously larger series examined by him.

In the systematic part of the work old-fashioned naturalists will find more to object to, mainly in regard to the extreme generic and specific splitting employed. For of the one old genus *Geomys* Dr. Merriam now makes nine, partly on the characters of the enamel-plates, and partly on those observable within the brain case, a region somewhat awkward to examine for those who have not material sufficient to justify them in cutting open a skull of each species, although, of course, this is no argument against the soundness of the conclusions arrived at. Of species and subspecies, thirty-seven are admitted, no less than twenty-one being new, numbers which contrast somewhat strikingly with the seven recognised by Baird in 1857, these even having been reduced to five by Coues in 1877. Still, it must not be supposed that any considerable proportions of the thirty-seven species are mere "splits" of those previously known, as a most astonishing number of genuinely distinct forms have been discovered by recent collectors in Mexico, and mainly by Mr. E. W. Nelson, of the Department of Agriculture, during his exploration of Mexico in 1892 and 1893.

The book, as a whole, is exceedingly well illustrated, the nineteen plates and seventy-one text-figures being, to our mind, as nearly perfect for their purpose as can be. For, while not equalling in artistic beauty certain German plates of a similar nature, such as those of J. D. L. Franz Wagner, yet they express to the eye the objects represented, which are for the most part skulls and teeth, more exactly than any figures that we ever remember to have seen, and Dr. Merriam and his artists are heartily to be congratulated on them.

Of the very few misprints we have noticed, we may venture to point out one which, occurring in the synopsis of genera, may easily cause confusion to workers. This is that the top line of p. 24 should be transposed to four lines lower, below the diagnostic character of *Zygogeomys*.

Altogether, apart from the splitting, which will undoubtedly be considered too extreme by the majority of Old World naturalists, we do not hesitate to assert that this is one of the finest and most careful monographs of a mammalian group that has ever appeared, and we trust it may be followed by many like it, thus properly utilising the magnificent material contained in the department from which it has issued. O. T.

A Primer of Evolution.

A Primer of Evolution. By Edward Clodd. 8vo. Pp. 186. London: Longmans, Green & Co. 1895. Price 1s.

This little book has an importance out of proportion to its size, for it is designed to bring the "comfortable word evolution" home to the great heart of the people. While the "Origin of Species" has its tens of thousands, Mr. Clodd's readers will be a great crowd, innumerable as the sands of the sea-shore. For such a book, a special standard of criticism is necessary. It were fruitless to object to the hypothetical inclusion among organisms of the discredited *Eözoon canadense*, to the practical neglect of the wonderful series of American beds that represent the gap that occurs in Europe, between the end of the Chalk period and the beginning of the Tertiary strata, or to the identification of the cavity in Hydra-like polyps with the body-cavity of higher animals. Mr. Clodd's readers have no examinations to pass; it is the general and not the particular for which they are ahungered. Each one of us, coming into the world and struggling into a tiny place, sometimes must forget the perpetual personal conflict in larger questions. It may be the wandering wind, or the silent stars, or the repulsive profusion of life under an up-turned stone that sets us thinking. What are we? What is the place of our littleness in the greatness of the universe?

For such questioners Mr. Clodd provides what answers are possible. He shows the universe as an ordered system of change, of evolution and devolution. "The universe is made up of matter and motion, or, to put this in other words, the universe is made up of distinct particles which are never still, whether they compose things living or dead." . . . "Nebulæ, circular, elliptical, spiral are the raw material of which suns and systems are formed. The nebulæ; the fixed stars, with whatever belongs to the system of each; the wandering comets; and the myriad meteor streams . . . comprise the ponderable matter of the universe." . . . "The star nearest to us is known as the sun, a mass of gas burning at so high a temperature that all the chemical elements of which it is composed are in a state of vapour." With the sun comes his system of planets, which, "like the stars, are in different stages of progress and decay. Some, as the Earth and Venus, at least in her Polar Regions, have cooled down sufficiently to be covered by a hard crust, and to be fit abodes for living creatures; others, like Jupiter and his fellow giant planets, are still in a more or less heated and partly self-luminous condition."

Having, so to say, put the earth in her place, Mr. Clodd proceeds to give a brief review of her geological history and of the fossil record of gradually evolving forms of life. Then with a pleasant lucidity he describes the gradations that exist among the existing forms of animals and plants.

The second section of his book Mr. Clodd entitles "explanatory." In that, positing matter and force and the unequal distribution of the masses of matter in a first chaos of the universe, he describes the origin of stars and systems, and in particular the origin of our own planetary system. Then he gives a brief review of the evolution of organic life on our own globe, and of the arguments supporting Darwin's principle that Natural Selection is the chief agency in this. All this part is done admirably and clearly, in a fashion that will command the respect of those familiar with the difficulty of presenting so complicated a series of arguments in a compact and coherent form.

Mr. Clodd has done his work well, and the inquiring reader will find, not indeed an answer to the problem of the universe, but something on which to stay his mind. The acceptance of the principle of evolution as ruling all the changes of matter and force, whether they be displayed in the great masses of stellar space or in the surging changes of the atoms and molecules of organic matter, merely pushes back the "why" of things to a further place. But the grandeur and the universality of the "explanation" afforded by evolution may narcotise many minds into peace; and when there are no ultimate explanations one narcotic is as good as another.

DR. HERTWIG'S TEXT-BOOK OF ZOOLOGY.

LEHRBUCH DER ZOOLOGIE. By Dr. Richard Hertwig. Third improved edition. Pp. 600, many illustrations. Jena: Gustaf Fischer, 1895. Price 11 marks 50 pf.

THE third edition of Dr. Hertwig's text-book has been carefully revised, and a considerable amount of new material introduced. The account of the Porifera has been much modified, a very large addition to our knowledge of this group having been made since the earlier edition appeared. The Lamellibranchs, too, appear in an altered form, as recent work of Neumayr, Pelseneer, Jackson, Grobben, and others make our former views untenable. The palæontological side has been slightly extended, but it still remains the weakest side of the book. In many respects we regard Dr. Hertwig's text-book as considerably in advance of other books in English, German, or French, and we think it the more to be regretted that due importance is not given to extinct forms. The divorce between palæontology and zoology, so lamentable in museums, is even more lamentable in text-books. Where the zoologist ventures upon palæontology he is always insufficient, and not infrequently erroneous. Where the palæontologist ventures upon zoology, so far as we have had opportunity of judging, he is almost invariably ludicrous. A true text-book of zoology remains to be written; but of those in existence Dr. Hertwig's, in lucidity and correctness, is probably the first, certainly among the first three.

Birds and Books.

Summer Studies of Birds and Books. By W. Warde Fowler. 8vo. Pp. 288. London: Macmillan & Co. Price 6s.

A series of unconnected but interesting essays are brought together into this volume from various tributary sources. Articles from magazines, papers read before natural history societies, and one separate publication in pamphlet form constitute the eleven chapters of which Mr. Warde Fowler's most recent book is made up. They are all worth reading, some of them particularly readable. Mr. Fowler dips deeper into his subject than do some ornithologists, and emerges with many valuable and not easily accessible facts. These are the result, not only of personal observation, but also of literary research. As an Oxford man, Mr. Warde Fowler is naturally a student of Aristotle; and he presents us with a highly interesting sketch of Aristotle as an ornithologist, a character in which he probably is not familiar to Mr. Fowler's colleagues of the University. It appears from what we are told that Aristotle was not altogether irreproachable as an ornithologist. His statements are not entirely the result of observation, and, as a consequence, are constantly erroneous. But mingled with much error are a few pearls of some price; it is interesting, for instance, to learn, rather from what is left unsaid than from what is said, that in the days of the father of natural history the House Martin had not yet acquired that mode of nesting which has given to it its name. His observations upon the Nightingale teaching its young to sing have been since confirmed. And Aristotle knew three centuries before Christ, "what we moderns only learnt a century ago, that a bird does not sing its own song by instinct, but will sing another bird's song if the proper chance is given it." One of Aristotle's weak points appears to have been colour, a defect which, according to certain authorities, he shared with no less a person than Homer. "Aristotle is often content with telling us that a bird is of a bad colour or a good colour without troubling himself further, as if he well knew that his countrymen were not gifted with an acute colour-sense." Besides, as Mr. Warde Fowler remarks, the Greeks were not by any means amply provided with words for the different colours, let alone diverse shades of a given colour.

Strongly though Mr. Fowler speaks in praise of Aristotle, he has plenty of commendation left for Gilbert White, to whose life and merits another excellent essay is devoted. In these days of unceasing "vorlaüfige Mittheilungen" and disjointed publication, it is good to read how Mr. Fowler emphasizes the fact that White did not print his famous book until he was close upon seventy. On the other hand, though the author particularly points out that his hero was "White of Selborne, not White of Oxford" he, obviously, came perilously near to being as barren (intellectually) as the average Oxford Don; for he died only four years after the book was published. We fear very much that the "ease and isolation, the complete absence of hurry and worry" to which Mr. Fowler attributes the greatness of the Natural History of Selborne, will soon cease to be attainable. Brixton and Clapham prefer something more useful than a "good monography of worms"; while the author of such a monograph would not be, in the opinion of a certain politician, one of those men who are worth more than five hundred a year.

The remaining essays of Mr. Fowler's volume are nearly all the results of direct observation upon bird-life, while an appendix, partly contributed by Mr. Pycraft, of Oxford, helps to elucidate a chapter upon the song of birds.

F. E. B.

Dorset Botany.

The Flora of Dorsetshire, with a Sketch of the Topography, River System, and Geology of the County. By. J. C. Mansel-Pleydell, B.A., F.G.S., etc. Second edition, with two maps. 8vo. Pp. 345 and 63. Dorchester: *Dorset County Chronicle* Printing Works, 1895.

We are glad to see that Mr. Mansel-Pleydell has brought out a second edition of his "Flora of Dorsetshire," in which is incorporated the additional knowledge gained since 1874, the date of the first edition. Though "printed for private circulation only," this valuable work will doubtless find its way into the hands of all interested in the botany of Dorset in particular and of Great Britain in general. The flora is prefaced by a useful sketch of the topography and geology of the county, and there are also two very good maps, geological and botanical respectively. Under each species the author cites the first record, the habitat of the plant, the localities in which it occurs, and its general distribution as regards the neighbouring counties of Devon, Hants, Somerset, and Wiltshire, and also Normandy. With vascular Cryptogams and Characeæ, there are 1,173 species; this includes, however, certain denizens and aliens.

The fair number of records credited to his own name shows that Mr. Mansel-Pleydell has himself laboured in the field, as well as entered into the labour of others, the latter, indeed, with full acknowledgment.

Injurious Insects.

Report of Observations of Injurious Insects and Common Farm Pests, during the year 1894, with Methods of Prevention and Remedy. Eighteenth Report. By Eleanor A. Ormerod. 8vo. Pp. viii., 124, lxii., 4. London: Simpkin, Marshall & Co., 1895. Price 1s. 6d.

This is a book distinctly aggressive to insects. It is most interesting to the general reader, and invaluable to the farmer. It would be instructive to know how many farmers buy it, and then how many of the buyers put the valuable advice it contains into practice.

In a short preface, Miss Ormerod says "the most important discovery of the year is that which we owe to the skilled researches of Professor John Percival, of the South Eastern Agricultural College, Wye, near Ashford, Kent, of the presence of two kinds of eelworms at the roots of Hops, of which one kind, which is seriously destructive to various kinds of crops on the Continent, had not previously come under notice in this country, and neither had been previously observed at Hops." A full account of Professor Percival's discovery has been given by Miss Ormerod in her Report (pp. 47–61). She understood that it was to have been published in the *Journal of the Royal Agricultural Society* for 1894. As a matter of fact, that Society did not publish the paper at all, but it was published in Natural Science for March this year, and Miss Ormerod has acknowledged the unexpected alteration in a slip inserted in her Report.

The insects which receive honourable mention in Miss Ormerod's present Report are only two in number,—the Golden Eye (*Chrysopa perla*), and the Eyed Ladybird Beetle (*Coccinella ocellata*). The first of these, known also as the Lace-wing Fly, is carnivorous in the grub stage, and will eat aphides, its fellows, or a caterpillar much larger than itself; the second, the "ladybird," has a partiality for Red Spiders, and while in the maggot state devours them voraciously. All other insects mentioned in the Report are condemned, and the best methods for their destruction fully set forth. One of the most destructive insects of the past year was the "Antler," or "Grass" Moth (*Charæas graminis*, Stephens; *Cerapteryx*, Curtis), which appeared in great numbers in the South-West of Scotland, seven counties being affected. So bad, indeed, was the plague that Professor Wallace remarked early in July, "they are said to be worse than the voles in some places." Nature supplied a remedy, however; for the caterpillars were attacked by a threadworm of the genus *Mermis*, and by the disease known as "flacherie."

Perhaps the most interesting part of the whole Report is the appendix of 62 pages, devoted entirely to "Observations on Warble Fly or Ox Bot-Fly" (*Œstrus bovis*, Linn.; *Hypoderma*, De Geer). Miss Ormerod has collected a great amount of additional information together regarding this pest,[1] which, besides causing serious pain and distress or even death to the animal, renders its skin, in bad cases, quite unfit for sale purposes. A picture of a piece of the skin of a yearling, 24 by 14 inches is given, which shows 402 warble holes, and this will give the reader some idea of the commercial value of such a hide for leather. Full particulars are given of the condition, value, and depreciation of the hides of oxen attacked by warble, and if this commercial point could be forced upon the farmer, methods might be adopted which would relieve the cattle, and, at the same time, increase the value of his property. The remedies suggested and approved of by practical men are large shallow ponds, where the animals can stand during the heat of the day, trees, and careful housing at night, smearing the back with Stockholm or green tar, brine, sulphur, train-oil, McDougall's Smear, or some other strong-smelling or fatty matter. If the beast is warbled the grub must be extracted by pressure after a small incision with a penknife.

We might quote many pages from this interesting Report, but advise our readers to get it and read it for themselves; they will then, perhaps, realise the debt of the agriculturist to Miss Ormerod, and the scope of the good work that she is doing.

The United States is, however, the true home of economic entomology, and we have frequently called the attention of our readers to the excellent reports issued by the various institutions of that country. The latest of these reports received by us is the eighteenth of the State Entomologist on the Noxious and Beneficial Insects of the State of Illinois, which contains a monograph of "Insect Injuries to Indian Corn," part 1, by Mr. S. A. Forbes, the State Entomologist. This deals with classification of injuries, injuries to the seed in the earth, injuries by ants, beetles, and the special insects that affect the seed, injuries to the roots, with the special insects that inflict them. The majority of these pests are figured, and many of them described for the first time. Fifteen good plates accompany the monograph, which is well and fully indexed, and which cannot fail to be of the greatest service to growers in America and elsewhere.

A Bold Undertaking.

The Great Problem of Substance and its Attributes, involving the Relationship and laws of Matter and of Mind as the Phenomena of the World, derived from the Absolute. 8vo. Pp. 197. London: Kegan Paul, 1895.

In this little book an attempt is made to solve the problem of the universe in terms of Ether regarded as absolute substance. We are

[1] See Reports for 1884, 5, 6, 7, 8, 9 and 90.

told, to begin with, that the luminiferous ether radiates or carries forth the forms and conditions of external objects from the objects to the external organ of perception. Thus we are introduced to the ether as mediating between the perceived object and the perceiving subject. It is then assumed that this ether is the absolute substance, and that it is to be regarded as peculiarly of the nature of spirit. Furthermore, it has the capacity of "taking on and setting forth in its own bosom what we may designate spirit copies of all material objects." But, as absolute, it must constitute the material objects themselves. To the question how the qualities of the ether can become the foundations of matter, the author replies, By the process of thickening. "How the spirit form is converted into a material form we have no means of showing further than this, that there is a process of incrassation." The ether thus becomes both the object and the medium by which it is revealed to the subject-mind. What, then, is the nature of this subject-mind? In answer to this question we are told that "there is much, very, very much, to lead us to believe that the operation of mind is neither more nor less than our old friend in a new position—the ether, found as the spirit medium in physics, and always working in and along with matter." Having now got the ether "incrassated" as matter, the ether "in a new position" as mind, and the ether as spirit medium to mediate between itself as "incrassated" and itself "in a new position," we may regard our solution of the problem of the universe with such complacency as it is in us (not being the author thereof) to summon up.

It is obvious that we cannot, in NATURAL SCIENCE, do more than barely indicate the nature of the author's thesis. One example of the kind of use to which the principles reached are put must suffice. "The ethereal representation of the sun," we are told, "meets the ethereal representation of the earth, and that of the other planets of the solar system, and thereby maintaining the centripetal relation of each to the sun, while their movement given to each centrifugally balances this relation into a definite circumambience." The illumination thrown upon other matters of physics and of psychology is of like nature.

C. LL. M.

GEOLOGY OF SWEDEN.

SVERIGES GEOLOGI ALLMÄNFATTLIGT FRAMSTÄLLD MED EN INLEDANDE HISTORIK OM DEN GEOLOGISKA FORSKNINGEN I SVERIGE JEMTE EN KORT ÖFVERSIGT AF DE GEOLOGISKA SYSTEMEN. By A. G. Nathorst. 8vo. Pp. 336, with several hundred illustrations. 1892–94. Stockholm: F. & G. Beijers. Price 8 kronor [9s.].

THE mass of geological literature is now so overwhelming, that everyone will be thankful to an author who gives a clear account of the geology of his own country within a reasonable number of pages. But when the writer is also the person best qualified to deal with the subject, and is in the front rank of original observers, our obligation, and our confidence, become still greater. On looking through Professor Nathorst's handsome volume we find that he has done for Sweden what has been done for few other countries. He has written a geology, or we may, perhaps, better describe it as a geological textbook, in which the greater number of pages is devoted to the formations and types of deposit represented in his own land. Formations absent in that country are only mentioned in the introductory sketch (pp. 20–38), and if good material is available the author always figures by preference Swedish specimens. This makes the book of great value for comparative studies, and we should advise English geologists, even if they do not read Swedish, at least to examine the illustrations and tables, for the volume is full of original or otherwise inaccessible material. We might particularly point to the chapters relating to the Cambro-Silurian, Jurassic, Cretaceous, and Quaternary, and to the illustrations and remarks about fossil plants throughout. It should be remembered that Professor Nathorst, though well-known as a field-geologist, is almost more celebrated as a student of fossil plants. So far as we can call to mind, no previous geological manual has been written by a botanist, and the botanical evidence, therefore, has been generally overlooked or misinterpreted. The book is provided with an exhaustive index, and a classified bibliography of nine closely-printed pages.

Dr. Ch. Janet has sent us a series of papers on ants, published in the *Comptes Rendus*, *Mémoires de la Société Zoologique de France*, and *Mémoires de la Société Académique de l'Oise*. Two other papers, from the *Comptes Rendus*, will be of some interest to those who, like our contributor, Mr. Latter, study the habits of wasps, and their resistance to unfavourable weather. They deal with the construction of the nest, the order of formation of its cells, and the preservation of heat within it. This last is a point of much importance, for if the temperature be reduced, the development of the eggs may be indefinitely arrested.

Another paper from Dr. Janet appeals to a very different class of readers, namely, the collectors and students of fossils. The Tertiary Bracheux Sands contain beautiful fossils, but, unfortunately, these are exceedingly friable. Dr. Janet shows how, by imitating a natural process, this powdery limestone may be transformed into solid gypsum, and the specimens extracted with safety. A strong solution of sulphate of lime, slightly acidified with sulphuric acid, is gently poured over the fossiliferous sand, thus the fossil is transformed into sulphate of lime without being in any way dissolved, and without being ruptured by the escape of gas. The note is contained in *Comptes Rendus de la Société Géologique de France.*

We understand that Messrs. Macmillan will publish early in May a work upon Birds by Mr. F. W. Headley. It will consist of some four hundred pages, octavo, with abundant illustrations, largely in the form of plates. The book will be suitable for those who are not experts in bird anatomy, though its treatment is strictly scientific. Great weight is laid upon the anatomy of the organs of flight; but such general topics as song and colour are dealt with also, though not at such great length.

The Clarendon Press will shortly issue a "Monograph of the Oligochæta" by Mr. F. E. Beddard, F.R.S. This is to be a quarto volume of nearly 800 pages, with five plates and a number of woodcuts. Our knowledge of the structure and of the species of the terrestrial worms has increased so largely of late that a general treatise embodying what is known upon the subject is much to be desired.

OBITUARY.

JAMES DWIGHT DANA.

BORN FEBRUARY 12, 1813. DIED APRIL 15, 1895.

AMERICA has to deplore the loss of one of the greatest of her sons in the death of this eminent zoologist, mineralogist, and geologist, which occurred at New York on April 15. James Dwight Dana was born at Utica, New York, February 12, 1813. He entered Yale College, where he graduated in 1833, paying especial attention to the natural sciences and mathematics. His first position was that of teacher of mathematics to midshipmen in the United States navy, and in the course of his work he visited the Mediterranean. Returning to America in 1835 he became assistant to Benjamin Silliman, whom he ultimately succeeded in the chair of Mineralogy at Yale. So early as 1837, Dana published his "System of Mineralogy," a work which has run through many editions, and is undoubtedly the first on the subject. The sixth and last edition was published in 1892, and in this he was largely assisted by his son. In 1836 he was fortunate enough to be appointed Mineralogist and Geologist to the Wilkes' Exploring Expedition to the Southern and Pacific Oceans. This expedition sailed in 1838 and returned in 1843, and for the next thirteen years Dana was engaged in editing the various reports sent in for publication, and himself contributed the reports on Geology, on Crustacea, and on Zoophytes, making, with few exceptions, the drawings of the animals himself. In 1850 he was elected as Professor Silliman's successor; but it was not till five years later that he took up the work. In 1854 he was President of the American Association for the Advancement of Science, his annual address being delivered at Providence in 1855. In 1863 the first edition of his "Manual of Geology" appeared, and this book has attained, so far as American geology is concerned, a position equal to that of his "System of Mineralogy"; only this year (1895) a new edition has been published, and the manuscript for this, we are informed, was in the veteran Professor's own handwriting.

"Corals and Coral Islands," another well-known book, appeared in 1872, while his "Characteristics of Volcanoes" did not see the light till 1890. His contributions to serial and academical literature were very numerous, and since 1846 he had been an editor of the "American Journal of Science," the best known of scientific serials of the United States, which was founded by the elder Silliman in 1818. He resigned his Yale Professorship in 1894.

Professor Dana was a Member of the Academy of Sciences of Paris, a Fellow of the Royal Society, of the Academies of Berlin and Munich. He received the Copley Medal in 1877, and the Wollaston Medal of the Geological Society in 1872.

As a naturalist, Professor Dana's work was characterised by the grip of a master-mind. In the course of his long career he had to undertake many very different and difficult tasks. In each of them the same mental grasp of the principles of the subject was shown. His great report on the Corals of the Wilkes' Expedition is the most indispensable monograph on the Reef Corals. His classification of the Crustacea, though artificial, is still used by zoologists. His work on mineralogy was a masterly synopsis of the knowledge of the science. His contributions to geology profoundly influenced contemporary thought on such problems as the formation of coral-islands, the permanence of oceans and continents, and the characters of volcanic action. In each of these Dana's mastery of his subject and his powers of careful, detailed observation were exhibited. His work was done to last. In his soundness of judgment, his wide range of knowledge, his thoroughness in research, he ranks intellectually with the great legal codifiers rather than the naturalists. In his works on coral-islands, for example, he stated the contending theories with judicial fairness and lucidity. He gave his verdict in favour of Darwin's views, and added some original arguments of great importance in their support. But he was inferior to Darwin in imagination. His classifications were artificial, and his views were more often in error, owing to excessive caution and lack of daring than from any other failing. Thus his masterly paper on the submerged fiords at the mouth of the Hudson River exercised only a very small proportion of the influence it might have had if he had not thrown back the formation of the valley to the Trias, apparently because his imagination recoiled from the conclusions that followed from attributing it to a later date.

But caution is probably an error on the right side, especially in a man who worked on so many subjects and on so many lines as did Dana. His shorter papers, including several contributions to NATURAL SCIENCE, were very numerous and extraordinarily diverse in their range. It is however as a great monographer that he will be best remembered, and it is his great systematic reports on corals, crustacea, vulcanity, and mineralogy that will form the most enduring monument of his fame.

JOHN ADAM RYDER.

BORN 1852. DIED MARCH 26, 1895.

THE death of this distinguished American embryologist will come as a great shock to his many English friends. Professor Ryder was born near London, in Franklin Co., Pennsylvania, and received an ordinary school education; but his progress was interrupted by financial trouble in the family. He became a student at the Academy of Natural Sciences of Philadelphia under the Jessup Fund endowment in 1874, and the fund was renewed to him from time to time in recognition of his "extraordinary singleness of purpose, and his ability as an investigator." Indeed, he held the fund until March, 1880, when he entered the service of the United States Fish Commission at the instance of Professor Spencer Baird. From 1880 to 1883 he investigated the development, habits, and breeding-grounds of the mollusca and fishes, and his results have been published in a long series of papers in the Bulletins of the Commission. He was a particular authority on oyster culture, and the artificial propagation of the salmon and the sturgeon. In 1886 he became Professor of Comparative Embryology at the University of Pennsylvania, and, though actively engaged in teaching, was never idle with his pen. The great majority of his writings, outside the work done for the Fishery Board, were published in the *Proceedings* of the Academy of Natural Sciences of Philadelphia, of which he became a member in 1878; but the American Philosophical Society, the *American Naturalist*, and numerous European journals were enriched by his communications. Ryder was a great opponent of Weismannism, believing that all phenomena of living organisms could be explained by mechanical laws. He possessed a wonderful memory, and had a strong inventive genius, as witness his microtome, now so extensively used. His last two papers on "An arrangement of the retinal cells in the eyes of fishes, partially simulating compound eyes," and on "The true nature of the so-called 'nettle-threads'" are in the hands of the Philadelphia Academy.

TOYOKITSI HARADA.

WE regret to learn of the death of the eminent Vice-Director of the Geological Survey of Japan. Dr. Harada was well-known in Germany, where he spent a great part of his youth, studying at Harburg, Freiburg i/S., Heidelberg, Munich, and Vienna. In the summer of 1880 he joined the Austrian Geological Survey as a volunteer, and in 1881 began his independent researches with a study of the eruptive rocks near Lake Lugano, which formed his doctoral thesis at the University of Munich. During the summer of 1882 he mapped a part of Comelico and Western Carnia, in the Venetian Alps, and published an account of the geology (*Verh.* and *Jahrb. k. k. Geol. Reichs.*). In 1883, after an extended journey through middle and south Europe, he returned to his native land, where he soon succeeded Dr. E. Naumann as inspector of the geological and topographical surveys. His more important works in Japanese geology are "Versuch einer tektonischen Gliederung der Japanischen Inseln" (*Tokio*, 1888), and "Die Japanischen Inseln" (*Berlin*, 1890), a work which is still unfinished. Of the official work which he so largely organised and directed, a full account was given in our pages a year ago. Dr. Harada had for some time been in failing health; he died on December 1, 1894, and his funeral was attended by many Europeans, and especially by members of the German colony in Tokio.

ALEXANDER GOODMAN MORE.

Born 1830. Died March 22, 1895.

IRISH science has sustained a severe loss in the death of this accomplished and genial naturalist. Of Scottish extraction, and educated at Rugby, Cambridge, and Switzerland, Mr. More, after some valuable field-work in the South of England, settled in Ireland about 1860. Our knowledge of the flora and fauna of that country has been vastly increased by his labours. In 1866 he published, in conjunction with the late Dr. D. Moore, a "Cybele Hibernica," giving a summary of the distribution of plants in Ireland, of the greatest value, not only for the accuracy of the author's own facts, but for the careful sifting of evidence with regard to doubtful records. It is a matter for the deepest regret to Irish botanists that the second edition of this work, for which material had been assiduously collected by Mr. More almost up to the last, can now never be published by him. In 1867, More became assistant in the Dublin Natural History Museum, of which institution he was appointed Curator in 1881. In his zoological work he devoted most of his attention to birds, and issued, under the Museum auspices, a valuable list of the Birds of Ireland. His knowledge of other vertebrate groups was, however, very considerable, and he was also well acquainted with the Mollusca and Lepidoptera. Both in botany and zoology, Mr. More excelled as a systematist and field-worker. He made no claim to be a morphologist or anatomist. A lamentable accident and lengthened illness forced him to retire from the Dublin Museum in 1887. Since then, debarred from his well-loved outdoor studies, he has cheerfully borne much suffering, while taking a kindly interest in the work of younger men, who have always found him eager to unlock his stores of practical knowledge for their benefit.

EDWARD HAMILTON ACTON.

Born 1862. Died 1895.

THE sudden and premature death of Edward Hamilton Acton at St. John's College, Cambridge, on February 15, has deprived the University of an able and vigorous worker in the Natural Science School. Born at Wrexham in 1862, Acton was educated at Rugby and St. John's College; after obtaining a first-class in the second part of the Natural Sciences Tripos in 1885, he was elected Fellow of his College in 1888. For the last seven years he devoted himself chiefly to chemical teaching, and was recently appointed College Lecturer in that subject. In his chosen field of reseach, botanical chemistry, he had already proved himself an investigator of considerable ability. His last work on this subject is contained in the recently published "Practical Physiology of Plants," the authorship of which he shared with Mr. Francis Darwin. A perusal of the papers contributed by Acton to botanical science does not lead to an adequate appreciation of his scientific attainments; but those who were privileged to be his colleagues know too well the severe loss which botanical chemistry has sustained. As a friend has admirably expressed it, Acton was in truth "a man"; a teacher who won the best form of appreciation, the respect and friendship of his students, and a friend who will always be remembered by those who knew him, not only as an able scientific thinker, but as one in whose strong personality there was reflected a sterling manhood, a thoroughness of action, and an honesty of purpose.

ROBERT FITCH.

Born October 21, 1802. Died April 4, 1895.

THIS well-known Norfolk antiquary and geologist was born at Ipswich, and was educated at the grammar school of his native town. He was apprenticed to a chemist and druggist, removing for a few years to London, and in 1827 entering into partnership with his brother-in-law at Norwich, where he has ever since resided. He became a Fellow of the Geological Society in 1844, and devoted his energies in a geological direction to the collection of flint implements and vertebrate remains in particular. These remains, together with an extensive and valuable series of antiquarian interest, he presented in 1892 to the Norwich Museum, himself providing the cases in which they are displayed. Fitch's writings were mainly concerned with archæology, but his contributions to geology were of considerable value and interest. He numbered among his friends Sedgwick, Murchison, Darwin and Lyell, and was in constant correspondence with men of the younger school, one of whom, Mr. E. T. Newton, fittingly attached his name to a new *Cervus* found by him in the Forest Bed at Bacton.

Robert Fitch was a magistrate for the city of Norwich from 1858, he became sheriff in 1867, was a director of the Norwich Union Fire Office, and was a most important man to his town. He was one of the last of the old Norfolk geologists, who numbered among them such names as Samuel Woodward, R. C. Taylor, J. W. Robberds, C. Green, Anna Gurney, and a host of others of more or less importance.

Jean Edouard Bommer, Professor in the Brussels University, Curator of the State Botanic Garden, and formerly President of the Royal Botanic Society of Belgium, died at Brussels on February 19, in his 66th year.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Mr. Christopher Heath has been elected President of the Royal College of Surgeons in the room of the late Mr. Hulke. Dr. Patrick Manson has been appointed Lecturer on Tropical Diseases at St. George's Hospital. This hospital is the first institution, so far as we know, which has provided for special instruction in tropical medicine in London. Professor C. S. Sherrington, late Fellow of Gonville and Caius College, Cambridge, has been appointed to succeed Professor Gotch in the George Holt Chair of Physiology at the University College, Liverpool; Professor E. L. Greene succeeds to the Botanical Chair in the Catholic University of Washington; Mr. T. T. Groom has succeeded the late Professor Allen Harker as Professor of Natural History to the Royal Agricultural College, Cirencester; Mr. J. C. Sumner has been appointed by the Liverpool Marine Biological Committee to be Curator of the Port Erin Station; Mr. J. Percy Moore is taking temporary charge of the late Professor J. A. Ryder's classes at the University of Pennsylvania.

Sir Joseph Lister received at the hands of the Prince of Wales, on the 9th inst., the Albert Medal, awarded to him by the Society of Arts for "The discovery and establishment of the antiseptic method of treating wounds and injuries, by which not only has the art of surgery been greatly promoted and human life saved in all parts of the world, but extensive industries have also been created for the supply of materials required for carrying the treatment into effect."

Mr. John Ward, of Longton, has been awarded the Robert Garner Medal by the North Staffordshire Field Club for his researches into the fossil fauna of his county. Mr. Ward's fine collection of Coal-measure fishes has been added to the national collections, and he is now working up the molluscan fauna of the same series of rocks.

We learn from *Nature* that the Paris Geographical Society has awarded its prizes for 1895, as follows:—Gold medals, to Lieut. L. Mizon, for his explorations in West Africa; E. Gautier, for his explorations in Madagascar; F. Foureau, for his explorations in the Sahara; E. Ponel, for his explorations in the region of the French Congo; Th. Moureaux, for his magnetic map of France; Father Colin, for his observations and triangulations in Madagascar; A. Courtry, for the production of a map of the Congo; V. de la Blache, for his general atlas; and Dr. Thoroddsen, for his explorations in Iceland. Silver medals have been conferred upon E. D. Poncins, for his journey from Turkestan to Kashmir by the Pamirs; J. Gaultier, for his works on the production of plans by photography; B. d'Attanoux, for his exploration in the Sahara; and J. Forest, for his studies on the breeding and habits of the ostrich in the Sahara. The Jomard prize has been awarded to L. A. Rainaud, for his memoir entitled "Le Continent austral: hypothèse et découverte."

Professor John Milne had just resigned his appointment at Tokio, and was on the eve of his final return to England, when, on February 17, his house, his valuable seismometric apparatus, and his accumulated notes of twenty-five years work were destroyed by fire in half-an-hour. Merely to state the fact is enough to arouse the warmest sympathy for our energetic and genial colleague.

DR. WILLIAM MARCET will deliver the Croonian Lectures at the Royal College of Physicians in June. He will take for his subject "The Respiration of Man."

THE annual meeting of the Zoological Society of London will be held on April 29. The Report, which has just been issued, shows a falling off to the extent of £1,300 in admission to the gardens, with a corresponding decrease of £90 in elephant rides, etc. The admission fees, subscriptions, and composition fees increased as compared with the year 1893. With regard to payments, on the other hand, there has been a considerable decrease in expenditure, more particularly noticeable because some heavy items, which properly belonged to 1892, were included in the year 1893. The whole account shows an excess of income over expenditure to the amount of £1,500, and this is carried forward to 1895. The Society paid £4,205 1s. 10d. for provisions for the animals during 1894, spent £1,008 on purchase of animals and carriage, £1,800 on publications (including the *Zoological Record*), and £500 on the library. On the income side of the account we read the following items of interest:—Riding receipts, £680; rent for refreshment-rooms, £762; garden sales, £522.

FROM the sixty-third Annual Report of the Royal Dublin Zoological Society for 1894, we learn that the attendance at the gardens was 10,000 more than in 1893. This is a surprising increase, and we congratulate the Society on the improvement which this has made in the finances. Nine lion cubs have been born, five of which were males in one litter. Three pumas were also born in the gardens. Great improvements have been made in the aquarium, and among the chief accessions are an orang-outang, a pair of ostriches, and a pair of South African hunting-dogs.

THE Royal Geographical Society will hold a special meeting on Monday, May 20, to commemorate the fiftieth anniversary of the departure of the Arctic Exploring Expedition under Sir John Franklin. In connection with this it is proposed to form a party to visit Greenwich on the same day, to inspect the Franklin relics in the Museum.

THE Museums Association will hold its sixth annual meeting in Newcastle-upon-Tyne, under the presidency of the Rev. Canon A. M. Norman, during the fourth week of July. The meeting will begin on July 23. The mornings will be devoted to the reading and discussion of papers, while in the afternoons the members will inspect the Museums and Art Galleries in the city. Inquiries should be addressed to Professor M. C. Potter, Durham College of Science, Newcastle-upon-Tyne.

THE sixth Annual Report of the Epsom College Natural History Society is just to hand. The list of members shows an increase of twenty-nine over that of the previous year. We regret to hear that the adjudicators of the Fayrer Prize did not receive an essay of sufficient merit to allow of the award. A number of excellent lectures were given, and good work has been done in the botanical and entomological sections in recording the finds of the year.

THE plaster cast of the *Iguanodon* from the Brussels Museum, which we referred to last month, has now been mounted and can be seen in the gallery of Fossil Reptiles in the British Museum. The growth of this portion of the collection is so large, and the important discoveries made in recent years so inadequately represented for want of space, that it is to hoped some better accommodation for the class Reptilia will soon be made. The large series of interesting and remarkable dinosaurs from America are as yet known in this country only from pictures.

The Natural History Society of Queensland, which was founded in 1892, has just issued its first volume of Transactions.

The Danish Geological Society, which was founded January 16, 1893, has already published its *Meddelelser fra Dansk Geologiska Forening.* No. 2 of the *Meddelelser* consists of an important paper in Danish on the "Pleistocene Foraminifera of Denmark and Holstein" (Copenhagen, 1895, 230 pp., plate and map), by Victor Madsen. Dr. Madsen has collected from a very large number of localities, gives the results of his washings, and describes, in the latter pages of his paper, the foraminifera in detail. It is interesting to note that of his eighty-eight forms described, he only recognises one as new, a proof of the care the author has taken in identifying and working out his material. This new form is certainly curious: it is called *Lagena danica*, and consists of an oviform test, cut off at the greatest breadth by an oval margin; the base consists of a deep groove surrounded by a thick but flat margin, the whole structure reminding one of a thick flat india-rubber band seen sideways. Six examples have been found in three different localities. The paper permits of a valuable comparison of the Pleistocene foraminiferal fauna with that of the recent Baltic and North Seas.

The sum of £500 has been bequeathed to the Geological Society of Cornwall by Mr. William Bolitho. The income derived from this money is to be applied each year to the gift of a medal "to be presented to the member of the Society whose attainments, labours, or discoveries in geological or mineralogical science are found most deserving."

Dr. Lopez Vieira, of the University Museum of Coimbra, makes an appeal for the better development of Portuguese museums in *Annaes de Sciencias Naturaes* for January, 1895. He refers in particular to the museums of Washington, London, and New York, and the arrangements there in force with relation to the natural habits of birds and mammals. Dr. Vieira also notes the value to agriculture of the work done by the museums of the United States, an example which might well be followed in London, where systematic zoology alone seems to be in favour. The especial feature of agricultural value that Dr. Vieira quotes is an examination of the stomachs of 2,700 hawks and owls, in order to find out the particular nature of their ravages ("Hawks and Owls of the United States." By Merriam and Fisher. Washington, 1893).

A paragraph in our April number, on p. 286, made certain statements concerning the Museum at Colchester, which should properly have related to the Chelmsford Museum. The Colchester Museum is purely archæological, having recently been weeded of its few natural history specimens to make room for its fine collection of antiquities. It has never been closed, except for cleaning, since it was first opened some thirty years ago. The number of Essex newspapers that have offered us a prize of two guineas for our "extraordinary concatenation of journalistic blundering," shows an interesting ignorance of the English language and a lack of originality among our esteemed contemporaries, but so far has added nothing to our exchequer.

The Zoological Society of France has re-issued the "Rules of Nomenclature of Organised Beings adopted by the International Congress of Zoology at Paris, 1889," and confirmed at Moscow, 1892. Dr. Blanchard, 7 Rue des Grandes Augustins, Paris, will send a copy gratis to any professor, director of museum, assistant in museum, or any learned society, on application.

The *Kew Bulletin* announces that the Queen has ceded to Kew Gardens the tract of land known as the Palace Meadow. This gracious act will allow the public easy access to the arboretum.

Dr. Ferdinand Lataste has several communications in *Actes Soc. Sci. Chili*, iv., part 4, one of which is illustrated by a plate, on the abnormal development of the horns in ruminants, which will be of interest to those who followed Mr. Holding's remarks on the same subject before the Zoological Society on February 5 last.

We learn that Mr. Sinel has taken charge of an oyster culture establishment in Jersey, and that Mr. Hornell is therefore in entire charge of the Jersey Biological Station at present. The first instalment of the second series of Studies in Marine Zoology is to hand, the high standard of the preparations being fully kept up. The new slides are a colony of *Sphærozoum*, the larva of *Antedon*, and an entire Pteropod *Cresis*. We have also received the *Journal* containing the descriptions and plates; the latter are a great improvement, and leave scarcely anything to be desired. Mr. Hornell announces a new series of microscopical studies in botany at the absurd price of 21s. for 20 slides, with letterpress and descriptions, for which there should be no lack of subscribers.

The *American Geologist* informs us that the Legislature of Michigan is considering the advisability of entering upon a complete topographical survey of the state, in co-operation with the United States Geological Survey. Maine is also making efforts to found a geological survey of the state.

The first instalment of the International Geological Map of Europe, which was promoted at the Geological Congress held at Bologna in 1881, has just been issued. It consists of six sheets, and embraces Iceland, a small part of the coast of Greenland, Northern and Central Germany, Belgium, Holland, and portions of Eastern France, Southern Denmark, and Northern Austria. The second part, which will consist of ten sheets, will be issued during the current year, and will contain Great Britain, France, Portugal, Southern Germany, Switzerland, the greater part of Italy, and the western portion of Austria-Hungary. There will be forty-nine sheets altogether, on a scale of 1 : 1,500,000, or nearly twenty-four miles to the inch. Dr. Hauchecorne and Professor Beyrich are superintending the work, while the topographical details are being laid down by Professor H. Kiepert. The subscription price for this map, from Mr. Edward Stanford, is ninety shillings, if subscribed for before June 25, but after this date the price will be raised to six pounds.

The promoters of the Macleay "Memorial" volume state that it is not a financial success. We are not in the least surprised to hear it, and have already remarked on the inconvenience of this method of publication (Natural Science, vol. iv., p. 242).

The *Birmingham Post* states that Mr. Cecil Rhodes, as chairman of the British South Africa Company, has offered a tract of land of 200,000 acres in Mashonaland as a game preserve, conditionally on those promoting this preserve-scheme undertaking to provide the funds necessary to enclose, stock, and maintain such an area. It is suggested that a society thus started could hope to become self-supporting and possibly remunerative by the export of animals to menageries and museums. Mr. Selous, Mr. F. J. Jackson, and Mr. J. E. Harting are among those specially interested in the scheme.

By the kindness of Dr. Henry Woodward we are informed that a letter has been received from Dr. Forsyth Major, dated Central Madagascar, February 22. Dr. Major landed at Mananzari and proceeded to Fianarantsoa, afterwards journeying to Ambolismatrasua, and making Ivolismanitra and Ambolismanga. In the latter place he met with some opposition from the Governor; but by sending a special courier to the acting British Vice-Consul at Antananarivo he obtained a passport from the Prime Minister, informing the natives that he was a British subject and a naturalist, and has since been treated with the utmost consideration. He has made a detailed exploration of the Tanala country, and has been successful in finding several new mammals and reptiles, and has made an extensive collection of plants. Dr. Major is in good health. He has not had any communications from England, and this letter is the first that has been received from him in this country, although he mentions several others that he has written and despatched.

THE industries of farming and oyster-culture which are pursued in Essex, have caused the Council of that county to make biology an important part of its technical instruction. There is a biological laboratory at Chelmsford, and a useful little monthly, *Biology Notes*, is issued under the direction of the Staff-lecturer, Mr. D. Houston. This gives the programme of work for the current month, the returns for the month past, a Naturalist's Calendar compiled by the students, practical lessons for use in the classes, and articles on applied biology for the benefit of the agriculturist. So successful has the work been that it may possibly give rise to a permanent Agricultural College for East Anglia.

In connection with the experiments on oyster-culture, begun last year at Brightlingsea, with the help of the Colchester Corporation, a small marine biological station has been fitted up. Owing to the weather, which killed the first fall of spat, and to other causes, experiments to determine whether the continental system of rearing oysters on tiles would succeed in the River Colne were hardly conclusive; but as the Fishmonger's Company has subscribed £50 to their continuance, it is hoped that the question will soon be settled. Meanwhile, the expert in charge of the experiments, aided by the staff, has made a good collection of marine animals, which are now being used in successful demonstrations to the local fishermen. Subjects out of the scope of the practical fisherman are also being dealt with; thus Mr. W. M. Webb has recently published, in the *Journal of Malacology*, vol. iv., p. 15, an interesting description of the dimyarian stage passed through by the young oyster. Those in charge of the station hope that it will soon be recognised by London biologists as a place where work may be done, and whence important contributions to science may emanate, to the credit of the Essex County Council.

DR. SCHARFF'S paper on the Irish Caves has already extracted information, for Mr. R. J. Ussher and Mr. J. Coleman contribute to *The Irish Naturalist* for April some notes of interest on the subject.

WE are indebted to the *Pall Mall Gazette* for the following delicious extract taken textually from the *Unione Liberale*, a newspaper published in Perugia: "In case the Dervishes attack us the Admiralty has sent the warship 'Dogali' to the island of Cuba, now the scene of a bloody strife between the Spanish and the English." We commend Perugia to the notice of Professor Guido Cora and his admirers.

CORRESPONDENCE.

A Complaint from the Wilderness.

Some time ago, having occasion to write to the Secretary of the Zoological Society about another matter, I enclosed the description of a new species of Coccidæ, of special interest, which I had just found at that time. In my letter I remarked that it could be printed in the *P.Z.S.*, or, if the Society did not care to print it, I should be glad if he would kindly send it on to the *Annals*.

After a good while, I saw by the papers that the description had been read at the Society, and presumed that it would be published in due course. But to-day I get it back, with no further explanation than the statement that the publication committee do not consider it suitable. The Society had kept the MS. over two and a half months.

Now, I have no complaint to make, of course, about the non-publication of the MS.; indeed, I had some little doubt in my own mind as to the suitability of the *P.Z.S.* for the introduction of a new insect, however excellent it might be for a new monkey. The wording of my letter to the Secretary was the result of this doubt.

The point I want to raise is this. Is it right for a Society to temporarily accept a MS., retain it some months, have it presented at a meeting, and *then* decline to publish it? Is not such a proceeding discourteous to the author, to say the least? Is it not calculated to deter those not among the select few from contributing MSS. to the Society? I do not say that it is intended to have this result; but whatever may be intended, surely such a method of procedure is both objectionable and unnecessary. I am raising the point now in reference to a particular instance, but the same thing has happened to my knowledge on other occasions, and probably happens often. At one of the very first meetings of the Zoological Society which I attended, a paper on fishes was read, which had been sent in by an old and distinguished South American naturalist; it was received with remarks about its being not up-to-date, and unsuitable for publication. The principal speaker was a very learned authority on fishes, and I can well believe that all he said was true; but, all the same, I thought the Society had not acted very courteously to the absent author.

T. D. A. Cockerell.

New Mexico Agricultural Experiment Station (U.S.A.).
February 25, 1895.

[We print Mr. Cockerell's letter because we wish to keep our columns open, so far as is possible, to any scientific man who has a complaint of general interest to make. We have dealt elsewhere with the question of the publication of papers. In the present instance, however, we have no doubt that the Publication Committee, which is elected by the Fellows, was acting only in the interest of the Society.—Editor, Natural Science.]

The Methods of Organic Evolution.

Mr. Galton has recently repeated, in *Mind*, his suggestion that *species* have originated by *discontinuous* variation, *i.e.*, by a series of sudden leaps, or sports, from one position of organic stability to another; and that this method of evolution is independent of the processes of *selection*. This theory, which is based upon Mr. Galton's statistical enquiries into heredity and regression, has been criticised by Mr. Wallace in the *Fortnightly Review* for February and March.

Mr. Galton, strangely enough, appears to overlook the fact that this theory of discontinuous variation between definite positions of organic stability, so far from in any way running counter to the theory of selection, entirely harmonises therewith: for the alleged process, if indeed a *vera causa*, must always be controlled by the master-process of Natural Selection. Clearly, if the—organically stable—characters produced by such discontinuous variation be disadvantageous to the species and ill-adapted to its requirements, they will immediately be swept away by Natural Selection.

This I find—as might be expected—insisted upon by Mr. Wallace in the second instalment of his article; but he strangely ignores the consideration that if such (alleged) variations prove advantageous, the work of Natural Selection is really far shorter and easier than we had hitherto supposed possible. This may be shown by a series of propositions, based on the assumption that Mr. Galton has found a *vera causa*. (1) Such sports, being the expression of definite positions of organic stability, will occur more frequently than mere "chance" variations. (2) They are, therefore, less likely to be swamped by the interbreeding which Darwin recognised as fatal to "chance" sports; and, especially when *particulate*, they are likely to hold their own in at least a proportion of the offspring. (3) Even were all such sports swept away by some disease or catastrophe, yet precisely the same varieties would soon re-occur in another generation, since they are the result of definite positions of organic stability. (4) Since such sports may, in one generation, achieve by a bound a position that could be attained only after many generations by the cumulation of small varieties, then clearly the process of species-making is greatly shortened. (5) It would appear, therefore, that a species may appear more than once, *i.e.*, may reappear even if once extinguished. (6) Obviously all such sports—the result of discontinuous variation—will be, when advantageous, preserved by selection even more certainly than will slight variations.

On the other hand, however, a sudden sport may often prove prejudicial to the species, although the same variation, reached by slow stages, would be beneficial; for the suddenly varying species may outstrip the slowly varying environment. For instance, the protective efficacy of mimicry depends upon the mimicking insect keeping pace with the mimicked; and if the latter—here the "environment"—vary slowly, so must the former: a sport of the mimickers, anticipating by centuries the final coloration of the mimicked, would be fatal. But sports in insects mimicking leaves would probably be beneficial.

F. H. Perry Coste.

Ravenshoe, Burnt Ash Hill, S.E.

Minneapolis and the Central Zoological Bureau.

Recent inquiries and suggestions from some of your immediate neighbours impel me to send you these few lines bearing upon your editorial "A Universal Zoological Record," in No. 34, vol. v., of your valued periodical.

The editorial seemed rather unfair to Mr. Barrows, who is printing a card catalogue of some zoological literature under my supervision, and for that reason I desire to give you a few facts in regard to the card catalogue we offered.

For several years we have been printing a card catalogue of the zoological literature available to the students in the Department of Animal Biology of the University of Minnesota. Last year we decided, at the suggestion of Dr. Thos. G. Lee, of this University, to offer to others what we had, and in the event of receiving sufficient encouragement make arrangements for obtaining the titles of all zoological articles.

At this time there was no evidence that Dr. Field's international bureau had any vitality, and we felt the way was clear for Mr. Barrows to issue his circular under date of August 10 without any apologies. To this circular we received encouraging and highly complimentary letters from all parts of our own country and from England. Some, without further questioning, sent in their subscriptions on the rather indefinite and extreme terms of the circular letter. The circular also brought us a kind letter of surprise from Dr. Field. To this we replied promptly, and asked some definite and pertinent questions. We waited so long in vain for answers to our questions that we felt none were contemplated, presumably because the international bureau was even less than a castle-in-the-air. Accordingly, we began to make arrangements for enlarging our list of periodicals, and were about to issue a second circular guaranteeing at least ninety-five per cent. of all zoological titles at a definite and very low price, when we received a reply to our questions from Dr. Field (who had been as prompt as circumstances would permit).

Dr. Field's letter indicated an actual move towards a realisation of the international bureau; and in the light of the facts disclosed we dropped all our plans, and expressed to Dr. Field our best wishes.

We present no claims for glory, priority, or anything of that sort in this matter. Our only desire is to see this very important work begun. Once begun it will be continued. At present, however, there is altogether too much importance attached to the active co-operation of certain excellent but painfully "slow" societies.

We are still printing our own card catalogue, and are furnishing it to a few friends, but are no longer offering it at any price. We urge everyone to encourage the movement aiming at the establishment of a central bureau at once.

University of Minnesota, Minneapolis, Minn. Henry F. Nachtrieb.

March 27, 1895.

The Leeward Islands.

In our March number we praised the colonists of these islands for their practical appreciation of science, especially as applied to agriculture. We regret to hear as follows from Mr. C. A. Barber :—

"By vote of the Local Legislature the office of Superintendent of Agriculture has been abolished, and the Department of Agriculture of the Leeward Islands, as such, no longer exists. The Library which I have been engaged in collecting during the last few years, and to which any journals and reports received have been added, I have handed over to Mr. Watts, the Government Analyst, Antigua, as Secretary of the Antigua Agricultural Society." Mr. Barber's address for the present is 16 Holborn Viaduct, London, E.C.

WANTED.

The following numbers of Natural Science : *No.* 8, *October*, 1892 ; *No.* 11, *January*, 1893 ; *No.* 12, *February*, 1893 ; *No.* 23, *January*, 1894. *The Publishers are prepared to give half-a-crown apiece for clean copies of the above-named numbers until further notice.*

CHANGE OF ADDRESS.

In future the Publishing and Editorial Business *of* "Natural Science" *will be carried on at the Offices of* Messrs. Rait, Henderson & Co., Ltd., 22 St. Andrew Street, Holborn Circus, London, E.C.

To Contributors.—*All communications to be addressed to the* Editor *of* "Natural Science" *at the above address. Correspondence and Notes intended for any particular month should be sent in not later than the* 10*th of the preceding month.*

"Natural Science" *is published on the* 25*th of each month ; all advertisements should be in the Publishers' hands not later than the* 20*th.*

Publishers sending Books for Review are particularly requested to take note of our Change of Address.

NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

No. 40. Vol. VI. JUNE, 1895.

NOTES AND COMMENTS.

The Royal Society's Selection.

THE following candidates have been selected by the Council of the Royal Society for election as Fellows :—Mr. J. Wolfe Barry, Professor A. G. Bourne, Mr. G. H. Bryan, Mr. J. Eliot, Professor J. R. Green, Mr. E. H. Griffiths, Mr. C. T. Heycock, Professor S. J. Hickson, Major H. C. L. Holden, Mr. F. C. M'Lean, Professor W. MacEwen, Dr. S. Martin, Professor G. M. Minchin, Mr. W. H. Power, Professor T. Purdie. We have neither the intention nor the desire to join the protestations of a certain disaffected set who take an annual opportunity of cavilling at the selected list. We believe all the candidates to be worthy of their places, and we offer them our sincere congratulations. Last year, in noticing the list, we said :— "It is not likely that anyone whose opinion is worth having will cavil at this year's selection. Still we cannot help regretting that no geologist has been nominated." The fact that this year again zoologists, botanists, physiologists, medical men, engineers, chemists, physicists, and mathematicians have found a place, while geology is unnoticed, deserves more than a passing mention. There are geologists and to spare in England ; indeed, geology for long has been a characteristically English branch of science, and, in our opinion, its English exponents are as numerous and as ardent as ever they have been. The Royal Society's annual list may be taken as a fair index of the estimation in which the exponents of each science are held by the exponents of all the sciences. It is plain that either the exponents of geology already in the Royal Society, and especially on its Council, or the exponents of geology outside the Royal Society, are behind the exponents of other sciences in convincing scientific men of the relative claims of geology. It is for geologists to search out the reason and to set their house in order.

The Harveian Oration.

Dr. Lauder Brunton has recently published his oration for 1894. Taking as his subject the modern developments of Harvey's work, he has succeeded in transforming what tends to be a perfunctory restatement of the obvious into a living account of the progress of science. Into the medical details, into the advances made in the treatment of disease, we do not propose to enter; we would merely point out a curious recurrence to the position of Harvey. The advance of science exhibits in a marked degree what Hegel called the dialectic of philosophy. In the history of knowledge, any two successive phases appear contradictory to each other; but the third phase is the reconciliation of contradictories in a higher unit.

When Harvey discovered the circulation of the blood, men's minds were receptive of vitalistic interpretations of nature. The harmonious unity of the animal body, for instance, they did not hold as a theory, but accepted with a childlike faith. In the matter of the varying flow of blood "*ubi stimulus ibi affluxus*," "increased flow goes where it is wanted," seemed to them so natural and necessary, that they did not inquire into the reason of it.

When it was realised that the heart was a force-pump, and the vessels a closed system communicating with it, mechanical interpretations held the field, and with each discovery of accelerating and depressing nerves the mechanical conception became stronger and stronger. But now that the intrinsic nerve-supply of the system, the nerves from the brain, and the controlling vaso-motor system have been demonstrated, we have reached a point where the mechanical conception—apparently complete—breaks down. All the strings and wires of a perfect mechanism are known, but the system works, not as a perfect mechanism, but as a self-regulating, vital whole. "Ubi stimulus ibi affluxus" represents exactly the position of our knowledge of to-day; and it is a statement, not an explanation.

Geological Literature.

We congratulate the Geological Society on showing itself alive to the needs of the time, and entering the ranks of the bibliographers. We alluded some time ago to the valuable list of accessions to the library, issued every year by this society, and we then pointed out the special value of that list, in that it gave one the complete geological contents of every journal received, which knowledge was not to be obtained elsewhere in this country. The Geological Society has now dropped this mode of recording, and has issued, separately from its *Journal*, an octavo pamphlet, entitled "Geological Literature added to the Geological Society's Library during the Half-year ended December, 1894." In future, therefore, the list will be concurrent with the years, instead of with the sessions of the Society—a convenient change. The present instalment, which is sold at 2s., contains a four-page list of abbreviations of titles of periodical literature, a list of all geological papers contained in the literature received, arranged alphabetically under authors, and occupying thirty-four pages, and a "Subject Index" of twenty pages.

Until certain conditions are fulfilled, we cannot pretend to welcome this change; and we trust that those responsible will not regard us as undue cavillers if we venture on certain suggestions. Let us say that the abbreviations employed in the author-index are so good that any glossary of them is really unnecessary. The space would be better devoted to informing the Fellows, as heretofore, precisely what parts of the journals listed have been received. The omission of a separate list of maps received is also to be regretted, and must be due to some oversight. As for the author-index, we have always said, and shall ever continue to say, that the value of such a list depends on its completeness. We do not complain that the compilers have omitted to index the paper on "The Homes and Migrations of the Earliest known Forms of Animal Life," contributed by Dr. Hicks to our December number, although they have indexed a similar paper by Dr. Hicks from another periodical, and have not omitted other papers in our number. But we mean to say that a list of only those papers which have been received by the Geological Society is necessarily and admittedly incomplete as a list of geological literature, and is, therefore, of small value to the geological student. In all lists of this kind it is the out-of-the-way papers that one wants to discover, and these are the very ones absent from the present list. With the disappearance of Dagincourt's *Annuaire*, an opening is presented to the Geological Society of doing a piece of work of world-wide importance, and not merely one for the delectation of its own Fellows. We cannot altogether overlook the fact that the present list is disfigured by some ugly mistakes, not to be excused as misprints, which ought never to have been passed by the Council, or which might at least have been corrected on an erratum-slip. The names of contributors to the *Quarterly Journal* at all events might be spelled correctly.

Our chief objection to the subject-index is that it is not a subject-index at all, merely an index, and a very incomplete one, to the catch-words of the titles. We will only ask any worker, who is sufficiently interested, to look up his own subjects in this index and to see if he considers the references at all adequate. If a subject-index is to be made at all, it must be made properly, and with a due appreciation of the difficulty of the task. We have found the twenty pages here devoted to it useless as a guide, and have had to go through the author-index on our own account in order to complete and correct the so-called subject-index.

If these few suggestions be accepted by the Geological Society in the spirit in which they are proffered, we shall hope next year to be able to congratulate it, not merely on its intentions, but on its accomplishment of them.

The Earthquake at Laibach.

The recent Austrian earthquakes furnish an instructive commentary on an article we publish this month on "The After-Shocks of Earthquakes," by Mr. C. Davison. The first great shock occurred on April 14, at 11.13 p.m. During the same night twenty-five shocks were felt at Laibach, and within the next three days at least forty more were observed, some of them strong, though not approaching the first in severity. From April 19, if we may trust the newspaper accounts, they became much less frequent, from two to four noticeable ones, besides a few weak tremors, generally occurring every day until the end of the month. After this, except for a severe shock on May 19, they do not seem to have attracted much attention, probably from their comparative slightness. Laibach forms the centre of one of the most interesting and most promising earthquake districts in Europe, and if some Austrian seismologist, with the energy of Professor Milne, would take the matter up, we should be provided in about as many years as it would require generations in England with the materials we need for the study of the growth of faults.

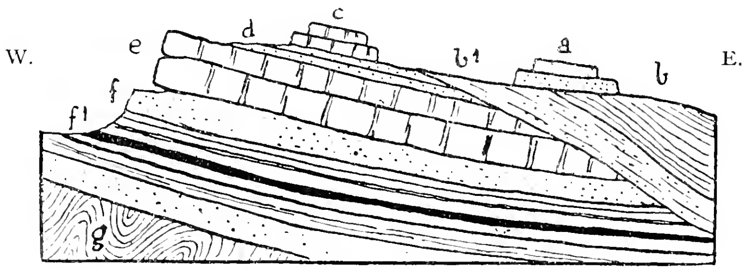
Artesian Water in Queensland.

Last December we commented on the excellent labours of the Geological Survey of Queensland, laying special emphasis on the intimacy that should exist between practice and pure science. Shortly afterwards an interesting address, enforcing the same moral, was sent us by the Government geologist, Mr. R. L. Jack; and now we have received from him a substantial proof of the truth of our remarks.

In our previous comment we alluded to the inquiries concerning an artesian or deep-seated water supply for the interior of Queensland. No great acquaintance with Australian physiography is needed for us to appreciate the enormous importance of such a supply. We have all heard of the terrible droughts in the interior of the continent, we know how the creeks dry up, and a glance at the map shows us how colonisation has hitherto been restricted almost to the sea-board. The soil in the western interior of Queensland is kindly, it supports grasses infinitely superior to those of the coast; and yet settlement is meagre, or impossible, because nearly all the rain falls on the belt of elevated country near the east coast, and only a half or a third as much reaches the western country. Though dams and tanks were constructed at a great cost, yet in 1885 cattle and sheep died by hundreds of thousands, and some of the western towns were threatened with extinction. Since then, on the recommendation of Mr. Jack, and Mr. J. B. Henderson, hydraulic engineer, over 200 bore-holes have been sunk along the western downs, which together have a daily yield of 125 million gallons. What this means may be understood by comparison with the daily public supply of London from all sources, which, as stated by the last Report of the Royal Commission, is about 171 million gallons. Last year further surveys and investigations were prosecuted by Messrs. Jack and A. G. Maitland,

and the preliminary results of their researches form *Bulletin No. 1 of the Geological Survey of Queensland.*

Somewhat remarkable explanations have been given to account for the occurrence of artesian water in Queensland. The populace imagines underground rivers, or reservoirs under great pressure in the bowels of the earth. The geological speculator pictures permeable beds passing beneath Queensland, and rising to the surface in the mountains of New Guinea. More profound philosophers have gone so far afield as the Himalayas or the Andes to find a collecting-ground for the Queensland water. The facts, for once, are not so strange as the fictions, and may readily be gathered from the accompanying diagram. On the right are seen the shales (b) of the Rolling Downs Formation, Lower Cretaceous in age; these stretch away to the east till they meet the sea, or are covered by the alluvial and other deposits near the coast. The lower part of the Rolling Downs Formation (b 1) is that which is of chief importance; it is called the Blythesdale Braystone, and consists of a series of soft, gray, very friable sandstones, grits and conglomerates, which absorb water with avidity. The outcrop of this rock forms a belt with an average width of five miles, and is estimated to present a gathering-ground for water of 5,000 square miles. These beds rest unconformably against a series of Triassic and Jurassic rocks, viz., the Darling Downs Basalt (c); Slates, Sandstones, and Coal-seams (d); Toowoomba Basalt (e); Murphy's Creek Sandstone (f); Ipswich Shales, Sandstones, and Coals (f 1). Below all come the folded Palæozoic Rocks (g). Above the Rolling Downs Formation lie patches of an Upper Cretaceous Rock, the Desert Sandstone, which once extended as a sheet over all the rocks just mentioned, but which is now found only in isolated patches, some of which obscure the outcrop of the Blythesdale Braystone.



GEOLOGICAL SECTION, WESTERN QUEENSLAND.—*R. L. Jack.*

There are a few water-bearing beds in the higher part of the Rolling Downs Formation; but for effective supplies the bores have to penetrate to the Blythesdale Braystone. This derives its water mainly from the annual rainfall of twenty-seven inches over its intake area. Much, also, is absorbed from numerous large streams which cross its outcrop, but run for only a small portion of the year. The Desert Sandstone is also very absorbent, and where it "lies like a full sponge on the top. . . . of the Blythesdale Braystone, must tend to equalise the supply by feeding the latter long after the rivers have ceased to run." There is also evidence that the sandy beds d, f, and f[1] of the diagram may abut in places on the Blythesdale Braystone, and still further tend to equalise the supply. These auxiliary beds are of greater importance if it be the case, as some suppose, that the Blythesdale Braystone crops out on the sea-floor, and thus parts with some of its fresh water by leakage into the ocean. In any case it appears, from the figures collected by Mr. Jack, that the amount of water passing through this bibulous bed is far more than can ever be exhausted by artificial means, so that the multiplication of bores is not a danger to be dreaded, but a remedial measure to be urged on by all means. We hope that Queensland will support its Geological Survey, which has made its deserts blossom like the rose.

Niagara and the Great Lakes.

So much has been written on Niagara Gorge and the Great Lakes, without exhausting the subject, that we venture to bring before English geologists the conclusions arrived at by Mr. Frank Bursley Taylor, and published by him in *The American Journal of Science* for April. We reprint here merely his chronological conspectus, and must refer our readers to his paper for the details of the various steps, so concisely set down by him, and so easily followed by anyone who troubles to read the conspectus with a map in front of him.

Mr. Taylor says:—"At its maximum the great Laurentide glacier covered the whole area of the Great Lakes. By a correlation of the abandoned shore lines, moraines and outlets, and the gorges, recently submerged shore lines and rivers of this region, the following order of events is made out for the post-glacial history of the Great Lakes. They are set down in seven principal stages, with transitions or critical stages between.

"I. Glacial, ice-dammed lakes. Outlets at Fort Wayne, Chicago, and other places. Beaches correlated with moraines in Ohio. Glacial lakes fall by stages as outlets change on withdrawal of the glacier-dams. Land relatively high in the north but slowly subsiding.

"*First Transition:* By withdrawal of glacier the Niagara river is opened and the upper lakes become united.

"II. First Niagara lakes. First epoch of Niagara Falls begins at Lewiston. For a short time glacial Lake Iroquois receives the water from Niagara. Shore lines of lower levels of this glacial lake washed over and obliterated by later marine invasion. Gradual depression of land continues at north, finally opening Nipissing outlet.

"*Second Transition:* First two-outlet climax. Marked by the ALGONQUIN BEACH. (Possible subdivision here for supposed Trent river outlet.) Gradual northward depression continues. First epoch of Niagara Falls closes at the Whirlpool. Epoch of Erigan Fall begins.

"III. First Lake Algonquin. Outlet eastward over Nipissing pass.

"*Third Transition:* Gradual northward depression continues. Nipissing outlet brought down to sea level. Lakes become marine.

"IV. Warren Gulf (rising stage). Marine waters fill the three upper lakes, the Ontario, St. Lawrence and Winnipeg basins.

"*Fourth Transition:* Marine climax. Marked by the CHIPPEWA BEACH. Northward depression ceases and gradual elevation begins. Iroquois and Herman marine beaches made at the same time as the Chippewa. This was probably the climax of the post-glacial warm epoch.

"V. Warren Gulf (falling stage). Gradual northward elevation. Irregular uplifts in the north deforming Chippewa and Algonquin beaches.

"*Fifth Transition:* Nipissing outlet raised to sea level. Upper lakes become fresh.

"VI. Second Lake Algonquin. Outlet eastward over Nipissing pass. Probably a small amount of local uplift at outlet in early stage.

"*Sixth Transition:* Second two-outlet climax. Marked by the NIPISSING BEACH. Epoch of Erigan Fall closes at a point between 40 and 80 rods above the cantilever bridge. Second (present) epoch of Niagara Falls begins.

"VII. Second Niagara lakes (present stage). Lake Superior becomes independent. Great Champlain uplift at the north-east. Formation of St. Clair delta begins and continues to the present time."

PRIMÆVAL MAN.

LAST November we published a paper by Professor Rupert Jones on the discovery by Dr. Fritz Noetling of Flint Chips in the Upper Miocene of Burma. Dr. W. T. Blanford writes to our contemporary, *Nature*, that a letter received by him from Dr. Noetling states that the beds in question have since been definitely ascertained to be Pliocene and not Miocene. This, though it reduces the proved limits of man's antiquity, does not diminish the interest and importance of the discovery.

Some remarkable finds have recently been made in the loess of Predmost, in Moravia. Carbon, chipped flints, and bones of extinct animals were previously known, but it was not till May, 1894, that Mr. K. J. Maska, a well-known Czech archæologist, found remains definitely human. His researches have since been rewarded by the discovery of ten almost complete skeletons, in the natural position as they had been buried in the brick-earth. The state of preservation of the bones, and the occurrence in their immediate neighbourhood of bones of the arctic fox and the mammoth, leave no doubt that they belonged to Quaternary man. We learn from *L'Anthropologie* that the remains are supposed to have belonged to a family, which, after having perished in some catastrophe or other, was interred in this place before the culture-layer found above had begun to form—that is to say, at the earliest period of habitation at Predmost. A block of stone was placed on the tomb to prevent the ravages of wild beasts, an end which was not completely attained, since some of the bones have been found at a little distance from the tomb and marked by the teeth of animals. The shape of the skulls shows that the Quaternary inhabitants of Predmost belonged to a dolichocephalic race, that their forehead was retreating, especially in the males, with strong superciliary ridges, and a snub nose. The superciliary ridges are, however, less developed than in the Neanderthal cranium.

The skeleton of a primitive man is said to have been found some years ago in the alluvial deposits of the lower Thames. We learn that the remains consist of a human skull and limb-bones found in the Palæolithic Terrace-gravel of Galley Hill, Greenhithe, Kent. Mr. E. T. Newton read an account of them before the Geological Society on May 22, after we had gone to press.

The Shells of the Antilles.

The West Indies may well be considered a paradise for the malacologist. Apart from the abundant marine fauna, their superficial area of 95,000 square miles supports some 1,600 species of terrestrial molluscs, or nearly as many as are found on the mainland of the entire continent of America. The distribution of these through the various islands of the group forms the theme of a valuable paper by Mr. C. T. Simpson (*Proc. U.S. Nat. Mus.*, vol. xvii.), who prefaces his remarks with a few words on the physical geology of the West Indies, and a lucid summary of the means of dispersal of non-marine mollusca. The tables of distribution themselves, dealing as they do with strings of names, are too technical for all save the very elect; but the details concerning the relationships of the faunas of the several groups of islands, both to one another and to the various adjacent portions of mainland, yield much interesting information. The conclusions arrived at concern the geologist quite as much as the malacologist. Briefly summed up, they are that "a considerable portion of the land-snail fauna of the Greater Antilles seems to be ancient, and to have developed on the islands where it is now found. There appears to be good evidence of a general elevation of the Greater Antillean region, probably some time during the Eocene, after most of the more important groups of snails had come into existence, at which time the larger islands were united, and there was land connection with Central America by way of Jamaica and possibly across the Yucatan Channel, and there was then a considerable exchange of species between the two regions. At some time during this elevation there was probably a landway from Cuba across the Bahama Plateau to the Floridian area, over which certain groups of Antillean land-molluscs crossed. At this time it is likely that the more northern isles of the Lesser Antilles, which seem to be volcanoes of later Tertiary and Post Pliocene date, were not yet elevated above the sea or if so they have probably been submerged since. After the period of elevation then followed one of general subsidence. During this, the island of Jamaica . . . was first to be isolated, then Cuba, and afterwards Haiti and Puerto Rico were separated. The connection between the Antilles and the mainland was broken, and the Bahama region, if it had been previously elevated above the sea, was submerged; the subsidence continuing until only the summits of the mountains of the four Greater Antillean Islands remained above water. Then followed another period of elevation, which has lasted no doubt until the present time. . . . The Bahamas . . . have been peopled by forms drifted from Cuba and Haiti. . . . The Lesser Antilles have been peopled for the most part from South America."

Hanging Foliage.

In the latest issue of the *Annals of Botany* (March), the hanging foliage of certain tropical trees forms the subject of a paper by F. W. Keeble. It is well known that the young leaves and branches of several species of leguminous plants hang vertically downward in the early stages of their life, and the writer has been seeking the causes which have induced this habit. Stahl saw in the occurrence merely a means of protection of the young leaves from the force of the violent tropical rain, which, owing to the stillness of the atmosphere in the tropics, generally falls vertically. From certain experiments of his own he concluded that the pendulous position was not a protection of the tender foliage from the sun's rays, or from too great a loss of water in transpiration. Mr. Keeble, however, found that the alcohol extract from leaves of *Amherstia nobilis* which had been fixed in a horizontal position, was of a lighter green than that from leaves which hung vertically. Moreover, he found that the delicate miniature leaves of the same plant withered more rapidly than those more fully developed, under similar conditions of exposure. As the older full-grown leaves were also found to transpire much more rapidly than the young hanging leaves, it was inferred that the tougher, more leathery mature organs could bear with impunity a loss far greater than that which sufficed to damage the delicate younger ones. Hence it seems probable that the pendulous position has been assumed, in part at least, as a protection from injury by exposure to direct sunlight and also against excessive loss of water by transpiration; for the young leaves transpire more rapidly when outspread than when hanging. As a brightly coloured cell-sap is often characteristic of young foliage in the tropics, it has been suggested that some protection might thereby be afforded against too strong thermal or chemical action of the sun's rays; and thermometric observations showed that the surface layers of the red leaf reflected more heat than those of the green, and conversely, that the green leaf absorbed more heat than the red. That is to say, the red colouring-matter acts as a screen by which the heating effect of the sun's rays is moderated. Another observer has recently shewn that it also is a powerful agent in protecting the leaf from too intense light.

We might suggest the common ash as a subject of investigation on the same lines. The opening leaves are tinged with a beautiful red from the sap contained in the hairs and outer cell-layers of the young leaves and leaf-stalks. Plunging in hot water at once extracts the dye, giving a brilliant crimson solution, with an acid reaction, becoming greenish when rendered alkaline with potash or soda. Not many years ago a tree grew ready to hand outside the window of the Botanical Laboratory at Cambridge.

Some criticism is passed on Stahl's view of the functions of the long tips of the leaves as means for running off water from the upper surface. It is pointed out that in trees of *Amherstia nobilis*, growing in the Peradeniya Botanical Gardens, the rather long tips consist of tissue that does not become so tough or leathery as the rest of the eaf, and while uninjured in the pendent position, these thin extremities are rapidly killed when the leaves rise up and expose themselves to the sun. Moreover, the tips, which were very long in the earlier stages, become, as the full green adult condition is assumed, less well marked. If the pointed end is a "drip-tip," why then is it better developed when it is useless?—for by the young leaf's position no rain falls directly on it, and water-drops splashed on it are at once rolled off by the hairs which are then present but which disappear with maturity.

In addition, however, to these temporary advantages, the author suggests an additional "permanent" advantage. Besides affording protection to the young foliage, the pendent habit enables the branch and petioles to wait in a safe position till the conditions of shade or sun, as far as these are governed by the arrangement of other branches, can, so to speak, be ascertained. Then on rising up, the branch with its leaves meets conditions more akin to those under which it must pass the rest of its existence than if it had grown out horizontally from the first. By means of the waiting and the rising up of the branch and the petiole, the shoot behaves like one of the leaflets—both are able to adjust themselves to a nicety to the prevailing conditions of sunlight. The two movements, those of the shoot with its leaf and of the leaflets, are compared to a coarse and fine adjustment.

A Primitive Lily.

In the same magazine Mr. Ridley describes as a new genus, with the suggestive name *Protolirion,* a saprophytic plant collected by himself in the Malay Peninsula. Mr. Groom gives a detailed account of its structure, and also discusses its affinities. "*Protolirion,*" he says, "may be regarded as closely related to the archetype of the Liliaceæ, and connecting the modern Liliaceæ with the Triuridaceæ." This interesting little plant is only three inches high, devoid of chlorophyll, and consists merely of a slender underground rootstock from which rises a simple scape bearing scales, and ending in a few small flowers. As the embryo in the seed shows no differentiation, the evidence for its being a monocotyledon rests on the arrangement of the parts of the flower in threes and on the presence of the nectar-secreting glands in the dividing walls of the ovary, known as septal glands, which hitherto have been found only in the petaloid monocotyledons. On the principle that union of parts denotes a later stage in phylogeny as compared with the case where they are free, *Protolirion* will hardly do for a near relative of the ancestor of the lilies. For while the typical lily has sepals, petals, and stamens, quite free and situated below the ovary, its supposed progenitor has the two former series (perianth) attached at the base to the ovary wall, the three outer stamens are similarly inserted, the three inner rising from the bases of the inner segments of the perianth. The pistil is remarkable from the fact that the lower portion of the ovary is inferior and three-chambered, while above it separates into three distinct portions.

We are much inclined to doubt the connection between this plant and the ancestral lily. Its saprophytic habit, which, as Mr. Groom suggests, may account for the apocarpous condition of the upper part of the pistil (a point which has to be explained away somehow), may also be responsible for points on which he lays more stress.

A most commendable feature about the whole thing is the co-operation of the systematist and anatomist to put forward an account which tells us something of internal structure as well as the mere external diagnostic characters to which papers on systematic botany are too often restricted.

The Action of Light upon Bacteria.

Professor Marshall Ward is continuing his researches into the nature of the inhibitory action of light upon the growth of bacteria. In the *Philosophical Transactions* of the Royal Society (1895, p. 961) he describes and illustrates by photographs some of his more recent results. When a pure culture of *Bacillus anthracis* was grown on a glass plate, to which light was admitted only by the aperture of a stencil-letter, after 24 hours incubation the shape of the letter was most clearly marked upon the plate. The bacteria had grown thickly where they were protected from the light. Where the light passed through the stencil-letter all growth was stopped. After a longer incubation, under the same conditions, a few colonies appeared over the illuminated area. For several reasons Professor Ward inferred that these colonies had grown from spores that in some way were protected from the light, possibly by being concealed behind other spores, as the layer of jelly was not thin enough to contain a set of spores only one deep.

In another set of experiments, conducted both with solar light and with electric light, the influence of the different rays of the spectrum was examined. It was found that there was no bactericidal action, at least in the case of *Bacillus anthracis* grown on agar-agar, in the red, orange, yellow, and true green parts of the spectrum. The action began near the line F and extended right on into the violet. All the blue-violet rays were effective to some extent, but the maximum action was about the line G.

For a variety of reasons into which we cannot enter here, Professor Ward concludes that the bactericidal action of these rays is a direct action upon the living protoplasm of the bacteria. Many other observers have shewn that light hinders the growth of other bacteria. It has been shewn to have such an effect upon the typhoid bacillus and upon several bacteria not associated with disease. Professor Ward found that the growth of several fungi and moulds was hindered by it and he is inclined to the view that the direct action of light hinders the growth of all living protoplasm.

We need not point out that Professor Ward's results, combined with the results of previous investigators, have an important bearing upon public health. It is unnecessary in these days to insist upon the necessity of sunlight, or at least of daylight in all parts of dwelling houses. But there is a minor although important issue raised. Especially in large towns the washer-woman and the private laundry are gradually being replaced by steam laundries, and we have grounds for saying that in the majority of the larger institutions the drying of linen is carried out by artificial heat in closed chambers. Care is usually taken to secure proper ventilation of these, but no care is taken to secure that light and especially direct sunlight has due access to them. The clothes that used to dry on the village green or in the private yard were thoroughly disinfected by the kindly rays of the sun. Clothes dried in a heated chamber are returned to their owners without having undergone this natural and harmless disinfecting process.

A New Worm.

We have received an interesting excerpt from the *Proceedings* of the Academy of Natural Sciences of Philadelphia dealing with a new genus of the Discodrilidæ, which the author, Mr. J. P. Moore, terms

Pterodrilus. The family, it should be explained, was formerly referred to the leeches on account of the jaws and the suckers. It is familiar to most zoologists in this country by the genus *Branchiobdella*. The new genus shows, perhaps even more obviously than *Branchiobdella*, the justice of placing these worms among the Oligochæta and of removing them from the Hirudinea. As a matter of fact, the gulf between those two groups is not now by any means so great as it was when we were only acquainted with the anatomy of *Lumbricus* among earthworms. There are many worms now known, belonging to the family Eudrilidæ, in which the generative pores are single and median, and in which there is a reduction of the cœlom by the extraordinary development of perivisceral sacs surrounding certain of the generative organs; these to some extent bridge over the gulf which has hitherto lain between the leeches and the earthworms. *Pterodrilus* is chiefly remarkable for the paired processes upon the back, which clearly suggest gills. There are now quite a crowd of Oligochæta, which, contrary to the Cuvierian definition of the group, are gilled. *Dero* has been long known with its circlet of gills round the anus. Professor Bourne has lately made known the Indian Naid, *Chætobranchus*, in which there are paired processes of the body-wall, which contain blood-vessels, and in which also, in certain parts of the body, the setæ are implanted. More recently still, Mr. Beddard has described the Tubificid *Branchiura*, with retractile gills upon the posterior segments of the body, containing blood-vessels, and an extension of the body-cavity separated from the general body-cavity by a diaphragm. And, lastly, there is the genus *Alma*, as it was originally called, which Dr. Michaelsen has lately shown to be identical with *Siphonogaster*. This is a true earthworm, with retractile gills that can be withdrawn into pits upon the body. *Pterodrilus* has a series of paired outgrowths of the dorsal body-wall which suggest gills; but, as the author of the paper points out, they do not altogether fit in with what might be expected of a branchial organ, in that they have no blood-vessels; they have no internal cavity either, but are solid throughout.

In the male reproductive organs this little Discodrilid is curiously like the aquatic family Lumbriculidæ. There are, as in the last-mentioned family, two pairs of sperm-ducts, which open, however, into a common atrium; in the segment in front of this lies the single spermatheca. Although the terminal gland of the male ducts, which is often termed the atrium, is single in this as in other Discodrilids, it shows traces of division, and there are four male ducts. There are two species of the genus, which, like their European allies, are parasitic upon crayfishes (*Cambarus*); they are of minute size, hardly exceeding a millimetre in length. If there was any doubt about the systematic position of the Discodrilidæ after the researches of Voigt and the monograph of Vejdovsky, Mr. Moore's paper settles the matter—at least to our satisfaction.

Mesenchyme.

In a recent valuable memoir published by the Royal Society (*Philosophical Transactions*, vol. clxxxvi., p. 163), Dr. Gadow gives the result of an elaborate investigation made by Miss Abbott and himself upon the evolution of the vertebral column in fishes. He takes occasion to dismiss Hertwig's theory of the mesenchyme as a "gratuitous hypothesis." While we are far from accepting it in the full sense of Dr. Hertwig, we are unable to agree to Dr. Gadow's casual dismissal of it. It seems to us that it is worth while to emphasise the double nature of the blasts between the epiblast and hypoblast. In Echinoderms and in *Amphioxus*, to take two examples, there is the clearest possible distinction between the amœboid wandering cells that form the blood and connective tissues and the invaginations that form the cœlom. In the higher vertebrates the cœlom is not formed as a series of invaginations, and thus the distinctive character of its mode of origin is lost. But still there seems to remain the distinction between the set of cells that form the true cœlomic system and the set of cells that form the skeletogenous layer and the blood system. The origin of the two is more difficult to distinguish, and we think that Dr. Gadow's paper is most valuable as clearing up the exact mode of origin of the skeletogenous layer. But the difference in behaviour of the cells of the skeletogenous layers and of the cœlomic cells is equally striking. We do not think that the mesenchyme is yet to be abandoned.

The Vertebral Column of Fishes.

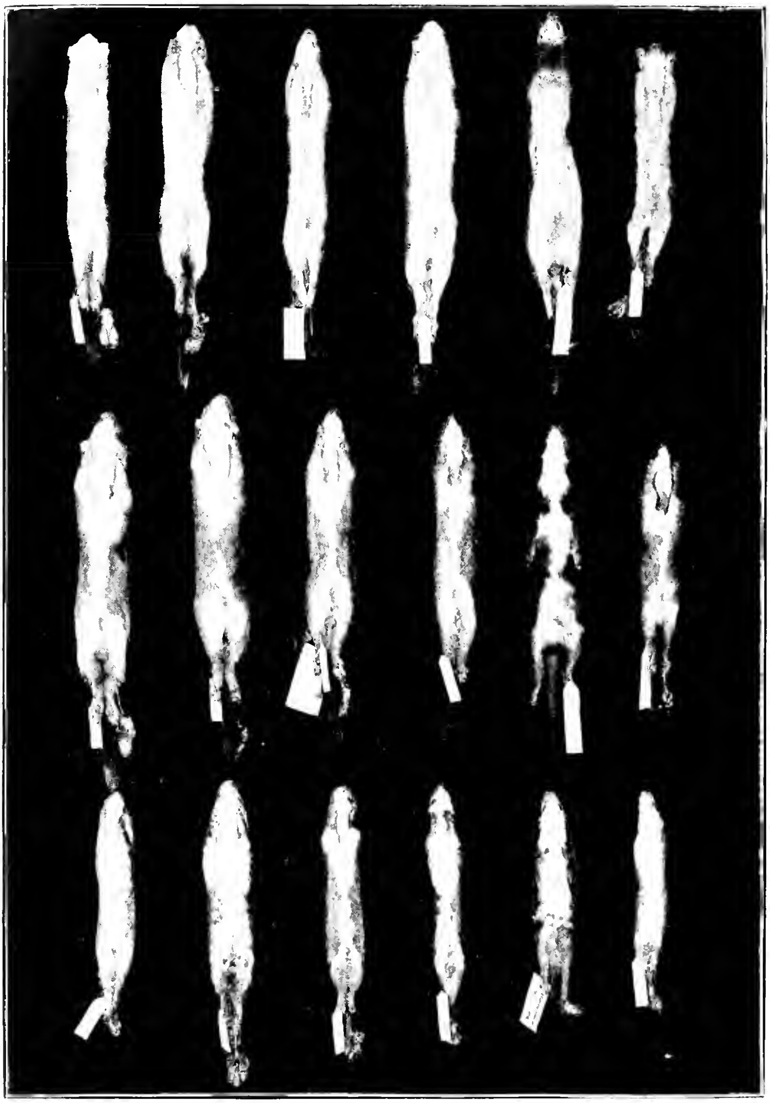
Even after the most careful study of the paper, in the absence of the necessary specimens, it is not easy to follow Dr. Gadow's summary of his results. The protovertebræ of primitive mesodermal segments divide into two parts. One of these, called the myotome by Dr. Gadow, divides to form a myomere, or section of the trunk musculature, and a part of the cutis. The other part forms a sklerotome, which produces skleromeres or skeletal trunk segments. The sklerotome or skeletogenous centre of each protovertebra consists of a separate dorsal and ventral half. Each of these is a curved S-shaped piece. In subsequent growth the dorsal half of each sklerotome grows downwards and comes to lie behind the ventral half of the sklerotome in front of it. Similarly, the ventral half of each sklerotome grows upwards, and comes to lie in front of and below the dorsal half of the sklerotome behind it. The acting units of the body are formed by the union of the unequally numbered sklerotomic halves in such a way that the dorsal half lies behind and above the ventral half. From this mode of union it results that the myomere or segment of the general trunk-musculature of the body comes into relation, not with the skeletogenous element formed from the same protovertebra, but with a compound element formed from the dorsal and ventral parts of the skeletogenous elements formed from two protovertebræ. This alteration of relations has been known for long, and is always referred to, as the resegmentation of the body, the teleological explanation being that the muscles must pull upon two vertebræ in order to make the whole segmented organism a physiological unit. But, according to Dr. Gadow, it takes place in a manner fundamentally different to what has been supposed. Each new or permanent skleromere is not formed by the vertical splitting of two successive sklerotomes, but is composed of the dorsal half of the posterior and the ventral half of the more anterior sklerotome.

Apart from the interest of this for vertebrate morphology generally, it has a biological interest that may appeal to many naturalists who neither know nor care to know the details of the embryological history of the skeleton and musculature. For it is one of the many instances in which the doctrine of a sudden and inexplicable (save teleologically) change has been abandoned after minuter investigation for a doctrine of slow metamorphosis. It is almost impossible to believe, if one reflect upon it, that "resegmentation" actually could occur, that the primitive myotomes, having been formed, should be cut in two, and that the adjacent halves of successive myotomes should unite. But some such theory as that was accepted unquestionably for long, and, so far as we know, it has not been criticised before the appearance of this paper by Dr. Gadow.

A Monograph of Crinoids.

The crinoids of the Palæozoic rocks of North America are so rich and varied in form, so numerous in individuals, that they have long been the delight and the despair of naturalists. Especially is this the case with that order of the crinoids to which the name Camerata is now generally applied, the order that includes such well-known forms as the Nave Encrinite, *Actinocrinus*, the Rose Encrinite, *Rhodocrinus*, which are common enough in our own Mountain Limestone, together with the flatter and simpler form, *Platycrinus*. For, in America, there are added to these ordinary genera such remarkable creatures as the huge *Megistocrinus*, the speared and spined *Dorycrinus*, the peculiar mushroom-like *Agaricocrinus*, *Strotocrinus* like a college don in his mortar-board, *Eretmocrinus* with its broad oar-like arms, *Pterotocrinus* whose lofty dome is surmounted by wings, *Gilbertsocrinus* with strange drooping appendages of unknown function, and *Batocrinus* whose pores at the bases of the arms are equally mysterious. But this list does not include a quarter of the camerate or vaulted genera known from the Carboniferous rocks of America alone; while, if we accept the work of Mr. S. A. Miller and kindred spirits, the long line will stretch out to the crack of doom. Such, indeed, is the variety of form, and such the rashness of interpretation of some of the more enthusiastic collectors and describers, that to us European students the subject has become one of inextricable complexity. It is, therefore, with peculiar pleasure that we learn that an authoritative monograph of these wonderful and beautiful beings is shortly to be issued.

Since the year 1859, or thereby, Charles Wachsmuth, who lives at Burlington, Iowa, in the very heart of the crinoid country, has devoted his life to the study of these animals. A large collection which he made was bought for the Museum at Cambridge, Mass., by Professor Louis Agassiz, at whose invitation Wachsmuth settled at the University to take charge of the whole collection of crinoids. The firstfruits of his study were published in 1877. After a time Wachsmuth returned to Burlington and began to form a second collection; much of this he was, unfortunately for himself, forced to part with, this time to the enrichment of the British Museum, in whose galleries some of his magnificent specimens are displayed. Association with Frank Springer enabled him to continue his collection and his studies, so that the series of fossil crinoidea made by the two friends is unrivalled even by the great collections of London, Harvard, or Stockholm, and their "Revision of the Palæozoic Crinoids" has long held the front rank among all works on the subject. In their knowledge of the writings of others, in their accurate discrimination of generic and specific characters, and in their important contributions to the morphology of the crinoids, these gentlemen have shown themselves most fitted to prepare that desired necessity, a monograph of the fossil crinoids of North America. The magnitude of the task, the failing health of the elder worker, and the business cares of the younger, have prevented the completion of more than a portion, that, namely, which deals with the Crinoidea Camerata. The text of this portion alone will fill between 600 and 700 quarto pages, while no less than eighty-three plates, of extreme beauty, have been drawn by A. M. Westergren, J. Ridgway, and C. R. Keyes, under the immediate supervision of the authors. It is fitting that Professor Alexander Agassiz and the Museum of Comparative Zoology at Harvard should undertake the publication of this monograph. It will appear as one of the Memoirs of the Museum, so soon as the plates can be photographically reproduced from the original pencil drawings—that is, it is hoped, early in 1896. The price will be thirty dollars. As the edition will be limited, intending subscribers are requested to send their names to Professor Agassiz at the earliest possible date. A work of such usefulness and importance needs no recommendation from us; we can only hope that the enterprise of the publishers and the devotion of the workers may meet with due appreciation from the scientific public, and that Charles Wachsmuth and Frank Springer may be spared many years of health and leisure, to place the crown on this worthy monument of American palæontology.



THE STOAT, THE ASSOGUE, AND THE WEASEL.

Photographed by Mr. A. Gepp, from specimens in the British Museum. The right-hand specimen in each row is a female; the rest are males.

I.

The Assogue (*Putorius hibernicus*): A Peculiar British Mammal.

ALTHOUGH several birds, recognised either as species or sub-species, are peculiar to the British Islands, no mammal has hitherto been considered sufficiently distinct from its Continental allies to deserve a peculiar name. Therefore, although notices of mere new species do not, as a rule, lie within the province of NATURAL SCIENCE, the recent discovery[1] that a well-known member of the Irish fauna represents a distinct species peculiar to that island seems worth a passing reference, especially as by the kindness of my colleague, Mr. A. Gepp, its characteristics can be so well illustrated as they are on the accompanying Plate iv.

This plate shows half a dozen specimens each of the Stoat, the Assogue, and the Weasel, those of the Irish animal being all that were available when the photograph was taken, and the others being picked at random from large series of each species, without any conscious selection to prove special points.

Briefly put, the differences between the three forms are as follows:

Stoat (*Putorius ermineus*).—Top row of plate.

Size large; colour-distribution (in summer dress) perfectly constant; the light of the under surface covering the upper lips, the whole breadth of the throat, chest, and belly, and the inner sides of the limbs, and running on to the fingers and toes; line of demarcation between colours, straight and regular; rims of ears white; tail long black-tipped.

Assogue (*P. hibernicus*).—Middle row.

Size medium; colour-distribution very variable, but the white always much reduced in extent, not extending on to the upper lip, narrowed, or even interrupted, on the chest and belly, and rarely extending on to the feet; line of demarcation between colours irregular; rims of ears brown; tail black-tipped.

[1] See Thomas and Barrett-Hamilton, *Ann. Mag. N. H.* (6), xv., p. 374, April, and *Zoologist* (3), xix., p. 124, 1895.

Weasel (*P. nivalis*).—Bottom row.

Size small; colour-distribution variable, but the white, as a rule, of less extent than in the Stoat, and of greater than in the Assogue; line of junction between the two colours irregular, as in the latter; rims of ears brown; tail short, uniformly brown.

It may be thought that these colour characteristics are of little importance, especially when it is granted that they vary in the Assogue; but the noticeable point is, that not a trace of variation, in these respects, can be found in Stoats elsewhere. Not less than some sixty or eighty specimens have been examined, coming from Scotland, England, and France on the west, through Scandinavia and Russia, to Siberia on the east, while even in those of Western North America, however other characters may differ, the colour distribution remains absolutely the same.

Compared to this vast area, that the little island of Erin, with a climate not unlike those of Cornwall or Brittany, and nearer to S.W. Scotland than we are to France, should have a local recognisable form is a most astonishing fact, and one for which it seems almost impossible to offer any reasonable explanation.

That, later on, we may find the Assogue to vary so as to meet the Stoat in colour, and, therefore, to be, according to some people, only a sub-species or "local race" of the latter, is, no doubt, possible; but this would, of course, be perfectly immaterial from the point of view of anything beyond mere species-naming, the fact remaining that the Stoat of Ireland is, even in its variability, an inherently different form from that found elsewhere. The discovery of the influences which have caused the one animal to become fixed in its colour-markings while the other has remained, or become, variable, is a nice problem which may be commended to the notice of geographical zoologists.

As yet, however, although the possibility is granted, no Assogue coloured like a Stoat has turned up out of more than a score of specimens seen; while the majority are of the very well-marked type represented by the five right-hand specimens in the figure.

I may take this opportunity of thanking the friends to whom we are indebted for specimens of this interesting animal, namely, Colonel J. W. Yerbury, Mr. J. E. Harting, Mr. Deane Drake, Mr. G. H. Pentland, and, most of all, Sir Douglas Brooke, from whom we have had nearly a dozen specimens. Lord Powerscourt, besides several dead examples, has also sent a pair of live ones, which may now be seen in the Small Cats' House in the Zoological Gardens.

Further specimens will still be acceptable, almost to any extent, as one of the most interesting points about the Assogue is its variability, and this can only be properly gauged when really considerable series are available for examination.

OLDFIELD THOMAS.

II.

The Cañons of Southern France.

THE great cañons of America are so well known that the very name of cañon has now an American flavour about it, and people are apt to forget that it is a Spanish word applicable to many valleys in Spain and in Southern France. Indeed, some remarkable examples occur in a part of France which it is now by no means difficult to reach, though it is only within the present century that Frenchmen themselves have become aware of the marvels to be found in a corner of their own country; and it is only by the recent extension of the Ligne du Midi, and by the conversion of ancient tracks into good roadways, that the "causses" and cañons of Lozère have been made accessible to the ordinary traveller.

Bounded on the north by the granite hills of central France, on the east by the range of the Cévennes, and rising high above the lowlands that border the Gulf of Lyons, are the "causses" of Lot and Lozère; high-lying plains or plateaux, consisting of thick Jurassic limestones disposed in nearly horizontal beds, and resting on clays and marls of Liassic age, from beneath which, to the north and east, emerge the gneiss and granite of the Central Highlands. Southwestwards, the causses descend in a series of broad steps toward the valley of the Garonne.

All the water which falls on the surface of the limestones is absorbed by those rocks and sinks into the ground, finding its way by subterranean passages and caverns down to the level of the springs, which feed the deep-sunk rivers of the country. The causses themselves are dry, rocky, and barren plains, without soil and without trees. But Mr. Baring-Gould[1] tells us that before the Revolution they were covered with forests, and that the impoverishment of the entire limestone district is due to the ruthless denudation which followed on that event. The seigneurial forests were cut down, the soil was bared to the winter storms, and, no longer held together by the roots of trees and shrubs, it was rapidly swept away and carried down into the depths of the valleys or into the swallow-holes, which gape like gigantic wells in the bare surface of the causses. "One hundred years has sufficed to sweep every particle of soil from

[1] "The Deserts of Southern France." Methuen & Co, 1894.

the causses, which it took countless ages to accumulate, and land that once maintained a well-to-do population is reduced to a desert."

"Swept by the cold winds of winter and parched by the heats of summer, only a Caussenard," says Reclus, "can love the causses, but every citizen of the world can admire the gorges of mighty depth that cleave it, and the precipices that form the walls of this gigantic acropolis. In decending by goat paths these bordering precipices, one is suddenly transported from parched wastes to pleasant pastures, from vast horizons, vague in outline and sad in tone, to smiling nooks of blended Heaven and earth . . . The startling contrasts between some of the cañons and their causses form one of the most phenomenal beauties of beautiful France."

But the pleasant pastures are generally very small, and the smiling nooks are often perched on steep slopes or on a narrow ledge above the rushing river. One or two of these gorges are indeed wide enough to admit of river, road, and rail; but most of them are in places so narrow that the river washes precipitous cliffs on each side.

It is these deep ravines or cañons, with their vertical walls, rising sometimes in a single sheer, continuous cliff, sometimes in a series of steps, with mural scarps and faces, that are the distinctive feature of the causses country. The tableland is divided by them into a number of isolated masses, and it is impossible to pass from one causse to another without descending into the depths of the cañon, and climbing again up the opposite cliff; there is no way round, for the river enters the cañon between limestone walls, and completely traverses the limestone region.

The average depth of these chasms is from 1,300 to 1,500 feet, and their width at the bottom varies from 160 to 1,500 feet. Their rocky walls are carved by rain and frost into an infinite variety of buttresses and turrets, alcoves and recesses, which recall the fantastic conceptions of Gustave Doré. Still, there is nothing monotonous or sombre about the scenery of the cañons, for though in places their precipitous sides close in till there is only just room for the river to pass, yet soon they widen out again and give space for fields and vineyards and orchards.

The finest of the cañons is that of the Tarn. "The whole of its course," writes Mr. Baring-Gould, "from Ispagnac to Roziers, a distance of thirty miles, is one succession of marvels. At every turn comes a surprise. The forms of the rocks are not alone singular and beautiful, the colouring is rich as it is surprising. The dolomitic limestone, which rises in nakedness to the height of 600 feet and even 1,800 feet on each side of the water is tinged and splashed with colour. It is fawn or salmon colour, with patches of red ochre, here stained black, there it gleams white. Everywhere it is sprinkled with the green of the box and the juniper clinging to the interstices. Overhead gleams down the azure sky and below flashes the foaming river." (*Op. cit.*, p. 80.)

Readers who wish for further particulars about this curious country will find excellent descriptions in the readable book by Mr. Baring-Gould; and so far as its physical features are concerned, in E. A. Martel's marvellously cheap and beautifully illustrated volume on the Cévennes.[2] My present intention is to discuss a point on which both these writers seem to be behind the knowledge of the day—namely, the manner in which these wonderful ravines have been formed.

Mr. Martel argues, and Mr. Gould takes it for granted, that the cañons were originally subterranean watercourses, and that the caverns formed by these underground waters became open valleys by the falling in of their roofs. This idea has commended itself to other travellers, and is even shared by some who have a considerable knowledge of geological causes. It is a possible explanation, and looking to the cañons alone, without considering the valleys beyond and below them, it might seem a reasonable way of accounting for their existence; but it is not the way in which other gorges and cañons have been formed, and there is no reason that I can learn why these should have been formed in so exceptional a manner.

A few decades ago it was generally supposed that such deep ravines were simply cracks in the earth's crust, and travellers described them as resulting from some "great convulsion of nature," laying stress on the fact that every concavity on one side was opposed by a convexity or promontory on the other, and taking it for granted that the two walls had originally fitted into one another and had simply gaped apart. Now we believe that a river is not there because of the valley, but the valley is there because of the river, which has gradually cut and carved its way through the solid land, and has thus led to the formation of the valley.

Let us grant, therefore, that running water has made the cañons of Lozère; but did the water begin its work below, or did it cut its way down from the surface? That is the question at issue. Limestone formations are always full of fissures, caves, and caverns which have evidently been occupied and enlarged by subterranean streams. If the roof of such a cavern fell in, would it not present the features of a cañon? It is quite possible that it would, in many respects; but it is exceedingly unlikely that the roof of a deep-seated cavern should fall in completely for any great distance.

The cañons are in some places over 1,500 feet deep, and that of the Tarn is thirty miles long; and one would not expect the roof of a cavern of such a length and such a depth to fall in along the whole of its course. Again, the floor of the cañon is a continuous slope, while most of the caverns have very irregular floors, here descending with a vertical fall, there blocked by a barrier which forms a lake. The roof is as irregular as the floor, being sometimes low and some-

[2] "Les Cévennes," par E. A. Martel. Paris, 5th edition, 1894.

times hollowed into lofty domes. It would, therefore, be very unlikely for the roof of a cavern to fall in all at once.

But supposing it fell in piece by piece, how is it that the roofs of those which are now cañons should have disappeared so completely, while the roofs of the many other caverns which do exist in the limestone have hardly fallen in at all? It is true there are swallow-holes and *avens*, which are holes in the roofs of caverns lying 200 or 300 feet below the surface; but if this was the way in which open cañons began to be formed, one would expect to find them in all stages of formation—some lengths fallen in and some still roofed over—so that natural tunnels and arches should be frequent along some of the cañons. But this is not the case; no such tunnel or arch occurs in the course of any one of the rivers. The cañons are everywhere clean-cut, open ravines.

Again, the main rivers do not take their rise within the causses district; it is true that one or two streams spring out of caverns within that district, and that all are largely fed by such springs, but the three chief rivers, the Tarn, the Joute, and the Doubies, rise outside the limestone area and cut right through it, separating one causse or plateau from another. The cañons of these streams are, therefore, merely the continuations of the valleys which they have made for themselves in the gneiss and granite.

Finally, there is no necessity for imagining the previous existence of a cavern. The streams running off the older rocks would be quite capable of excavating these gorges by ordinary mechanical means if they were first set running over the surface of the limestone plain from a watershed at a higher level. That they did once run over this surface and carved out the channel which has become a cañon, few geologists would doubt. The causses most certainly did not always exist as an isolated limestone district; they are part of a great formation which extended over the whole of southern France, from the Bay of Biscay to the Maritime Alps, stretching completely over the heights of the Cévennes, and spreading northward for some distance up the slopes of the Central Highlands. Much of the limestone formation may have been removed before the rivers came into existence, but when the rivers began to cut their channels, the surface of the limestone plateau doubtless extended northward and eastward till it met the rising plane of the older rocks which form the Cévennes and the Central Highlands. At the present time, the highest parts of the causses hardly reach 4,000 feet above the sea, while the Cévennes rise in places to over 5,000 feet. Consequently, if the cañons did not exist, and if the space between the eastern escarpment of the causses and the peaks of the Cévennes were bridged, or rather filled up, with limestone, the watersheds would remain where they now are, and the streams would run over the surface of the limestone plateau.

So also with the rivers Lot, Dordogne, and Vesère, which traverse the causses of Guyenne. They rise in the highlands of Corrèze and

Cantal, and there was a time when they meandered over the surface of the limestone which is now so deeply trenched by their valleys.

Many people admit that ordinary valleys have been excavated by rain and rivers, but when they see a river passing between lofty walls of solid rock, they ask how liquid water can saw out such a passage. The geologist replies that a river in flood is not only water, it is full of sand and mud, and, moreover, it sweeps stones along the bottom of its channel. It is the silt, sand, and stones that erode; the stream is only the motive power which works and guides the machinery.

Still, it is objected that few rivers, either in England or on the Continent, seem to be deepening their channels at the present time, even when they are in flood. This is perfectly true. A river above the influence of the tides cannot sink its channel below the level of the sea, and, when it has cut its valley down to a certain slope, it ceases to have any power of deepening *so long as the land remains stationary.* If, however, the land is raised, the erosive power of the rivers is renewed. Upheaval increases the height through which the water has to fall before it reaches the sea, and enables the rivers to cut their way deeper into the land. Subsidence lessens the relative height of the land, and not only stops the deepening process, but causes the streams to deposit the materials they carry.

It is because geologists for a long time failed to perceive that the erosive power of a river is controlled and limited by the movements of the land that certain sceptics have refused to believe that valleys have been made by rivers alone. The anonymous author of a book entitled "Scepticism in Geology," referring to the rocky barrier known as the Iron Gate just below the great gorge of the Danube, remarks that 'geologists have failed to explain to us how water-erosion, having (as they assert) cut through cliffs 2,000 feet high, should have stopped short at this petty barrier-reef." The answer, I should think, is that the river had reached its base level of erosion when it had cut down through the 2,000 feet of rock, and it will never remove the Iron Gate unless that part of Europe is once more raised to a higher level above the sea than that at which it now stands. The rivers of Lozère are not now deepening their channels, because their work is done; but their present incapacity for erosion is no proof of their having been incapable of making the cañons during a period of upheaval.

Those who imagine that the gorges which trench these limestone districts have been formed by the enlargement of fissures or by the falling in of caverns, forget that they are only part of a river-channel and that this river-channel is part of a great system of drainage. They forget, too, that in other countries similar ravines have been cut through rocks in which caverns do not exist.

The fact that the main rivers of the causses have their ultimate sources beyond and outside the limits of the causses country is alone sufficient to point out the manner in which the cañons have been formed. This has not escaped the notice of some of the French geologists, and I may conclude with an apt quotation from a recent French work on Earth-sculpture[3], the authors of which write as follows (p. 170) :—" The deep valleys of the causses would not have existed if their drainage basins had been limited to the permeable limestone areas. The rain-water would have sunk into the ground instead of forming rivers large enough to excavate such deep ravines. On the contrary, the watercourses formed on the impervious rocks . . . entered the permeable region with a sufficient volume to erode their channels . . . The rocks in the upper reaches of the valleys were crumbled and washed down by the action of the rain ; while the limestones which crown the causses, thanks to their permeability, escaped this detritive action. This is sufficient to explain the aspect of the region round Florac, where the Tarn and its affluents seem to penetrate the thickness of the plateau as if they passed through a wall" ; meaning, of course, that the outer wall of the plateau has been developed *pari passu* with the excavation of the valleys, and was not there when the streams commenced their work.

For the inception of this work we must look back to early Tertiary times (Eocene or Oligocene), when the country was being elevated after the great Cretaceous submergence and when the Cretaceous and Jurassic limestones formed an extensive plain sloping gently away from the central highlands of France. It was then that the drainage system of the central provinces was marked out ; the decomposition of the granites, gneisses, and lavas of Auvergne and Limousin supplied plenty of grit for erosive purposes, and the rivers, having once established their channels, slowly sawed their way down through the layers of limestone. The upward movement of the land, whether this was continuous or episodic, kept the machinery in action and enabled the rivers to eat deeper and deeper into the tracts which subsequently became, by isolation, the elevated plateaux or causses of modern times.

Such I conceive to be the history of the formation of these remarkable valleys, and all the other physical features of the region are really dependent on the gradual formation of these deep ravines. The caverns which open at various levels in their walls register stages in the process of erosion, for the highest caves mark the time when the water issued at that level ; but as the cañons were cut deeper and deeper the subterranean waters were drawn off at lower and lower levels, either deserting their former tunnels or deepening them in places into underground cañons. Mr. Martel has explored many of these gloomy corridors and found them open here and there into lofty halls curtained by a profusion of stalactitic ornaments, like the grottoes of Adelsberg and other well-known subterranean palaces.[4]

A. J. Jukes-Browne.

[3] " Les Formes du Terrain," par MM. de la Nóe et de Margerie. Paris, 1888.

[4] See " Les Abîmes," reviewed in Natural Science, vol. vi., p. 131. Feb., 1895.

III.

Individual Variations.

IN a Note with the heading "Variation and Probability" in NATURAL SCIENCE (vol. vi., p. 217) it is said:—"There are two classes of variation," viz., "large or sports," and "numerous small variations." . . . "Have species come from the large or from the small variations?" I am inclined to answer—rarely from either or chiefly from neither. Mr. Wallace expresses himself somewhat differently, for he says[1]:—"In securing the development of new forms in adaptation to the new environment, Natural Selection is supreme. Hence arises the real distinction, though we may not always be able to distinguish them, between specific and non-specific or developmental characters. The former are those definite, though slight, modifications through which each new species actually became adapted to its changed environment. They are, therefore, in their very nature useful. The latter are due to the laws which determine the growth and development of the organism, and, therefore, rarely coincide exactly with the limits of a species."

Mr. Wallace here leaves "sports" out of consideration altogether, but separates the "definite," *i.e.*, the adaptive characters or variations, out of the numerous "indefinite," ill-adapted, or injurious variations (which he also omits to mention), which, according to Darwinism, Natural Selection has eliminated.

So that we have the following groups of variations:—

1. "Large and striking variations or sports" (writer in NATURAL SCIENCE).

2. "Numerous small" (writer in NATURAL SCIENCE), *i.e.*, the same as the "developmental or non-specific characters" (Wallace). These I will call "Individual variations."

3. "Adaptive or definite characters" of true varieties and species (Wallace and myself).

4. Ill-adapted, or useless, or injurious, or "indefinite" variations, eliminated by Natural Selection (Darwinism).

As far as plants are concerned I would suggest instead the three following groups:—

1. Sports, occurring mainly under cultivation, and probably rarely supplying the source of varieties and species in nature.

[1] *Fortnightly Review*, March, 1895, p. 444.

2. Innumerable, slight, individual variations—the "developmental characters" of Wallace, because they are due to the ever varying growth and development of the plant. They are usually transient, *i.e.*, not hereditary and "non-specific" (Wallace), though they may become true varietal or specific characters under circumstances different from their normal conditions. Thus, *e.g.*, probably no two leaves of *Ranunculus Ficaria* are absolutely alike in this country, but the variations are insufficient to suggest any varietal differences. In the Mediterranean regions, however, the whole plant bears finer and larger flowers and leaves, so that it is generally recognised as the variety *Calthæfolia*.

3. Self-adapted varietal characters, which do not exist until called into existence by a pronouncedly different environment. These are the main sources of true varietal and specific characters, and give rise to the well marked "facies" of plants growing in deserts, marshes, water, etc.

Variations (as of number 2) in the parts of plants of the same species, when growing in the same place and under the same conditions, are very common. Mr. Burkill, for instance, has lately alluded to variations in floral symmetry, which I called "symmetrical reduction."[2] He describes the numbers of stamens and carpels in the flowers of several plants; thus he says:—"*Ranunculus Ficaria* showed that towards the end of the flowering period both the stamens and carpels become reduced in number without their proportion being changed."[3] He regards temperature as a cause. I have had more than one occasion to allude to this feature of "reduction," as well as of "symmetrical increase."[4] The former is sometimes attributable to a barren soil (tetramerous flowers of *Potentilla Tormentilla*), or to a feeble vitality (early flowering trimerous Fuchsias), or an accidental and locally insufficient supply of nutriment (tetramerous flowers in a corymb of elder, etc., in pentamerous instead of hexamerous flowers of *Lythrum Salicaria*, and in dimerous flowers of iris, orchids, etc.) On the other hand, symmetrical increase is due to hypertrophy; as in a large tetramerous crocus now before me, in heptamerous, central flowers of a glomerule of *Lythrum Salicaria*, and in the polymerous flowers of cultivated auriculas, etc.

Symmetrical increase is, of course, quite a different thing from "doubling"; as this latter is due to the conversion of stamens and carpels into petals, coupled with their multiplication.

I would here observe, that a reduction as well as an increase of the symmetry may be transient, as in all the preceding plants mentioned, except the Tormentil, in which the quaternary flowers have now assumed a truly specific character. Similarly, speaking

[2] Note on the "Causes of Numerical Increase of Parts of Plants," *Journ. Linn. Soc.*, 1877; "Origin of Floral Structures," p. 18, etc., 1888.

[3] *Nature*, Feb. 7, p. 359, 1895.

[4] The reader may be referred to my paper on "Self-Fertilisation" (1877), and that on "Æstivations" (1876) in the *Trans Linn. Soc.*

generally, all plants can become temporarily depauperised by atrophy; but if the poor environment be perpetual, then the variety "*depauperata*" can become relatively "fixed," and is often recognisable as such by systematists.

As another class of variations in individuals, their æstivation may be mentioned. If buds of *Laurustinus*, primroses, wallflowers, etc., be examined, every variety of æstivation can be pretty well met with in each. If æstivations, however, be constant, as in mallow, then they become classificatory characters. Lastly, Mr. Scott Elliot has given interesting statistics of the varying dimensions of leaves due to varying amounts of illumination, etc.[5]

Now, all these comparatively slight individual differences, and very many others that might be mentioned, arise, as it seems to me, because *no two organisms of the same species can grow with a mathematically exact likeness;* so that the laws of growth and development of organic beings cannot be compared with, say, gravitation and the laws regulating the constant angles of crystals.

As long, however, as wild plants are all growing together in the same natural environment, such variations in one and the same species, or in different flowers on one and the same individual plant, do not amount to true varietal or specific differences in the eyes of a systematist. Yet it is just out of these that Natural Selection is supposed to make its choice; but in point of fact it cannot do so.

Though this statement may be opposed to the prevailing creed, I think it is correct and in agreement with Mr. Wallace's statement of such characters being "non-specific." Thus the writer in NATURAL SCIENCE says (p. 218):—"It may be taken for granted that most biologists agree that variations of a nature similar to those now occurring among plants and animals have been the source of the differences between the varieties, species, genera, and so forth, in which existing animals and plants are classified." According to Darwin, such variations supply the materials for Natural Selection; but observations do not seem to me to support this view. The writer proceeds to observe, and very rightly, that "the chief difficulty in [this] view is that it is impossible to see in many cases, perhaps in any case, how small variations could have a value in selection; how, in fact, the gradual increments in a continuous change could each have had a determining value in the selection of the animal or plant."

Let us test it by an example. *Caltha palustris* has itself no recorded variety in low-lying situations; yet Mr. Cockerell[6] has been able to enumerate a great variety in the number of carpels of plants growing in one place, Corfe; and, presumably—as it, at least, is usually the case with *Caltha*—growing more or less crowded over marshy ground. Notwithstanding all its variations, Natural Selection appears to stand aside and to look upon all of the innumerable small

[5] *Journ. Linn. Soc.* xxviii., p. 371, 1891.
[6] *Nature*, March 21, 1895, p. 487.

variations, due to growth and development of each individual specimen, with perfect indifference, year after year, and in every one of the numerous localities where marsh-marigolds grow; and it makes no effort to see if it can select any particular form which might possibly be able to start on a new evolutionary career. For, *no variety has ever arisen in England in low-lying situations.*

When, however, this plant manages to get away from its habitual environment and to reach "mountainous places" (Hooker), it puts on characters which descriptive botanists have independently noticed and variously named as varietal or specific.[7] It is commonly known as *Caltha minor.*

These two kinds, or the highland and lowland forms of *Caltha palustris*, would, therefore, seem to illustrate Mr. Wallace's two terms "definite or adaptive" and "developmental or non-specific" characters respectively; only he has omitted to mention, as I said above, the "indefinite" characters of the majority of individuals, out of which Natural Selection is supposed to have selected the "definite" form known as *Caltha minor.* The fact is, there is no evidence whatever that they ever did exist. *Caltha minor* illustrates Darwin's assertion about the "definite action" of an environment, namely, that under such, "a new sub-variety would be produced without the aid of Natural Selection."[8]

But, let us not forget that what is true for *Caltha* is, of course, true for all other plants. It may stand as a type of the origin of *all* true varieties of plants, or "incipient species," as Darwin calls them. In other words, of the primeval seeds of *Caltha palustris*, which happened to be transferred from lowlands to mountainous places, *all* that survived grew up in direct adaptation to the new alpine environment, and so formed the so-called species *Caltha minor*, "without the aid of Natural Selection." But this change could never have occurred without the physiological self-adaptibility with which living protoplasm is endowed. It is here, therefore, as I take it, that the writer in NATURAL SCIENCE very rightly insists upon the *sine quâ non* of "function" being taken into account when "the question of species and of changes in them are to be dealt with" (p. 219). "The question as to whether such small dimensions [as observed by Professor Weldon] have, or do not have, a selection-value for the animal remains to be answered" (p. 220.) My reply is that under usual conditions of growth and development, in plants at least, they have no selection-value at all.

The Note adds: "Professor Weldon's method concerns itself merely with the fact that in certain cases there is a *relation* between selective destruction and certain specified dimensions." Is the word "destruction" the right word to use? The crabs measured, however abnormal, were all alive and well, and of the same age. I would

[7] See Hooker's "Students' Flora of the British Islands," p. 11.

[8] "Animals and Plants under Domestication," ii., pp. 171, 279.

suppose that they are like plants, in that the varying features of their structure, that is of numerous individual crabs respectively, were simply due to growth and development being never exactly alike in any two, and, therefore, not to be trusted as hereditary. It is not, therefore, that any crabs are "destroyed" in consequence of slight abnormalities or deviations from the average form, but that their variations are not necessarily reproduced in their offspring, which might vary slightly again, either towards the average form or in some other way. Hence, I do not quite agree with the writer in saying: "It is clear from the data that there must be a special death-rate among crabs which have a certain deviation from the normal in the case of the frontal breadth, and that there is no special death-rate among crabs with deviations of the right dentary margin." Without evidence, I would suggest the above interpretation as probably the correct one, as none of the 7,000 specimens appear to have been actually malformed, or more sickly or more likely to die sooner than the rest, or less likely to produce healthy offspring.

To return to the origin of *Caltha minor*. It might, of course, be said that all the "indefinite," ill-adapted variations, which may be supposed to have put in an appearance on the mountains, together with the few adaptive individuals, have all died out ages ago; and this could neither be proved nor disproved to have been the case. But many experiments have shown that if plants, or even half of an individual plant, or their seeds, be taken from lowlands and planted on alpine regions, all those that change their structures, at once begin to assume more or less the same anatomical and morphological characters as the plants normally growing on highland regions. On the other hand, they never produce any "indefinite," much less any "injurious" variations, as has been so completely proved by Messrs. Bonnier, Flahault, and others. We may look, but we shall look in vain for them; for there are no *facts* in support of the existence of innumerable indefinite variations for the benefit of Natural Selection, other than those due to growth and development; but these, as in the case of *Caltha*, supply no sufficient material for Natural Selection; as a rule they are "non-specific," according to Mr. Wallace, and I entirely agree with him.

On the other hand, there is an overwhelming amount of evidence, both from nature and experiment, that self-adaptation to a new environment is the true and only method of evolution, without any aid from Natural Selection at all.

The point I would, therefore, emphasise is, that Professor Weldon's statistics among crabs (and cows, horses, pigs, dogs, and human beings themselves, would readily supply analogous ones), as well as similar data among plants, prove nothing of themselves, beyond the patent fact that they are all due to the irregularities of nutrition, and are, consequently, the "inexact" results of the laws of growth and development of organic beings. Whenever, however, any one or more of these numerous slight variations become extra stimulated by a new environment, *then*, they may become varietal, provided the stimulus be prolonged sufficiently to "fix" the character. Thus *Ranunculus Ficaria* has become as stated, the var. *Calthæfolia* in Malta, etc.; while *Caltha palustris* has become *C. minor* on mountains, and has (it is believed) developed the variety *plena* under cultivation.

One more quotation from the writer in Natural Science:—"Although Professor Weldon did not say so, it must have occurred to many listeners that this first result of statistical inquiry on variation was in direct contradiction to those who assert that variation is not a matter of 'chance,' but that it occurs in determined directions (p. 218)." As far as the ordinary, but innumerable, slight variations, due to growth and development, are concerned, they may very well be attributed to chance; for they are the results of accidental differences of nutrition, light, etc., and are, as Mr. Wallace rightly calls them, "non-specific." But I, for one, do maintain that *true* varietal and specific variations *do* occur in determined—but certainly not *pre*-determined—directions; because they do not arise until some external and sufficient cause occur to determine the special direction in which any particular organ, or organs, of the organism shall grow and develope—as I have shown in the example of *Caltha palustris* and *C. minor*. *Ex uno disce omnes.*

George Henslow.

IV.

The After-Shocks of Earthquakes.

NO severe earthquake ever occurs without numerous attendants of a slighter character, a few of them preceding, but by far the greater number following it, and continuing to be felt for many months afterwards. Their unusual number, their immediate succession, and their gradual decline in frequency with the lapse of time, as well as their geographical distribution, point unmistakably to the existence of an intimate connexion between the earthquake and its after-shocks.

From whatever point of view we look at earthquakes, but especially if we regard them as the results of fault-slipping, the study of after-shocks is of great importance. At the same time, it is a study frequently encompassed with serious difficulties. In the presence of a terrible disaster, slight shocks are only too likely to be neglected. Their number is often so great that even a cool-headed observer may give up in despair the attempt to chronicle them. And, again, supposing his record to be nearly complete, the interval between successive shocks may be so small that their identification with those noted by other observers may be nearly, if not quite, impossible. Thus, the distribution of after-shocks, with regard to space, is a subject on which we must at present be content to remain in partial ignorance, except in those cases where the shocks are few, or where seismographs are abundant, and escape injury during the chief disturbance.

The distribution of after-shocks, with regard to time, is a subject which lies more within the range of investigation. It has recently been discussed with great ability by Mr. F. Omori, of the Seismological Institute at Tokio (3, 4). Within the last six years there have been three earthquakes of unusual severity in Japan: those of Kumamoto, on July 28, 1889, Mino-Owari, on October 28, 1891, and Kagoshima, on September 7, 1893; and, fortunately, in each case there has been an observatory provided with a seismograph in the immediate neighbourhood of the epicentre.[1] Except within a few hours of the earthquakes, the records of these instruments may be

[1] The "epicentre" is the area on the earth's surface vertically above the seismic focus or centre of disturbance.

regarded as complete, and Mr. Omori has subjected them to a careful analysis, of which the principal results are summarised in this paper.

One of the most interesting sections in Mr. Omori's memoir is that on the periodicity of the fluctuations in the number of after-shocks, but as this does not seem to have any necessary connexion with the present subject, I have, in order to save room, omitted all reference to it here.

As the results, to a certain extent, are the same in each case, it will be sufficient to describe a single earthquake in some detail, with a brief reference to the others. That on which our knowledge is most complete is the Mino-Owari earthquake, generally known in this country as the great Japanese earthquake of 1891.

Fig. 1. AREA AFFECTED BY THE EARTHQUAKE OF 1891.

This figure is reduced from a map appended by Mr. Omori to his paper on the isoseismal lines of the great Mino-Owari earthquake of 1891. I regret that I am unable to give the proper reference. But the omission is probably of little moment, for the paper is written entirely in Japanese.

The Mino-Owari Earthquake of 1891.—This great earthquake, one of the most disastrous within historic times, occurred on October 28, at 6.37 a.m. (mean time of 135 degrees E.). The area disturbed by it, including both land and sea, was not less than 320,000 square miles. Its boundary and the principal isoseismal lines[2] are shown in Fig. 1. In the region enclosed by the first isoseismal, the destruction of buildings was nearly complete; in the area of the second isoseismal,

[2] "Isoseismal lines" are lines of equal intensity of the earthquake force.

houses, walls, bridges, etc., were partially damaged; in that of the third, walls were cracked, clocks were stopped, and crockery fell off shelves; while between the third and fourth isoseismals the shock was distinctly felt. The first of these areas, which includes the provinces of Mino and Owari, and portions of those adjoining, is mapped on a large scale in Fig. 2. The continuous lines marked 1

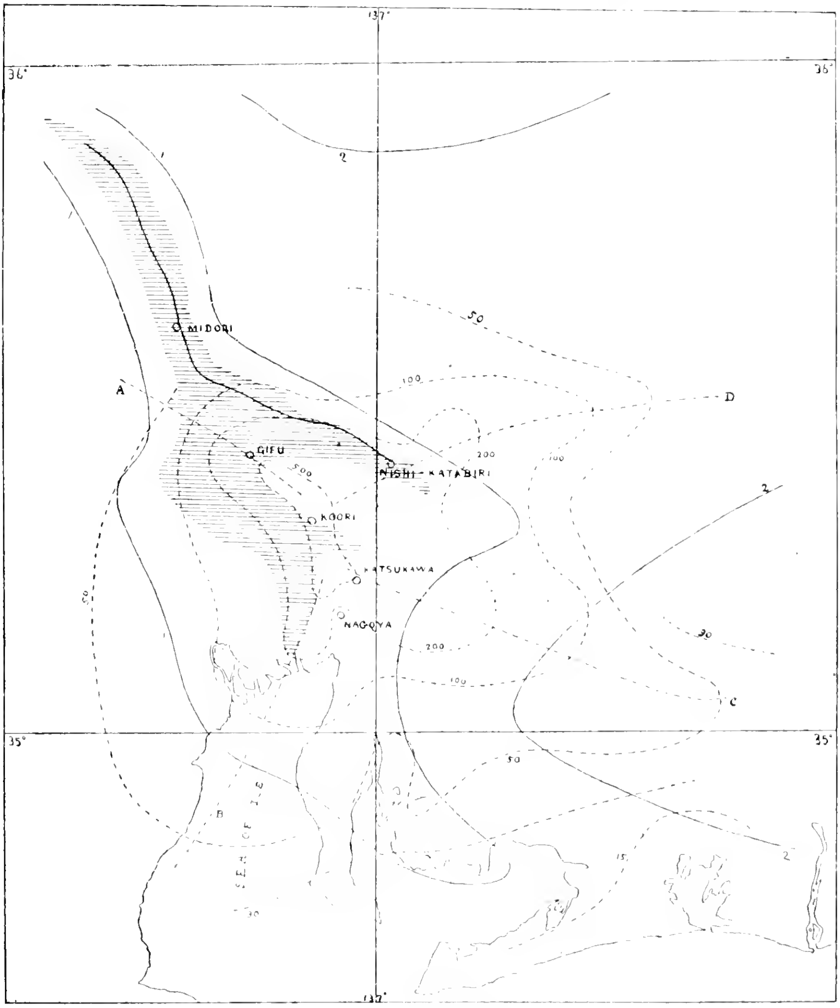


FIG. 2. AREA OF FIRST ISOSEISMAL OF MINO-OWARI EARTHQUAKE.

and 2 are isoseismal lines, along which the maximum accelerations were respectively 2,000 and 800 mm. per second. The shaded area is that which was shaken most strongly. The dotted lines will be referred to in a later section (*see* p. 397).

Within the shaded area will be noticed a somewhat sinuous line. This marks part of the course of the great fault-scarp, which has been admirably studied and described by Professor Koto. He succeeded in tracing it from Nishi-katabiri to Haku-san, a distance of 40 miles, and there is good reason for believing that it extends as far as Fukui, *i.e.*, for a total length of 70 miles. Proceeding in a general south-east and north-west direction, it cuts through hills and plains alike. With one exception, the ground on the north-east side is relatively depressed, and it is also constantly shifted by as much as 5 or 6 feet towards the north-west. Where the vertical displacement is small, a mark was left like the track of a ploughshare or of a gigantic mole. In other places, where the fault crosses flat ground, it has the appearance from a distance of a railway embankment. This is the case at Midori, in the Neo Valley, where the bed of the valley is split longitudinally, and an abrupt step formed, from 18 to 20 feet in height. When we think of the remarkable length of this fault, the magnitude of its throw, and the friction that must have resulted from the rapid displacement of such enormous masses of rock, we can entertain but little doubt as to the correctness of Professor Koto's conclusion that the formation of the fault-scarp was the cause of the great earthquake (1).

Distribution of After-Shocks in Time.—Gifu, the capital of Mino, lies about 4½ miles from the nearest point of the fault-scarp. The seismograph at the meteorological observatory there recorded the first half-dozen vibrations, and then the building fell and the instrument was buried. At 2 p.m., *i.e.*, about seven and a half hours after the earthquake, it was again in working order, and the registration of the after-shocks commenced. From this time until the end of December, 1893, or within a little more than two years, the total number of after-shocks recorded is 3,365. Of these, ten are described as violent, ninety-seven strong, 1,808 weak, and 1,041 feeble; the remaining 409 being merely sounds unaccompanied by any perceptible motion.

The relation between the number of after-shocks and the time since the great earthquake is shown graphically in Fig. 3. The dots denote by their distance from the horizontal line the total number of shocks recorded during each successive month from November, 1891, to December, 1893. The continuous curve, which passes through these dots, exhibits very clearly the rapid decline of frequency during the first two months and the much slower rate of decrease at the end of five or six months. For instance, in November and December, 1891, there were 1,087 and 416 after-shocks respectively; while in December, 1892, there were only thirty-nine, and in the December following not more than sixteen. At the same time, this decline is not continuous, there being several maxima and minima, though not of great importance. Smoothing away these irregularities, we get the dotted curve in the figure, and this, it will be seen, approximates very closely to the form of a rectangular hyperbola. If this curve represents the true law of decline in frequency, it follows that the monthly number of shocks must go on diminishing, until, after a lapse of about forty years, the ground will have so far recovered its former stability that there will be no more than one slight shock a month. By this time the total number of after-shocks recorded at Gifu will probably not fall short of four thousand. Before 1891, the average yearly number of shocks at the same place was about fifteen.

Another point of some importance is the connexion between the intensity of the shocks and their time of occurrence. Of the ten violent shocks recorded at Gifu, nine occurred within the first four months, and the last in September, 1892. All of the strong shocks took place within the first thirteen months, and all the weak ones but

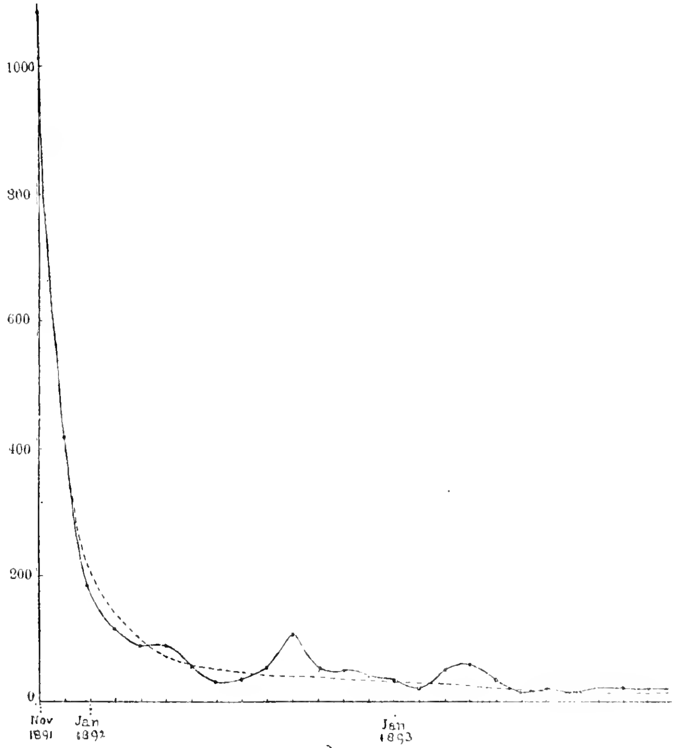


FIG. 3. CURVE SHOWING DIMINUTION OF AFTER-SHOCKS IN TIME.

four within the first twenty months. During the few days immediately succeeding the earthquake, feeble shocks and sounds were comparatively rare, and it was only after the lapse of several weeks that they came into prominence. Towards the close of the year 1893, with the four exceptions just alluded to, feeble shocks and sounds alone were recorded.

The remaining earthquakes discussed by Mr. Omori were less intense, and the number of after-shocks was, in each case, much smaller. They yield, however, somewhat similar results.

The Kumamoto earthquake occurred on July 28, 1889, at

11.49 p.m., and disturbed an area of more than 38,000 square miles. Up to the end of 1893, 922 after-shocks were recorded at Kumamoto. One of these, five days after the principal shock, was violent, seventy-six were strong, and 845 weak or feeble shocks, or else merely sounds. The curve representing the decline in frequency is somewhat similar in form to that of the Mino-Owari earthquake, and shows that, after a total lapse of about seven or eight years, the annual number of shocks will once more have attained the value which it had before the earthquake of 1889, namely, three or four a year. Thus, the disturbed condition of the earth's crust in this district is now approaching its end, and the complete number of after-shocks will probably be about 950 or 1,000.

The Kagoshima earthquake, which took place on September 7, 1893, at 2.46 a.m., disturbed an area of about 30,000 square miles. The record of after-shocks at Chiran, in the epicentral district, began at about 9 p.m. In the interval, there were probably about a hundred shocks, so that by the end of January, 1894, the total number must have been nearly 480. The annual number will probably return to its original value after a lapse of three or four years, by which time some six hundred shocks may have been felt.

The number of earthquakes discussed is too small to draw any general conclusions; but so far it would appear that the more intense the original earthquake the greater is the number of its after-shocks.

Distribution of After-Shock Frequency in Space.—When many earthquakes occur in the same district, it does not follow that they all proceed from one centre of disturbance. Probably, a continual displacement of the centre, resulting from a relief and transference of stress at each successive shock, is the more general rule. To make our history of an earthquake-series approach completeness, we ought, therefore, to know the disturbed area and epicentre of every shock, and this would enable us to trace the laws according to which the disturbed portion of the earth's crust gradually settles down into equilibrium again. But, as already remarked, this is in many cases a task of peculiar difficulty, and, with the Mino-Owari earthquake, one that it was impossible to perform. The large number of after-shocks in this case, however, renders another method applicable, for the suggestion of which we are, I believe, indebted to Mr. Omori. This is the distribution of after-shock frequency with regard to space.

Besides the seismographic records from Gifu and Nagoya, earthquake reports were sent in from district offices and other stations in the three provinces of Mino, Owari, and Mikawa. These give, with a general approach to accuracy, the monthly numbers of after-shocks from November, 1891, to February, 1894. Plotting such numbers on the seismic map, curves may be drawn through places where equal numbers of after-shocks occurred during stated intervals. Mr. Omori gives three such maps—one for 1892, another for 1893, and the third for January, 1894. In Fig. 2 the curves on the map for 1892 are reproduced as dotted lines.

It will be obvious from a glance at these curves that the after-shocks were most frequent, not in the Neo Valley, where the great fault-scarp was formed, but in a district to the south-east of it, in the Mino-Owari plain. For some time after the great earthquake the most active centre of after-shocks was situated close to Gifu, probably just to the west of the town. During 1892, as is shown in Fig. 2, there were four axial lines (A–D) radiating from the neighbourhood of Koori, along which after-shocks were more numerous than in other parts of the district.

In succeeding years the distribution of seismic activity underwent some changes. The curves of equal frequency during 1893 show a diminution along the axial line marked A, while the prominence of shocks along the axial line B almost entirely disappears. The other two lines, however, remain still distinct, and, during January, 1894, are the only representatives of the original number. This is due to the fact that, on the 10th of this month, a severe earthquake occurred, whose origin was situated at the point where these two axial lines intersect. Two other severe earthquakes on January 3 and September 7, 1892, also originated in the Mino-Owari plain, one close to Gifu and the other near Katsukawa. All three, therefore, had their centres close to the axial line A C running W.N.W. and E.S.E.

The interpretation of these facts seems to be, as Mr. Omori suggests, that a prominent fracture or series of fractures exists beneath the Neo Valley, and that by the great earthquake the stress in this district was almost completely removed. The axial lines radiating from Koori probably indicate the existence of four slighter or deeper fractures, along which the crust is still gradually settling down into equilibrium, that condition not being yet attained. The centre of after-shocks, Mr. Omori concludes, may still further change its position; but he considers it more probable that it will recede from, rather than approach, the Neo Valley.

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4. ————.—"On the After-shocks of Earthquakes." *Journ. Coll. Sci. Imp. Univ. Japan*, vol. vii., pt. ii., 1894, pp. 111–200.

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V.

Eozoön and the Monte Somna Blocks.

IN consequence of the Note in our February number, entitled "Eozoön: Requiescat in Pace," as well as our remarks on the letter written to us by Sir William Dawson and printed on p. 288, we have received several more letters from Sir William, strongly protesting against the re-burial of *Eozoön*. He has also enclosed a criticism on what he calls "the strange mis-statements" contained in the paper on "Eozoönal Structure of the Ejected Blocks of Monte Somna," by Drs. Johnston-Lavis and Gregory (*Trans. R. Dublin Soc.*, vol. v., ser. 2). In consequence of the great interest that has been aroused by this paper, we print Sir William's criticism in full, together with a short reply by Drs. Johnston-Lavis and Gregory.

I DESIRE to make a few remarks on the paper by Dr. Johnston-Lavis and Dr. J. W. Gregory, not for the purpose of entering into the general questions relating to the organic structure of *Eozoön*, but to indicate certain reasons for the belief that the appearances described in the paper have no relation to *Eozoön canadense*, either in mode of occurrence and mineral character or in microscopic structure, and consequently that, whatever the interest and value of the paper in other connections, the references to the Laurentian fossil are wholly gratuitous and unnecessary, while they are also in very important respects incorrect.

In making these criticisms I shall, for the sake of simplicity, refer only to those typical specimens of *Eozoön* in which the laminæ remain as calcite, while the chambers are filled with serpentine or more rarely with malacolite, and the canals and tubuli with serpentine or dolomite.

1. *Mode of Occurrence and Mineral Character.*—The paper states in three distinct places that the typical *Eozoön* is enclosed in a pyroxenic igneous rock, as follows:—

Page 260. "The typical Eozoönal nodules occur enclosed in a rock of which a white pyroxene is the leading constituent."

Page 275. "Concentric Eozoönal masses are included in a coarse-grained rock which is composed of white pyroxene, and which, therefore, may safely be regarded as igneous in origin."

Page 277. "The Cote St. Pierre spheroids (of *Eozoön*) are probably cases of blocks included in either a volcanic or plutonic mass."

By implication this mode of occurrence is also assumed throughout the paper. The statement is, however, entirely without foundation, and I am at a loss to understand whence it has been derived. It certainly could not have been from any competent observer who has studied *Eozoön* in situ. In point of fact the best specimens have all been found in a thick limestone, the Grenville Limestone of Sir William Logan, estimated by him at 750 feet in its average thickness, though with a few intercalated thin bands of gneiss and quartzite.[1] In the vicinity of Cote St. Pierre in the Seigniory of Petite Nation, where some of the best specimens of *Eozoön* are found, the outcrop of this limestone has been traced continuously and mapped by the Geological Survey for twenty-five miles, and in the same district it occurs over an extent of more than one hundred miles on the reverse sides of synclinal and anticlinal folds, where it may be recognised by its character and associations as well as by its holding *Eozoön*.[2] It is true that grains, nodules, and thin interrupted bands of a white variety of pyroxene (malacolite) occur sparingly in this limestone; but neither in their chemical composition nor in their mode of occurrence have we any proof or even probability of an igneous (intrusive) origin. This was the matured conclusion of the late Dr. Sterry Hunt; and Dr. F. D. Adams, at present our best authority on these rocks, is of the same opinion.

The Grenville limestone has been much bent and folded, and with its accompanying beds has been subjected to regional metamorphism. In the Petite Nation localities, however, it has not, as far as known, been invaded by igneous dykes or masses.

The specimens of *Eozoön* are included in this limestone, and vary from single individuals ranging from an inch to six inches in diameter to aggregated groups of a foot or more; and microscopic examination shows that, in some of the beds in which they occur, there are innumerable fragments showing the same structures scattered on the strata planes, and associated with the minute globular chamberlets which I have named *Archæospherinæ*. The specimens of *Eozoön* may be seen weathered out on the surfaces of the limestone exactly in the manner of stromatopores on the surfaces of the calcareous rocks of the Cambrian, Ordovician and Silurian, though I have not seen them so crowded together as in some of the beds of these later formations.

From the statements quoted from Mr. Blake (p. 274) and some remarks on p. 275, I would infer that the authors possibly contemplate *Eozoön* in an inverted position, with the acervuline part within or

[1] "Geology of Canada," p. 45. See also "Life's Dawn on Earth," and Memoir on "Specimens of Eozoön Canadense." By the Author. Montreal, 1888.

[2] It must be evident that the singular opinion quoted on p. 277, that even these great continuous Laurentian limestones may themselves have been "inclusions in intrusive igneous rocks," is too monstrous to be entertained for a moment, especially in opposition to the judgment of all the ablest geologists who have studied them.

below, and the thicker and more regular laminæ above or without. I need scarcely say that this would be in direct opposition to the descriptions of Dr. Carpenter and myself, based on the study of a great number of specimens in situ. I observe, however, that Zittel (Palæontologie, fig. 47) places *Eozoön* in this inverted position, which may have led to the error in question.

It is proper perhaps to repeat in this connection that, in certain layers of the Grenville limestone, grains and concretions of serpentine and malacolite occur without *Eozoön*, and specimens of *Eozoön* with only so much of such minerals as may be contained in their chambers. There are also instances in which specimens of *Eozoön* occur attached to or partially imbedded in such nodules, just as sponges and other organisms occur associated with flints in chalk, or as stromatopores occur in connection with concretions of chert in Palæozoic limestones. As to the origin of the concretions themselves, from whatever source their materials may have been originally derived, their isolation in the limestone and the manner in which they occur in connection with surfaces of deposition, render it certain that their introduction as well as that of the individuals of *Eozoön* was contemporaneous with the formation of the limestone.

Quite recently Kemp and Smyth, two able U.S. geologists, have published (*Bull. Geol. Soc. Amer.*, March, 1895), Observations on the Laurentian Limestones of the Adirondack Region of New York, in which they fully recognise their extended and sedimentary character, though in that mountainous region they are even more altered and disturbed, and also more invaded by igneous masses than in the Canadian area. They do not seem to have met with any "contact-banding" comparable to *Eozoön*, though from microscopic examination I know that detached fragments of *Eozoön* exist in some of the limestones of the Adirondack region.

The above plain statements of facts are, I think, sufficient to show that the specimens of *Eozoön* found in the Laurentian limestones of Canada in no respect resemble in their associations and mode of occurrence the banded forms from Mt. Somna described in the paper in question. Additional details may be found in my Memoir on the "Specimens of Eozoön in the Peter Redpath Museum," and in other publications referred to in that work.[3]

2. *Form and Structure.*—In so far as can be judged from the plates, some of the forms described in the paper lie in bands parallel to igneous veins, like the laminated borders or selvages common in every part of the world, and of which we have good instances in the trappean dykes of the Montreal mountain; or they appear as rounded masses in the manner of nodules or geodes. Thus, in so far as mere lamination or banding is concerned, they have a certain resemblance to *Eozoön;* but, on closer inspection, essential differences may be

[3] Montreal, 1888, pp. 106. See also "Salient Points in the Science of the Earth" by the same author, 1893.

observed which I have no doubt would be farther accentuated by comparison of actual specimens.

I have already referred to the apparent inversion of acervuline and laminated structures; but there is a more important difference than this, at least, in several of the figures. The Vesuvian specimens, as shown therein, consist of continuous laminæ of crystalline igneous matter, including interrupted or lenticular layers of calcite. *Eozoön*, on the contrary, when well preserved, consists of a continuous skeleton of calcite composed of broad layers slightly pitted on their surfaces, and connected at intervals; while the silicious material appears as a substance filling wide, flattened, mammillated chambers, more or less limited, and presenting amœboid lobes at their extreme edges, and passing finally, in the upper part, into rounded chamberlets. This difference should commend itself to any palæontologist; but I am aware that it may be overlooked by cursory observers. Scores of specimens have been sent to me of banded rocks, supposed by their finders to resemble *Eozoön*, though, in arrangement of parts, the converse of it.

The authors of the paper seem to have peculiar ideas respecting even the general form of *Eozoön*. I have repeatedly shown, and have illustrated this by photographs, that when we find perfect detached individuals of *Eozoön*, these are usually of inverted conical form, springing from a narrow base and widening upward in the manner of some sponges and corals. When close together they often become confluent, and when these confluent masses or layers appear to be hollow or doubled, I believe that this usually results from the folding of the containing bed. This can, indeed, often be seen to be the case, and the laminæ may be observed to be bent and crushed at the flexures.

In the specimens figured in the paper, the characteristic microscopic structures of *Eozoön* are entirely absent. There is no trace of the beautiful and complicated system of canals, and the fibrous structures compared with the minute tubulation are merely prismatic fibrous crystals like the secondary veins of chrysotile which sometimes cross and deteriorate our specimens of *Eozoön*. With reference to these chrysotile veins, while their filling of minute and often transverse and branching cracks show that they are merely aqueous deposits of later origin than the structures which they traverse, and while their appearance under high powers is very different from that of the tubuli of the calcite layers, they have no doubt been when parallel to the layers, and in poor specimens, fertile causes of error. Fortunately, however, they are absent from the more perfect specimens. I may also explain that while the finely tubulated margin of the calcareous layers can be seen to terminate abruptly against the filling of the chambers, it passes gradually in the interior of the layer into the larger canals when these are present. Naturally also, the finely tubulated wall often fails to show its structure, just as anyone who has examined large series of sections of nummulites may observe in these fossils: and the tubuli are often filled with dolomite or calcite very difficult to distinguish from the substance of the calcareous lamina.

The authors of the paper refer (p. 272) to the late Dr. Carpenter as having failed to distinguish the chrysotile veins from the proper wall; but when in England, not long before his lamented decease, I had the pleasure of going over his collection of slices with him, and of ascertaining that he quite understood the distinction between the veins of asbestiform serpentine and the organic structures. At that time he hoped to have prepared an exhaustive memoir on the subject, including my material as well as his own. Had he been permitted to fulfil this intention, many subsequent mistakes might have been avoided. Even his collections, though varied and admirable, are, in a case of this kind, of comparatively little value without their skilled interpreter, so full of varied knowledge.

The writers of the paper do not seem to notice that in the St. Pierre specimens the fine canals and tubuli are often filled with transparent dolomite, difficult to perceive without very good preparations and properly managed light, but of extreme beauty when these conditions are fulfilled. In roughly prepared specimens, indeed, and without careful attention to illumination, these delicate structures are often quite invisible. I have sections properly prepared which show the finest and most complicated tubulation in a manner equal to anything I have seen in any fossil foraminifera from more recent formations; while other slices cut from the same specimen, but possibly slightly heated or subjected to mechanical jars in polishing, show little except the curdled appearance of the serpentine and a multitude of cleavage planes in the calcite. In like manner in preparing decalcified specimens, a little heat or an acid too strong or not quite pure, may remove all the dolomitic casts of tubuli, and may erode those of serpentine. From causes of this kind I fear many who have pronounced very decided opinions on *Eozoön* have not actually seen perfect examples of its structure.

While, therefore, I must agree with the writers of the paper that their specimens from Somna belong to the category of those banded structures found in concretions and geodes, and at the lines of contact of igneous and aqueous rocks, which are not unfamiliar to those who have advocated the organic origin of *Eozoön*, and which they have all along been solicitous to distinguish from it, I must emphatically deny that they resemble either in composition, mode of occurrence, or form and structure, the Laurentian *Eozoön* of Canada. The present writer, indeed, regards it as little complimentary to his long experience of rocks and fossils to suppose him guilty of the error of judgment implied in such a resemblance, and, therefore, begs to say that had he been privileged to examine the now celebrated specimen from Somna in the British Museum, or had he seen such things in the escarpment of that old volcano, or had his friend Dr. Johnston-

Lavis been kind enough to show him such specimens in his collection at Naples, he would probably have declined to acknowledge their true affinity to *Eozoön*. But, of course, we must bear in mind the organic possibilities in metamorphosed rocks which Brögger has so well illustrated in Norway; though even in Canada we cannot yet parallel his *Orthis calligramma* moulded in garnet.

The gradual discovery of new forms of pre-Cambrian life in Europe and America, will soon compel palæontologists to give more attention to this neglected portion of their science. The discoveries of Billings, Cayeux, Walcott, Matthew, and others have already established a probability that, besides *Eozoön*, the pre-Cambrian rocks contain other stromatoporoid forms (*Archæozoön Cryptozoön*) as well as sponges, radiolarians, worms, and possibly elementary types of mollusks and crustacea. Dr. Adams has also made much progress in separating the truly sedimentary beds of the Canadian Laurentian from those that may be referred to the crushing and deformation of igneous products. Perhaps, however, the time has scarcely yet come for summing up the evidence.

J. William Dawson.

Sir William Dawson's remarks call for some reply, as he seems to have overlooked or misunderstood some points having an important bearing.

He distinctly passes over the table of necessary conditions for the occurrence of eozoönal structure on pp. 264–65 of our paper, and then describes the mode of occurrence of the so-called fossil in Canada, which is provided for under section (*b*), on p. 265. All we require for the formation of eozoönal structure is the contact of a rock containing silicates, with a limestone more or less magnesian, both being exposed to a high temperature. Whether such an association be due to the injection of veins of igneous rock (either remaining as dykes or broken up into separate masses); whether boulders of old volcanic or other rocks containing silicates were enclosed at the time of the sedimentation of the limestone; or whether beds of detrital silicates were interstratified with limestones, does not matter. We repeat that all we want is the association of these two types of rocks in close contact with each other, and a sufficient supply of heat, time, and pressure. Then, along the line of contact, both rocks will undergo alteration, and this eozoönal structure be developed.

Sir William Dawson expresses surprise at our statement that a white pyroxene occurs as a leading constituent in the rock in which the masses of *Eozoön* occur, and is at a loss to understand how it can have been made. He assures us that it is entirely without foundation. In reply, we refer Sir William Dawson to some of Dr. Carpenter's choicest specimens, to some which he had photographed to illustrate the "exhaustive memoir," which Sir William reminds us he intended to prepare. The matrix of some of these is composed almost entirely of white pyroxene.

We understand that further information will be published shortly in relation to the mode of occurrence of some of the typical Canadian *Eozoöns*, and that this is far more in agreement with the theory we proposed than with that of the organic origin of *Eozoön*.

Our specimens show both the silicate bands and the calcite bands as lenticular, as in Figs. 1 and 3 (especially at top and right side of 3), pl. xxx. In pl. xxxiv. the silicate laminæ are distinctly lenticular and are interrupted. In many of Carpenter's specimens the silicate bands are certainly continuous, and the calcite bands lenticular and interrupted.

The mammillation of the chambers in the Canadian *Eozoön* is observable with the serpentine, but not with the so-called malacolite bands, and is simply due to an arrangement commonly assumed by amorphous serpentine.

As to the shape of masses of *Eozoön* we can place at Sir William Dawson's disposal a great variety, and we could have devoted a hundred pages to descriptions of variations both in shape and structure; but space and the patience of our readers forbade. Both of us have had some practice in the examination of organic and mineral structures under the microscope, and can appreciate the resemblances and differences between them. Of the two, some of the Vesuvian specimens far better mimic organic structure than do those we have seen from Canada.

Sir William Dawson tells us that Dr. Carpenter fully understood the differences between the chrysotile veins and the proper wall. He certainly did maintain this view for about twenty years, but then he admitted that he had been in error, and in the last published statement of his that we know, he accepted the proper wall as inorganic.

Sir William Dawson's memory has failed him, for the first author of the paper did show him some specimens when in Naples over a decade ago; but his suggestion as to their nature was so peremptorily dismissed that he feared to push the matter further, more especially as then no microscopic or even detailed examination had been made.

Neither of us wish to deny the existence of life in pre-Cambrian times; though we doubt whether all the cases quoted by Sir William will be verified by further research.

Finally, when Sir William Dawson denies that the specimens we described "resemble either in composition, mode of occurrence, or form or structure, the Laurentian *Eozoön* of Canada," we would only remark that when one of them was shown to the only geologist in England whom we could find who still had any faith in *Eozoön*, he declared it was an excellent specimen of the Laurentian *Eozoön* of Canada; and when he was told it came from Monte Somna he rejected the idea with scorn, and said that if it had, it had been first carried there from Canada.

H. J. Johnston-Lavis.
J. W. Gregory.

VI.

In the Home of the Nautilus.

DR. ARTHUR WILLEY (formerly of University College, London) last year resigned the post of instructor in biology in Columbia College, New York, in order to accept the Balfour Studentship of the University of Cambridge. He undertook as a condition of his election to the studentship to proceed to New Britain in order to attempt the investigation of the embryology of the Pearly Nautilus. Dr. Willey was assisted in his equipment by a grant from the Government Grant Committee of the Royal Society of London, and started for his destination in last September. The following letter is the first received from Dr. Willey since he arrived in New Britain, and it shows that he is in a fair way to carry out the purpose of his journey, whilst it is full of matter interesting to naturalists, and will, I feel sure, be welcome to the readers of NATURAL SCIENCE.

E. RAY LANKESTER.

DEAR PROFESSOR LANKESTER,

When I arrived at Batavia at the beginning of last November, I had still upwards of three weeks to wait before going on to the Bismarck Archipelago by the North German Lloyd ss. "Lübeck," and accordingly wended my way to the magnificent botanical gardens, known as "'s Lands Plantentuin," at Buitenzorg. Here every kindness was shown me by the director, Dr. Melchior Treub, and by Dr. Janse.

My imagination having been for a long time excited by what I had heard of the extraordinary parasitic flowers of the Rafflesiaceæ, I made an excursion, accompanied by a most intelligent native employé of the gardens, to the virgin forest on the flank of the huge extinct volcano, Salak (West Java), where *Rafflesia Rochussenii*[1] was discovered by Teijsmann and Binnendijk in 1850, and is till now only known from this locality.

After a somewhat fatiguing scramble through the bush, the astonishing instinct of my guide led us to several groups of the sessile flowers, which are about the size of the closed fist, growing on the roots of *Cissus*, and almost concealed under the humus.

[1] *Rafflesia* is known to the Malays by the name of "padma besar," while the allied hermaphrodite genus *Brugmansia* is called "padma k'chil" (small padma). *Balanophora* is also called "padma."

It is a very rare thing to see the flowers expanded, and we were not fortunate in this respect, but in some cases the brown bract-like structures which surround the inflorescences had parted on the top so as to expose the flesh-coloured corolla.[2]

After this, through the kindness of Dr. Treub, I spent eight days at the mountain laboratory of Tjibodas, which has been erected within the last two or three years under his direction. It stands on the side of the still active volcano Gedeh, some 1,425 metres above the sea-level.

There is an extensive area of virgin forest here, which has been granted by the Government to 's Lands Plantentuin, and is now administrated by the well-known horticulturist, Mr. Henry Couperus.

In the immediate vicinity of the laboratory, a number of exotic trees have been introduced, most noticeable among them being a quantity of Australian conifers, chiefly species of *Araucaria*. But at this elevation they do not undergo fructification; they bear cones, but they are quite sterile. *Cupressus excelsa* (Australia) is the only conifer that bears fruit at Tjibodas. In the same way, cocoa-nut palms do not bear fruit here. On the sides of the pathway leading up to the laboratory are two of the curious Australian grass gum trees (*Xanthorrhœa hastilis*. Nat. Ord. Liliaceæ).

In the laboratory book at Buitenzorg, I had previously found an entry by Professor Max Weber to the effect that giant earthworms, called by the natives "tjatjing sondari," occurred in the neighbourhood of Tjibodas, and I gave my chief attention to these during my stay there.

The giant earthworms that are to be dug up here belong to the family of the Perichætidæ (as, in fact, did all the earthworms I saw in Java). When lying still upon the ground they may measure from eighteen to twenty inches, and when crawling may exceed twenty-five inches, with the girth of a small snake or blindworm. When first taken out of the ground the worms, especially the older ones, exuded a viscous dull greenish-yellow fluid from the dorsal pores in the hinder region only. They may even squirt the fluid up, by violent muscular contraction, to a height of some inches, which is rather an unpleasant fact when one is bending over them.

Once, while walking through the forest, one of these worms, a young one, dropped from a tree at my feet. On my expressing surprise, my native guide, whose intimate knowledge of bionomics filled me with envy, informed me that the "tjatjing sondari" were also to be found in the mould in which the large epiphytic fern *Asplenium nidus*, or "kadaka," grows, high up on the trees. On going later to verify this statement, I found it to be perfectly correct. From one kadaka I took three specimens, and from another six, and so on.

[2] For a recent admirable account of the Rafflesiaceæ *cf.* H. Graf zu Solms-Laubach "Ueber die Species in der Gattung Rafflesia . . . " in *Annales du Jardin Bot. de Buitenzorg*, vol. ix., 1891.

Of course, we could only examine those within our reach, but there is no telling how high the worms clamber. One kadaka, from which we obtained specimens, was growing far out on an overhanging branch about nine feet from the ground.

We got individuals from the kadakas, ranging from five to twenty inches in length, but always young, never with a well-developed clitellum, and my guide assured me that one never finds old worms in the kadakas. "Kalau k'chil ada di-kadaka, kalau bĕsar masok di-tana:" when small they live in the kadakas, when large they go into the ground. We did, however, obtain specimens without a clitellum in the ground, but not with a clitellum in the kadakas. Another singular fact in connection with those individuals which were obtained from the kadakas was that they never exuded the viscous fluid through the dorsal pores, but, strangely enough, squirted out a fluid, sometimes with great force, through the mouth. I repeatedly observed this. The very small ones did not exude any fluid. Both in the ground and in the kadakas the "tjatjing sondari" are accompanied by a common small species.

From the waxy contents of the tuber-like excrescences of *Balanophora*, which is very common on the Gedeh, the natives manufacture effective candles, or, more correctly, small torches, by plastering the viscous substance in an even layer, about one-eighth of an inch thick, round a long thin slip of bamboo. It is interesting to observe the manifold uses to which the natives put all kinds of plants.

On another occasion we came across the nest of a species of the lizard *Calotes*, containing four eggs. Under ordinary circumstances the *Calotes* is of a bright green colour, but one put into my pocket emerged a dark brown. The nest consisted of a hole in the ground, two to three inches in depth and nearly two inches in diameter. My guide had seen the lizard making this nest some three days previously, and on opening the eggs I found the young embryos to be at the corresponding stage of development, with the heart beating. This was another welcome proof of the trustworthiness of the native.

On the faces of rocks near Tjibodas I found a quantity of small caterpillars living on the powdery alga, which makes greenish-white patches on the rocks. The caterpillars had so completely covered themselves with the alga as to be only discernible by their movements on close inspection, and their disguise must effectually protect them from foes.

The name "djambilong" is applied both to large black freshwater leeches and to *Bipalium*. I did not obtain any of the latter, although it is known to occur here.

On returning to Batavia, I enjoyed the hospitality of the British Consul, Mr. Rushton Lankester, for a few days, and finally left by the "Lübeck" on November 25.

During the first three days of the voyage from Batavia, I noticed a large number of snakes (Hydrophidæ), averaging from 18 inches to 2 feet in length, and ranging in colour from pure white, through yellow, to dark brown, floating on the surface of the sea. After the third day I saw no more of them; but they occur here in Blanche Bay.

After touching at several points on the coast of New Guinea to land goods, we at last anchored off Herbertshöhe on the evening of December 15, and next day I landed at Ralum, where I was received with great kindness into the house of Mr. Richard Parkinson, to whom I had brought a generous introduction from Dr. Anton Dohrn.

Mr. Parkinson, as is well-known, has himself produced works[3] of great ethnographic value, and my surroundings were, therefore, at once of a sympathetic description.

Later on Mr. Parkinson caused a commodious bamboo laboratory to be erected on the beach at Ralum, and with the assistance of Mrs. Parkinson, whose knowledge of the native language and character is most intimate, I purchased a small native hut on an island which was thrown up in Blanche Bay during an eruption in 1879, so that I could have an accessory laboratory and shelter in the immediate vicinity of the habitat of the Pearly Nautilus.

This island is called by the natives Rakaiya, which means "the spirit"; on the maps it is marked Raluan or Vulcan Island. It is rather badly placed for squalls and earthquakes, but is otherwise not unpleasant.

The native canoes are of excellent, though simple, construction, and I was anxious to purchase one for use in surface-dredging; but up to the present it has been impossible to do so, since the natives will only accept native shell-money[4] (dewarra or tambu) in payment. It is very difficult for white men to get hold of any of this dewarra. Gold will not tempt the natives to part with it. And, indeed, unless one is willing to purchase it from traders at the rate of three shillings a fathom, the only way to obtain it is by attending the funerals of chiefs and great men. This is, however, rather a tedious process, as we found on one occasion, when Mr. Parkinson and I attended the funeral of a man called Tononat, the son of a former great chief. We had to wait seven continuous hours before the dewarra was distributed.[5] During this time the women were wailing, the men were

[3] *Cf.* "Im Bismarck Archipel. Ergebnisse und Beobachtungen auf der Insel Neu-Pommern (Neu-Britannien)." Leipzig: 1887. Also "Album von Papua-Typen," in conjunction with A. B. Meyer, Dresden (Stengel and Markert), 1894. In this work the photographic reproductions are of surpassing excellence.

[4] The shells are the small *Nassa callosa* var. *camelus* v. Martens. (See Dr. O. Finsch). "Ethnologische Erfahrungen aus der Südsee." Wien: 1893. P. 387.

[5] The distribution of dewarra which occurs at the funeral of a rich man, among all the men who are, as it were, in at the death, is a remarkable spectacle. On the above occasion the amount of dewarra thus disbursed would correspond roughly to £150 sterling.

decorating, painting, and pouring sand and ashes on the body, and all were chewing the betel. After the body had been laid in the grave, I, for my part, received a fathom and a half of dewarra. The price of a canoe is 20 fathoms.

Before speaking of the Nautilus I may record the occurrence of a Tornaria in Blanche Bay. I have also found metamorphosing larvæ of *Balanoglossus*, but cannot say whether they and the Tornaria belong to the same cycle of development. The small Heteropod, *Atlanta*, is very abundant, but, while those dredged from the surface are sluggish and usually retracted in their shells, those dredged from a depth of 20 to 30 fathoms are as active and brisk as possible.

The native name of the Nautilus in the Gazelle Peninsula is "pal-a-lialia." [6] It is found in Blanche Bay at a depth of more than 70 fathoms, and is caught in large barrel-shaped baskets made of bamboo slips bound together by rattan, and provided with an internal basket-work which serves as a trap (see vignette at end). These fish-baskets are baited by suspending a number of small, smooth-skinned fish, called "malabur," inside. We obtain the malabur by exploding a charge of dynamite in the midst of a shoal of the fish. The baited baskets are lowered to the bottom after sunset and left there for several hours, when they are hauled up and may, or may not, contain several individuals. Often as many as six are obtained in one haul, and if several canoes are engaged in the fishing it is possible to secure some two dozen individuals in one night.

I will now give a brief summary of the observations I have so far made on *N. pompilius*.

1. *Mode of Occurrence*.—The Nautilus occurs in Blanche Bay throughout the season of the north-west monsoon, and this is already a fact of some importance, since it is thereby available for investigation the whole year round [and not merely during the S.E. monsoon]. The specimen on which Owen founded his classical memoir was obtained by Bennett off the New Hebrides in the month of August, *i.e.*, in the south-east season.

In the laboratory book at Buitenzorg there is an entry, made by Semon in 1893, containing the following statement referring to the Molluccas:—"Nach übereinstimmender Aussage alter Fischerdort, ist das Meer während der Zeit des S. O. Mons. (Mai–October) sehr viel tierreicher als in der des N.W. Mons. Nautilus wird beinah ausschliesslich nur in ersterer Jahreszeit gefangen (geangelt)." Further, in response to an inquiry as to the occurrence of the Nautilus at the Philippine Islands, the necessity of going there at the right time of the year, viz., April to July, was strongly impressed upon me by Dr. D. C. Worcester, of the University of Michigan.

[6] "Pal" means a "house," and "lia" means "above" or "beyond," so that "pal-a-lialia" signifies "house above house," and evidently refers to the chambered structure of the shell.

I have myself not yet seen the Nautilus swimming at the surface of the sea, all my specimens having come from the bottom.

2. *Nature and Temperature of the Sea-bottom in Blanche Bay.*—I have dredged in Blanche Bay in nearly all directions with the assistance of a small steam launch which I hired for the purpose, and everywhere, apart from the reefs which occur here and there near the shore, have found the bottom to consist of firm mud (which at some points is consolidated to a clay) and loose pumice-stones. This, then, is the feeding-ground of the Nautilus. I have not yet ascertained whether it is also the breeding-ground.

The temperature at the bottom, as found by attaching a deep-sea thermometer (kindly lent by Mr. Parkinson) to a fish-basket, is between 19 and 20° Celsius. This means that there is a difference of some 10° C. between the water in which the Nautilus lives and that into which it is brought when hauled up to the surface, and may account, in some measure, for the difficulty in keeping it alive for any length of time.

3. *Food.*—When a Nautilus, which has been taken in the fish-basket, is examined, the stomach, or crop, is found to be filled to its utmost capacity with fragments of the small fish used as bait. Among these are fragments of small Decapod Crustacea (probably Carididæ), and it is probable that the latter form its chief article of food under ordinary circumstances, since they also come up, sometimes in large numbers, in the fish-baskets, and I have seen the Nautilus attack and swallow them. Owen also found Crustacean remains in the stomach of his specimen. It would seem as though they would prefer soft-bodied fish if only they could catch them. Dr. Woodford informed me by letter that in the Philippines the baskets in which the Nautili are caught are baited with pieces of raw chicken.

Nautilus itself affords very good eating. Not one that I capture is wasted. The taste is very much like that of other Cephalopods, and the toughness is between that of a young squid and an old octopus.

4. *Young Individual.*—The youngest individual that I have as yet obtained was a male with the following dimensions:—

Length from root of siphuncle to mid-anterior point of hood (measured along the dorsum)	25 mm.
Length of hood in middle line...	10·5 ,,
Breadth of body across middle of eyes ...	15 ,,

The surface of the hood was perfectly white and unpigmented. The branchiæ of opposite sides were in close apposition in the median line, and, curiously enough, the larger posterior pair extended forwards far into the interior of the funnel.

The shell was perforated at the umbilicus, as it is throughout life in *N. umbilicatus*.

5.—*Relations of male and female* (*N. pompilius*).—Out of sixty-seven individuals, fifty-one were male and sixteen female, so that the statement in Pelseneer's recent "Introduction à l'étude des Mollusques," that "hyperpolygynie" occurs with the Nautilus, requires modification.

Most of the females that I have obtained have been immature, whereas many of the males have been mature, and have had spermatophores in the Needhamian vesicle and motile spermatozoa in the testis, and, in rarer instances, I have found a spermatophore in

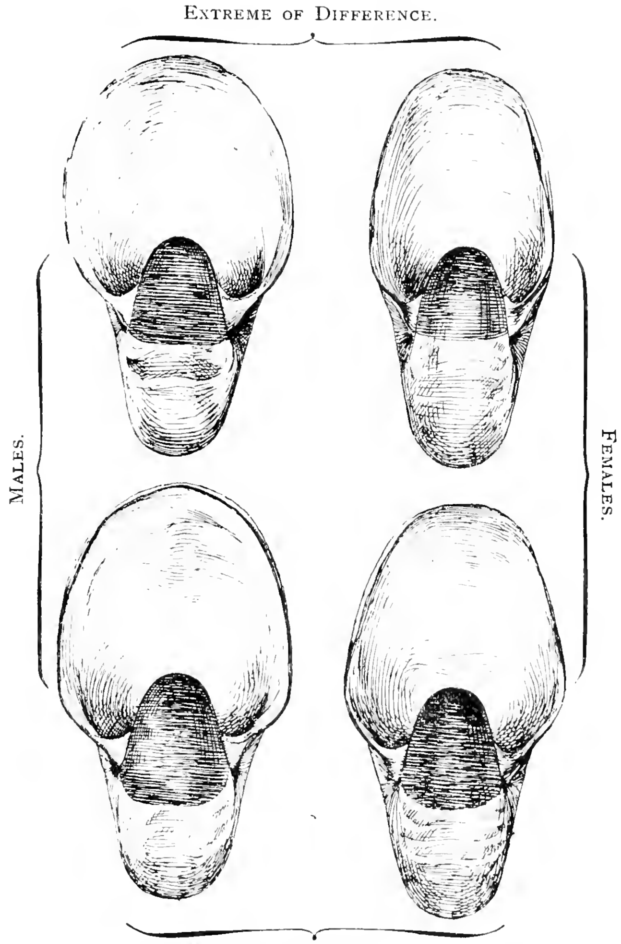

Fig. 1. Secondary sexual characters in shells of *Nautilus pompilius*.

the angle formed by the buccal mass with the dorsal hood, attached to the innermost tentacles of the inner lobes of the cephalic tentacular system, as first described by Van der Hoeven. There are reasons for thinking with Van der Hoeven that the spermatophore remains here for a long time.

Van der Hoeven and Keferstein describe a difference in the dimensions of the hood in male and female. I have made a large number of measurements of the shells to ascertain if there was any constant difference between male and female. The male shells average larger than the females, but there is an astonishing amount of variation in the proportional dimensions of the shells of both sexes, into which, however, I need not now enter. The essential differences between the male and female shells will be seen clearly in the accompanying figures (Fig. 1), which are pen-and-ink tracings from photographs kindly taken for me by Mr. Parkinson. In most adults the shell can be readily identified as belonging to male or female, but often the identification is very difficult, and in young shells impossible.

The shell also varies to a great degree in thickness, being sometimes so thin that the slightest touch is almost sufficient to break it at the free edge, and in other cases as thick as cardboard.

6. *Significance of the so-called post-anal or supra-anal papilla.*—This curious structure, which immediately strikes the eye on drawing back the mantle, lies in the wall of the mantle across the middle line, immediately behind[7] the region of the renal sacs and, in the female, between that region and the nidamental gland.[8] It is identical in the two sexes. Hitherto this has been of unknown insignificance, but I have no hesitation in suggesting that it represents a pair of osphradia or branchial sense organs, corresponding metamerically with the pair described by Lankester and Bourne, between the bases of the gill plumes. This conclusion rests at present on two main grounds, namely, (1) Variation and (2) Relation to visceral nerves.

As to the innervation, I will say at once that it is very difficult to see the actual nerves or nerve-fibres (because the nerves are often not compact trunks, but broken up into loose strands) which pass into the osphradia; but the anatomical relations of the visceral nerves to the osphradia, which, I think, have never been fully described, are such as to leave no doubt as to the source from which the osphradia derive their innervation.

In the first place, one readily sees in fresh specimens that we have not to do with one papilla, but with a pair of papillæ, which are more or less deeply bifid or cleft at their free projecting extremities, and so approximated to one another in the middle line as generally to be fused together, but always with a deeper or shallower groove between them. In two examples, I have observed that the two papillæ were absolutely independent of one another, and separated by an interval (in one case, which I measured) of two and a half millimetres. (*Cf.* Fig. 2.)

Secondly, there are two main visceral nerves on each side, which run side by side in the wall of the vena cava immediately below the skin. On arriving at the region of the renal sacs, the two nerves separate, one of them, the outer and larger, proceeding to the branchiæ,

[7] That is when regarded with the mantle drawn back.

[8] In the fresh condition the nidamental gland is of a bright yellow colour.

and dividing into two shortly before reaching the osphradium of Lankester and Bourne, to which I am confident of having traced a small nerve, arising from the point of bifurcation.[9] The inner and smaller visceral nerve passes over the region of the renal sacs on each side to the base of what I may call the posterior osphradia (os^2), to which it undoubtedly sends nerve-fibres, although I cannot say positively that I have definitely traced these. In the

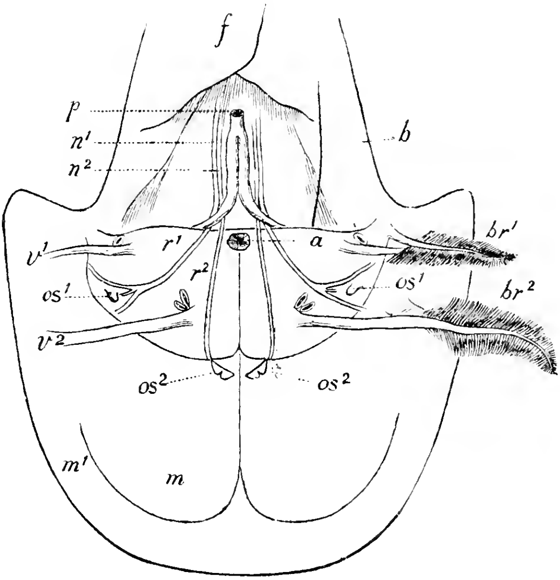

Fig. 2. Dissection of the Pallial cavity of *Nautilus pompilius*.

f. Funnel. *b*. Bristle passed into aperture of left rudimentary genital duct. *p*. Male genital aperture. *a*. Anus. *m*. Mantle reflected back. m^1. Muscular margin of mantle. n^1, n^2. Visceral nerves showing through the skin. br^1, br^2. The branchiæ. v^1, v^2. Branchial veins. r^1, r^2. Region of anterior and posterior renal sacs. The excretory granules with which the latter are usually filled produce a brilliant vermilion colour in the fresh condition. os^1, os^2. Osphradia.

N.B.—os^2 = post-anal papilla; in this example its two component papillæ were separated by an interval of 2½ millimetres. A line drawn from the anterior renal opening to the posterior osphradium (os^2) will pass over the posterior renal opening. The inner and larger of the two posterior openings on each side is the viscero-pericardial aperture.

female, I have followed the main trunk of this nerve through the nidamental gland. The two visceral nerves on each side arise close together from the visceral ganglion[10] (posterior subœsophageal ganglion of Owen).

Finally, one can see from the accompanying figure that the two papillæ, discussed above, have essentially the same topographical

[9] As it crosses the renal sacs, the outer visceral nerve runs along the line dividing the outer anterior sac from the inner posterior sac.

[10] The figure of the visceral nerves given by Keferstein corresponds more to what I have found than does that of Owen.

relation to the posterior branchiæ that the osphradia of Lankester and Bourne have to the anterior branchiæ. Their greater proximity to the middle line is shared in common with the posterior renal sacs and renal apertures, and even the posterior branchial veins, as compared with the corresponding anterior structures.

That they are bifid and, therefore, more highly developed than the anterior osphradia,[11] is in keeping with their position in the living Nautilus in the anterior region of the mantle cavity, and also with the fact that the posterior branchiæ, with which they would be associated in the metameric system, are considerably larger than the anterior branchiæ.

I remain, dear Professor Lankester,

Ralum, New Britain, Yours very truly,

February 10, 1895. ARTHUR WILLEY.

[11] It must be remembered that the terms anterior and posterior as used above refer to the reflected position of the mantle and not to the actual position in the living animal.

SOME NEW BOOKS.

The Text-book Writer among the Echinoderms.

Lehrbuch der Vergleichenden Anatomie . . . Vierte Abtheilung. Vergleichende Anatomie der Echinodermen und Enteropneusten. By Dr. Arnold Lang. 8vo. Pp. xvi., 871–1198, 251 text figures. Jena: G Fischer, 1894. Price, 7 marks.

As a condensed account of the morphology of the Echinoderma, this should prove a useful compilation, and one cannot but admire the assiduity of the compiler. But it is a compilation and little more. One has not to read many paragraphs before becoming aware that the writer is no specialist in this subject. From one point of view, this may be thought an advantage; from another it is certainly a disadvantage. The author of a text-book such as this should combine the learning of the specialist with the wide outlook and broad grasp of the philosophical biologist. The demand, however, is, at least at the present day, an impossible one. It can never be stated too often or too strongly that the single author text-book, on any such extended scale, is foredoomed to failure. The specialist in each branch knows how many years it takes him to grasp the rudiments of his subject, and he simply laughs at the idea of any man, even so clever a man as this eminent Swiss investigator and professor, venturing to collect and retail the knowledge of all the groups of the Invertebrata which has been and is being laboriously acquired by a multitude of scattered workers.

It seems, however, inevitable that such a book as this, devoting as it does 284 pages to a single group of animals, the Echinoderma, should be criticised from the standpoint of the specialist. It is all very well for the student to say that he understands a certain book, and reads it with pleasure, or for the philosopher to admire the masterly handling of facts and theories; but what if your expert comes along the next minute and says—as he alone has a right to do—that the language is incorrect, the very facts wrong, and the theories those of the day before yesterday? I propose, therefore, to deal with this book from such vantage ground as I may have gained by several years study of some of the animals dealt with in its pages; and, though such a proceeding may seem rather unfair to the author, it is surely fairer to the public, and, in the long run, of more use to the author. It may also advantage specialists themselves. For, if specialists write in such a way that an acute observer and able writer cannot render their views correctly, then it is just possible that some of the fault may lie with the specialists and not with the compiler.

The class to which I propose chiefly to direct attention is that of the Crinoidea. These, it must be admitted, are remarkably difficult animals to study, and the difficulty is enhanced by the fact that so many, and those the more important, are found only in the fossil state. One can but sympathise with the poor man who undertakes, in a fraction of two or three years, to master the problems presented by the literature alone. It is not surprising that the writer of this book has repeated a few errors that seem to be the common property of text-books. It will be well to correct these at some length, and with the lucidity that must hitherto have been lacking in our attempts.

First, we find this lamentable confusion between the dorso-central and centrodorsal, as well as a misconception of the true meaning of the latter. It may be granted willingly that the two terms are very similar, and that it was quite wicked of anybody ever to give such similar names to two distinct elements of the crinoid skeleton; but this does not alter the fact that the elements are distinct. The confusion occurs in the following sentences. On p. 904 it is written, "In the apical system [of the Echinoderma] the following plates occur: (1) a central plate at the apical pole; (2) a circlet of five radially-placed plates, the Infrabasals," etc. From p. 888 we learn, quite correctly, that this central apical plate is called the "Dorsocentral." On p. 915 follows this remarkable statement: "A central plate has been observed in the larva of *Antedon.* Later on it fuses with the uppermost ossicle of the larval stem, which supports the calyx, and with the infrabasals, to form the Centro-dorsal." That is to say, according to Dr. Lang, the dorsocentral forms part of the centrodorsal. The facts are simple; they are not as above stated, but they are as follow. The plate in the *Antedon* larva, to which the name "dorsocentral" has been applied, lies at the distal or aboral end of the stem, and differs from the other calcareous elements of the stem in being larger and flatter. It is by this plate that the so-called "pentacrinoid" larva becomes attached. When the larva begins to assume the characters of the adult *Antedon*, the proximal ossicle of the stem, nearest the calyx, increases in thick-ness, and develops first one, then two and three, whorls of cirri; it also becomes fused, as is now known, with the remains of the infra-basals. The *Antedon* eventually breaks away from its stem, carrying with it only this proximal, cirrus-bearing ossicle; and it is to this ossicle that the name "centrodorsal" is properly restricted. It is obvious, then, that the dorsocentral has been left behind in the place to which it became attached, and that it has no part or lot in the centrodorsal.

The centrodorsal is considered to represent more than one stem-ossicle, as has been most clearly put by Ludwig (Zur Anatomie des Rhizocrinus Lofotensis; *Zeitschr. f. wiss. Zool.* xxix., p. 73, 1877): "The centrodorsal is a compressed upper portion of the stem, in which the calcified tissue has undergone no separation into ossicles lying one under the other." The fact that the infrabasals become fused to this structure is interesting, but cannot be considered to con-flict with the statement of Ludwig—that is to say, this morphological element would remain a centrodorsal even if the infrabasals did not become so fused: indeed, it is possible that they were not so fused in many of the earlier Antedonidæ. The fact that the centrodorsal bears whorls of cirri is, however, very characteristic, for it is chiefly this fact that shows it to be a compound structure and not one simple stem-ossicle. The early stages in the evolution of a centrodorsal may be seen in *Millericrinus Pratti*, as described by P. H. Carpenter (*Quart. Jour. Geol. Soc.*, vol. xxxviii., p. 21), and in *Thiollievicrinus* (De Loriol: "Faune jurassique du Portugal"), and this palæontological evidence confirms the view that the centrodorsal is a compound structure. Essentially it is a part of the stem, and though it may, in certain cases, become fused with some elements of the calyx, it cannot in a morphological sense be said to form a part of the calyx or to have anything to do with the apical system of plates. The greater or less extension of the lobes of the chambered organ below the actual limits of the calyx has nothing whatever to do with a centrodorsal, and is of no morphological importance. Such is the conception of the centrodorsal as we receive it from the most eminent writers on the Crinoidea—Wyville Thomson, the two Carpenters, De Loriol, and Ludwig. It is, therefore, incorrect and misleading of Dr. Lang to say (p. 897) that the expanded centrodorsal forms part of the calyx of Apiocrinidæ, or (p. 898) that there is a centrodorsal in the calyx of the Bourgueticrinidæ. The structure to which he is here alluding is the proximal stem-ossicle, and it is not a centrodorsal in any sense of the term, although that name has occasionally been applied to it by the compilers of text-books.

Let me add a word on my own account as to the dorsocentral. Dr. Lang cannot be blamed for considering some plate with this name to be an essential element of the echinoderm apical system, since he is merely following the highest authorities, among whom may be mentioned Loven, P. H. Carpenter, and Sladen. Personally, I have long considered this plate to be, like the now abandoned orocentral, no more than a myth of transcendental morphology. On purely palæontological grounds, and specially from a consideration of the Cystidea, I have found it impossible to swallow the extended homologies that have been maintained, chiefly by the writers just named, between the apical systems of the various orders of Echinoderma. This, however, is a vast question, only to be settled by reference to decided facts, too many to be dealt with here. But, so far as the dorsocentral is concerned, the following facts may be recalled to mind. In the whole of the Crinoidea, Cystidea, and Blastoidea, the actual existence of such a plate is known only in the embryo *Antedon* and the adult *Uintacrinus* and *Marsupites*. Indeed, even *Marsupites* should probably be set on one side, for it is quite open to us to believe, as Seeliger for one does believe, that the central apical plate of this genus is merely the top segment of a stem, a legacy from stalked ancestors which has become modified and incorporated in the calyx wall. If this plate were a true centrodorsal, that is, if it bore cirri, the question would be settled; but the absence of cirri does not necessarily prevent it being derived from one or more stem-ossicles. Unfortunately, in the picture of ancient crinoids that present knowledge permits us to call up, *Marsupites* is a kind of Mahomet's coffin hanging between heaven and hell; just as it seems to have no descendant, so also it has no visible ancestry. In this inquiry, then, it offers no evidence worth attention. The plate in *Uintacrinus* may likewise be a stem-ossicle, or it may represent fused infrabasals. The belief in a crinoid dorsocentral must, therefore, rest on the facts of development in *Antedon*. Here it should be remembered that the ascription of this plate to the apical system of the calyx is a pure assumption; at least, I cannot find that anyone has ever seen it forming a part of the calyx. When it is first seen it is already separated from the calyx by a varying number of columnals, and, at this early period, the basals and orals, which are the only calycal plates yet formed, are still remote from one another and unsymmetrically disposed. There is, in short, no evidence to show that this plate ever lay at the apical pole symmetrically surrounded by the five basals. By those who regard it as a calycal element some weight is attached to its shape and structure, so different from those of the developing columnals. But many stalked crinoids present an expanded base of attachment, often involving more than one columnal. Such a structure, inherited from ancestors, still of functional importance, and specially necessary in all cases during larval life, would surely not be developed in precisely the same way as an ordinary columnal. Such considerations, it is true, would not be enough to upset the theory of a dorsocentral, were any evidence in its favour forthcoming; but, in the absence of such evidence, they are enough to suggest caution in the acceptance of far-fetched homologies. The foregoing conclusions were based, as stated, chiefly on palæontological grounds, and were opposed to the views of those who laid more stress on embryology. But now even an embryologist has struck a severe blow at the generally received homologies. Mr. MacBride states (*Proc. Roy. Soc.*, vol. liv., p. 433) that in *Asterina* as in *Antedon*, the præ-oral lobe of the larva becomes a fixing organ or stalk, but the relations of this stalk to the body of the adult are totally different in the two cases. "It follows that *the abactinal poles of* Asterina *and* Comatula (Antedon) *are not comparable with each other*, and that all conclusions based on the supposed homology of the dorsocentral in Echinids and Asterids, and that in Crinoids, are incorrect." Of course if this be so, not merely are the conclusions to which Mr. MacBride refers untenable, but the last support of the dorsocentral itself is also demolished. The dorso-central, considered as a morphological element of the Echinoderm type, has got to go.

To return to Dr. Lang, a similar error in the use of terms occurs in his treatment of the perisomatic skeleton. On p. 923 this is defined as composed of all those skeletal pieces which protect the body between the apical and oral systems. Further, in the Crinoidea (p. 949) the perisomatic skeleton is said to consist of (1) the perisomatic skeleton of the calyx; (2) the skeleton of the arms and pinnules; (3) the skeleton of the stem. And in the perisomatic skeleton of the calyx are included all skeletal pieces other than the dorsocentral, infrabasals, basals, radials, and orals.

Now the term perisomatic skeleton appears to have been first employed by Sir Wyville Thomson in his paper, "On the Embryogeny of *Antedon rosaceus*" (*Phil. Trans.* 1865, p. 540) in these words: "The skeleton of the pentacrinoid is composed of two systems of plates, which I shall term respectively the *radial* and the *perisomatic* system, thoroughly distinct in their structure and mode of growth. The radial system consists of the joints of the stem, the centrodorsal plate, the radial plates, and the joints of the arms (and subsequently of the pinnules). The perisomatic system includes the basal and oral plates, the anal plate, the interradial plates, and any other plates or spicula which may be developed in the perisom of the cup or disk." The lateral and covering plates of the ambulacral grooves were likewise included by Sir Wyville in his perisomatic system. Precisely the same significance was attached to the term by subsequent writers on the Crinoidea, at least down to and including the "Challenger" reports of P. H. Carpenter. In their important paper, "The Perisomic Plates of the Crinoids" (*Proc. Ac. Nat. Sci., Philadelphia*, 1890, p. 345), Messrs. Wachsmuth & Springer seem to have used the term in a different sense, or rather they substituted for the old division one that is simpler and, to my mind, more logical. They divide the skeletal elements of a crinoid into *primary* and *secondary*. *Primary* elements include: (*a*) The abactinal plates, developed on the right antimere and connected with the axial nerve-cords, viz., columnals, infrabasals, basals, radials, and all brachials; and (*b*) the actinal plates, developed on the left antimere and connected with the mouth, viz., orals and all plates of the ambulacra. The *secondary* or *supplementary* elements are all the interradial, interbrachial, and interambulacral plates, including the anal plates and those of the tube or sac. It is, apparently, to these secondary plates that Wachsmuth and Springer restrict the term perisomic.

It is, then, perfectly clear that Dr. Lang's definition is something quite new; and he ought, therefore, to have said as much, and to have given the grounds on which it was based. What those grounds may be I am at a loss to discover: to include brachials and interbrachials under one head seems a hopeless confusion. However advisable it may be to discriminate between the supposed primitive apical and oral systems on the one hand, and the supposed newer additions on the other, it must always remain unjustifiable to wrest the word perisomatic from its original and usual meaning. It seemed possible that Dr. Lang might have been influenced by the use of the word in other orders of the Echinoderma; but it does not appear that even there the word is used (or, if used, properly used) in the sense here attached to it. Huxley, in "A Manual of the Anatomy of Invertebrated Animals," 1877, uses "perisoma" in its obvious sense for the general body wall of Echinoderma; but he does not use the words "perisomic" or "perisomatic," and does not differentiate the skeletal elements. In its simplest meaning, derived from the foregoing simple meaning of "perisoma," the word "perisomatic" would naturally be applied to the skeleton of the body-wall as opposed to that occasionally deposited in the viscera; but this would bring us no nearer Dr. Lang's use of the term. In no other of the standard text-books is the word used in any such sense, and it, therefore, seems right to raise an immediate protest. For my own part, I should prefer to drop the term altogether.

It was hardly to be expected that the difficulties that have been introduced into the study of crinoid arms by many generations of writers should have been entirely mastered by Dr. Lang. He is correct in discriminating the primary brachials, which are purely arm structures, from the radials, which belong to the apical system. He also points out very clearly how certain brachials may become included, as fixed brachials, in the limits of the dorsal cup, and yet remain morphologically part of the arms. He does not, however, bring out with similar clearness the fact that the number of arms in every crinoid, with a few well-known exceptions, is five, and that any apparently greater number is produced solely by the branching of these five arms. Had this been clear to the writer's mind, he would not have wasted space in repeating the fact that there are five arms in *Thaumatocrinus*, five in the Bourgueticrinidæ, and so on; nor would he have spoken of "ten pairs of arms" in the Eucalyptocrinidæ when he meant ten pairs of arm-branches; nor would he have said "arms usually branched" in the Apiocrinidæ, since they are, if I remember rightly, always branched, though sometimes only once.

There is a similar hesitancy in the various references to pinnules. It is suggested, with undoubted correctness, that "the pinnules are best regarded as the ultimate arm-branches"; but had the author really understood what this sentence implied, he could not have written, "pinnules appear to be absent" in *Crotalocrinus*. It is no question of appearance: pinnules simply could not be present, any more than the most angelic of us could sprout wings.

This confusion extends to the description of the covering-plates of the arms. These are minute plates developed on either side of the food-groove, both in arms and pinnules. They vary considerably in the extent of their development; but they occur in almost all crinoids, and the species in which they are so atrophied as to be indistinguishable are very few indeed. They close over the food-groove, so as to protect its delicate ciliated lining from mud or other destructive agents; but during active life they are kept open. They are more highly developed in many of the Palæozoic crinoids than in those of our own day, and are, in some of the former, very complicated structures; but even here there is evidence that they could be opened. I have myself figured specimens of *Botryocrinus* from the Wenlock limestone preserved with the covering-plates erect, which is direct evidence, and I have demonstrated in *Cyathocrini* from Gotland the presence of an articular facet and apparent innervation from the axial nerve-cords, which is indirect evidence. Consequently, Dr. Lang is speaking far less than the truth when (on p. 890) he refers to the Inadunata as possessing covering-plates, which "could probably be erected," in non-pinnulate arms alone; or when (on p. 960) he speaks of covering-plates in the past tense only, and of their absence as quite a common occurrence.

The section on the Mode of Union between the skeletal pieces (pp. 964, 965), betrays a hopeless confusion, perhaps pardonable considering the difficulty of the subject. The first mode of union is thus described. "Two plates are firmly and immovably joined. There is no fibrous connection between them; between the two plates is an unbroken deposit of calcareous substance, which, however, is less solid than that of the plates themselves." So far, so good; this is a fair enough description of fusion or ankylosis, such as obtains between the basals of *Bathycrinus* or the radials of *Rhizocrinus*. But mark how our author proceeds! "In this way all or some of the plates of the cup are frequently united, *e.g.*, the radials of *Antedon* with one another and with the centrodorsal. Such a union is called a suture or synostosis." This is entirely incorrect. The term "suture" is a general one, which may be applied to every mode of union short of muscular articulation. The union above described is an ankylosis, not a synostosis. The radials of *Antedon* are not united in this way, but are united by "synostosis," or Close Suture, as Carpenter, Wachsmuth and Springer, and myself subsequently preferred to call it. In a close suture (I quote P. H. Carpenter) the skeletal elements are united "by means of connective tissue fibres, which pass from the . . . organic basis of the one joint into that of the other. These fibres are sometimes quite short, and their ends are surrounded by the denser layers of calcareous reticulation on the apposed surfaces of the two joints, which are thus closely and immovably fitted together, though they can be separated by the action of alkalies." This kind of suture is probably that which Dr. Lang had in his mind when he penned his next paragraph, in which he erroneously calls it a Syzygy. It is true that a syzygy is a form of close suture; but it is untrue that "all or most of the ossicles of the stem are united by syzygial sutures," that "syzygial sutures may occur in the cup," or that "the occurrence of syzygial sutures in a series of ossicles renders a certain flexibility possible." The term "syzygy" was invented by Johannes Müller, and by him applied to that particular case of close suture between two brachials in which the proximal or lower of the two loses its pinnule (supposing pinnules to occur). The term has since been extended to the precisely parallel case of close suture between two columnals, in which the lower of the two loses its cirri (supposing cirri to occur on all columnals), while the upper still bears them. The two components of a syzygy are termed "epizygal" and "hypozygal." The next stage to the close suture is the Loose Suture. This is either overlooked by Dr. Lang, or has been confused by him with his "syzygy." In the loose suture the two elements are connected by fibres, as in the close suture; but the connection is more elastic and flexible, and there is a more or less developed facet cut out of the edge of the plate, or else a system of ridges or striæ. Finally comes the Muscular Articulation, which receives a nerve supply from the apical, aboral nerve centre. This is correctly described by Dr. Lang, though he is wrong in saying that "all ossicles of the pinnules [in living crinoids] are united by a muscle-joint." Most are united by loose suture. It is also misleading to say that "where the costals [*i.e.*, primary brachials] are incorporated in the cup, a certain flexibility of the calycal skeleton is the consequence." This is true for the Articulata (*Taxocrinus*, *Ichthyocrinus*, and allies); but it is quite untrue for the typical Camerata (*Actinocrinus* and allies). Flexibility between the plates of the cup is, where it occurs, an independent development, and has nothing to do with the incorporation of brachials; it is found even in some species of Inadunata, where, of course, no brachials enter into the cup.

Naturally it is difficult to draw hard and fast lines between these various modes of union; and it is a vain attempt of this kind that has made the subject more puzzling than it need be. On this point Dr. Lang holds a rational opinion, and exercises due caution. Perhaps the whole matter may be most conveniently stated in the historical or evolutionary method. The plates of Echinoderma are deposited in a stroma of connective tissue fibrils, any of which may, as occasion arises, assume muscularity. It is reasonable to suppose that the skeleton, either in each group or in an ancestral stock, developed from scattered spicules like those of most modern holothurians. This does not mean that such holothurians are necessarily primitive, nor is the evolution of radial symmetry, pentamerous or otherwise, now in question. Spicules grew into plates or ossicles; and these were at first united loosely by the connective tissue fibrils, as yet undifferentiated into special muscles: this first stage was the Loose Suture. From this the development proceeded in opposite directions. One, in the direction of greater fixity, through the Close Suture or Synostosis, with its special case—the Syzygy, to the Ankylosis. The other, in the direction of greater flexibility and voluntary motion, through the Imperforate Articulation to the Perforate Articulation with its axial nerve-cords, its highly developed muscles, teeth, and ligaments. Since there is no essential difference between any of these modes of union, it has always been possible, as fossils give instance, for a close suture to pass back again through the loose suture to a stage almost worthy the name of articulation. The stem of a crinoid, for example, presents us with instances of ankylosis, close suture, syzygy, loose suture, and complete articulation, each, as need arises, developed independently from the loose suture, which is the normal type.

The remaining chapters, which deal with the morphology of the other systems and organs of the body, and with embryology, ontogeny, and phylogeny, appear to present a remarkably exhaustive digest of our knowledge, so far as recent crinoids go, well up to date, and interspersed with critical suggestions of some value. Exception, however, has rightly been taken by previous critics to the omission of the names of authorities. One does not necessarily want a historical sketch; but the value of some of the paragraphs would be greatly enhanced if one could tell on whose authority certain statements or opinions were given; and this might have been rendered possible by the insertion of a name here and there between brackets. For instance, in chapter xx., on Regeneration and Asexual Reproduction, it is stated that "In Ophiurids regeneration of the whole body from one arm never occurs." Has Dr. Lang considered Semon's case of *Ophiopsila aranea* (*Jena. Zeitschr.*, xxix., p. 585), and has he rejected it? We cannot tell. In the same chapter it is doubted whether "Division of the body into two nearly equal halves with subsequent regeneration" has been observed in Holothurians. Does Dr. Lang doubt the observations of Chadwick (see NATURAL SCIENCE, vol. v., p. 4), or is he ignorant of them? We cannot tell. Was the cautious paragraph on autotomy in crinoids written before or after Dr. Lang had read the interesting account given by Danielssen and quoted in NATURAL SCIENCE (vol. v., p. 5)? We cannot tell.

In his account of the development of *Antedon*, Dr. Lang has followed Seeliger throughout, as the student may for this once gather from the figures. I am glad to find him criticising the expression "Cystid stage," in so far as it implies any recapitulation or morphological affinity; but I must protest against his falling into a precisely similar error in his next section. He says, with all the emphasis of spaced type, that in the pentacrinoid stage "the correspondence of the attached and stalked larva of *Antedon* with the Inadunata, especially with the so-called Larviformia, is quite remarkable, immediately striking the eye." With equal emphasis I deny that any except a superficial resemblance exists. The stem is totally different: the base is, we know, dicyclic; in nearly all so-called Larviformia it is monocyclic: the radials and basals are quite regular; in most Larviformia they are more or less irregular, owing to the bisection of some radials: the arms are branched; in the Larviformia they are single: the temporary anal plate, though no doubt homologous with that occurring in some Larviformia, is not comparable in situation or course of development. In short, Professor Lang's emphatic dictum is a typical example of those rash and inexact comparisons that drew so just and vigorous a protest from Professor von Zittel in his address to the Geological Congress (NATURAL SCIENCE, vol. vi., p. 308).

Adequately to discuss the chapter on Phylogeny would require a special article, though it deals only with the origin of the Echinoderma and the relations of the classes. A primitive bilateral form, corresponding to the Dipleurula larva, is sketched out. Since radiate structure is a consequence of an attached mode of life, "all echinoderms must once have been attached animals." Attachment took place by the anterior end of the body (larval organ, præ-oral lobe). "The body then developed chiefly in the region of the mouth and tentacles. The hind part with the anus was at first a lateral projection, which gradually subsided and less and less disturbed the radiate arrangement. On this view the greater extension of the anal interradius, found especially in palæozoic crinoids, may possibly be an original condition, with which is connected the occurrence of special anal plates in that interradius." Here, again, the facts of phylogenetic development, as deduced from actual fossils and not from imaginary archetypes, force me to join issue with Dr. Lang. Granting the lateral position of the anus, the evidence, at all events of the Inadunata, shows that the widening of the anal interradius and intercalation of anal plates was a secondary condition, of which the initial rise can be traced just as easily as its final decline.

This attached stage has been persisted in by the Crinoidea, although many genera have, at different periods, relinquished it for a free mode of life. Other classes relinquished it at an earlier period in their history, and of these the Holothurians, says Dr. Lang, were the first to become free. They remained attached, however, long enough for five food-grooves with their rows of tentacles to be developed. But as the various classes became free, and took in food directly by the mouth, these food-grooves gave up their original function, and they with their underlying nerves became transformed into the subepithelial radial nerves with their epineural canals. I myself suggested, in 1889, that while radiate symmetry was, in the case of Echinoderma, due to fixation, the special case of pentamerous symmetry was induced by its mechanical superiority to the tetramerous or hexamerous type, and that pentamerism in Holothurians might be due to the fact that they were descended from ancestors in which the typical skeleton was better developed (*Quart. Journ. Geol. Soc.* xlv., p. 166). It is interesting to find that Dr. Lang is brought by his views to a similar suggestion. Or is he adopting mine? We cannot tell.

It is, however, impossible to agree with Dr. Lang that the ancestor (Stammform) of the Crinoids had a determined composition of the dorsal cup, viz., five infrabasals, five basals, five radials, and the anals. Either there were two ancestral types, one with infrabasals, the other without; or the dicyclic type was derived from the monocyclic by a modification of the cup-plates. In either case, the anals were a secondary development.

From such an attached "Crinoid-phantom," as the Sarasins would call it, Dr. Lang derives the Echinoids, Ophiurids, and Asteroids. His belief that the Echinoids are descended from attached ancestral forms with arms is due to the fact that the gonads have become influenced by the pentamerism, and must, therefore, have originated as fertile outgrowths of the axial organ contained in arms. The Ophiurids branched off later still, and the Asteroids were last of all to adopt a free life, an idea confirmed by the temporary fixation of the larva still seen in their ontogeny.

These ingenious views present many difficulties, to some of which allusion has been made; but they are distinctly stimulating and worthy of careful study in the original. Fortunately, a translation of this book, by Mr. and Mrs. H. M. Bernard, is being published by Messrs. Macmillan, so that English students will soon have an opportunity of carefully considering Dr. Lang's ideas. Despite the slips to which I have thought it right to call attention, the book is a wonderful compendium of our knowledge of the Echinoderma, clearly written and beautifully illustrated. It concludes with a short but adequate account of the Enteropneusta, the larval forms of which have been shown by recent research to indicate some remote connection with the ancestral Echinoderm stem. F. A. Bather.

Three Books on Botany.

Lehrbuch der Botanik für Hochschulen. Von Dr. Eduard Strasburger, Dr. Fritz Noll, Dr. Heinrich Schenck, Dr. A. F. W. Schimper. Gr. 8vo. Pp. vi., 558, with 577 illustrations. Jena: Gustav Fischer, 1894. Price 7 marks.

A Student's Text-book of Botany. By Sydney H. Vines, M.A., D.Sc., F.R.S. 8vo. Pp. xvi., 821, with 483 illustrations. London: Swan, Sonnenschein & Co., 1895. Price 15s.

A Hand-book of Systematic Botany. By Dr. E. Warming, with a revision of the Fungi, by Dr. E. Knoblauch. Translated and edited by M. C. Potter, M.A., F.L.S. 8vo. Pp. xii., 620. London: Swan, Sonnenschein & Co., 1895. Price 15s.

How is it done? Here is a text-book of the first order, bearing the names of at least two of the best botanists in Germany, well printed and profusely illustrated, some of the figures being new, and all for the equivalent of seven shillings. Is the cost of production so much less in Germany; or is the publisher satisfied with less profit; or is it an advertisement for Bonn University, with which all the authors are connected?

A text-book is generally the work of one man, whose qualifications to speak with authority may be of very unequal value in the different branches. This difficulty is satisfactorily avoided in the present work. Professor Strasburger supplies the first section, "Morphology," including external form and internal structure. We could have no higher authority for this branch, and the 130 pages form an excellent introduction to the general facts of form and structure. The portion dealing with the cell and its contents is of special interest, embodying, as it does, so much of the writer's own work. Dr. Noll is responsible for the Physiology (pp. 132–256), which is a well-arranged general account of the life-processes in plants. We are surprised, however, to see the process of assimilation represented on p. 169 as consisting of the union of carbonic acid gas and water to form starch. This is scarcely in accordance with modern views. The second half of the book, "Special Botany," is divided between Dr. Schenck and Professor Schimper. The former deals with the Cryptogams (pp. 258–363), the latter with the Phanerogams (pp. 364–512). This systematic portion is very well illustrated, one feature being the indication of the common poisonous plants by coloured figures. But why are *Lolium temulentum* and *Vincetoxicum officinale* left uncoloured? The scheme of classification is similar to those generally adopted on the continent.

A translation of this work by Dr. A. C. Porter, assistant instructor in botany at the University of Pennsylvania, is to be issued by Messrs. Macmillan. We are glad that America and not England is concerned. The slavish adoption of continental text-books has gone quite far enough in our own country. We are well able to supply our own students with indigenous productions, as evidenced by the admirable hand-book which we now proceed to notice.

We have already reviewed the first half of Professor Vines' text-book, which appeared in January of last year, and are now glad to welcome the complete work. Dr. Vines' English edition of Prantl, which appeared fourteen years ago, has long been insufficient for the advanced student, who has been restricted to new editions of time-honoured but woefully out-of-date handbooks, or else forced to select from the excellent but somewhat too exhaustive German works of Sachs, Goebel, and De Bary, translations of which have been issued by the Oxford University Press. The second half of the book deals with the classification of flowering-plants and physiology. The introduction to the former is a most able account of the life-history of seed-plants, and is the fullest expression we have seen of the relation existing between these and the Pteridophyta. The scheme of classification resembles that adopted in Prantl and Vines' text-book, with slight differences. Among others we notice the insertion of many of the Monochlamydeous orders among the Polypetalæ, though the sub-class Monochlamydeæ is still kept up. A strange mistake occurs on p. 590, where the old-world *Balanophora* is described as a "Brazilian" genus. The section on Physiology (pp. 666–783) is an excellent sketch of the subject, but we expected a somewhat fuller treatment from Dr. Vines. For this, however, he will perhaps refer us to his

"Text-book on Physiology," of which we should be glad to see a new edition.

It is unfortunate that the English edition of Dr. Warming's excellent work on Systematic Botany should have appeared almost simultaneously with the completion of Dr. Vines' Text-book. In the latter the ordinary advanced student will find what he requires in Systematic Botany with the addition of Morphology and Physiology; while the Warming-Potter handbook is, to a great extent, a repetition of the portion dealing with classification only. The price of the smaller book is the same, and, for a translation, seems rather a heavy one. Many teachers as well as the higher class of students will, however, be glad to have the opportunity of reading a book which was inaccessible to them in the original Danish.

Recent tendencies in the arrangement of algæ are to lay continually less stress on colour grouping, and in Dr. Knoblauch's revision, presented to us here, we find them divided into ten classes, three only of which are Chlorophyceæ, Phæophyceæ, and Rhodophyceæ respectively. It is somewhat surprising to find the bacteria included as colourless algæ. The fungi fall into three classes: Phycomycetes, Mesomycetes—a small class including intermediate forms between the first and third, viz., the Hemiasci and Hemibasidii or Ustilagineæ, and the Mycomycetes or higher fungi, comprising Ascomycetes and Basidiomycetes. Lichens form appendices to each of the two latter sub-classes, according to the nature of the fungal element.

The Angiosperms occupy the larger half of the book (pp. 273–end), and here we find a departure from the methods adopted in English works, which are based on the system elaborated in the Genera Plantarum. Dr. Warming's aim was to get a scheme which should represent as nearly as possible the course of evolution of complicated and "derived" floral structures from the simplest forms. He therefore starts with the willows (Salicifloræ), where the male flower consists only of a few stamens and the female of a single ovary, and works up through the monochlamydeous and dichlamydeous polypetalous orders of Dicotyledons to the Sympetalæ, which culminate in the Compositæ. A careful study of the whole reveals the fact that we are still a long way from a phylogenetic classification, if, indeed, such is possible with the materials now in existence.

From a literary point of view the work is poor. It abounds in slovenly writing and bad spelling. Many of the illustrations will be new to the English student. These, however, have the effect of making some of the old familiar figures look rather worn.

The British Museum Catalogue of Mycetozoa.

A Monograph of the Mycetozoa, being a descriptive catalogue of the species in the herbarium of the British Museum. Illustrated with 78 plates and 51 woodcuts. By Arthur Lister, F.L.S. Printed by order of the Trustees, and sold by Longmans & Co., B. Quaritch, Dulau & Co., Kegan Paul & Co., and at the British Museum (Natural History), Cromwell Road. London. 1894. Pp. 224 and 78 plates. Price 15s.

The British Museum and the scientific public are indebted to Mr. Lister for more than this beautiful and careful monograph. Mr. Carruthers, in a preface, states that owing to the generosity of Mr. Lister, the national collection has been extended so as to include all the types described in this catalogue.

The slime-fungi have a very peculiar biological interest. While it is by no means certain that some of them, or all of them, may not be degenerate from higher forms of fungi, yet the majority of them exhibit characters that suggest affinities with both the animal and vegetable kingdoms. Many persons see in them a hint of the primitive appearance of living matter. The life-history of a slime-fungus may be taken as beginning with a spore enclosed in a firm cell-wall. In suitable surroundings the cell-wall bursts and there creeps out a tiny "swarm-spore," a little naked mass of protoplasm with a contractile vacuole and a nucleus, and provided with a single flagellum or cilium. In this stage the little cell wriggles through the water, in its appearance and in its method of feeding being quite like an animal. In this stage, too, it multiplies rapidly by division. The numerous swarm-spores thus produced become amœboid; instead of being rowed through the water by the lashing movements of a cilium, they creep through it by the protrusion of pseudopodia. In this condition they are to be found chiefly where there are decaying, comparatively solid, masses of organic matter. The amœbæ fuse together, forming a plasmodium, in which the outlines of the original amœbæ sometimes remain visible, sometimes are lost. The whole plasmodium creeps about and exhibits streaming movements like a gigantic amœba. Such a plasmodium offers, from its size, many facilities for the investigation of protoplasm in the mass, and a number of remarkable experiments have been performed upon the plasmodia of various species showing the positive and negative attraction between the protoplasm and various chemical substances. Into these general biological questions Mr. Lister, of course, does not enter, as his monograph is chiefly systematic.

The plasmodium contains a number of nuclei, and these may multiply. After a time, especially when the substance upon which the plasmodium is creeping becomes dry, the process of spore-formation begins. The plasmodium comes to rest, and it either forms a single sporangium or divides into several portions, each of which forms a sporangium. The external wall hardens to form the wall of the sporangium, and the internal protoplasm breaks up into cells surrounding each of the nuclei. Each of these cells in turn forms a thick cell-wall and becomes a spore.

Among creatures so simple it is plain that the difficulty of establishing systematic criteria must be great. Indeed, in many respects the classification of them in the absence of an extended physiological examination of their habits and properties, their response to the stimulations of different media, and the results of such investigations as have been made upon bacteria, must remain rather an empirical index serving for their identification than a classification in the modern scientific sense. With such necessary limitations, Mr. Lister's system is very good; and all biologists who take an interest in this group must remain indebted to him for the beautiful series of figures, which illustrate with great clearness the characteristic forms of the sporangia and of the plasmodia in each of the species.

Mr. Lister follows earlier authorities in separating the small group of Mycetozoa in which the spores are not formed inside sporangia from the great majority of forms in which sporangia are formed. We think, however, that he uses the word sporophore unfortunately in his synopsis. The relation between the spores and the sporophore in the Exosporeæ is not different in nature from the relation between the spores and the stalk of the sporangium in those Endosporeæ in which the sporangium is carried upon a stalk. Indeed, the stalk is most usually called a sporophore; the point of difference to be insisted upon is, of course, the existence or non-existence of the sporangium, not the relation of the spores to the sporophore.

OBITUARY.

KARL VOGT.

BORN JULY 5, 1817. DIED MAY 6, 1895.

THE death of Karl Vogt robs science of one of its masters and Switzerland of one of her most devoted friends. Vogt was born at Giessen, and received his schooling in his native town. In 1833 he went to Berne, where he graduated in medicine, taking his doctor's degree. In 1839 he became the collaborator of Louis Agassiz in "l'Histoire naturelle des poissons d'eau douce," and from Neuchâtel went on to Paris, where he lived two years. After a brief visit to Italy, he returned to Giessen, where he held a chair at the University. A too ardent following of the democratic movement cost him his professorship; he had been deputy to the German National Assembly, had supported the National Party, and had gained considerable distinction as an orator. Retiring to Berne and to Nice he pursued his zoological studies, and in 1849 became professor at Geneva, a position he still held at his death. In Switzerland also he closely followed political life, and was a member of both the Federal and National Councils. Vogt's name is associated with anthropology, comparative anatomy, zoology, and geology, each group having been illuminated by his genius. His "Science and Superstition" did much to check religious bigotry, and his exposure of humanity in "Animal Communities" created a considerable sensation throughout the civilised world.

KARL LUDWIG.

BORN OCTOBER 29, 1816. DIED APRIL 24, 1895.

THIS eminent physiologist was born at Wissenhaufen in Hesse. He graduated at Marburg, receiving his doctorate at the University in 1840, and resided successively at Marburg, Zürich and Vienna. In 1861 he was appointed to Leipzig, where he remained till his death. Of his numerous papers and books the best known, perhaps, is his "Lehrbuch der Physiologie." He also edited the annual, *Gesammelte Arbeit des Leipziger Physiologischen Instituts.*

DR. EUGENIO DUGES, the eminent zoologist, died at Morelia, Mexico, on January 13. He was born at Montpellier seventy years ago, and was educated at Melun and Paris. In 1865 he went to Mexico, and practised medicine at Guanajuato, later on proceeding to Morelia to take charge of the Museum, the College of Anatomy, and the School of Medicine. He published numerous papers on the Coleoptera of Mexico.

Henry John Carter, who died May 4, at Budleigh Salterton, was a distinguished medical officer in the Bombay army. He was a voluminous writer on medical and natural history subjects, his chief work being done on the Geology of Western India, and on the Foraminifera and other low Invertebrata.

Dr. George A. Rex, the eminent fungologist, died at Philadelphia on February 4. He was the first authority on the Myxomycetes in the United States, and was a cautious and exact botanist, avoiding the establishment of new species until thoroughly convinced that they were really new to science. Dr. Franz Posepny, the well-known geologist, died at Vienna on March 27. He was born in 1836. Dr. Alfred Wilhelm Stelzner, professor of geology at Freiburg, died on February 25. William Parker Snow, so intimately associated with the Franklin expedition, died on March 12, in his seventy-seventh year. He was purser of the "Prince Albert," Lady Franklin's search vessel, and published an account of the voyage in 1851. Six years later he issued a second volume entitled "Two years' cruise off Tierra del Fuego and Patagonia," a record of his work in connection with the Fuegian mission. His one object in life was the rescue of the Franklin expedition, of which he protested, again and again, there might still be survivors.

A portrait of Mr. A. G. More will be found in the *Irish Naturalist* for May.

From *Naturae Novitates*, which now comes to hand with greater punctuality, we learn of the death of Dr. Eduard Rostan, an eminent investigator of the Piedmontese Alpine flora, on January 15, at the age of 69. Also of Claudius Rey, the entomologist, in Lyon; and of Dr. Robert Sachsse, Professor of Agricultural Chemistry at Leipzig, which took place on April 26.

Sir George Buchanan, who died on May 5, aged 64, had resigned the post of medical officer to the Local Government Board three years before. All his sanitary work, of such eminently practical importance, was based on thoroughly scientific investigation. Some account of his work and of his honours is contained in *Nature* for May 16.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Dr. A. von Mojsisovics has become Professor of Zoology in the University of Graz; Dr. R. Köhler, Professor of Zoology in the Faculty of Sciences at Lyon; Dr. Eugène Canu, Director of the "Station Aquicole" (Fish and Fisheries) of Boulogne-sur-Mer; Dr. Pechuel-Loesche, Professor of Physiology at the University of Erlangen; Dr. A. N. Berlese, Professor of Botany at the Camerino University; Mr. W. G. P. Ellis, additional Demonstrator in Botany to the University of Cambridge. Professor Dr. K. Weierstrass has been made a member of the Academy of Sciences of Paris in the room of Dr. Kummer. Dr. H. Simroth, of Leipzig, has been appointed Extraordinary Professor. We have the best authority for stating that Dr. Branco, of Tübingen, is to be succeeded neither by G. Steinmann nor by R. Brauns, as has been widely reported, but by Professor E. Koken, of Königsberg. Professor Brauns goes to Giessen.

Dr. Hanitsch has been appointed Curator of the Singapore Museum, thus vacating the post of Demonstrator of Zoology at University College, Liverpool. Professor Oliver Wolcott Gibbs, the well-known chemist, succeeds Professor Marsh as President of the New York Academy of Sciences for the next six years.

The following appointments have recently been made known officially in the Geological Survey:—Alfred Harker and Herbert Kynaston join the Geological Survey of Scotland, replacing the late William Topley and Edward Greenly, retired. Mr. Harker, it is understood, will not relinquish his duties at Cambridge. J. R. Dakyns has been transferred from the Scottish to the English Survey, and this accounts for the two vacancies in Scotland. The Museum of Practical Geology loses its librarian, T. W. Newton. It is now open on Fridays.

Sir William Vernon Harcourt has been elected the new trustee of the British Museum.

On May 29 Mr. William Carruthers, of the Botanical Department, British Museum (Natural History), will bid farewell to his colleagues, and enter upon his non-official life. Mr. Carruthers has been for so long identified with the department that it is difficult to realise that he will still be in the flesh though absent. Mr. George Murray will succeed him as head of the department.

This is but the beginning of a series of changes at the Cromwell Road establishment, the new Treasury minute, with respect to age-limit, acting rather sweepingly at the Museum. We are not at liberty to make known these changes at present, official announcement not yet having been made.

A *Central News* telegram from Brussels states that negotiations have been opened for an International University Extension scheme. It is proposed that some Oxford professors should deliver a series of lectures in Brussels, while some Belgian professors should deliver addresses in Oxford.

The arrangements for the summer meeting of University Extension students at Oxford, which will take place during August, include lectures by Professor Green, Professor Odling, Dr. Kimmins, Dr. Fison, Dr. Wade, Mr. Carus-Wilson, Mr. G. C. Bourne, Mr. J. E. Marsh, and Mr. Percy Groom, accompanied by classes for practical work.

THE Edinburgh Summer Meeting Classes will be held at the Normal School, Johnstone Terrace, Edinburgh, from August 5–31. The courses include all subjects, but those of special interest to our readers are the following:—Applied Physiology of the Nervous System, by Dr. Irvine; The Savage Mind, by Professor Haddon; Biology of the Seasons, by J. Arthur Thompson; Botany Applied to Field and Garden, by R. Turnbull; The Evolution of Cities, by Geddes, Elisée Reclus, and A. J. Henderson; Field Geology, by J. G. Goodchild. Details can be obtained from Dr. R. Stephens, University Hall, Edinburgh.

THE summer plans for University Extension Courses at Toynbee Hall include the following: Darwinism, by H. de Haviland; Primitive Man, by F. W. Rudler; Botany, by G. May; Biology, by Miss Hall; Geology, by Miss Raisin; Physiology, by S. Rowland. The Natural History Society, under the presidency of Dr. J. W. Gregory, still flourishes, ten excursions being arranged for during the present session.

WE have before commended the summer activities of the University of Kansas. The present season will see five different scientific expeditions. Professor Dyche goes to collect and study the birds and mammals of Greenland and adjacent regions; Professor Williston will have two expeditions for the collection of vertebrate fossils, one in Western Kansas and one in Wyoming; Chancellor Snow, it is expected, will spend the summer in the south-west with a party collecting entomological specimens; a fifth party under Professor Hawortb will be in the field during the next six months engaged in mapping the Tertiary outcrops of the State. The cost of the three geological expeditions is, we learn from *Science*, borne by special appropriations from the State Legislature.

DR. R. H. TRAQUAIR secured the skin of the Great Auk in summer plumage, which, as the property of Sir Frederick Milner, was bought in at a sale at Stevens' on April 23. The price paid by Dr. Traquair was £350, and the bird becomes the property of the Edinburgh Museum. The Great Auk egg, which was put up at the same time, realised £180. The well-known collection of eggs and nests belonging to Mr. Leopold Field was also dispersed at the beginning of May at Stevens' rooms.

THE Berlin Zoological Gardens have received from Sumatra an orang-utang, locally known as the maroch, and two gibbons from the same place, neither of which have been seen alive in Europe before. They were presented by Dr. Heinrich Dohrn. The orang is the *Simia bicolor* of Geoffrey St. Hilaire, and, although held in reverence by the inhabitants of Sumatra, but little is known as to its habits. The local name of the gibbons is siamang or imban.

THE State of Indiana is making special efforts to conduct a biological survey of the State, the objects being to ascertain the character and extent of the life, to associate workers that they may labour to one end, to stimulate teachers, and to secure for the Academy a representative collection.

THE Geological Society of South Africa has already issued its Rules and Regulations, together with a list of Officers and Council for 1895. The first ordinary meeting of the Society was held in April, when Dr. Exton delivered his presidential address, in which he dealt especially with the "Coal of South Africa." The second meeting was held on May 10, when Mr. David Draper read a paper on "The Primary System of South Africa," with special reference to the conglomerates of the Witwatersrand.

THE German Zoological Society meets at Strassburg from June 4 to 6.

At a meeting of the Victoria Institute on Monday, May 6, there was read a paper by Sir J. W. Dawson, on the physical character and affinities of the Gaunches, or extinct people of the Canary Islands, illustrated by photographs. The author reviewed the historical facts as to the Canary Islands and their inhabitants, the characters of the crania found and the weapons, ornaments, etc., and described the conclusions he had arrived at with reference to the relationship of the Gaunches to ancient peoples of Western Europe and Africa, and their possible connection with the colonisation of Eastern America.

Further particulars of the third International Zoological Congress to be held at Leyden in September are to hand. The sectional meetings will be divided into (1) General Zoology; Geographical Distribution including the fossil faunas; the Theory of Evolution. (2) Classification of living and extinct Vertebrates; Bionomy; Geographical Distribution including fossil Vertebrates. (3) Comparative Anatomy of living and extinct Vertebrates; Embryology. (4) Classification of living and extinct Invertebrate Animals; Bionomy. (5) Entomology. (6) Comparative Anatomy and Embryology of Invertebrate Animals. A long list of zoologists who have promised to attend the Meeting has been published, and those desiring accommodation should apply to Dr. Lidth de Jeude, of Leyden, who will supply information as to hotels, etc.

The subscription list for the International Geographical Congress had, on April 24, reached £2,136. The Royal Geographical Society, therefore, issued a letter on that date pointing out that at least £3,500 would be wanted, and stating that "if the Congress is not the brilliant success which it is expected to be by foreign geographers, the discredit will fall on the Royal Geographical Society." We hope after this gentle squeeze that the foreign geographers will not be disappointed. The anniversary dinner of the Society has been postponed to Tuesday, July 30.

At the February meeting of the Hull Scientific Club a recently-killed specimen of the English Black Rat was exhibited by Mr. J. W. Boult.

M. de Mauroy, of Wassy (Haute-Marne), is preparing an account of all French meteorites or meteoric falls, and will be glad of any communications on the subject.

Mr. Jaroslav Perner, of Prague Museum, has been in London. He is engaged on the Graptolites of the Silurian of Bohemia, in continuation of Barrande's great work. Mr. Perner has also visited Birmingham to see the rich collection of graptolites formed by Professor Lapworth, and we may hope shortly to see the results of his later studies.

With reference to our note last month on Wild Fowl in Norfolk, it should be mentioned that both the Bittern and the Avocet, though not specially mentioned in the recent Order, were included in the Schedule of 1880; and we are informed that protection has been extended to all areas suitable for them to nest in. The birds mentioned by name in the Order were only such species as were too scattered to be protected by the more effective method of naming areas.

We understand that Lord Kelvin is resigning the Presidency of the Royal Society. We sincerely hope that the high scientific reputation of the chair will be considered in the choice of his successor.

At the moment of going to press we are informed that Professor Green has resigned his position on the Council of the Royal Society. Professor Bonney has been mentioned as the probable successor.

CORRESPONDENCE.

Bibliographical Reform and the *Zoological Record*.

In "Natural Science, May, 1895, pp. 333-338, there appeared an article with the above title, signed Herbert Haviland Field. From the general tone of the article, and from some of the expressions made use of by Mr. Field, it might be inferred that he is writing on behalf of the *Zoological Record*, and with the sanction of those connected therewith. I, therefore, ask to be allowed to inform your readers that this is not the case, and that the article must not be taken as in any way authorised by those engaged in carrying on the *Record*. The statement in the article as to the annual deficit is quite incorrect.

The existing and long-established *Zoological Record* is superintended by a committee of zoologists appointed by the Council of the Zoological Society of London, to which Society the *Record* belongs. I may, therefore, say, in reference to Mr. Field's phrase about the desire of "zoologists personally connected with the *Record* . . . to raise the standard of the work," that I shall be very much obliged if such persons will communicate with the *Zoological Record* Committee (3 Hanover Square, London), or with myself, as I should much regret that such aspirations should be fruitless.

University Museum of Zoology, Cambridge, *May 7th*, 1895.

D. Sharp,
Editor of *Zoological Record*.

[From our editorial communications with Dr. Field, who is now abroad, we are able to say that nothing was further from his intention than any attempt to criticise Dr. Sharp's conduct of the *Record*, or to interpose between the Zoological Society and the *Record*. The statement as to the annual deficit was taken from Dr. Sclater's official letter to the secretary of the Geological Society, published in *Nature* for December 7th, 1893. It is greatly to Dr. Sharp's credit that the deficit has been reduced since. But Dr. Field's general point was that there is an annual loss, and that the Zoological Society, by making use of international co-operation, might be relieved of this financial burden, while the actual conduct of the *Record* would remain in the hands of its committee, and of the present editor.—Editor, Natural Science.]

CHANGE OF ADDRESS.

In future the Publishing and Editorial Business *of* "Natural Science" *will be carried on at the Offices of* Messrs. Rait, Henderson & Co., Ltd., 22 St. Andrew Street, Holborn Circus, London, E.C.

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Publishers sending Books for Review are particularly requested to take note of our Change of Address.

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