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A Monthly Review of Scientific Progress.

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HUNTERS IN ROCK-SHELTER AT NIGHT IN SOUTHERN FRANCE.

(*From a drawing by Cecil Aldin, illustrating H. N. Hutchinson's "Prehistoric Man and Beast."*)

To face p. 54.]

NATURAL SCIENCE:

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NOTES AND COMMENTS.

The Little Trip of the "Fram."

WELL! we *have* made a mistake. "Skål to the Norseman!" we cried in the innocent enthusiasm of our leading article, after reading Nansen's first telegram to the *Daily Chronicle*, for we thought he had achieved a magnificent success. But a paper by Mr. A. M. Brice in the December number of the Geographical Society's *Journal* shows up our error: "the drift of the Fram—full of marvel though it be—has only extended our knowledge a few degrees in one direction."

When this paper was read, the President of the Geographical Society assured the meeting of the brilliant ability of its author. Its estimate of Nansen's results appears therefore with some authority, and we turn eagerly to see what were the achievements wrought in Franz Josef Land, beside which those of Nansen pale. The map illustrating the paper represents two years' work in Franz Josef Land. It is nothing that we are disappointed when comparing it with the work done by Payer in one month's march, or with the survey made by Leigh Smith in two short cruises along the south coast; for if Mr. Harmsworth, who has so generously paid for the work, does not grumble at the time, no one else need object—except when previous explorers are unfairly treated.

What is the great result of this two years? Mr. Jackson left his village at Cape Flora "to discover if possible, new land to the westward." This boat-cruise resulted in the "discovery" of a majestic headland, which "newly found cape received the name of Cape Mary Harmsworth," a discovery which, it is claimed, "has advanced Franz Josef Land a considerable distance westward of the previously known limit," and rediscovered Gilies Land. Interested in this great discovery, we turn to the map only to recognise in the "newly found cape" our old friend Cape Lofley, and to see that the southern coast of Franz Josef Land has been extended westward no further than it was already known from the work of Leigh Smith. That courageous but modest explorer was unable to take his yacht the "Eira" further west than the entrance to the fjord, which he named Cambridge Bay.

He saw to the west two capes forming the south-eastern and apparently the south-western extremities of the island called Alexandra Land: the former he named Cape Ludlow, the latter, Cape Lofley. Cape Lofley, though seen only in the distance, stood up distinctly as a bold rocky headland, and was marked on Leigh Smith's chart as far on one side of Cape Ludlow as Cape Crowther is on the other, a position exactly agreeing with that assigned in this new map to Cape Mary Harmsworth. Not that the name Cape Lofley has disappeared: Jackson has removed it to an insignificant point between Leigh Smith's Cape Lofley and Cape Ludlow, a point which Leigh Smith, considering the distance which the "Eira" was from it, could not have distinguished from the land behind it.

Mr. Jackson's detailed map of the southern coast of Alexandra Land will doubtless be of great value when this part of the world is divided into building lots; but the claim that his exploration has advanced Franz Josef Land "a considerable distance to the westward of the previously known limits" is not supported by his map. The identification of Alexandra Land with Gilies Land is probably correct, but is by no means a new idea. Markham's "Threshold of the Unknown Regions" includes Gilies Land as part of Franz Josef Land. Reference to Sir Clement Markham's admirable work reminds us of another instance of the Jackson-Harmsworthian method of treating the names of their predecessors. Payer named the great sound which extends along the south-eastern shore of Zichy Land, and thence to Cape Fiume, "Markham Sound." Leigh Smith accepted the name and mapped the south-western end of the sound. Now on the present map this great sound is re-christened the "British Channel," while "Markham Sound" is restricted to an unimportant passage between the island on which stand Payer's Capes Fiume and Triest, and the three opposite islands. We regret a proposal to remove the name "Markham" from the channel that is likely to be the main route followed in future exploration of the area north of Franz Josef Land.

Perhaps the most valuable item of information given by Mr. Jackson's map is the position of Nansen's winter quarters; for it enables us to understand that explorer's criticism of Payer's map. According to one of Jackson's letters :" Payer's map being so utterly wrong, had completely fogged Nansen; and at his winter hut, he believed himself near Cape Lofley, and that the land to the westward, which we had discovered (!) was Spitzbergen." Nansen, therefore, according to Mr. Jackson was very considerably fogged, for he was about 13° out in longitude and three-quarters of a degree in latitude. No wonder then that he could not reconcile his observations with Payer's map, and the fact that he did not see Petermann Land where he expected it, is no proof of its non-existence.

Nansen, however, may pluck up heart of grace. His journey will yet live in history, thanks to the magnanimity of his rescuers.

Finding that "Nansen has very little of his own to name," and that he was apparently disappointed at being "unable to name anything in the neighbourhood where he passed a winter under such conditions," Mr. Jackson offered to allow Nansen to name the island. This offer Mr. Jackson's expositor describes as an act of "single-mindedness, generosity, and an entire absence of anything approaching a petty sense of rivalry . . . difficult to beat in the annals of exploration." Jackson remarked that the naming of this country "only rests between himself, [Nansen] and me." The fact that it had been previously named the Karl Alexander Land by Payer, as the new map in the *Geographical Journal* shows, is a trivial detail that Mr. Jackson seems to have overlooked. Nansen, however, appears to have understood this; he is a man of humour, and entered into the spirit of the joke. So he promptly called the country "Frederick Jackson Island," apparently as a mark of his appreciation of the originality of the discoveries of his rescuer, whose gracious permission to re-name an already named island has conferred immortality upon the little trip of the "Fram."

The Value of Morphology.

The suggestive paper by Mr. MacBride, appearing in our present number, states many of the problems now engaging the earnest attention of biologists, botanists no less than zoologists. The great question before us really is: What do we mean by Biology; what is the aim of our research? Put broadly, the object of those who think on such matters is no doubt to fit all the phenomena of life, both physical and psychical, into some general scheme of the philosophy of the universe, alongside the phenomena of inorganic matter. If once this could be accomplished, all questions of minor philosophic, but major practical, interest would receive their answers by simple deduction. Till then, of course, our energies will constantly be distracted towards the solution of problems bearing on the daily life of our own species. But setting bread-and-butter considerations aside for the moment, and placing before us only the grand enigma, we have each of us to test our own line of investigation by its concordance with this main direction of philosophic enquiry.

Mr. MacBride discusses a few of the methods of zoology, but chiefly the morphological method. How far, he says in effect, does the comparison of the structure of the various species and genera of animals shed light on the relation of life to not-life, on the steps and the causes of evolution, on the problems of heredity, and on other obscurities that need illumination? This line of attack has been much criticised of late: there are earnest workers, to whom shallowness of thought cannot be imputed, who reject the methods of dissection, description, minute comparison, and systematic analysis of species. They say that these methods will not stand the touch-stone; that they are barren of philosophic fruit. Mr. MacBride does not agree, or, at all events, he is not ready to swap horses yet. While realising the value of Statistical Zoology, of Experimental Embryology, and other new methods, we, too, are not inclined to discard either morphological or systematic investigation. Indeed, we consider that morphology is only just beginning to see the size and shape of the field that it has to till. But we trust that this paper may open a discussion of much interest and value.

"A Plague o' both your Houses!"

After all, what do we know of the plan of the universe? We do not even know that there is a plan. Our need still is for facts. And the immediate value of any method or any theory is its incitement to collect and to discover fresh facts. Within the limits of biology we seem to see many hypotheses at present incapable of proof, but many methods of research all capable of advancing knowledge. We would suggest that the present is not the time for speculation on the merits or failings of Natural Selection or any other theory as to origin of species. A truce to library papers for ten or twenty years! Meanwhile let those naturalists for whom the subject has any fascination forget their pet theories, and set to work. They may find a pattern in a familiar series of green-bound books published by Murray. Perhaps at the end of the suggested period it may be possible to formulate important results based on experimental evidence, say, for instance, whether variation is indefinite or definite; to understand something of correlated variation and of the extent to which it must be considered as a factor in the problem, and so on, and so on. Perhaps we may seem no nearer the solution of these and similar questions. But a great deal of useful work will have been done, our stock of knowledge will have been increased, and we shall not have to lament the waste of valuable time on empty discussion. We are urged to these remarks by a paper read at a recent meeting of the Linnean Society, entitled "Does Natural Selection play any part in the Origin of Species among plants?" In the opinion of its reader it did not, the key of the whole problem being the reaction of plant and environment. This view is, of course, not a new one. Nor were any new experiments advanced in support of it. The chief evidence put forward lay in the production of several of our useful vegetables, like the parsnip and carrot, from a wild plant by cultivation. The same and similar facts, in the opinion of many who, following Darwin, find in Natural Selection a conceivable hypothesis, point in the opposite direction. In view of such marked disagreement it is manifest that existing evidence is insufficient. There is nothing for it but to continue the patient plodding investigations initiated by the author of the "Origin of Species" and (let us not forget) of all those other works choke-full of experiment, ingeniously planned and carefully carried out.

Alternation of Generations in Vertebrates.

We have had some difficulty in finding an appropriate heading for this comment on a recent treatise by Mr. John Beard, entitled "On Certain Problems of Vertebrate Embryology," and published at Jena by Fischer. It deals incidentally with speculations as to the ancestry of vertebrates, such as Dr. Gaskell's theory of arthropod origin alluded to in our November number; it discusses many particular problems of embryology, such as the fate of the yolk and Mr. Beard's own discovery of an ephemeral nervous system in vertebrate larvæ; it has also an important bearing upon what is called the recapitulation theory. But probably the central and most important idea in it is the suggestion that all Metazoa, and in particular the Vertebrata, have representatives of the two generations well known in plants, the sporophyte or asexual generation, and the gametophyte or sexual generation. Mr. Beard's position among vertebrate embryologists is so assured, and this particular volume is so important a summing-up of his work for the last eight years, that specialists certainly will study the treatise for themselves. We shall attempt, therefore, in an untechnical fashion to set forth the general lines of his brilliant theory.

Two types of larvæ are familiar to the naturalist, although in many instances there is difficulty in determining under which type particular larvæ must be grouped. In one of these types the larva, although it may attain a large size and be provided with special organs, is no more than a bearer of the growing adult. The real embryo appears as a small bud in some part of the large bearer, and gradually grows until a point is reached at which it has become more important than its nurse. The nurse or bearer then degenerates, and frequently its broken down tissues are absorbed and digested by the growing embryo. Such a larva, familiar among the echinoderms, may be compared with the sporogonium of a moss, and, indeed, Mr. Beard declares it to be, like the sporogonium, an alternating, sexless generation. On the other hand, there are larvæ, like tadpoles, and probably like those of at least most insects, which pass directly into the adult form, changes no doubt taking place, but yet the general structure of the larva being directly transformed into the general structure of the adult. Such a larva may be compared to a young seedling, the leaves and shape of which may differ from the adult but which directly becomes the adult. Mr. Beard's great thesis is that in all vertebrate development there is a stage easy to make out and directly comparable to the first kind of larva, that is to say, to an asexual, alternating generation. At a definite point of the development, different in date from the beginning of the development in different vertebrates but corresponding very closely in the differentiation of organs attained, what he calls the critical stage is reached. The embryo has become a fœtus; the main lines of the future organism have been laid down, and a sudden change in the mode of nutrition marks the degeneration of those structures which correspond to the asexual generation. Thus in a dogfish at the critical stage the embryo is definitely a dogfish, with signs of the peculiar snout, paired and unpaired fins. Up to this stage the nutrition has been through the medium of special cells of the external yolk-sack which have emulsified, and, so to speak, made pap of the yolk. After this stage, the yolk is gradually drawn up into a new and internal yolk-sack, formed from tissue which becomes an actual part of the future fish; and the yolk is digested directly by the alimentary canal of the future fish, the pancreas of which has begun to secrete. The external yolk-sack disappears. A corresponding stage Mr. Beard thinks to have made out, or at the least to have pointed to, in all the vertebrate groups. In the case of opossums and marsupials without the allantoic placenta described by Mr. Hill, the embryo is born at the critical stage, and, as the yolk-sack of these and higher animals is already devoid of yolk, the change in nutrition is not from external to internal yolk-feeding, but to the new diet of milk. Mr. Beard considers that the milk-glands are older than the true placenta, and that their coming into play marks the change in the nature of the embryo. In animals with an allantoic placenta the critical stage is marked, not by birth, but by change from yolk-sack placenta, such as is found in the marsupials, to the allantoic placenta, which is an outgrowth from the true embryo, not from the yolk-sack. As the allantoic placenta became more important, the sphere of the mammæ became less important, and indeed Mr. Beard looks to the ultimate disappearance of suckling. Thus, in some ungulates the young are nourished so long in the uterus that a few hours after birth they are able to feed on grass. As having a possible bearing in favour of his view that nutrition by milk is older than nutrition by a true placenta, we may remind him that one of the earliest signs of pregnancy in woman is change and activity in the milk-glands.

At the critical stage in all vertebrate embryos, a similar degree of development has been attained: the central canal of the nervous system has begun to form, constriction of the notochord has begun, the sex has been determined, and the permanent kidney has been formed. But where Mr. Beard charges against the theory of recapitulation is this: at the critical stage the final characters of the adult are sufficiently marked to determine the young embryos as respectively fish, amphibian, reptile, bird, or mammal. Indeed, Mr. Beard goes further, and declares that in the case of the human embryo the critical stage is definitely human. To put the difficulty plainly: in its crudest form the recapitulation theory supposes that the mammalian embryo passes through a fish-like stage when the gill-slits are developed. Mr. Beard declares that before the gill-slits have appeared the embryo has already assumed definite mammalian characters: that it becomes a mammal before it becomes a fish.

It would be unfair and unwise to criticise Mr. Beard's conclusions when we have found too little space for his arguments; but it is worth pointing out that when Mr. Beard carries so far back in ontogeny the meeting place of the different vertebrate groups, he is only taking an unusually long step in the direction in which zoologists generally have been proceeding. At first, the attempt was made to arrange vertebrate groups in linear form, so that on the recapitulation theory any high mammal would pass through a number of stages corresponding, as one traced it backwards, first to lower mammals, then to a series of reptiles, then to a series of fish, and so on. Now zoologists have been busy at work arranging the groups in diverging lines from remote common ancestors. In fact, we should not be surprised to see it argued on the grounds of comparative anatomy that mammals, Sauropsida, and Ichthyopsida had come independently from a common stock.

Although the law of recapitulation was drawn from considerations of mammalian embryology, upon which Mr. Beard places an interpretation opposing it, we are prepared to support the apparent paradox that a mammal may have been a mammal before it was a fish, and may yet recapitulate in its ontogeny some of its ancestral history.

Wild Hybrids between the Arctic and Common Foxes.

One of the causes which is most frequently invoked by various persons to account for the presence of unexpected characters in animals is crossing, and yet, as a matter of fact, hybridisation occurring in the wild state and not in a menagerie, is one of the very rarest phenomena possible, and, within the group of Mammalia, the undoubted instances of it known might almost be counted on the fingers.

This being the case, we need make no apology for drawing the attention of naturalists to Dr. Einar Lönnberg's interesting account, published in a Swedish sporting journal, of an undoubted case of hybridisation in the wild state between those very different species, the Arctic and Common Foxes. ("En Bastard mellan fjellräf och vanlig räf," in *Svenska Jägarförbundets Nya Tidskrift*, xxxiv. pp. 154–164, with coloured plate, 1896.)

In this paper Dr. Lönnberg carefully describes two hybrid specimens between the above species, now in the Upsala Museum. He conclusively shows their intermediate characters, and has happily been also able to make out something of their history. For in a private letter he tells us that no less than nine hybrid foxes were shot or trapped within a period of two years, and these are clearly shown to have been the offspring of a female Arctic fox, which had mated with the common foxes of the district of Öregrund. He furthermore tells us, which is highly important, that the place where the hybrids were bred is far south of the usual limit of the Arctic fox, and that no other specimen but the presumed parent had ever been seen there.

It is, no doubt, very difficult to obtain direct and conclusive evidence of such cases of wild hybridisation as may occur, but it may be remarked that of thousands of specimens that have passed under the writer's ken during the last twenty years, only one single individual could be at all confidently presumed to be a hybrid, and in that case, just as in this, the occurrence took place in a district where, owing to much shooting down of game, one or both parents might have found much difficulty in finding a mate of their own species.

Where this difficulty does not exist, owing to the abundance of both forms, there seems, as yet, no evidence that crossing ever takes place among mammals in a state of Nature, whatever may be the case with birds. But the very absence of such evidence makes it the more important that every apparent case should be properly investigated, and we are therefore very grateful to have Dr. Lönnberg's careful account of the Bastard Swedish Foxes.

The paper is illustrated by an excellent figure of one of the hybrids, showing very clearly how intermediate in colour it is between the two parent species.

The Biology of the Great Lakes.

In connection with Mr. Scourfield's plea for a British fresh-water biological station, appearing in this number, it is very instructive to note the increasing attention paid to the study of fresh-water animals and plants in the United States. As a good instance of this may be taken the work of the Michigan Fish Commission. This Commission not only performs the practical duties of fish-hatching and "planting," but has also carried out a considerable amount of valuable biological work, as may be seen from the six *Bulletins* it has already issued. The ultimate object in view has naturally been a practical one, namely, to ascertain the cause of the decreasing yield of whitefish (*Coregonus clupeiformis*) in spite of artificial propagation, evidently a subject of some importance to the State of Michigan, touching as it does four of the five Great Lakes. Recognising that the only basis for intelligent action in the future would be a more complete knowledge of all the conditions surrounding the whitefish, the Commission very wisely began a biological examination of the Great Lakes. During the summer of 1893 a party of naturalists, under the direction of Professor J. E. Reighard, was maintained on Lake St. Clair, a great spawning-ground of the whitefish, lying between Lake Huron and Lake Erie, and in the following year a similar party, under Professor H. B. Ward, was stationed at Charlevoix on Lake Michigan. In both instances every effort was made to get as complete a picture as possible of the whole aquatic fauna and flora. Most of the groups of any importance were worked out by specialists, whose reports, published either as separate *Bulletins* or as appendices to the two principal reports (*Bulletins* Nos. 4 and 6), contain a mass of information, of use not only to American workers but to biologists everywhere. The two reports on Rotatoria by Mr. H. S. Jennings, that on Cyclopidæ and Calanidæ by Professor C. D. Marsh, that on the Turbellaria by Dr. W. M. Woodworth, and that on the Parasitic Worms by Professor Ward, are especially noteworthy. But even more important than this work on the different groups of animals and plants was the application of the methods, recently developed by Hensen and Apstein, to the estimation of the amount and distribution of the "plankton," *i.e.*, of the minute plants and animals found floating free in the water, which form, in the last resort, almost the only source of the food-supply of fishes in large and deep lakes, and play a not unimportant part in this respect in smaller and shallower ones. The results obtained in this way are of the greatest scientific interest, confirming, as they do, many of the observations of Apstein, Zacharias, and others on the plankton of European lakes; nor can they fail to be of value in the practical work of fish-culture. The Commission is certainly deserving of all praise for undertaking this biological examination of the Great Lakes, and we can only regret that the work cannot be carried on continuously, instead of being limited to a few weeks in the summer.

In connection with this subject, we may mention that we have received a copy of "The Illini," the weekly journal of the University of Illinois, giving an account of its biological experiment station, the first to be started in the United States with an adequate equipment. This station also differs from others in having for its basis of work the system of life in a river. The article, which is written by Mr. C. A. Kofoid, has some excellent illustrations, and we hope it will soon be possible to give an equally interesting account of a British fresh-water biological station.

A Starfish "Struggle-for-Lifer."

There lives in the Mediterranean, as well as in the adjacent parts of the Atlantic, an ally of our common crossfish, known as *Asterias richardi*. Professor Perrier, who first described the species, noticed the peculiar fact that the young were provided with six arms, whereas the adults had no more than five. Since he also observed cases in which three of the arms were much smaller than the rest, he assumed that the species was accustomed to reproduce by fission during its youth, but that in its old age it sobered down to the more usual method of reproduction. It appears, however, from the observations of Dr. E. von Marenzeller, that the arms are thrown off, after the not uncommon custom of echinoderms, while the disc that remains behind buds out fresh arms (*See* Natural Science, vol. v., p. 4, July, 1894.). The curious point remains to be explained, that the young is more given to this operation than is the adult, as well as the fact that in old age there seems to be a cessation of regeneration, so that some individuals have only two or three arms.

The reason, according to Dr. von Marenzeller, is as follows:—

There is a small parasitic animal, whose affinities can hardly as yet be considered fully determined, but which is well known as a parasite in and upon crinoids, and is called *Myzostoma*. A species of this enters the mouth of *Asterias richardi*, and, taking up its abode in the blind extensions of the stomach that pass into the arms, it there grows to a size and breadth that are quite remarkable for a *Myzostoma*. The *Asterias* soon feels inconvenienced, but, being unable to get rid of the intruder by the way through which it entered, it is compelled to adopt the rather severe measure of getting rid of the arm in which the *Myzostoma* has esconced itself. The more numerous the parasites, the greater the number of arms that have to be disposed of in this summary fashion. At first the starfish, endowed as all starfish are with strong powers of recuperation, re-grows all its six arms; but after a time it finds it less trouble to close up the gap, and to content itself with five arms. And since by these repeated struggles in its youth its powers have been reduced to a greater extent than is customary in this class, it often finds itself forced to hobble out the remainder of its days with the miserable allowance (for a starfish) of three arms. The same parasite is found to occur in *Stolasterias neglecta*, another starfish with the same distribution, and it appears to have like effects. This case of autotomy or self-division has, then, no connection with reproduction ; it is a simple example of a continuous struggle for life.

The complete account of this will be found in Dr. von Marenzeller's report on the Echinoderma collected in the Mediterranean in 1893–4 (*Denkschr. Akad. Wien, Math.-Nat. Classe*, lxii., pp. 123–148, 1895).

An Eastern Question.

What has Japan got to do with the East Coast of America ? More than once of late have we drawn attention to curious instances where the distribution of a genus was confined to these two localities (vol. viii., p. 10, the Chimæroid *Hariotta*, and p. 350, the Octopod, *Opisthoteuthis*) ; and now, on glancing at Dr. Sluiter's recently-issued Catalogue of the Starfishes in the Amsterdam Museum, we note that the well-known species *Echinaster spinosus*, hitherto known only from the east coast of America, has—if labels are to be trusted—been found on the shores of Japan.

A Revision of North American Slugs.

A paper recently published by Messrs. H. E. Pilsbry and E. G. Vanatta in the *Proceedings* of the Academy of Natural Sciences, Philadelphia (1896, pp. 339–350) marks a great advance in the study of North American slugs, and forms the first of a series which will prove invaluable to all interested in those animals. Writers on the slugs of America have hitherto been accustomed to distinguish them according to their external characters, an imperfect method of work which it may be remembered we have found it necessary to censure in other branches of systematic zoology. Here for the first time the introduction of new species is accompanied by descriptions and figures of the internal parts as well as of the external form, in a manner similar to that employed by such English and Continental malacologists as Simroth, Lessona, Pollonera, Godwin-Austen, Collinge, and Scharff. The present instalment treats of *Ariolimax* and a new genus *Aphallarion*. Two species of the former genus are figured and described, and descriptions given of varieties and insufficiently known forms. The genus *Aphallarion* is constituted for a new species, *A. buttoni*, from California, which, although very like an *Ariolimax* both externally and internally, differs in the complete absence of a penis and retractor muscle, agreeing in this feature with the condition which obtains in *Arion* and *Prophysaon*.

The *Ariolimax* from Costa Rica in the British Museum, described as a sub-species by Cockerell, is treated by the present authors as probably a good species "on account of its locality (if correct), but a diagnosis is still wanting." Perhaps Mr. Cockerell might retort that it is only a "full diagnosis" that is wanting. That may be; but how many forms there are in our valuable national collection of which we have as yet nothing more than a preliminary description! how many varieties which may be good species, or species which may be only varieties! And these remarks do not apply to slugs alone.

All Smoke and no Fire.

The informal smoking evening of the Geological Society, to celebrate the displacement of gas by electric light, to which we alluded in our last number, was not quite such a success as had been hoped. The President was unable to attend, owing to an indisposition from which he has since recovered, and very few of the Fellows themselves were present. The President, however, had sent down some excellent photographic lantern slides illustrating points in the geology of Devonshire and Cornwall, and these were explained by Mr. W. W. Watts. Perhaps, as Fellows only were admitted to this meeting, we should not be in order in giving a lengthy account of the proceedings. Fortunately, it is unnecessary to dwell upon the magic lantern entertainment on the Holy Land which Prof. Hull might have borrowed from the S.P.C.K. to amuse the Fellows with; but we may allude to the successful attempt made by Mr. Teall to project actual rock-sections upon the screen. The exhibits, too, were neither numerous nor, with one or two exceptions, particularly exciting. Dr. Wheelton Hind exhibited the specimens of *Carbonicola* described in the monograph recently reviewed by us, also a series of goniatites intended to show the differences in the cephalopod fauna of the Carboniferous shales and limestones. Professor Grenville Cole sent up specimens illustrating his paper on the igneous intrusions of Port

Glasdruman, Co. Down, and Professor Blake showed a sample of a ferruginous oolite from Cutch, the grains of which being covered with iron pyrites, caused it to be called "golden oolite."

THE ANTIQUITY OF MAN IN BRITAIN.

THE honours of this geological *soirée* went to Mr. W. J. Lewis Abbott, some of whose exhibits were indeed remarkable. From the Ightham Fissure alone he has increased Prestwich's list of thirty-seven British cave and fissure vertebrates to about ninety, all of which were shown, and among them one of the most interesting was *Canis lagopus*, the arctic fox. From the Hastings kitchen midden he has secured a large assemblage of diminutive implements, supposed for the most part to be fish-hooks, and to have been used by a peaceful race that in many parts of Europe were settled on the seashore, often in proximity to more warlike tribes. Concerning the customs of this race much information has been accumulated, and we hope in a forthcoming number to publish a paper by Mr. Abbott with illustrations of the extraordinary relics that he has found. He also had some remarkable specimens of stone-working discovered on the supposed sites of ruined cities of India. Their strangeness consisted in the fact that the stone had been chipped into almost perfect cubes and globes, a feat which the modern imitators of the stone-workers, including Mr. Abbott himself, are quite unable to perform; many of these specimens, too, were delicately ornamented, presumably by the burning of an alkali into patterns incised upon them.

But the interest of all these specimens was completely cast into the shade by some rough-looking stones lying on the table. These were flints which certainly bore a striking resemblance to the work of man, which we believe the most critical expert would say probably were the work of man, and which had been obtained by Mr. Abbott's own hands, in the presence of a witness, from the Cromer Forest Bed at Runton, where they were found sticking in the iron pan, portions of which were still attached to them. One of them showed an undoubted bulb of percussion. We shall publish next month an illustrated account of these specimens, which are among the most interesting evidences of human antiquity that have been turned up for many a long year. The Forest Bed, we may remind those of our readers who are not geologists, lies, according to Prestwich, at the base of the Pleistocene or Quaternary system, but is now usually regarded as forming the top of the Pliocene series; it contains remains of the cave-bear, of the rhinoceros, of the hippopotamus, various species of elephant, deer, and other species of mammals, both living and extinct. In this country, at all events, no one has ever professed to find the remains of man at so low a horizon, although the opinion has before now been hazarded that if they occurred at this horizon at all, they would be found at the place where Mr. Abbott has actually discovered them.

Giant Ammonites.

We understand that the British Museum (Natural History) has acquired a magnificent specimen of the ammonite *Haploceras leptophyllus*, Sharpe, from the Upper Chalk of Rottingdean. It was one of seven that were to be seen this summer between Brighton and Newhaven, the largest of which reached the gigantic size of 5ft. 6in. across, without the body chamber, but was almost destroyed when discovered, and could not be moved. The specimen now at the Museum is in a perfect condition, is 44 inches across, and shows a few inches only of the body chamber. We believe that Mr. Crick will contribute a note on its features and peculiarities to the *Geological Magazine*, a note we shall await with considerable interest.

Had the Belemnite an Aptychus?

Our recent remarks on the relations between stratigraphers and palæontologists find an appropriate illustration in a paper contributed by Dr. H. P. Blackmore, of Salisbury, to the *Geological Magazine* for December. There are found in the Chalk, as in earlier Mesozoic strata, many of those peculiar bodies known as aptychi, and now generally accepted as being the opercula of ammonites, corresponding to the hood of the recent *Nautilus* (see Natural Science, vol. viii., p. 84, Feb., 1896). There is no question as to the correctness of this view in the majority of cases; but Dr. Blackmore, carefully collecting zone by zone in his own neighbourhood, established the facts "that *Aptychus leptophyllus* and *Belemnitella lanceolata* always occurred together, and that *Aptychus Portlockii* and *Belemnitella quadrata* were similarly associated," and "that *Aptychus rugosus* was associated with *B. mucronata*"; further, no ammonite or nautilus is known in the zone of *B. lanceolata* at this locality, but *Haploceras leptophyllus*, which occurs at a lower zone, is associated with a coarsely tuberculate aptychus of very different appearance. From these facts Dr. Blackmore infers that the three aptychi mentioned above belong to no ammonite, but to the belemnites with which they are respectively associated.

As to the value and suggestiveness of these facts, there is no question. Even if we doubt the main conclusion of Dr. Blackmore, we receive from him a new guide for the determination of these zones in all parts of the country; and such facts can only be obtained by the local collector and stratigrapher, whom we are doing what we can to encourage and assist. But when we turn to the zoological inferences of Dr. Blackmore, we find the existence and intervention of the specialist fully justified.

Dr. Blackmore figures specimens of *Belemnitella lanceolata* and *B. quadrata* [we follow his nomenclature], as evidence that the lower margins of the aptychi fitted the upper margins of the belemnite-guards; but as to the imperfection of these specimens no one acquainted with belemnite structure can have any doubt. Then again, if it were possible to accept the homology of these aptychi with the pro-ostracum, as advocated by Dr. Blackmore, it would be impossible to regard their median line of junction as "probably a continuation of the ventral sulcus or the siphuncle"; for whereas these structures are ventral, the pro-ostracum, as the schoolboy knows, is dorsal. In any case, the whole idea that the pro-ostracum of a belemnite could become detached and form these twinned independent calcified masses, is foreign to every conception of the cephalopod shell, and would require substantiation by careful comparative studies.

Until some observer with Dr. Blackmore's care and enthusiasm shall have discovered, *in situ*, specimens of these belemnites and their supposed aptychi in juxtaposition, we do not think the suggestion demands serious discussion. Not that we refuse to admit the possibility of such a discovery; only we should expect to find these aptychi dorsal in position, not representing or replacing the pro-ostracum, but lying within it at its anterior end. They would, in fact, probably occupy the position of the nuchal cartilage in *Sepia*, a structure which, as Von Ihering long ago pointed out, bears a striking resemblance to an aptychus in form, and which, it may be added, bears to the shell and soft parts much the same anatomical relation as does the hood in a nautilus, or as the aptychus is presumed to in an ammonite. Such a discovery would not strengthen the case for the dibranchiate nature of the Ammonoidea; nevertheless, it would be of such interest that we hope Dr. Blackmore and others will do their best to confirm the novel proposition now put forward by him.

JAMES HALL.

FEW persons with any pretensions to geological knowledge are not familiar with the works of James Hall, for sixty years the State Geologist of New York. At the recent meeting of the American Association for the Advancement of Science, held at Buffalo, a special celebration was inaugurated in his honour, and the veteran geologist was present to receive the congratulations and acclamations of his friends. The celebration was opened by Professor B. K. Emerson, and Professsor Le Conte spoke on behalf of the Geological Society of America. Professor Hall briefly replied.

To give any sufficient account of the contributions to science by this remarkable man, would be to fill a number of this journal. We can therefore only briefly refer to some of the chief labours to which his long and active life has been devoted. In 1836 the official survey of

New York was organised under the direction of Mather, Emmons, Conrad, and Vanuxem, James Hall being appointed assistant geologist. Within a year the districts assigned to these men were changed and Hall was placed in charge of the Fourth District. Other changes brought him finally to the charge of palæontology; and in the elucidation of the fossil remains of the State of New York, Hall has therefore laboured for upwards of sixty years. The noble series of volumes issued by the State, and known as the *Palæontology of the State of New York*, will remain a great and permanent monument of the untiring energy of James Hall, the sole survivor of that little band, the first generation of American geologists. Fortunately for geology in general, the United States has ever been famous for the singular liberality with which she has distributed world-wide the labours of her sons, and Hall's works are as accessible to English geologists as are Lyell's Elements.

But it is not in palæontology alone that Hall has gained his laurels; while hard at work upon the fossils of the State, he was also engaged in stratigraphical matters, and found time to deal with the broader philosophical questions which lie at the root of the subject. He wrote brilliantly on the loading of the earth's crust and on oscillations of level due to denudation and removal of pressure, and on the origin of mountains and sedimentation. He mapped the State of New York several times, and worked away on the complex relations of the rocks until their secrets were fairly wrested from them. As active, apparently, at 80 as he was at 25, James Hall's figure could be seen in all weathers tramping the hillsides in sunshine or snow, never seeming to tire or flag under any difficulties, and it was in this present decade that one of us drove in his trap at imminent risk to his life, driven as it were by a young buck of 20, instead of a veteran of almost 80. And even more remarkable still is the fact that the Professor crossed the United States from West to East to take part in this celebration, and has once again taken the field in active service to the science he has loved so long, rather than seek his well-merited repose.

Professor Hall has gathered round him a large following of the younger men, who have rendered him great service in carrying out his plans, and he would be the first to give them his ungrudging acknowledgments. We are glad to offer him our congratulations, and indeed those of every geologist in this country, and to hope that he may long enjoy that health, which alone makes a multiplicity of years a great and pleasurable gift.

A full account of Professor James Hall's life and work, with portraits, will be found in *Science* for November 13.

The Eocene Beds of Dorset.

In the August number of the *Quarterly Journal of the Geological Society*, Mr. Clement Reid has given a sketch of these deposits, our knowledge of which has been greatly enlarged owing to the re-survey of the western end of the Hampshire Basin. The London Clay thins to less than 100 feet in Dorset and becomes more sandy and pebbly, though still apparently of marine origin. The Woolwich and Reading series becomes more fluviatile than at Newhaven and Portslade, as it contains lenticular patches of subangular gravel to the west of Wareham. The Lower Bagshots also become coarser and more purely fluviatile westward, the change near Dorchester being singularly rapid. Mr. Reid's study of the composition of the Eocene gravels in this area shows him that the rivers which deposited them must have flowed from the west or south-west. The paper, of course, gives the details of which this note is the mere conclusion.

Botanical Scraps from America.

We have just received some portions of vol. x. of the *Proceedings of the Washington Biological Society*, containing small contributions to the botany of the mountain regions in the Western United States. C. Hart Merriam describes a new fir (*Abies arizonica*) from the San Francisco and Kendrick Mountains, Arizona, where it grows at an altitude of between 9,000 ft. and 10,000 ft. It is a tree about 40 feet high with fine-grained elastic bark, the creamy-white colour of which makes it a conspicuous object in the mountains. Its nearest ally is *Abies lasiocarpa*, a tree ranging from Southern Alaska and British Columbia southward, over the Rocky Mountains into Utah and Colorado, and over the Cascade Range to Southern Oregon. The smaller cones and broader cone-scales are distinguishing features of the Arizona fir.

F. V. Coville describes a new rush (*Juncus confusus*) from the Rocky Mountain region, and gives a synopsis of the group of (chiefly) American species to which it belongs. He also gives an account of a new currant (*Ribes erythrocarpum*) found growing near Crater Lake, Oregon. Crater Lake is described as "a remarkable body of the purest water, nearly circular in form, about six miles in diameter and 2,000 ft. in depth, without a visible outlet, occupying the bowl of an extinct volcano in the southern part of the Cascade Mountains of Oregon." The surface of the water has an altitude of 6,239 ft., and the surrounding cliffs rise 1,000 ft. to 1,500 ft. higher. The mountain slopes are densely forested, and as no botanist seems to have heretofore explored this part of the Cascades, the plants collected by Mr. Coville and his friend may prove of unusual interest.

I.

Wanted, a British Fresh-water Biological Station.[1]

ALTHOUGH there are fresh-water biological stations actively at work in Germany, France, Switzerland, Bohemia, (see NATURAL SCIENCE, vol. v., p. 370., Nov., 1894), Sweden, Finland, and the United States, besides several in contemplation in various other countries, the necessity for the establishment of a fresh-water station in this country has received very little attention. In fact, with the exception of the proposal to found such an institution in Norfolk (see *Transactions* of the Norfolk and Norwich Naturalists' Society, Vol. vi., Part 1., p. 108., also NATURAL SCIENCE, Jan., 1896, p. 8.), the very idea of working methodically at the fauna and flora of our inland waters never seems to have been seriously entertained by our biologists.

There can, however, be no question that from the purely scientific point of view, a knowledge of the life-histories, habits, and inter-relations of fresh-water organisms is of equal importance with similar knowledge concerning marine organisms. It may even be of greater importance in some cases, for there can be little doubt that many of the fundamental biological problems can be answered (if answerable at all by a study of aquatic organisms) more easily by observations upon fresh-water than upon marine forms, because their conditions of existence are less complex. From the practical point of view the claims of fresh-water biology are, it must be admitted, not so great as those of marine biology, but they are by no means inconsiderable, and will tend to become stronger with every increase in the amount of attention paid to fresh-water fishes as food.

Surely it is time, therefore, now that the more pressing need for British marine biological stations has been largely satisfied, and the anticipations as to their value are being steadily realised, to consider if the careful study of fresh-water biology in this country cannot be helped forward by the establishment of a properly equipped station. Comparatively, no doubt, a greater amount of work can be done upon fresh-water animals and plants without the aid of a special station, than is possible in the case of marine forms, but it is nevertheless true that there remain a long series of problems in fresh-water biology which will never be solved without the continuity of observations practically only to be secured by means of a station definitely

[1] Paper read before Section D of the British Association, September 18, 1896.

working towards this end. And these problems are, for the most part, not the narrower specialist questions, but those of great general importance bearing on vexed points of variation, heredity, selection, and the influence of environment.

As to the position of such a British station, it would probably be well, for reasons of accessibility, to limit the possible situation to England and Wales, although the claims of several localities in Scotland and Ireland are not light. Undoubtedly the three districts in England and Wales offering most suitable conditions for the establishment of a fresh-water station, are the Lake District, North Wales, and the Norfolk Broads. The claims of the Lake District and North Wales are very much of the same character, though probably the former has a somewhat richer aquatic fauna and flora than the latter. In both cases some of the principal work to be done would be carried out upon the free-swimming organisms constituting the fresh-water "plankton." That the detailed study of these forms is likely to lead to important results may be seen from the following considerations. The number of species constituting the bulk of the plankton of fresh-water lakes is remarkably small, the conditions of existence are comparatively simple and uniform, and the certainty with which typical collections may be made is at its maximum. In fact, in studying the fresh-water plankton one gets, under perfectly natural conditions, almost the simplicity of an artificial experiment, and it is here, if anywhere, that some of the problems of selection, variation, and heredity will find a satisfactory solution. Many other lines of work besides observations on the plankton could easily be undertaken by a station in either of the two districts named, but the latter would still form its characteristic feature. The Norfolk Broads and rivers, on the other hand, offer little opportunity for plankton work, but possess the unique character of presenting continuous and nearly imperceptible gradations from absolutely fresh to brackish, and even salt water. This is a condition, the influence of which has never been sufficiently studied, and has certainly never been touched by any existing biological station. That this peculiarly favourable example of gradation from fresh to salt water is leading to important changes in the habits of various species is almost certain, and it has already been shown for one group of animals, viz., the Ostracoda (see Brady and Norman's Monograph of the Ostracoda, &c., Royal Dublin Society's *Transactions*, 1889 and 1896), that several species, which are typically brackish (*Cypridopsis aculeata*, *Cythere pellucida*, *C. fuscata*, &c.), are now living in broads and dykes that are quite fresh, whilst other forms, essentially marine (*Cythere antiquata*, *Loxoconcha guttata*, &c.), have succeeded in pushing their way into the brackish waters of the district. These facts suggest a subject for systematic observation which should be taken up by a biological station on the Norfolk Broads as one of its most important duties. It is certainly not too much to hope that in this way some very important additions to our knowledge of the origin of fresh-water faunas would be made in the course of a few years. As in the case of a station in the Lake District or North Wales, in addition to the more characteristic work determined by the nature of the locality, a considerable number of valuable investigations in fresh-water biology of a more general nature could be carried out on the Norfolk Broads, and it is also to be borne in mind when discussing the utility of a fresh-water biological station, that, wherever it is situated, the mere working out of the aquatic fauna and flora of the immediately surrounding district, which is almost essential as a preliminary step to deeper investigations, would be in itself no small gain to science.

The cost of a fresh-water biological station would depend somewhat upon whether it was floating or fixed. A floating station would no doubt be cheaper in the first instance, but a fixed station offers much greater accommodation for workers, and also greater facilities for experimental investigations. The minimum initial cost, however, even of a floating station, could not be much under £500, and in addition to this it would be absolutely essential to provide for the salary of a trained biologist. If the undertaking is to be in any measure a success, there must be one man at least to live and work continuously at the station. Anything less than this would prevent the continuity of endeavour which alone lifts such a station above the level of an attempt to catalogue the local fauna and flora, an undertaking which, however praiseworthy in itself, is not likely to receive much more than local support.

Compared with the large sums spent on marine biological stations, the amount required for a fresh-water station, even if provided with a little more than the minimum outfit, is very modest, and it is scarcely necessary to advocate the formation of a special society to carry through the proposal to found such a station. At any rate, it seems clear that in the present state of public opinion on the question of the value of fresh-water fishes as food, such an association could not be formed on the broad basis of the Marine Biological Association; but the work could probably be accomplished without the aid of a new society. A little co-operation on the part of the many existing Institutions interested in biology, with a local society willing to undertake the work of organisation and supervision, seems to be all that is required. At least, so far as the establishment of a station on the Norfolk Broads is concerned, this method would be sufficient, for there is the proposal of the Norfolk and Norwich Naturalists' Society already in the field, and it would be a great pity if, from want of support, a scheme should be allowed to fall through which, if carried out, would not only add materially to the general stock of scientific knowledge, but would also remove the reproach that the United Kingdom is almost the only country in Europe without a fresh-water biological station.

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II.

The Position of Morphology in Zoological Science.[1]

A PAPER which proposed to insist on the cardinal value of morphology in the science of zoology, would have been held some years ago to be an entirely superfluous statement of a universally recognised truth. It cannot, however, be denied that there has been growing up amongst zoologists in the past ten years a feeling of dissatisfaction with morphological methods, and a tendency to disparage altogether morphology as a means of research. This feeling finds expression, for instance, in Ray Lankester's article on Zoology in the "Encyclopædia Britannica," where we read that "pure morphography has long since ceased to be a principal line of research," and that the attention of young students should not be confined to "what are now, comparatively speaking, the less productive lines of research." In Bateson's "Materials for the Study of Variation" we meet the broad statement that the morphological method has failed. Driesch and others who work on the same lines estimate the value of morphology at a very low figure.

I propose in the present essay to examine briefly the rival methods which have been put forward as demanding chief attention from zoologists, and shall show that all of them, valuable as they undoubtedly are, suffer from defects from which morphology is free. I shall further inquire to what causes the feeling of discontent with morphological methods is due, and finally I shall tentatively indicate certain ways of dealing with morphological facts which seem to me likely to be free from the objections which have been raised to other methods, and promise to throw fresh light on the general problems of zoology.

The lines of research which have been put forward as being more important than morphological investigations and comparisons, are mainly three, viz., Experimental Embryology, Study of Individual Variations, and Statistical Studies in Variation, or Mathematical Zoology. All are avowedly attempts to solve one of the chief problems of zoology, viz., the nature and causes of animal change or variation. Experimental embryology founded by Roux and carried on with such success by Driesch, Hertwig, and Loeb, has for its aim the isolation of the various processes the conjoint working of which con-

[1] Paper read before Section D of the British Association, September 22, 1896.

stitutes ontogeny. It is therefore to be looked on as a new kind of dissection. Many embryologists have attributed to the visible units observed in a segmenting egg, independence of growth and a definite function in building up the organism. The experimental embryologist, however, by forcing these units into abnormal positions, or by isolating them, is able to demonstrate that such attributes are impossible, and that the real factors into which development is separable are not identical with the visible differentiation which the germ exhibits. Driesch distinctly states that this dissection is the whole aim of experimental embryology, but some of his followers seem to imagine that by such means it is possible to discover the ultimate causes which bring about variation. This idea is, I think, a totally mistaken one. No examination of the changes which, by the application of physical or chemical means, we are enabled to bring about in the outer structure of the organism, will bring us one step nearer the discovery of those causes which are able to modify its inner hereditary potentiality. We may, like Driesch, force that part of the egg which normally produces the head to give rise to the tail, or, like Herbst, turn endoderm into ectoderm; all we arrive at is a resultant of the combined working of hereditary tendency and effect of environment.

The study of individual variations stands on a different basis. The modifications which species undergo are *ex hypothesi* made up of a summary of individual variations, and it seems quite proper to begin with a consideration of these; but at the outset we are met with a most serious difficulty. We have no means of distinguishing modifications, which have been produced by the action of environment on the particular individual we examine, from those due to variations in its hereditary qualities. Still further, the individual variations which appear most prominent to us are often those which there is strong reason to believe have never taken part in the evolution of species. In his "Materials for the Study of Variation," Mr. Bateson has collected numerous instances of supernumerary digits in Vertebrata and of branched legs among the Insecta; yet, in all the hundreds of species of Mammalia, and the hundreds of thousands of species of Insecta, no solitary instance is known where a variation of this kind has become the distinguishing mark of a species. Even of those changes due to variation in the hereditary powers, it is clear that only a small proportion concern us. For, in order to become a real factor in evolution, it is necessary that a variation should not only be transmissible to the offspring, but further, that it should occur sufficiently frequently to give natural selection an opportunity of taking advantage of it.

The Statistical Study of Variations, which we owe to Dr. Galton and Professor Weldon, cannot, I think, be assailed on theoretical grounds. This method aims at representing in a curve, not only the extent of variation in a given character met with in a species, but also the number of individuals exhibiting any particular variation. We can thus see at a glance the proportion between the average and the departures from it, and if we should find, by comparing curves constructed from the examination of individuals of different ages, that the death-rates of the possessors of different variations were different, we should have reasonable ground for supposing that natural selection was weeding out certain variations. We should further have an answer to the question whether the species was being modified or not—this answer being positive or negative according as the death-rate was greater on one side of the average than on the other, or equal on both sides.

The main objections which can be urged against this method are of a practical nature. First, we isolate a character, and try to determine whether or no its possessor suffers by its presence. But "characters" are mere mental conceptions, they do not exist by themselves, and natural selection acts, so to speak, on the balance of all the characters. Of course, if we could prove that individuals possessing a certain character had a lower death-rate than individuals without it, we might expect that that character would become a distinguishing feature of the species, though the survival of those that possessed it might not be due to its utility, but to some constitutional peculiarity of which it was the by-product. How are we to determine such an association of death-rate and variation? The attempt has been made by drawing a curve for the same character in the case of young and of older animals. The curve was flatter in the case of the younger animals; that is, the number of deviations from the mean was found to be greater in them: hence it is assumed that these abnormal forms are continually being weeded out. It is possible, however, that there may be a self-regulating tendency in growth. Are not abnormalities observed in children often toned down in later years?

There is another objection, the validity of which would not be admitted by many, but which seems to me to have a certain weight, and that is this: So far as we can discover, the condition of the organic world has remained relatively stable during the historical epoch, and no new species are known to have been formed. It is a question, therefore, on the one hand whether the conditions which brought about the great variability of which we see evidence in the geological record, still persist, and if not, whether the process of evolution be not so slow, that the infinitesimal period over which our observations can extend gives us no clue as to its direction.

From the standpoint of morphology it seems to me that we are free from all these difficulties. We take as our units not individuals but species, and if the doctrine of descent be admitted, we must infer that specific characters have themselves been of importance in the struggle for existence, or have been inextricably bound to others which have. If specific characters then are our material, certainly they offer a sufficiently wide field to work on and give us a unit small enough to start with, since there are many specific distinctions far less obvious than many individual variations. Many specific characters are doubtless merely associated with other characters which really determine the survival of their possessors, but this is true to a less degree of generic characters, and as we pass over to family and ordinal groups we get on safer and safer ground in assuming that we are dealing with features directly due to natural selection.

Why then has this feeling of dissatisfaction with the method of morphology grown up? The main reason is, I think, a conviction that it proves too much. The most discordant views as to the relationship of animals and their ancestry have been drawn from the same facts, and there does not seem to be any court of appeal before which rival views can be brought. Then again, continued study has forced home the conviction, that the processes of evolution are much more complex than was at first imagined, and that so far from being a simple process from the less to the more differentiated, the converse, viz., *degeneration* or simplification of structure, is also going on; further, that similar structures are sometimes independently developed along different lines of descent, in virtue of what was called "parallel" or "convergent evolution," but termed by Professor Lankester *homoplasy*. Now the discovery of the great principles of degeneracy and homoplasy, whilst it explained many points, has caused considerable doubt as to the certainty of morphological reasoning. For really, when armed with the principle of progressive degeneracy as well as with that of progressive differentiation, there is no limit to the powers of the evolutionary theorist; one can derive literally any one animal from any other by first deleting all the obnoxious organs in the supposed ancestor, and then evolving any number of new ones. Then again, if similar structures may have been developed independently in two different stocks by the action of similar external conditions, where is one to draw the line? How far is one justified in relying on similarity in structure as a criterion of community of descent at all? These, I think, are some of the questions which have underlain the feeling of scepticism as to the value of morphology which has crept over many zoologists, and which have caused, I confess, much trouble and distrust in my own mind. I now venture to suggest ways of looking at morphological facts, which seem to me more fruitful than the ordinary methods, and which have given me fresh hope in the pursuit of morphological study.

First, it is a mistake to assume that in tracing a supposed line of descent we are at liberty to assume that any conceivable variation may have occurred, variations, for instance, the utility of which we are not bound to explain. I think if we take specific and generic distinctions as our units, we are bound to show that some parallel change to the one we postulate has in all probability taken place. Thus there has been an immense amount of discussion as to how the pentadactyle limb was derived from the fin of a fish; but no one, so far as I am aware, has examined with care the cases of those Teleostean fish, like some species of blenny, which come out of the water and seek prey between tide-marks, where a fin has been to some extent transformed into a supporting limb. I do not wish to dogmatise against the possibility of changes having taken place in past time altogether different in kind to those of which we find evidence in present specific differences; but I do maintain that if we assume such changes in explaining the present structures of animals we are on utterly unsafe ground. My suggestion then is that by carefully comparing and tabulating specific and even generic differences we may be able by induction to arrive at "laws of successful variation." An example or two will serve to make clear what I mean. If we review the group of the Lamellibranchiata, we are struck by such forms as *Teredo* and *Pecten*. *A priori* it is possible to argue that the idea that these were derived from the ordinary type of Mollusca is hypothetical. Practically all zoologists are agreed about it, no different explanation having so far as I know ever been suggested. Now my point is that cases like these give us definite data to go on; they really enable us to ascertain now,—what Bateson has looked forward to doing only in the remote future—that certain changes are possible to certain animals. Perhaps it may be retorted that all this is known and acted on. My reply is that many changes have been postulated, which have no analogue amongst variations that we know must have occurred. Balfour postulated the formation of a new mouth; Dohrn that of a new arm. All through the Mollusca we find no such fundamental change, nor the foundation of any new type skeleton such as the supporters of the Annelid theory are bound to postulate.

If we now turn to the difficulty of distinguishing primitive and undifferentiated from degenerated structures, we shall find, I think, that objections may be made to the current modes of reasoning on this subject. It is often implicitly assumed (1) that if an animal can be shown to be degenerate in one respect, it is not primitive at all, and throws no light on the ancestry of a group; (2) that any amount of degeneracy may be followed in the history of the race by any amount of forward evolution. I shall give reasons for questioning both suppositions. To prove that the first is a real factor in theorising, we have only to remember the war waged round *Amphioxus*. Few would deny that it is degenerate in some points; the question at issue is, Does it in its general organisation represent the ancestor of Vertebrata or not? Now it seems to me that on *à priori* grounds we have every reason to expect that all animals, which possess on the whole a primitive organisation will be degenerate in some features. For their modified relatives which have departed from the ancestral type have *ex hypothesi* been forced to do so by natural selection, and how have these primitive animals been able to escape from that pressure of environment which modified allied forms? In some cases, perhaps, isolation on oceanic islands, or in small lakes may have allowed them to do so; but in the case of what we may term phylum-ancestors such as *Amphioxus* and *Peripatus* such explanations are futile. The grade of structure they present, dates from such a remote epoch that no physical barrier will have remained constant in the interval, added to which their wide distribution at once negatives such an idea. No; the general method in which such animals escaped from the stress of environment, was by taking to burrowing or sessile modes of life, and this has inevitably carried a certain amount of degeneracy with it. The idea that active vigorous vertebrates are descended fnom a sluggish mud-eating worm like *Balanoglossus* must strike many people as highly improbable, but that *Balanoglossus* is descended from a free swimming form with well developed eyes we know from the structure of its larva to be extremely probable, and there is nothing violent in the supposition that this same form may have given rise to the Vertebrata. I think, if once the dictum, "All primitive animals are also in some degree degenerate" were accepted, a harmonious explanation of many discordant facts would be attained.

In this connection I may say that such phrases as "degree of modification" are in need of definition. I think many zoologists have admitted there are two kinds of modification at work: first, in the intensity of metabolism or the degree of vitality, by which we mean the degree of specialisation of the organs for carrying on the vital processes—digestion, respiration, circulation, excretion, &c.; and secondly, in shape and form of the outer appendages, or of the general shape of animals. Now, it is the first kind of modification to which zoologists attach weight. What we desire above all to know is how the complicated physiological mechanisms represented by the higher vertebrates have been built up. It is assumed that variations in form and size—the outgrowth of flaps and lobes, &c.—are things which may have been accomplished in a comparatively short time. When, therefore, zoologists speak of animals being primitive, they mean with regard to internal organisation. On grounds of the relatively greater importance of the latter, they separate the beaver and wombat, the great ant-eater and the pangolin, &c., &c. This distinction between internal and external structures is sometimes explained by saying that the latter are adaptive, as if all characters were not ultimately adaptive. Explicitly stated, the belief underlying such principles of classification is: Improvement and elaboration of internal structure and constitution is a slow process; modification of external structure is a rapid one, and has given similar results again and again. The Brachiopoda are a good instance of a group where the external form is highly specialised, but where inner differentiation is at a very low ebb. They exhibit a most primitive condition of the excretory, generative, circulatory, and nervous organs. These animals form a part of the oldest fauna known to us; but if the view which I have been trying to propound is correct, the conclusion that the date of the appearance of this fauna is enormously removed from the beginning of life, because it contains highly specialised forms, must be taken with considerable abatement.

Turning now to the question whether we can legitimately assume that degeneracy and differentiation have alternated in the history of a species—in a word, whether degenerate ancestors have given rise to active forms with highly developed sense-organs—let us glance at those cases where all will admit that a certain amount of recovery has occurred. One of the best-known cases is that of *Pecten.* There is strong reason for believing that all lamellibranchs are descended from more active forms provided with cephalic eyes and tentacles, and separate cerebral and pleural ganglia. Owing to their sluggish, burrowing mode of life, and gross non-selective feeding, these features have been lost. The oysters, to which *Pecten* is not distantly allied, have carried this degeneracy to its extreme point—the foot, one adductor muscle, and the pedal ganglia being likewise absent. *Pecten* has recovered the free-swimming life, and has developed a new set of sense-organs; but none of the lost structures have been recovered. I know of no other case where it is at all probable that recovery has taken place, and there is no ground for assuming that burrowing forms could ever give rise to active descendants. This brings us to the question, how primitive or undifferentiate can be distinguished from degenerate features. How are we to say, for instance, what features in *Amphioxus* we may regard as ancestral, and what as secondary; or in *Peripatus* or *Chiton*? I do not assert that any absolute criteria can be put forward; each case must be judged on its merits: but certain suggestions can be made. The criterion of primitive characters is their synthetic character—that is, they serve to link together different groups. Why are the gill and foot of *Nucula* said to be of a primitive character? Because they agree, in contradistinction to those of the lamellibranchs, with the gastropod gill and foot. An organ may be said also to be primitive if it combines functions distributed in other animals amongst different sets of organs. Thus, the cœlom of brachiopods combines the functions of excretory organ, generative organ, and body-cavity: so we regard it as being in a very primitive condition. Underlying such reasoning there is a principle I believe to be sound; it is that new organs are never developed from functionless rudiments—as Darwin at one time thought himself forced to believe—but arise by modification of pre-existing organs. Where the function of an organ is changed, the newer function must have existed with, but subsidiary to, the old one. Eyes and ears would thus be local spots of peculiar sensitiveness on a skin which responded to both light and sound. Degenerate organs are no doubt simplified, but not only are they not synthetic in the sense mentioned above, that they do not recall the conditions of affairs in other groups, but they are not correlated with the state of development of other organs. A good instance is found in the limbs of perennibranchiate Urodela. These are, it is true, constructed on the pentadactyle plan, but they are exceedingly feeble, and the number of digits is in almost every case reduced. They are thus unable to support the weight of the body, the purpose for which the pentadactyle limb was originally evolved, nor do they, on the other hand, show any of the characters of fins. Hence, we conclude that perennibranchiates have been derived from caducibranchiate forms—forms, at any rate, which walked more and swam less,—and the life-histories of *Siredon* and *Menobranchus* have now proved that we are right. Then, again, in the case of the head of *Amphioxus* we notice that it is asymmetrical, and that the brain and sense-organs are almost entirely obsolete. But we know that the whole organisation of Vertebrata is permeated by bilateral symmetry, and that they all have well-developed brains and sense-organs, and lead typically a free, roving life. To this life only can we attribute the fact that *Amphioxus*, like other vertebrates, is flattened in a vertical plane, since only in swimming could a body of such a shape be easily maintained in equilibrium; asymmetry and feeble sense-organs are inconsistent with such a life, and hence we interpret them as secondary features, due to the secondarily-acquired burrowing habits.

One of the most vexed questions in zoology has been the value of the evidence afforded by embryology. The older anatomists roundly asserted that the ontogeny of the individual was a recapitulation of the phylogeny of the race. They endeavoured to find ancestral meanings for all the embryonic structures which they had observed. Lately it has become fashionable to look coldly on such theorising, and some have even gone so far as to deny that there is any evidence that ontogeny is in any sense a repetition at all. I cannot but think that the latter class of zoologists are in much the same position as the theological opponents of the doctrine of evolution —they are most imperfectly acquainted with the facts. It must be remembered that comparative embryology is only an extension of comparative anatomy; that it is most arbitrary to say that only sexually adult forms are to be compared with one another; and that the conclusion that resemblance between the immature stage of one animal and the mature stage of another is indicative of affinity, is precisely on all fours with a similar conclusion drawn from a comparison with one another of two adult stages. But there are many cases where no one really doubts that the affinity indicated by ontogeny is correct; in other words, that in these particular cases ontogeny is a repetition of past history. Such cases are the pentacrinoid young of *Antedon*, with the conspicuous basals and orals, the cyclops-like larva of the parasitic copepods, the tadpole-like larva of the ascidians, and, in general features, the tadpole of the frog. Now, if there be this undeniable hereditary basis for ontogeny in some cases, it is exceedingly unlikely that it is a factor which is only sporadically present. The process of development of all animals from the egg is in all probability fundamentally the same kind of process in all cases, and made up of the same factors, though some may be more prominent in some cases than in others.

Of course, everyone admits that there are numerous features in ontogeny which are not due to ancestral repetition, but to subsequent modification of the larva, and the question is how are the results, due to these two factors, to be distinguished from one another. Now, an important step towards attaining this result has been made by Mr. Sedgwick in his theory of the relation to each other of the embryonic and larval types of development. This theory is briefly as follows: An embryonic stage of development is nothing but a larval stage, which has been sheltered from the external world, by either being enclosed in an egg-shell or retained in the body of the mother, and modified in consequence. The larva retains ancestral features, because it is subjected to ancestral conditions of life; when, however, as in the case of reptile ontogeny, it has become converted into the embryonic type, then a change, such as the loss of limbs, when it occurs, can affect the development as a whole, and not even embryonic vestiges remain, as is the case with snakes. On the other hand, so long as the larva of the frog lives in water, it must retain many fish-like features. I think, however, we should make a mistake if we limited the influences tending to retain ancestral structure to the outer conditions, when these latter happen to be of an ancestral type. It is to me impossible to suppose that the general form of insect larvæ, the vermiform shape, and the large number of almost similar segments, is not an ancestral feature; but in view of the extraordinarily diversified habits of these larvæ, we cannot suppose that the influence of outer conditions has retained it. If, however, we say that the larval form is the combined result of outer and inner conditions, we shall, I think, be nearer the truth. By inner conditions I mean the intensity of the metabolism of an animal, correlated with which is the differentiation from one another of the organs fulfilling the various functions; such a level of metabolism as the adult possesses being only gradually obtained, when the said level is a comparatively high one. Thus, when the long and comparatively undifferentiated nervous system of the caterpillar becomes converted into the concentrated nervous ganglia of the butterfly, there is no doubt that we have reached a higher level of life.

Suppose, then, that the ancestral features of ontogeny are due to the repetition of ancestral outer and inner conditions, we can make a rough estimate of characters which will *not* tend to be preserved by these causes, but which, for the most part, will be obliterated by subsequent modification. Chief among these is *size*: most larvæ are comparatively minute; only in a few cases does the larval stage rival in size the adult, while in very few cases (urodele amphibians) it tends even to supplant it. Now, along with reduction in size goes, in all probability, reduction in number of a series of homologous organs performing the same kind of function, since there is little doubt that the repetition of members is a phenomenon of fundamentally the same nature as vegetative reproduction, and that it is, primarily at any rate, correlated with growth in size. Thus, we can understand why the nauplius larva should lose the long series of post-oral parapodia-like appendages which we have reason to believe the annelid ancestor of arthropods possessed, and should retain only those first three modified ones, the modification of which was probably one of the chief changes by which an annelid was converted into an arthropod. A most interesting confirmation of this view is obtained by comparing the nauplii of the various crustacean groups. The most annelid-like order is that of the phyllopod Branchipoda, and here the nauplius still retains traces of the post-oral segmentation. In the other groups, notably the cirripedes and the ostracods, the nauplius has lost all traces of this segmentation, and is obviously secondarily modified, since in each case it shows precociously some features of the adult.

The general result of this way of regarding ontogeny will be that we shall regard no larva as purely secondary in all its features, and that where we compare two animals more or less allied, we shall attach the greatest importance to the ontogeny of that one which has the longest larval history. It is, so far as I know, a rule without any exception that when we compare corresponding stages, what is obscure and difficult to interpret in the embryo, becomes clear and instructive in the larva. The Echinodermata have perhaps the longest larval history in the animal kingdom; they begin free life as blastulæ, and in no group is the process of the formation of the primary germinal layers so diagrammatically clear.

Finally, we have to consider the greatest difficulty of all those involved in morphological reasoning, namely, that which the recognition of the principle of homoplasy brings with it. The proposition that similar organs might arise in different animals under the stress of similar conditions, seemed at first to offer merely a convenient solution of some difficult problems, so long as the organs in question belonged to members of widely different families, such as the tracheæ of insects and arachnids for example. It is, however, becoming every day clearer, that such parallel evolution has taken place again and again within narrow circles of affinity. Let any morphologist examine the structure of a large group; let him by careful comparison of the various species with one another, arrive at an idea of the primitive form from which all were derived, and then let him attempt to indicate the lines along which the different forms have been evolved. He will find himself driven to the conclusion that important structural features on which he would be naturally inclined to found his system of classification, have been developed twice or thrice. Numberless instances of this might be given. Calcareous sponges used to be divided into three well-marked classes, according to the character of their canal system, viz:—the ascons, the sycons, and the leucons, forming three stages in an ascending scale of differentiation. Now Sollas and other competent spongiologists consider that the leucon type has been evolved many times amongst the different families into which the sycons are divided. The horny sponges and the sponges devoid of spicules have likewise had in each case two or three roots. Among the echinoderms we meet with similar cases on every hand. One of Perrier's main divisions of the asterids is that of the Valvata, in which the plates which form the skeleton are covered with a uniform granular coating, there being practically no spines. This division includes long-armed and short-armed forms; the latter glide by insensible gradations into the short-armed forms of the group Paxillosa, in which the armature is composed of circles of spinelets borne on a button, of which *Astropecten* is the best known example. The long-armed forms probably have no affinity with Paxillosa. The term "short-armed" is not quite correct; it is not only that the radii are comparatively short, but that the arms have coalesced with each other laterally. Amongst the ophiurids, the habit of carrying the young in the genital bursæ, which gives rise to great differences in the development, has been independently acquired in four or five distinct genera, other species of which give rise to free-swimming larvæ. Among echinids the eccentric position of the anus, and the peculiar modification of the ambulacra, involved in their assuming the petaloid form, when the tube-feet become broad respiratory leaves, is found in the Clypeastridæ and Spatangidæ. As the first group retain the jaws of the regular echinids whilst the second have lost them, one might at first sight infer that the spatangids were merely a further development of the clypeastroid type, but this supposition is precluded by the fact that in both groups we find fossil forms with perfectly normal ambulacra and almost central anus. Among the annelids, the group of the tubicolous annelids is now divided up into families which are placed with families of the Errantia. Amongst arthropods, those air-breathing arachnids which retain lung-books, the Pedipalpi on the one hand and the scorpions and spiders on the other, have, according to Laurie, been derived from the water-breathing eurypterids along two distinct lines. The land-crabs, which have so completely acquired the air-breathing habit that they die when immersed in water, have been derived from various marine ancestors. The molluscs offer perhaps the best examples of all of homoplasy. The pteropods, once supposed to be a most clearly defined order, are now admitted to have been derived along two lines from the opisthobranchs; the Thecosomata from *Bulla*-like forms, and the Gymnosomata from nudibranch genera. In the newest classification of the Pulmonata, the two common slugs *Arion* and *Limax* are separated from each other, the first is classed in the same family as *Helix*, and the second is made

[1] M. Laurie, "Anatomy and relation of the Eurypterida." *Trans. R. Soc. Edinburgh*, vol. xxxvii., pp. 509–528, 1893.

the type of a family by itself. Amongst the vertebrates we find numerous examples wherever we choose to look. The Teleostei are polyphyletic, and so are the ganoids; so marked does the parallelism in evolution seem to have been, that our ideas as to the mutual relationships of the two groups are in hopeless confusion. Snake-like reptiles have been derived from many families of lizards. Huxley arbitrarily selected the characters of the absence of all trace of a pectoral girdle, and of an allantoic bladder as a criterion to determine the true snakes; but it is obvious that this is a mere makeshift. Among birds, parallelism is seen everywhere, and instances of it can be found in the works of Gadow and Fürbringer.

Turning finally to the Mammalia, we find Sir William Flower writing thus of the Ruminants:—"The great difficulty which all zoologists have felt in subdividing them into natural minor groups arises from the fact that the changes in different organs (feet, skull frontal appendages, teeth, cutaneous glands, &c.) have proceeded with such apparent irregularity and absence of correlation, that the different modifications of these parts are most variously combined in different members of the group."

Such facts as these are apt to have a disheartening effect on the student of morphology. If we try to analyse the feeling of disappointment to which they give rise, we shall find, I think, that it is due to a theory we are accustomed to assume as the base of our speculations on phylogeny, with which such facts are irreconcilable. This theory, rarely explicitly stated, but everywhere postulated, holds that when one large natural group of animals was derived from another (as for instance Amphibia from Pisces), this took place by *one species* of the lower group acquiring new characters and taking to a new method of life. It is then imagined that all the species of the higher group have been derived from the modification of this single ancestral species. The view suggested, however, by the ever increasing number of cases, in which we are forced to assume a parallel development, is that a complete homology or homogeny, and a homoplasy are only after all extreme terms in a series, in which the successive terms are very closely related to each other. We seem driven to the conclusion that when a large natural group of animals was being evolved, the changed environmental conditions, which were causing the evolutional progress, acted not merely on one species but on species belonging to the same or different genera, families, or even orders, and induced similar modifications in them. The course of evolution, therefore, instead of being represented by a single trunk of a tree repeatedly branching—the typical form of the Haeckelian genealogical tree—ought rather to be pictured as a column of parallel stems with interlacing branches, like the stipe of a mushroom.

The study of systematic zoology in fact suggests, that, given a definite set of environmental conditions, any species having a given general structure exposed to them, will undergo the same change.

On the Lamarckian hypothesis this result would be attributed to the direct action of the environment; on the Darwinian view to natural selection, the only assumption which it is necessary to make, being that the small variations on which selection acts may always be trusted to occur.

All admit that the larger differences in structure which separate animals are useful, that is, are adaptive in character. Most systematists are beginning to admit that such modifications may have been effected again and again, and in despair they are taking refuge in minute peculiarities of form, pattern and arrangement, which they hope are not adaptive. This seems to me to be a futile position, the provisional acceptance of which is only rendered possible because, except in the cases where closely allied forms follow each other in immediately superposed strata, we have no means of determining the exact ancestry of a species. What we really do when we determine the structure of an animal is to unravel a series of superposed adaptations, and we class together those animals in whose structure we detect evidence of their having undergone the same series of modifications in the same order.

All similarity in structure between two animals is primarily due to similar external conditions; the longer the period of action of the same environment has been, the more complete is the likeness; and, by identity of ancestry we can only mean the extreme case in which the action of similar environmental conditions has extended for an indefinite time back into the past.

Just, therefore, as it would be a hopeless task to attempt to trace a single hypha in a mushroom stipe back to the spore from which it arose, so it appears to me is the attempt to trace several modern species back to a single original species.

The important thing to know about any hypha is its relation to the general anatomy of the plant, and especially its level on the stem; and the important thing to know about the arthropod groups is not whether they were all descended from the same species of annelid, but whether they were derived from annelid ancestors by the same series of modifications. The morphologist should aim at establishing and defining definite grades or levels of structure, and correlating these with the environmental changes which produced them.

I do not flatter myself that any of the points of view which I have endeavoured to set forth in this paper is absolutely new. Each of them will be found to have been either explicitly stated or implicitly assumed by some zoologist or other; but there is perhaps no zoologist who has not argued in a manner inconsistent with some one of the principles I have endeavoured to establish; and it seemed to me most desirable to try to give a coherent and explicit account of the principles vaguely recognised and imperfectly understood on which phylogenetic speculation is based—and such an attempt has been made in this paper.

E. W. MACBRIDE.

III.

The Earliest Known Seat of Learning in Europe.

A REMARKABLE discovery relative to the culture of pre-historic times has recently been published by Ed. Piette in the third part of his series of "Études d'Ethnographie préhistorique" in *L'Anthropologie* (tome vii., no. 4). In the cave of Mas-d'Azil, on the left bank of the Arise, in the Department of Ariège, the lower layers indicated the previous existence of a cold, damp period; above them were found deposits characterised by bones of the reindeer, red deer, aurochs, horse, common bear, &c. Many of the antlers, and harpoons made from the bones of reindeer, were engraved. Next came a layer, having a maximum thickness of 65 cm. (2 ft. 1½ in.), containing a large number of pebbles, which had been painted with peroxide of iron. The coloured pebble layer, marking the Asylienne period, was followed by a snail-shell layer (*Helix nemoralis*). It contained remains similar to those in the previous one, and belonged to the Neolithic age, but in neither were polished axes found. That the climate was damp may be inferred from the great numbers of this species of snail. Polished axe-heads were found at a higher level in the Pélécique layer, which was also characterised by the presence of *Helix hortensis*, thereby indicating drier conditions of climate.

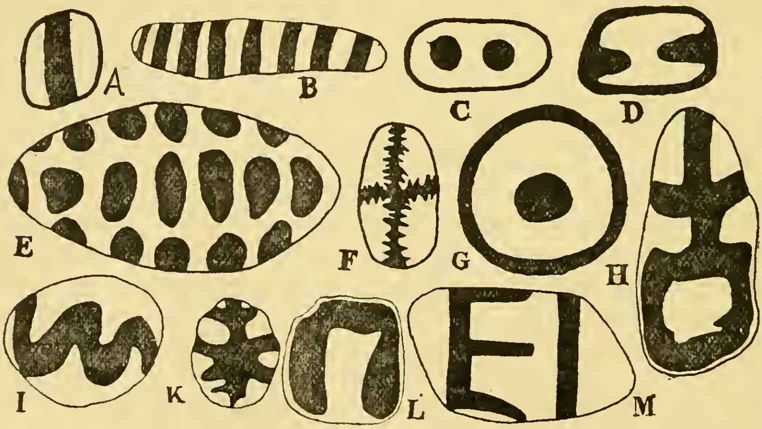
The humidity of the climate during the Cervian period was continued to a less extent during the Asylienne; the presence of trees shows that the climate was ameliorating at the time when the shell-layer was formed. Vegetation must have flourished under the influence of the warmer, though still damp, atmosphere, and men successfully cultivated several species of fruit-trees.

The Transition period, which followed the glacial period, began when the modern fauna had replaced the glacial fauna; that is, after the Equine or Eburnéene Period. The Transition Period had three phases: (1) the Cervian, during which the Quaternary industries continued, the implements remained unchanged, and the art of the reindeer-hunter was perfected; (2) the Asylienne phase, or period of the coloured pebbles, which came after the disappearance of the reindeer: by this time man had forgotten the arts of engraving and carving, but took to painting stones; (3) the Shell phase, marked by cinders mixed with snail-shells, and remarkable for the richness of its vegetation.

We can now turn to the coloured pebbles themselves. These are usually rounded, oblong and flattened water-worn stones of quartz or schist, which have been very rudely painted with red. Sometimes the whole surface is coloured, but more frequently a stone is marked on one or both sides with simple devices; occasionally the edge of the pebble is coloured so as to form a kind of border to the decoration.

Mr. Piette classifies these markings under four headings, viz.:—(1) numerals, (2) symbols, (3) pictographic signs, and (4) alphabetical characters.

Numerals: These markings may be straight lines which run across a pebble (A, B), or may be irregular rounded marks (C, D, E). No stone has as yet been found with more than eight bands (B). Mr. Piette considers the spots as representing units of higher groups, either nines, or more probably tens. In this connection, he points out that the Egyptians used strokes up to nine; ten was represented by a curve, the tens were grouped like the units; a hundred was indicated by



DIAGRAMS OF A DOZEN PAINTED PEBBLES, traced and reduced by one-third from the figures which illustrate the text of Mr. Piette's paper.

a spiral. There may be numerous spots on a pebble, one being found which had as many as twenty-three. In some stones a spot or blotch arises from the margin of the pebble (D). The author points out that this mark may also have a different value, and suggests that it may be the square of a higher-grade unit; thus a stone with twelve marginal blotches and six central spots (E) is credited with indicating a total of 1260 in the decimal system and 1728 plus 60—or 1788 on the hypothesis of a duodecimal system! This rather staggers Mr. Piette himself, and so he makes the admission that perhaps the differences between the marginal or tangent ovals and the isolated spots have no significance. Then, recalling the fact that a disc has throughout all time been employed to represent the sun, he asks whether they may not be signs in a hieratic writing, or units adopted by great men of the tribe, or even used to denote special objects. The author certainly does not fail in ingenuity when he proceeds to throw out the hypothesis that these may have been counters in some game, or that they were painted to teach arithmetic.

Symbols : The occurrence of cruciform markings (F, H) naturally suggests sun-symbols, especially when they are inscribed within a border, a disc surrounded by a ring (G) being claimed as pictograph of a sun-deity—even the tau, or **T** cross, is represented. While admitting with the author that symbolism is a "perilous study," it would be too much to expect an enthusiastic archæologist to refrain from making the most of his material, and so Mr. Piette cannot be blamed for doing his best to read a meaning into these rude designs.

Pictographic Signs : Certain sinuous lines (I) are supposed to be pictographs of snakes. The author found in the Pyrenees a long wavy line of unhewn stones sunk in the ground, and resembling a serpent, with some granite blocks for a head, and near the bends was a beautiful group of cromlechs, in which he found neolithic remains. He also records two interesting experiences of his own, which prove that the cult of the serpent is not yet extinct in the Pyrenees or in the Department of Aisne. It requires some imagination to see trees, as does Mr. Piette, in some of the markings (for example, in K in the accompanying figure); some ovals, also, with central dots are called eyes, and certain elongated markings he names reeds.

Alphabetical Characters: A third of the paper is taken up with a discussion of markings (L, M) that recall the characters of primitive writings; and when we remember the recent discoveries of Mr. Arthur J. Evans in Crete, it is wiser to treat these markings with respect. At times, as in the case of the numerals, Mr. Piette rather gives himself away, but his discussion of these supposed script characters is worthy of very careful consideration. He claims that thirteen out of the twenty-three Phœnician characters have equally done duty as graphic signs on the Asylienne pebbles, and says: "This is a considerable proportion, especially as the number of the Phœnician letters would at first have been fewer, and therefore the idea that all letters have been borrowed entirely from Egyptian writing falls to the ground. The Phœnicians, a mercantile people, whose navies frequented all the Mediterranean shores, have everywhere taken with them, and notably to the neighbourhood of the Pyrenees, the graphic signs which appeared to them the most convenient for keeping their registers, for dealing, and for correspondence. They selected the most widely distributed signs in the countries where they trafficked, in order to spread more rapidly the innovations which they introduced into writing. They changed the meaning of the ancient signs. Those of Mas-d'Azil were probably originally syllabic. In becoming letters, their significance was necessarily modified; but their form was not altered. The Greeks, whose ancient syllabaries had many characters in common with the Asylienne alphabet, adopted the Phœnician reform all the more easily, since they found in the Phœnician letters forms with which they were familiar; later, by a very natural reaction, their tendency was to restore to the signs, which the Phœnicians had modified, their ancient configuration, which they still remembered."

The number of the signs which Mr. Piette believes to be syllabic is small compared with those with bands and spots, and an obvious objection to his view is that, with very few exceptions, each syllable is on a different stone; in M we reproduce the figure of one pebble which has two syllabic signs printed on its side. Have we here the traces of the oldest known seat of learning, which succeeded a still earlier art school, and at which the early Neolithic inhabitants of the Pyrenees learnt how to count and how to build up words from syllabic signs? It is tempting to follow up this line of speculation, but we do not wish to diminish the seriousness of Mr. Piette's labours by unintentional levity.

Specialists alone can determine the real value of Mr. Piette's discovery, but it is evident that he has opened a new chapter in the early history of man, which promises to be of exceeding interest, and we venture to offer him our sincere congratulations.

The paper is illustrated by 107 figures in the text, and it is accompanied by an atlas of 25 plates, containing some hundreds of figures, which are very beautifully printed in colours.

A. C. Haddon.

IV.

Cope's "Factors of Evolution."[1]

THE object of this book is "to select from the mass of facts accumulated by biologists, those which, in the author's opinion, throw a clear light on the problem of organic evolution, and especially that of the animal kingdom." The selection, however, is chiefly made from the facts of palæontology, with a view to interweaving them with the evidence from bionomics and embryology that has been set forth by others. The author poses not as a judge, but as an advocate, and the evidence that he marshals is intended to prove the following propositions: (1) Variations appear in definite directions; (2) Variations are caused by the interaction of the organism and its environment; (3) Acquired variations (which I shall call modifications) are inherited; (4) Variations survive directly as they are adapted to changing environments; (5) Movements of the organism are directed by sensation and other conscious states; (6) Habitual movements are derived from conscious experience; (7) The rational mind is developed by experience, through memory and classification.

Natural selection, then, is admitted by thesis 4, but, according to the other theses, is left little work to do. For, if variation be definite, if it, whether as congenital variation or as inheritable modification, be induced by change of environment, or be, as theses 5 and 6 seem to imply, a more or less conscious response thereto—then, if this method of evolution be a workable method at all, it is a self-sufficient method. Natural selection, if these six theses be true, will find no variations out of harmony with the environment, and will have to seek a situation in another universe. But one can admit the truth of all these propositions without admitting it to be the whole truth. There might still be variations that were indefinite and not in directions determined by environment, modifications that were not inherited, and movements that were not the result of consciousness in any accepted sense of the word. The march of events is not bound to be on any simple system dreamt of in our small philosophies. In short the Neo-Darwinian is not compelled to cut the throat of the Neo-Lamarckian, but may join the agnostics, of whom there are still a

[1] The Primary Factors of Organic Evolution. By E. D. Cope. 8vo. Pp. xiv., 548, and 121 text-figures. Chicago and London: The Open Court Publishing Co. 1896. Price 2 dols.

few, in discussing the arguments here ably presented by Professor Cope.

First, the author attempts to show that variations are definite and in determined directions. That there are limits of variation in each species may be admitted, whether the limits be wide or narrow, whether they be due to obvious physical and chemical conditions or to characters impressed on the race by ancestral history. That allied species, or the varieties of a species, and especially geographical sub-species, observe a successional relation to each other, is a fact that no one need hesitate to accept after reading the numerous instances here brought forward. A lizard, to select but one example, does not jump from a longitudinally striped to a transversely banded form, but the two are connected by intermediate series of broken stripes, spots, and broken bands, a circumstance tending to show that one has been derived from the other; and when we enquire which it is that has been derived, we cannot ignore the further fact that forms banded in the adult have young that are striped and pass through a spotted stage. Nor is this relation confined to superficial characters; it appears no less plainly in the teeth of mammals, in the shoulder-girdles of frogs, and in the arm-loops of brachiopods. We accept all this; but we accept it as a statement of the trend of evolution, not as expressing the direction of variation. With the exception of a paragraph quoted (without reference) from a paper in NATURAL SCIENCE by the Rev. G. Henslow, there is no attempt to show that all the offspring of any individual vary, if they vary, in one direction, and that the direction is constant for all individuals of a species. Indeed, it is hard to see why the main facts detailed by Professor Cope in this chapter cannot be explained just as well on a hypothesis of natural selection, although, in the enforced absence of evidence, the explanation would be worth just as little.

The next chapter deals with phylogeny, especially with various lines of descent within the Vertebrata, which, by reason of definite palæontological evidence, are better known than any others. The position of these facts in Professor Cope's argument appears to be stated in this paragraph:—"Examination of all these genealogical lines reveals a certain definiteness of end and directness of approach. We discover no accessions of character which are afterwards lost, as would naturally occur as a result of undirected variation. Nor do we discover anything like the appearance of sports along the line, the word sport being used in the sense of a variation widely divergent from its immediate ancestor. On the contrary, the more thorough becomes our knowledge of the series, the more evident does it become that progressive evolution has advanced by minute increments along a definite line, and that variations off this line have not exerted an appreciable influence on the result." This statement, just as the preceding ones, may be accepted; but, equally with them, its bearing on the question is not obvious. In saying that there are no accessions of characters afterwards lost, Professor Cope cannot mean to deny that there have been lines of progressive evolution (*e.g.* the Trimerellidæ among Brachiopoda and the Calceocrinidæ among Echinoderma), which rapidly became extinct; nor can he mean to deny that, in the history of a line, a character (such as the coiling of Ammonoidea) has gradually appeared and, as gradually, disappeared. What his examples show him to mean is, that within the limits of any one ascending series, there is no appearance of a character outside the general trend of development. The inception and evolution of characters are gradual processes, and a line of development once started is continued. What is this, other than to say that variations are so slight as to be imperceptible to our grosser sense; that those not in accord with environment are checked before they make their mark on the history of the race; that those in accord with environment are preserved; and, since environment changes gradually and definitely, that preserved variations and the consequent line of organic evolution are also definite and gradual? So far there seems nothing in the statements of our author that may not be accepted with untroubled conscience by the most orthodox of Neo-Darwinians.

The final section of the same chapter can offend none except those who would derive the Vertebrata from a highly specialised arthropod, for it merely restates the fact that "the highly developed, or specialised types of one geologic period have not been the parents of the types of succeeding periods, but that the descent has been derived from the less specialised of preceding ages." This, however, does not even profess to bear on the main argument.

Chapter III. is devoted to exemplifying what Professor Cope calls the Canon of Parallelism. This, put as broadly as possible, and without the qualifications that are required for each particular case, states that "all organisms in their embryonic and later growth pass through stages which recapitulate the successive permanent conditions of their ancestry. Hence those which traverse fewer stages resemble or are *parallel* with the young of those which traverse more numerous stages." As an instance of this may be mentioned the case of the lizard cited above; but Professor Cope has had no difficulty in adducing plenty of cases from various branches of the animal kingdom. He further explains how inexact parallelism (cænogenesis of Haeckel) may arise through certain characters developing more or less rapidly in certain lines of descent than they do in others. Inexact parallelism is, of course, the rule, while the necessity for compression in development, or the opposing influence of an altered environment, has often caused the partial or almost complete elimination from ontogeny of recapitulatory stages. Accepting this most freely, still one cannot shut one's eyes to the facts of parallelism, in many cases so obvious; and despite the amount of criticism that the principle has met with of late, it really seems as though the only differences between the various writers lay in their interpretation of admitted facts. What bearing he supposes the facts to have upon any of his theses, Professor Cope does not, so far as I can discover, deign to tell us; nor is it clear how the facts of successional relation in time are more valuable evidence than those of successional relation in space, which were dealt with in the first chapter.

The facts of retrogressive evolution or degeneration, which Professor Cope calls Catagenesis, are discussed in Chapter IV., and are supposed to prove the direct influence of disuse; but no attempt is made to combat the arguments of many who believe themselves to have shown that degeneration is merely a form of adaptation to surroundings, perfectly explicable on the principles of natural selection.

At this stage Professor Cope points out the conclusions he thinks he has reached. For my part, while prepared to admit that "phylogeny exhibits a progressive advance along certain main lines," and that evolution itself has been in definite directions, I cannot see that it has been proved that all or even the majority of variations were in definite directions. Definite variation may be possible, may exist, may be of constant occurrence; but as an attempt to prove it, these 200 pages seem to me utterly beside the point.

We come now to the second part of the book, and as this is entitled "The Causes of Variation," we look for something in support of the thesis that variations are caused by the interaction of the organism and its environment. Instead of this we find a lengthy and highly interesting account of various methods in which modifications are produced by the direct action of external influences, among which are included the mechanical actions of the parts of the organism itself. Everyone may not agree with everything here brought forward, but none can be prepared to deny all the instances. Let us, for the sake of the argument, admit modifications of the adult or of the growing organism to the extent that Professor Cope desires. The question remains: are these modifications inherited? Throughout this section such inheritance is boldly assumed by the author, and so all difficulties disappear. But the mere existence of modifications, or the occurrence of evolutionary series, the goal of which appears to be not merely definite but adaptive, of themselves prove nothing. Undoubtedly, if one could prove modifications to be inherited, one would have a simple and satisfactory explanation of a very large number of facts which at present have to be explained as due to some exceedingly tortuous process, and one that many straightforward people find difficult of conception. But we must remember how unsafe it is to assume that simplicity is the rule in nature.

Palæontologists, as a rule,—and Professor Cope is one of them—are so profoundly impressed by the adaptive nature of the evolutionary process and by the definiteness of its direction, that they cannot regard the restraining or selective action of the environment as enough to keep the breed true. They are so accustomed to see mutation after mutation, generation after generation, developing in apparent obedience to obvious physico-chemical or mechanical conditions, that they incline to regard these conditions as causes. And if it be suggested to them that the results they see may have been achieved by the selection of adaptive variations from among a number of promiscuous variations that were not adaptive, they ask why it is that they do not find evidence of these numerous non-adaptive variations in the rocks, when one would suppose that, on any hypothesis except that of definite variation, such forms must have been the more abundant of the two. It is useless to reply to them that the non-adaptive variations in each generation were killed off when young, and so, even if fossilised, are practically undistinguishable; because they will reply with abundant proof that the adaptive characters chiefly appear in the adult stages of the organism, possibly only in its senile stages, and so are incapable of coming under the action of natural selection during the early undifferentiated stages. How the conversation might continue does not much matter, for it is obvious that it has reached a point beyond which all must be speculation. The facts on which the palæontologist relies, the facts that Professor Cope adduces with such wealth of knowledge, are strong presumptive evidence in favour of his second thesis. But they are not proof.

"Chapter VII.—Natural Selection" is included in this part, not because the author regards natural selection as a cause of variation, but because he wishes to controvert that view. It is often stated in the writings of those who are not strict Neo-Darwinians, that some, at least, of the Neo-Darwinians regard natural selection as a cause of variation. If this be so, Professor Cope is surely right in affirming such Neo-Darwinians to be in error. "A selection cannot be the cause of those alternatives from which it selects." To state that natural selection may preserve the character of variability is another matter; for variability is just as much a character of some organisms as fixity is a character of others, and this without reference to the environment; and since variability is a character that may work good or ill to an organism, it inevitably falls under the action of selection. Apart from this, is there not a slight misunderstanding here, due perhaps to a lax use of the term "natural selection?" Whereas "selection" means selection and no more, the stereotyped phrase "natural selection" has come to imply the whole process that forms the fundamental conception of Darwinism. For evolution to take place through this process, we must postulate:—first, variation in the offspring; secondly, variation in the environment; thirdly, selection of such variations in the offspring as are in harmony with the variation in the environment. It is this whole series that results in change—change which is definite in kind, and which influences many individuals in the same way. In short, this is "the origin of species by natural selection."

However elementary the above exposition may seem to some, it seems to be needed by Professor Cope. For he says (p. 7) "Mr. Spencer occasionally falls into the error of ascribing the origin of structures to natural selection, as in the case of the forms of flowers (Principles of Biology, II, p. 153), and the armor-plates of paleozoic fishes (*Op. cit. p.* 288)." What Mr. Spencer does say is (on p. 153), "if we bear in mind the functions of flowers, we shall find in their adaptations to their functions, under conditions that are extremely varied, an adequate cause for the different types of symmetry, &c.," and again, "the forms and positions of those subsidiary parts which give the general shape to the flower, similarly arise by the survival of individuals which have the subsidiary parts so adjusted as to aid this fertilizing process." And what Mr. Spencer says on p. 288 is, "The contrasts between . . . . soft skin Fish and Fish in armour like the *Pterichthys*, must have been produced entirely by natural selection." Moreover, on page 192 Professor Cope says that Wallace ascribes the colour and form characters of animals "to natural selection *as a cause*." This time no reference is given, so one can merely draw attention to the fact that nothing in Mr. Wallace's book "Darwinism" supports this statement; on the contrary, its author continually postulates the existence of variation before selection, he accepts sexual reproduction as the main, if not the only, cause of variation, and, as for colour, he regards it "as a normal product of organisation, which has either been allowed free play, or has been checked and modified for the benefit of the species" (p. 299).

From the point of view of the argument, the third part of the book, entitled "The Inheritance of Variation," is by far the most important; for by "variation" Professor Cope usually means what is here called "modification." If the inheritance of modification be proved in a single instance, then it is a possibility, and as such may legitimately be drawn upon in constructing a theory of organic evolution. Is it proved here? The first argument is that certain characters were, at some time or other in the history of each line of descent, acquired, *i.e.*, they first appeared as modifications of adult structure; and that these characters are no longer induced afresh in each generation, but are inherited. In short, "*all characters now congenital have been at some period or another acquired.*" For instance, the groove-joints between the metapodial bones and the phalanges of the mammalian foot are assumed to have been due originally to mechanical causes; they are no longer so due, since they are to be found prefigured in cartilage in young animals that have never stood on their feet. The Neo-Darwinian sees no difficulty here: first, he simply denies that such characters were originally produced by mechanical causes; secondly, he maintains that, even if they were so produced, yet the inherited characters were really congenital variations that happened to be in the same direction as the modifications. It is clear that such an argument cannot be disproved, so we pass, with our author, to the evidence from palæontology. Here Professor Hyatt entirely fills the stage with his case of the impressed zone of the Nautiloidea, of which an account was given in NATURAL SCIENCE, vol. III, p. 408. "There is" says this palæontologist "every reason for regarding the impressed zone as a ctetic [*i.e.* acquired] characteristic acquired in the later stages of growth and not hereditary so far as is known in any shells of the earlier Paleozoic periods." Admitting this, one still remains without definite proof, and it is unfortunate that even Professor Hyatt, in a subsequent note, has to admit certain exceptions, of which "it is impossible to say at present whether the impressed zone appeared as a genetic character or as a mechanical necessity." Here again the evidence, however cumulative, will receive from the Neo-Darwinian the same explanation as the evidence from embryology.

The evidence from breeding is quoted chiefly from Professor W. H. Brewer, and puts the case very strongly. It is pointed out that "all the best breeders recognise the rule laid down by Darwin, that those characters are transmitted with most persistency which have been handed down through the longest line of ancestry." Hence, they do not expect modifications produced by temporary conditions, or by conditions that have been in operation for only a short period, to be inherited. On the other hand, says Brewer, "The art of breeding has become in a measure an applied science ; the enormous economic interests involved stimulate observation and study, and what is the practical result ? This ten years of active promulgation of the new theory has not resulted in the conversion of a single known breeder to the extent of inducing him to conform his methods and practice to the theory. My conclusion is that they are essentially right in their deductions founded on their experience and observations, namely, that acquired characters may be, and sometimes are, transmitted." The cases of inheritance of injuries cited by Brewer appear well authenticated, but what is the use of them when they can so easily be explained away, by Neo-Darwinians, as mere coincidences ? A case more akin to what would, on the hypothesis of the inheritance of modifications, be the action of nature, is the following : Sheep taken from Ohio, where the wool is fine and good, to the alkaline soil of Texas, have their wool rendered harsh in texture, while its behaviour under dyes is altered. So far this is a case of undoubted modification due to environment. The offspring are born with wool of the same harsh character, and this alone might be explained as a modification induced afresh in each generation. But it is noticed that the harshness increases with succeeding generations, and flocks that have inhabited such regions for several generations produce a harsher wool than do the new-comers. It thus appears that there is something more than a mere repetition of the modification, and this something more is explained most naturally as due to inheritance of the modification. To explain it as "a congenital adventitious variation coincident in all the individuals of immense flocks, is a mathematical absurdity." The only possible alternative appears to be that the environment, which *ex hypothesi* is favourable to individuals with harsher wool, selects such individuals from among the numerous lambs, some of which may of course vary in the direction of harsher wool, while others vary away from it. But now comes the final step in the Lamarckian's argument, which establishes him so firmly that it is hard to see how the Neo-Darwinian can dislodge him from his position. It is this: so far from environment having any selective action, the selection is all the other way, since the breeders do all they can to lessen the evil by selecting the lambs that vary least in the direction of harsher wool. Now, if these facts are really so (and we have them presented to us with others of similar nature under the highest authority), then the only conclusions I am able to draw are, first, that environment can modify individuals; second, that variation can be so influenced by environment as to be definite in direction; third, that modification can be transmitted from parent to offspring; fourth, that not merely individuals but the whole line of descent may respond to the direct action of environment in the face of equally direct and opposing action on the part of selection. In the case of these sheep, it is not definitely stated, doubtless because it is a well-recognised fact in this as in other cases cited, that the offspring of such animals, though born and reared in favourable localities, do not, in the first generation at all events, throw off their unfavourable characters. This is further proof that a definite effect has been produced, not merely on the individual but on the race, by the direct action of environment.

Professor Cope has been a long time in giving us this proof. It seems as though palæontological evidence alone will never be enough to furnish any of equal value, and that we must in the end look to the experimenter. One cannot have proof unless one has control of the conditions of experiment; mere historical results are capable of so many interpretations. To apportion guilt and innocence justly between Queen Elizabeth and Mary Queen of Scots is child's play compared with the task of deciding how much of the structure of an ammonite is due to congenital variation selected by environment, and how much is due to modification impressed by environment.

Yet let not Professor Cope lay to his soul the flattering unction that this case of the sheep, or any other that he quotes, will satisfy the Neo-Darwinians. Every Neo-Darwinian must be his own experimenter, and as few of them have the capital to invest in sheep-runs in Ohio and Texas, they will probably prefer the easy laziness of scepticism. And even should one of them verify this instance to his own satisfaction, he will at the same time have invented some new explanation, or some sweet arrangement in iddities, that will leave him as unconvinced as ever.

Let us, however, who are neither Neo-Darwinians nor Neo-Lamarckians, admit that Professor Cope has scored his first four points, if only that we may see the game out. Every explanation of the facts around us necessitates another explanation behind it, and so, if modifications are inherited, the biologist has to explain how they are inherited. This is why Weismann appealed to us: not because he explained evolution, for Darwin, Wallace, and Spencer had done that, but because he explained their explanation, showing how it worked through the known mechanics of reproduction and cell-division. Now the explanation of evolution by Lamarck is not opposed to the explanation by Darwin, it is complementary to it. What it is opposed to is the germ-plasm theory, at all events as first strictly stated by Weismann. Professor Cope, therefore, who follows Lamarck, is obliged to provide some explanation of the way in which a modification of the adult soma is transmitted to the already differentiated germ-plasm. His first step is to propound what he calls the Theory of Diplogenesis. This supposes that a cause affecting the soma actually, likewise affects the germ-plasm, but affects it potentially. It is hard for us to understand how an external cause influences the soma, but we know that it does so; it is hard for us to understand how the germ grows into the adult with the characters of the parent, but we know that it does so; is it any harder for us to understand how the same external cause may influence the germ while yet within the parent?

Professor Cope, it is true, does not present us with a wonderful mechanism of speculative *ids*, *idants*, and the rest, linked to a set of physical appearances upon which no two cytologists are agreed, and so we must admit that his explanation, even if it be accepted, requires some further explanation. But, so far as this goes, his theory is no worse than anyone else's, and it seems safer to wait for the principles of cellular biology to settle before applying them to these speculative questions. Rejecting all hypotheses based on pangenetic principles, *i.e.* on the transference of material character-bearing particles, Professor Cope relies on the action of a growth-force, to which he gives the name "bathmism." This in its turn requires explanation, and it is explained as the action of a kind of unconscious memory, with which faculty the reproductive cells are endued. Impressions are transmitted to these cells through the continuous protoplasm of the organism, and the cells retain, "first the impressions received during their primitive unicellular ancestral condition, and second, those which they have acquired through the organism of which they have been and are only a part." When we are able to give a physical explanation of conscious memory, it will be time enough to consider this heredity theory of Professor Cope, or rather of Hering. At present, like many other fascinating speculations, it is as incapable of proof as of disproof.

The final section of Chapter VIII. recapitulates some of the objections that have been raised to the doctrine of the inheritance of modifications. This, however, chiefly discusses special cases, with which the theory neither stands nor falls.

Chapter IX., headed "The Energy of Evolution," appears to be devoted rather to a recapitulation of the theory of bathmism, to the explanation of heredity as the transmission of a mode of motion, not the transmission of matter, and to the airing of those numerous neologisms in which the New World delights—Antichemism, Emphytism, Statogenesis, Autokinetogenesis, Cryptonoÿ, and the like. It is Chapter X. that tells us what, in Professor Cope's opinion, does form the energy of evolution. "It maintains that consciousness as well as life preceded organism, and has been the *primum mobile* in the creation of organic structure. . . . The true definition of life is, *energy directed by sensibility, or by a mechanism which has originated under the direction of sensibility*." To enter upon the discussion of such important theses would require not a paragraph, not an article, but a book. It is the less necessary since the views of Professor Cope have recently been fully criticised by professed philosophers and psychologists (see NATURAL SCIENCE, ix., p. 220).

Chapter XI., and last, contains a list of papers by American authors that have contributed to the evidence used in the book, and summarises in their own words the positions assumed by Ryder, Hyatt, Packard, Osborn, Dall, W. B. Scott, Eimer, and Naegeli. Whatever our own opinions may be on particular questions, we must admit that this chapter, and indeed the whole book, forms a useful guide to the views of the American school of biologists, and especially to those of its chief prophet, Professor Cope. At the same time we must remember that not all eminent biologists in America are Neo-Lamarckians, any more than all in England are Neo-Darwinians. In biology, as in other sciences, there is still plenty of room for that diversity of opinion without which knowledge cannot be advanced.

F. A. BATHER.

V.

Wasps and Weismann.

THE phenomena connected with the reproduction of the social Hymenoptera are of such interest, not only for their own sake but also on general considerations, that the results detailed by Dr. Paul Marchal in a paper entitled "La Réproduction et l'Évolution des Guêpes Sociales" (*Arch. Zool. Expér. et Gén.*, 3e sér. vol. iv. pp. 1–100, 1896), deserve more than cursory notice. His experiments deal with various species of wasps; and in order to give a fair presentation of his theoretical conclusions, the experimental results must be briefly stated.

In terrestrial species the size of the workers increases progressively from the beginning to the end of the season, culminating in intermediate forms not much smaller than the queens, which are the final product of any one complete season. Correspondingly, the lower and later combs are formed of larger cells for the reception of queen larvæ, though of these a few may at first contain large "intermediate workers"; while the small cells contain at first workers only, and later both workers and drones, the latter becoming more numerous as the season advances. Contrary to the statement of Réaumur, no special cells are constructed for the reception of the male (drone) larvæ. Combs containing both large and small cells occur at times, and in such the proportion of males is unusually low. The normal occurrence of fertile parthenogenetic workers producing male offspring only is beyond doubt. Such workers become extremely abundant if the queen is removed, or if her fecundity becomes impaired; but apparently only recently emerged imagines can attain to this distinction. The determining cause of this occurrence is the excess of food consumed by the workers, in consequence of the absence of larvæ, to whom normally they would impart a large share of the food-material brought to the nest. It is fully established that quantity and not quality of food is the determining factor in the rearing of worker or queen wasps.

The power of the queen to withhold spermatozoa from those eggs destined to be males, and with certainty to fertilise those deposited in queen cells, has been ascribed to the will. Dr. Marchal suggests a new explanation as follows:—towards the end of the season the spermatheca contracts with less regularity for various reasons, hence many unfertilised ova are deposited: the larger cells excite the queen and furnish a stimulus which causes the spermatheca to contract and expel spermatozoa, so that all large cells contain fertilised ova, of which the great majority give rise to queens. The explanation is ingenious, but does not appear to reach the bottom of the matter. It is probable that the explanation of the phenomena is the same for both bees and wasps, and, if so, the question arises,—how is it that the large drone-cells of bees do not act in the same, but in the contrary manner, on the nervous system of the queen bee? It is tempting to attribute the sure fertilisation of ova towards the end of the season to the elongation of the abdomen of the queen wasp in order to reach the bottom, or rather top, of the large cells; such elongation would cause a narrowing of the abdomen and put some pressure on the walls of the spermatheca. A similar view has been held with regard to the queen bee, but this supposition is effectually disposed of by the observations of Drory and others, that the queen bee will lay drone-ova in small cells if all large are removed, and, conversely, fertilised eggs in large cells if provided only with the latter. Incidentally it may be mentioned that Dr. Marchal finds that the bifurcation of the seminal duct as described by Cheshire is non-existent.

The arboreal species of wasps do not form special cells for the queen larvæ, but the existing small ones are increased in height, and, in consequence of their shape, also in diameter.

An evolutionary series in nest economy is thus presented, commencing with *Polistes*, and passing, by the arboreal and then the terrestrial wasps, to the honey-bee.

An observation of great interest consists in the establishment by experiment of the inability, despite desire, of male *Vespa germanica* to copulate with female *V. vulgaris*, or vice versâ. This fact at once demonstrates the importance of the male genital armature in determining species.

En passant the immunity of the parasitic fly *Volucella zonaria* is attributed to the quiet slow way in which it moves about within the nest, in striking contrast to the behaviour of a strange wasp.

After stating results and conclusions, Dr. Marchal proceeds to an examination of Weismann's theory, and an explanation of the phenomena of heredity here presented. Determinants, ids, and idants are rejected as having no real existence. The sterility of the early workers is regarded as being due (1) to insufficient food supplied by the over-prolific queen, (2) to these workers having to supply food to their very numerous brethren. With the advance of the season, and increase in numbers of active workers, the latter become capable of coping with their cares, and thus acquire more food for themselves, while each larva also gets greater abundance; thus there arises a series of larger workers, some of which are fertile, and eventually queens appear. Proceeding now to the effect on the germ-plasm, it is maintained that the present conditions of rearing at the end of the season differ from the variable conditions which obtained in pre-social ancestors; that these newer conditions constitute a constant modifying influence on the germ-plasm, which has thus acquired a new physico-chemical constitution. The ova that give rise to workers contain this same modified germ-plasm, but the resulting larvæ are reared under conditions different from those which have now, in the course of ages, become normal to the queens. The germ-plasm is therefore under new conditions, and does not follow its normal course, but gives rise to imperfectly developed forms (workers). As the conditions more and more nearly approach those habitual to queen-ova and queen-larvæ, so does the resulting imago more and more nearly attain the perfect form. All cases of dimorphism and polymorphism among social insects with sterile forms are capable of being similarly explained. These conditions, once reached, are maintained by natural selection.

In a second paper, "Observations sur les Polistes" (*Bull. Soc. Zool. France*, vol. xxi., pp. 15–21, 1896), Dr. Marchal points out the habit of *Polistes* of storing small quantities of honey temporarily; also that the hexagonal form of the cell is not due to instinct, but to compression, the first part of the cell being cylindrical.

Dr. Marchal is to be congratulated on having brought together by his own industry a number of valuable observations, and on furnishing a sound explanation of the main problem before him.

OSWALD H. LATTER.

Charterhouse, Godalming.

SOME NEW BOOKS.

Our Forgotten Forefathers.

Prehistoric Man and Beast. By Rev. H. N. Hutchinson. 8vo. Pp. xxii., and 298, with 10 plates. London: Smith, Elder, & Co., 1896. Price 10s. 6d.

This is a fairly good attempt to collect and summarize what has been published in books and memoirs (for the most part English) about Prehistoric Man, with illustrations of his probable ways of life, taken from the nature and customs of existing savages. It is comprehensive and plainly expressed; indeed, the author, intending to write in a popular style, condescends to use a slang word here and there.

The gradual outcome of creatures and things, not only the manifold inhabitants of the earth from earliest times to the present, but the structure and conditions of our globe, and the solar system itself, is one of the leading ideas throughout this well-wrought compilation of facts and notions about "Prehistoric Man and Beast."

Part I. comprises seven chapters treating of "The Man of the Older Stone Age" (Palæolithic). The stone implements are briefly described at page 28, and do not appear to have been specially studied by the author. He notes some of the geological deposits, chiefly in valleys and caves, in which these stone tools and weapons have been found. The "Plateau Implements" of Kent, and the gravel in which they lie, are better known now than when page 24 was written; and their value is not really influenced by the depreciative doubts of some referred to. The prehistoric *human remains* are carefully noticed, especially those found in Belgium.

The author expresses a strong distrust in the published evidence of Pliocene or Miocene Man. He might as well have been cautious in this matter. Dr. Noetling particularly refers the Burmese flint flakes (see Natural Science, vol. v., p. 345) to a ferruginous conglomerate of early Pliocene Age, and he writes that he did find them in the real conglomerate, notwithstanding what the author's "Old Schoolfellow" may state to the contrary. Dr. Hugh (why "Keith"?) Falconer's expectation that man or his works will be found in the Sivalik strata, or their equivalents, may still be well grounded.

Mr. W. G. Smith's interesting book on "Man the Primeval Savage," his implements and ways of life, is largely drawn upon; and like that laudable attempt to describe and illustrate prehistoric man and his surroundings, the book before us is calculated to raise and encourage a taste for this subject, putting the enquirer in the right line of research, and affording many clues to first-class systematic works on anthropology.

The rock-shelters and caves of Dordogne and neighbouring districts in France form the subject of chapter iii., taken from good sources. Their former occupants are known as "Reindeer Hunters." Some interesting notes taken by travellers and others about existing savages and their modes of living and thinking are brought in as appropriate to the probable condition of these prehistoric Aquitanians and others. At p. 69 is a lively description of plate iii. which represents two men trying to follow up some reindeer, one of which has been already wounded with a harpoon, probably when first met with while grazing. A far easier method of obtaining their venison, by the old folk, was probably by trapping in pitfalls and the associated run-ways and deer-fences, as in Scandinavia and Newfoundland. At p. 70 the curious pogamogans used in those old times in Western Europe, and by some wild races at the present day, are mentioned. We may inform the author that in Westminster Abbey a pogamogan is held in the hand of a North-American Indian, who has been sculptured as part of a tombstone in memory of some military officer in George the Third's day. It is on the north wall of the nave.

Information collected about ancient cave-dwellers, especially the men of the Older Stone Age, opens out the subject of rock-shelters, caverns, and especially bone-caves. In the last occur bones of animals formerly existing, but some of them now extinct altogether or removed by emigration from their former country—hence the question as to the time when the caves were occupied, and, of course, the age of the bones in the several caves, in relation especially to the so-called Glacial Period. These points are more or less dogmatically stated by different observers. It is often difficult to determine whether such animals as now inhabit warm climates were really associated at the same time with such as are now accustomed to live in cold regions. Also whether or no, clay and stones rubbed down by then existing glaciers entered or shut up the mouths of the said caves. These questions often cannot be decided for want of incontestable evidence.

The various proofs that Western Europe had a kind of Arctic climate, in palæolithic and probably neolithic ages, lead to the consideration of its possible causes. Although some extreme statements and fancies have been cleared away, the real cause or causes are still hypothetical. This is the subject of chapter iv., "On the Myth of the Great Ice-sheet, and Theories of the Flood." Mr. Hutchinson wisely regards the "Ice-age" as having been greatly exaggerated—that "the notion of a Polar Ice-cap must be for ever abandoned," and that "vast physical changes" took place at the end of the Glacial Period, with an "amelioration of the climate." He finds no room to compile an account of the peculiar Drift deposits of the time; but objects to Prestwich's careful and elaborate geological exposition of "a rapid submergence and emergence," producing all the apparent results of a deluge in Western and Southern Europe; also to his own friend Howorth's "wide-spread wave of waters" passing over the country, and sweeping away the mammoth and its contemporaries. This flood was presumably caused by earth-movements, as the foregoing was, but on a larger scale. The Reverend Author rejects these scientific hypotheses, because "both the account in 'Genesis' and that in the primitive Accadian or Chaldean version mention *rain* as the cause of the Flood"!

Changes in climate and their causes, in chapter v., form a sequel to the "Great Ice-sheet Myth"; and some enquirers may find the *resumé* of opinions and calculations of use to them in their studies. As for the author, he is judiciously cautious here; the old-fashioned idea of the relative positions of land and water—that is, geographical conditions and their changes—is mentioned; and the cause of the Glacial Period is left "an open question." Less than 20,000 years is taken as being fully the time between it and the present. So also in chapter vi., on "The Antiquity of Man," the recession of Niagara is known to give correlative time of 12,000 to 18,450 years since the Glacial Period. Other standards for the calculation of this time are quoted at p. 139.

In chapter ix., the abodes of the living and sepulchres of the dead of these prehistoric times are described. The kitchen-middens and shell-mounds are useful indications of modes and conditions of life in many parts of the world. So are the mounds, barrows, tumuli, and other tombs, of the character of the deceased, and of the culture of the survivors. Much interesting detail is collected from writers on these and collateral subjects.

The former existence of a short-statured race throughout Europe and the British Isles is strongly believed by Mr. Hutchinson. An early expression thereof occurs at p. 30, where he seems to suppose that small flint-flakes must have belonged to a small kind of men. He mentions "Moustier in the Dordogne district" as connected somehow with evidence of this. He gives no reference thereto.

Chapter x., for its main subject, has the "Little-folk or Fairies, and Mermen." The information has been collected *con amore*. The stone arrows of the Dwarfs seem to be the connecting link with the neolithic times just now alluded to. Boyd Dawkins' descriptive sketch of a neolithic homestead, and Flinders Petrie's neolithic intruders into Egypt about 5,000 years ago also find a place in this chapter.

In the two following chapters the author gives fuller remarks on the Dwarfs, Little-folk, Troglodytes, Dwellers in caves and underground, Goblins, Elves, and Fairies, all of whom originally, as well as the Mermen, he regards as having belonged to the race of the Finns, Lapps, and Esquimaux. To these Fairies or Dwarfs he ascribes the building of Stonehenge.

In connection with the indications of the important changes from the use of implements of wood and stone to those of bronze and iron, we would have liked to have seen our old acquaintance, cunning little Jack the Giant-killer, brought in with his metal sword, superior to the clubs of bigger and stupid men, although his history is far from being equivalent to the clever stone-using Dwarfs fighting and annoying, if not beating, men of metal.

Regarding the antiquity of the human race, it is thought "very probable that man originated at a somewhat early period in the great Tertiary era of the world's history" (p. 128); but, being under the the rule of evolution, and having his immediate ancestors among the Primates, the geological horizon of those ancestors must be taken as a limit before which he cannot have come upon the stage. The gradational characters of the skulls from Spy, Neanderthal, Naulette, etc., and the still lower type of the skull from Java, prepare us for the "missing link," whether coming from Pleistocene, Pliocene, or even Miocene strata. We have already intimated that Mr. Oldham's attack on the accuracy of Dr. Noetling's observations has not demolished the Burmese evidence; and the Javan skull still retains its semi-human, or even infra-human, status according to several experts.

In Part II., Mr. Hutchinson treats of Man of the Later Stone Age (Neolithic), and the Bronze Age; and herein he moves freely among the published discoveries and descriptions of Lake-villages, Pile-dwellings, and Crannogs, and the implements and other curiosities found in their remains; as well as in the tombs and burials of the same kind of peoples. Some pile-structures in London (Pitt-Rivers) and in Newbury (E. P. Richards) might have been mentioned for England.

In chapter xi., on "Rude Stone Monuments," Mr. Hutchinson has collected, from Fergusson and others, various notes about Cromlechs, Dolmens, Pillar-stones, Monoliths, Standing Stones, and Menhirs, also about lines and avenues of stones—these are well-known evidences of the work of some prehistoric peoples. Not knowing anything more than others about the feelings and intentions of the original makers of these rough stone-works and earth-works, the author uses a judicious caution for the most part. But he apparently gets out of temper with those who have called Stone-circles Temples (p. 265); and especially with those who have supposed the *Druids* to have been connected with their origin or uses; nor may the Keltic people be hypothetically connected therewith. Stonehenge is a sad trouble to our author; he knows apparently, like others, just what he has read about it, after or before an occasional inspection of the place. After suggesting that Pre-keltic Dwarfs may have built it, because they were clever at "chambered cairns and dolmens," he finishes with eighteen observations and arguments amounting to little, except that "Stonehenge may be purely memorial," but is not merely a burial place; he thinks that it has not any astronomical or solar meaning, but that it may have been used for public meetings and a court of justice. We cannot here indulge in any controversial remarks for the sake of helping the author or his readers, although there are many points of interest tempting to the philologist and archæologist. A new course of reading about Stonehenge, even in the writings quoted by himself, might improve the second edition to which this book is well entitled.

In a new edition the very interesting and suggestive researches of Dr. J. S. Phené and others on shaped mounds, having definite outlines of animals, with associated stone-work of probably sepulchral and sacred meanings, in different parts of the world, should be fully referred to.

The plates should be inscribed with the pages where they are described; the list of publications should have some kind of classification, or (best) a chronological arrangement; and there should be a better Index.

In the later chapters, as indeed with other parts of the volume, the author has conscientiously tried to fulfil his intention "to gather and put the scattered threads of inquiry into a continuous web and pattern" (page vii.), so that every-day readers may know something definite (if not conclusive) about the too frequently imperfect and confused statements and notions concerning Man in his early existence on the Earth.

The ten phototint plates, very suggestive of the author's views, have been carefully designed by Mr. Cecil Aldin, but the original drawings, of larger size, lately exhibited at the Geological Society's *soirée*, had a more artistic aspect.

Plate I., "An eviction scene at Wookey Hole, near Wells, Older Stone Age," is described at page 53, as the defence (of very doubtful success) of his cave, woman, and child, by a skin-clad man, against a select lot of the carnivorous beasts of the period.

Plate II., "Hunting the Mammoth in Southern France," shows one hairy elephant marching along with apparent disregard of javelins and stones, and another coming up behind with a man lifted up in his coiled trunk. The picture might be termed elephants hunting men. Possibly in front of the first elephant is a part of a badly constructed cover of a pitfall, and he has taken up a twig or two by his trunk.

Plate III., "Hunting the Reindeer in Southern France," described at page 59, we have already alluded to.

Plate IV., "Hunters Feasting on Horseflesh in Southern France." Four trowsered, half-bare, tattoed men, sitting, kneeling, squatting, and crossed-legged, with a shaggy dead horse near by, and a roughly clad man coming along with something heavy on his shoulder. One of the feasters is talking and pointing excitedly, or toasting a bit on his spear (?). Two Indian wigwams are placed at the side.

Plate V., "Hunters in Rock-shelter at Night in Southern France," is reproduced here by permission. The interior of a cave, with skin (?) hangings at the doorway, into which a trowsered and otherwise clad man is coming. Two bare little children are near the fire on the floor, one lying down, and one standing up with a long marrow-bone in her hand. The parents are sitting on a ledge of the cave; the man nude, and either scraping or carving a bone (?), apparently with only one leg, but rapt in artistic ecstasy. The woman, also unclothed, but having an armlet, and apparently sewing a long-haired skin (?) with long sinew thread. It is probably after supper, as the cooking sticks for roasting (?) are not at the fire now.

Plate VI., "Swiss Lake-dwellers; an evening scene, Bronze age." The full-dressed man is paddling home in his dug-out; the children, thinly clad, look out from the railing; the woman, wearing a black dress with a tartan-plaid skirt, is fishing with rod and line and float; cattle are coming along the bridge, followed by two men carrying a dead deer.

Plate VII., "An Interment in a Long Barrow, Later Stone Age." A dark group of mourners, bearing a corpse, which is tied up nose and knees (in orthodox fashion), towards the entrance of a chambered burial-mound.

Plate VIII., "The Warrior's Courtship, Denmark; all the clothing, weapons, ornaments, taken from actual discoveries in the peat of Denmark. Bronze Age." This rests upon Dr. Sophus Müller's outline restorations in his "Voroltid." The modest-looking young woman, without shoes and stockings, with her water-jar at hand, has been gently detained by a good-looking, trowserless spear-man. Possibly he will abduct her (according to the custom of marriage by capture) if she will not go with him willingly.

Plate IX., "Two British Warriors. Bronze Age." Specimens of cavalry and infantry, better than the Roman writers describe them. It is pleasant to see the old Britishers rehabilitated.

Plate X., "The Construction of Stonehenge" by the Dwarfs (!) according to H. N. Hutchinson, C. H. Read, and Flinders Petrie, see page 279.

American Ethnology.

The Thirteenth Annual Report of the Bureau of Ethnology. By J. W Powell. Pp. lix., 462, pls. lx. Washington: Smithsonian Inst., 1896.

The Annual Reports of the Bureau of Ethnology, addressed to the Smithsonian Institution, have so long been amongst the most valued publications devoted to the study of human races, both living and extinct, that general laudatory remarks are unnecessary on the appearance of a new volume. . Suffice it to say that the consistently high standard of excellence observable in the previous volumes of the series is fully maintained in the report issued in 1896, under the able directorship of Major J. W. Powell.

In his general introductory remarks, the Director gives information regarding the organisation and operations of the Bureau. He describes the gradual extension of the lines of research promoted by it, as new fields of investigation suggested themselves as necessary, in view of a complete elucidation of North American ethnology and archæology. Incidentally one may gather some idea of the enormously rich and seemingly inexhaustible field of exploration open to American investigators within the confines of their own national boundaries. One cannot but be struck with the great activity of researchers in ethnology in the United States, under the auspices of a government which liberally contributes funds for that most important branch of study, and lament the scarcity of funds devoted to the purpose in our own country.

The first paper in the volume is one by Professor W. H. Holmes on "Prehistoric Textile Art of the Eastern United States." This writer has already devoted much attention to the subject, and his skilful restoration models of ancient textiles from their negative impressions in clay (pottery, etc.) are well known. The work is important as forming a portion of the investigations of the condition of the Mound-Builders, conducted with a view to determining who these people were, and what relationship they bear to the modern Indian. Professor Holmes' researches have borne out those of others in other branches of the subject, and tend to identify the culture of the Mound-Builders with that of the modern Indians, who are presumably their descendants. Perishable structures, such as textiles, are apt to be conspicuous by their absence amongst prehistoric remains; but, as Professor Holmes relates, he has drawn his material largely from fragments preserved accidentally by contact with copper, whose oxides have proved preservative, or through the agency of salts contained in the earth, or by charring, or, as already stated, from negative impressions of textiles in burnt clay. The many illustrations are clear and well chosen.

A paper upon "Stone Art," by Mr. Gerard Fowke, surveys the different articles of stone found in the ancient mounds, and under various circumstances, on or below the surface of the ground, together with those obtained from living Indians. Here again, a comparison of the stone art-products of the Mound-Builders with those of the modern Indian, reveals a strong kinship between the two, helping to link their ancient and recent makers into one family. The classification adopted in the paper is one which has its conveniences, especially for cataloguing purposes, but its value is practical rather than scientific. Classification is difficult where function is unknown, and such group names as "Banner-stones," "Gorgets," "Cones," etc., must of necessity remain indefinite and unsatisfactory until the use of the articles comprised within the groups has been determined. The tables of geographical distribution are excellent for reference, and the illustrations numerous and good.

Elaborate surveys of aboriginal remains in the Verde Valley, Arizona, are described by Mr. Cosmos Mindeleff, who gives a sketch of the physical features of the district, which have played so important a part in determining, here as elsewhere, the nature of the habitations and mode of life of the inhabitants. The remains of stone-built villages, isolated lodges, and the curious and interesting cave-dwellings, are described in detail; and extensive irrigation works proclaim ingenuity in agricultural processes and skill in overcoming a natural aridity of soil. The "cavate" lodges were evidently applied largely to purposes of defence. Mr. Mindeleff refers the occupancy of this region, as indicated by the ruins, to a comparatively late period. Many excellent photographs and sketches are reproduced.

The Rev. J. O. Dorsey supplies a study of the Omaha tribe, or rather a description of their dwellings and implements, the result of personal observations dating from 1878. The Omahas were a truly warlike and hunting people, migratory in their habits, a characteristic which is at once revealed by the merest glance at their various appliances. For them, as for so many North American tribes, the extinction of the bison has led perforce to a gradual change in habits. The paper contains much information in a condensed form, and the descriptions notice details too often omitted in ethnographical accounts.

A second paper by Mr. C. Mindeleff is devoted to a description of the important "Casa Grande Ruin" of Arizona, discovered in 1694 by Padre Kino, and in ruins even then. It is, perhaps, the best known example of aboriginal architecture on a large scale in the United States, and is now being carefully preserved with its walls strengthened and supported, a special grant of money having been voted by the Senate for that purpose.

The last paper is one on "Outlines of Zuñi Creation-Myths" by Mr. Frank Hamilton Cushing, who is, perhaps, the chief authority upon matters connected with the inner life and philosophy of the natives of New Mexico. Few modern primitive peoples have received so much and such careful study as have the inhabitants of this region, and Mr. Cushing's initiation into a tribe, even into its "priesthood," has given him unrivalled opportunities for the investigation of the doctrines and folk-lore of the people. The importance of a study of myths and legends rests largely upon the light, shadowy it is true, which is thrown upon the early history and migrations of a tribe; and the cosmogony of the Zuñis is interesting, not only *per se*, but also because of the side-lights which illumine the often ill-defined transitional periods of culture. The creation-myths of the Zuñis are, in fact, highly suggestive, and have a value apart from their mere intrinsic interest. Mr. Cushing's style of writing is admirably adapted to the description and translation of the legends and folk-tales of the Zuñi Indians. Throughout them runs a truly poetic strain, and the primitive, though often somewhat intricate, philosophy is tempered with many a graceful conception and poetical allusion, whose quality can only be rendered by a sympathetic writer. The paper is but a portion of a wider survey of the subject, and its author promises fresh communications upon the cultural characteristics of the Zuñis, with a view to elucidating further the meaning of the Myths of Creation. H. BALFOUR.

BRITISH ARCHÆOLOGY.

THE RELIQUARY AND ILLUSTRATED ARCHÆOLOGIST. Edited by J. Romilly Allen. New series, vol. ii. 4to. 260 pp. Plates and many illustrations. London: Bemrose & Sons, 1896. Price 12s. nett.

THIS exceedingly interesting and well-illustrated quarterly, which has now reached the second volume of a new series, is devoted to the study of the early pagan and christian antiquities of Great Britain; mediæval architecture and ecclesiology; the development of the arts and industries of man in the past ages; and the survivals of ancient usages and appliances in the present.

As might be expected, we have a valuable paper on the Cupand-Ring sculptures by the editor, those of Ilkley-in-Wharfedale, the very interesting curved swastika of Woodhouse Crag, and a picture of the Doubler Stones on Silsden Moor, being reproduced among the many illustrations. Mr. Alfred W. Johnston writes on the "Dwarfie Stone" of Hoy, Orkney. This is a block of Old Red Sandstone, 28 feet, by 14 feet, by 3 to 6 feet high, and has been excavated near the larger end, to form two chambers, a large stone lying alongside having apparently served at one time as a door. The various legends connected with the stone, which was first referred to in 1529, are discussed, but Mr. Johnston inclines to the belief that it was originally a place of sepulture. Other papers in the volume, of special interest to us, deal with flint implements, and an exploration of Rains' Cave in Derbyshire, a note on which will be given in our Notes and Comments.

More Reliquiæ Diluvianæ.

The Testimony of Science to the Deluge. By W. B. Galloway, M.A., Chaplain to the Earl de Montalt. 8vo. Pp. vii., 166. London: Sampson Low, Marston & Co. [no date: 1896?]. Price 6s.

The object of this book is to prove that the "so-called glacial deposits" are due to a great deluge produced by the shifting of the earth's axis of rotation, any sudden change in which would undoubtedly have interesting results. The author's main qualification for the difficult task of the reconciliation of the teachings of Moses and Lyell apparently is that he is chaplain to the Earl de Montalt. The author is conscious of the importance of his mission and has been at elaborate pains, he tells us, "to make double sure of every step of" certain mental processes which he fondly calls "his reasoning." That we should not dignify them with this name is probably due to deficiencies in the author's expression rather than in anything else. He tells us "of a Great Ice Age of enormous extent, depth, and duration." We should not be surprised to hear that the author regards an ice age as the same thing as an ice-sheet; but if he does not, perhaps in his next edition he will be good enough to explain what is the extent and depth of an age. By the science of to-day the author sets little score; but there are hopes for it in the future, for he assures us that "science will right itself." He seems, however, doubtful about it, and so does his best to help the somersault. There is very little in the book that we agree with, so we hasten to quote his maxim that "science does not rest on the authority of names." Assuredly it does not rest on the authority of the names he adduces in support of his propositions; for he goes to Cuvier and Josephus for anatomy, to "a man of science" (Mr. Mungo Ponton, F.R.S.E., to wit), for biblical criticism, to the late Dr. Pye-Smith for botany, to Job for terrestrial physics, and to Sir William Dawson and Professor Hull for geology. The author had hopes of Sir Joseph Prestwich, but he died still encrusted by a tenacious theory; so that another good man is lost. The most important information we have derived from a perusal of this work is that the Earl de Montalt keeps a chaplain.

Good News for South Africa.

Stanford's Compendium of Geography and Travel. (New Issue). Africa, Vol II. South Africa. By A. H. Keane. Pp. xvi., 671, with numerous illustrations. London: Edward Stanford, 1895. Price 15s.

This volume represents an enlargement of half of Keith Johnston's once useful work on Africa, of which the last edition was issued in

1884. Information has been accumulating at such a pace and recorded in so many languages, that it was hardly to be expected that the work could be made as complete as the original edition was at the time of its publication. Mr. Keane tells us that "in some instances many carefully prepared pages have had to be greatly modified and even rewritten" during the preparation of this edition. It is no doubt due to the haste of these alterations, that numerous errors have crept into the work. The editor thinks the volume will be "welcome to students unable to consult the innumerable books of travel, scientific periodicals, and memoirs" on South Africa; but we hope that no serious student will trust to the present volume as a summary of the results of even the most important works on that district. Thus in spite of all the recent explorations of the Cameroons, the author concludes his short list of references with the journey of Lieutenant Morgen in 1890-91, (p. 4). The ethnographical sections of this volume and the maps by which it is accompanied are excellent; but the rest is less satisfactory. The author's political judgment may be estimated by his naive reference to the time "when the little gap between Tanganyika and Mfumbiro is obtained by agreement with Germany and the Congo State," (p 333); and his political knowledge by the assurance of "the supremacy of the English language as the almost exclusive instrument of education throughout the South African Republic." We wonder whether the people of Pretoria will be as pleased at this assertion of the unimportance of the Dutch language, as the inhabitants of Johannesburg are sure to be by this rude exposure of their most notorious grievance. The author's competence to handle zoölogical problems may be gauged by the statement that "the Cameroons do not come within the zone of the true tsetse fly, which is here represented by the *Glossina*, an apparently closely allied but harmless species." The compiler of a book on South Africa might have known that *Glossina* is not a species but a genus; that the fly thus named is not harmless but deadly; and that instead of being closely allied to the tsetse-fly, it is the tsetse-fly itself, the name of which is *Glossina morsitans*. The author's choice of authorities is not always happy; it is amusing to those who have followed on Bateman's track, and know the nickname by which he is called on the Congo, to find him ranked with a few men such as Grenfell, Johnston, and Arnot, as one of the "careful observers" who have studied the Congo natives.

As an example of the author's speculations on physical geography we may quote from p. 27; "Ascension lies right in the track of the south-east trades, under whose influence, perhaps increased by the occasional crash of huge icebergs from the Antarctic regions, the Atlantic billows, twenty to thirty feet high, break with fury against the windward coast." Ascension is situated only 9° from the Equator, and the Antarctic icebergs do not come within thousands of miles of it; their crashing can have no more effect on the surf of Ascension than on the ripples of the Hampstead ponds.

The book, however, ought to have a good sale among members of the Stock Exchange, for its remarks on the mineral resources of Bechuanaland and Rhodesia are most encouraging. The author quotes (pp. 391-392) a long paragraph from that optimistic reporter Mr. F. Mandy, to the effect that the northern slopes of Mashonaland "will eventually prove to be the alluvial goldfields of the world. The neighbourhood of the Amazoe and its tributary streams is a veritable El Dorado." This the editor states to be borne out by recent research, a remark which ought to be very encouraging to city financiers when coming from such a geographical authority as Mr. A. H. Keane. The

French in Madagascar seem also to have hit upon a rich thing in gold mines, if the quotation on p. 608 is to be relied on ; and it comes from a work by Zélie Colville, who, from the extent to which Mr. Keane quotes from her, is apparently *the* authority on the geography of Madagascar.

The Cell-Nucleus in Plants.

Die Morphologie und Physiologie des pflanzlichen Zellkernes; eine Kritische Litteraturstudie. Von A. Zimmermann. 8vo. Pp. viii., 188, with 84 figs. in text. Jena: G. Fischer, 1896. Price 5 marks.

Many botanists will probably remember that some years ago there appeared in the *Beihefte zum Botanischen Centralblatt* a series of useful abstracts of current cell-literature, from the pen of Dr. Zimmermann. Since that time an enormous amount of new facts has been accumulating concerning the cell-nucleus, and the object of the book before us is to place its readers *au courant* with the present state of our knowledge respecting this cell-constituent, together with such other protoplasmic structures as may be directly associated with it.

There is no doubt whatever as to the usefulness of the work. The author has fulfilled the task he set before himself in a business-like and thoroughly conscientious manner, and he well deserves the thanks of all who wish to get an idea of what is doing in cytology. At the same time, though it is, perhaps, rather ungracious to say it, we cannot help finding the book rather dull. The reason lies in the fact that the author, in his anxiety to omit nothing which may be considered as in any degree important, has poured out torrents of details, and these lie thickly strewn over his pages, with little or nothing to indicate their co-ordinate or subordinate relationships one with another. It is all very well to present, in a concise form, the facts and the conclusions which their discoverers may have drawn from them ; but if the reader is to get much of a living idea of what it is all about, he must have already devoted a good deal of time to the first-hand literature himself. In short, we confess the book strikes us as being rather too much of a note-book—a good note-book, it is true, tolerably complete, and containing very few mistakes withal, but a note-book still.

And seeing that Dr. Zimmermann has proved himself to be an able investigator in the domain of cytology, it is the more to be regretted that he has not seen fit to adopt the *rôle* of exponent a little more definitely. No doubt it is premature to expect to found theories which are likely to prove permanent, and possibly this consideration may have had due—we had almost said undue—weight with the author when he was outlining the plot of his book.

But, after all, our criticism is chiefly directed against the method which Dr. Zimmermann has decided to adopt ; for the way in which he has followed it out we have little but praise. The clear and succinct paragraphs, in which he deals with the memoirs of the huge number of authors whose works he cites, will earn for him the gratitude of many who are debarred, by lack of time or opportunity, from consulting the originals.

The book is divided into two sections. The first, or general, part contains an account of: 1. Methods of fixation, &c.; 2. The chemical composition (so far as it can be regarded as possibly known, of the nucleus; 3. The phenomena of Karyokinesis; 4. The physiology of the nucleus. The second part discusses the nucleus as it occurs in the various groups of the vegetable kingdom, and the volume concludes with a good bibliography and index of the whole subject.

Thus, as will be seen by this synopsis of its contents, the book is a thoroughly useful, and we may add a reliable, work of reference to the literature on the cell-nucleus in the vegetable kingdom.

J. B. F.

The Booth Birds.

Catalogue of the Cases of Birds in the Dyke Road Museum, Brighton. By E. T. Booth. Second edition (by A. F. Griffith). 8vo. pp. 217, with 16 full-page illustrations. Brighton: King, Thorne & Stace, 1896.

This is a Museum guide-book, not only admirable for the ordinary intelligent public, but containing much of special interest to the field-ornithologist. It is an illustrated reprint of the late Mr. Booth's "Catalogue" of his collection published twenty years ago, and now that this collection has become the property of the Brighton Corporation, the work appears as an official handbook. Apart from a brief history of the bequest there are very few additions to the letterpress, which is based almost entirely on Mr. Booth's personal observations; but the exquisite photographs of the cases with which it is illustrated add greatly to its value, while the portrait of the author and the view of his exhibition room are interesting. The observations on the birds are desultory and make no pretence to systematic treatment, but we think the utility of the volume would be much enhanced if the scientific name of each species were placed after the popular name which is always employed.

Baran.

In common with the *Times*, *Ally Sloper*, and the *Jam-i-jamshed* we have received from Balmokand, cloth-seller of Rawalpindi, a book published at Lahore, entitled "The Priceless Gem," pp. 242, price (including postage) one rupee, of Mr. Balmokand. "The Priceless Gem" sets forth the advantage and necessity of "Baran," the disregard of which in India's degenerate days has "vitiated its [India's] original form and has occasioned the disappearance of those Excellencies (*sic*) and the prevalence of these evils and vices."

"Baran" seems to signify the Eastern equivalent of the Western Temperament, and the four different Barans are set forth at length on pp. 36–38. The work attempts to prove the advantages of marriages between those of similar Baran. Mr. Balmokand is evidently a great reader both of his own and of English literature; and the volume will no doubt be of considerable interest to those who are interested in Indian customs or who make a special study of temperaments. It is translated from Urdu by J. W. Rockwell.

Varia.

L'Annuaire Géologique Universel for 1895 has made its appearance, and this time is confined to the geology of France and her colonies in Northern Africa. It is a great matter for regret that a universal record could not be kept up, for the old volumes were most valuable, but we still have cause to be thankful for the energy of Dr. Carez and his publisher, which gives us so useful a record of the geology of France.

The Winter Number of *The Artist* has been dedicated to a consideration of the art of Frederick Sandys. The public has now an opportunity of seeing the work of this exquisite draughtsman whose art has hitherto been appreciated by a select few. We would especially draw the attention of botanists to the wonderful drawings of trees and tangled vegetation made at Fairlight, near Hastings, and the flowers and plants that Sandys has introduced into his classical subjects. The grace and tenderness of his chalk drawings of women, and his marvellous rendering of a woman's hair make one more than ever lament the praise that has been wasted on the decadents. Messrs. Constable & Co. are to be congratulated on this beautiful number of their magazine.

The *Westminster Review* for October contains an able article by F. M. Lyte, explaining the metric system and advocating its use.

The *Revue Internationale des Archives des Bibliothèques et des Musées* ceases with the publication of its ninth number (December, 1896). The three numbers devoted to museums form a volume of 160 pp., sold separately for 7 frs. 50, by H. Welter, Paris.

The *Journal of Physical Chemistry*, edited by Profs. W. D. Bancroft and J. E. Trevor, is published as a new monthly at Cornell University.

A new semi-monthly, entitled *Chemische Rundschau*, appeared on October 1st, edited by Dr. F. Peters, and published at Berlin. The annual subscription is 16 marks.

A new international bi-monthly, devoted to medicine and medical geography, is published at Amsterdam. Its editor is H. Peypers, and its title *Janus*. Papers are published in various languages, and each number forms a quarto of about 100 pages.

Further Literature Received.

Cambridge Natural History, vol. ii., A. E. Shipley and S. F. Harmer: Macmillan. Habit and Instinct, C. L. Morgan: Arnold. Marketable Marine Fishes of the British Isles, J. T. Cunningham: Macmillan. Present Evolution of Man, G. A. Reid: Chapman and Hall. Report of the Horn Scientific Expedition, pts. i. and iv.: Dulau. Catalogue des Bibliographies Géologiques, E. de Margerie: Paris, Gauthier-Villars. The Cell in Development and Inheritance, E. B. Wilson: N.Y., Macmillan. Treatise on Deep-Sea and Pelagic Fishes, &c., G. B. Goode and T. H. Bean: Smithsonian Inst. British Lepidoptera, vol. iii., C. G. Barrett: Reeve. Determinative Mineralogy, G. J. Brush, revised S. L. Penfield: New York, Wiley.

Physical and Social Heredity, J. M. Baldwin: *Amer. Nat.* Early History of the Manchester Geol. Soc M. Stirrup: *Trans. M G.S.* System der Nordamerican. Schmetterlinge, A. R. Grote: *Mitt. Roemer Mus.* On the Fate of the Blastopore, K. Mitsukuri: *Journ. Coll. Sci. Tokyo.*

Nature, November 26, December 3, 10: Literary Digest, November 14, 21, 28, December 5; Revue Scientifique, November 21, 28, Dec. 5; Irish Nat., December; Feuille des Jeunes Nat., December; Amer. Journ. Sci., December; Naturæ Novitates, October (20), November (21); Science, November 13, 20, 27, December 4; Scott. Geogr. Mag., December; Westminster Rev., December; Botanical Gazette, November; Review of Reviews, December; Pop. Sci. News, December; Knowledge, December; Photogram, December; Journ. Essex Tech. Lab., October. Journ. Marine Zool., Sept. Bol. Mus. Para, Oct.

OBITUARY.

SIR BENJAMIN WARD RICHARDSON.

Born October 31, 1828. Died November 21, 1896.

IN Sir Benjamin Richardson science has lost a busy worker in many fields. Essentially a physician, he was at the same time an active experimentalist in pharmacology, and a prominent worker in the field of hygiene. To these labours he added those of a prolific writer on many topics, and of an ardent temperance reformer. In each of these fields he accomplished much valuable and enduring work, and if in none of them he reached the foremost rank, this is almost a necessary corollary to the many-sidedness of his character.

He was destined from boyhood for the medical profession, so that his aptitude for physical and natural science was early encouraged. At seventeen he was apprenticed to a country surgeon named Hudson, whose tastes lay in the direction of electrical science, and he proceeded thence to Anderson's College, Glasgow. In 1847 he returned, for the benefit of his health, to England, and became assistant first to Mr. Brown, of Saffron Walden, and later to Dr. Robert Willis, of Barnes, who was at that time editor of the *Medical Gazette*. Here he first turned his hand to literary work and became a frequent contributor to the *Gazette* and other periodicals.

He qualified at Glasgow in 1850, and in 1854 took the degrees of M.A. and M.D. at St. Andrew's. In 1854 also he won the Fothergill Medal of the Medical Society foı a thesis on "Diseases of the Fœtus in Utero." He was now appointed lecturer on Medical Jurisprudence, and, in 1857, lecturer on Physiology to the School of Medicine in Grosvenor Place, holding the latter post until 1865, when the School was incorporated with St. George's Hospital. During this period, he became intimate with Douglas Jerrold, Thackeray, George Cruickshank, and other literary celebrities of the time. He became a Member of the Royal College of Physicians, London, and in 1865 was elected a Fellow of that body : two years later he became a Fellow of the Royal Society.

Space fails us to record all the investigations which Richardson undertook during his busy career, but among the more important were his researches into the subject of anæsthetics and the question of alcohol. He experimented largely on a variety of bodies to test their anæsthetic power, and to him is due the introduction of methylene bichloride as an anæsthetic, and the local application of ether spray for producing insensibility in minor operations. His researches on the action of amyl nitrite were likewise of importance. From a humanitarian point of view the most striking outcome of his experiments has been the 'lethal chamber' now employed at Battersea for the painless destruction of superfluous dogs and other domestic animals. As the result of his investigations on the action of alcohol on human beings, he became a total abstainer, and in 1874 delivered the Cantor lectures at the Society of Arts on the subject of alcohol. He was Physician to the Temperance Hospital, and his influence in the promotion of temperance was wide and far-reaching, based, as it was, on sincere conviction and actual experiment.

He was deeply interested in questions of public health, and in 1862 he started the *Journal of Public Health and Sanitary Review*, and edited it for four years: to this he contributed some valuable papers, especially one on the hygienic treatment of phthisis. Later he inaugurated the short-lived *Social Science Review*, to which he contributed several poems and plays. His most important literary effort was, however, *The Asclepiad*, a quarterly periodical which for twelve years he produced—single-handed. Few men of his profession have been more widely known to the public, but this arose less from his actual professional abilities, great though these were, than from the part he took in allied questions, and especially those of hygiene and temperance reform. He was knighted in 1893, mainly in recognition of his services to the humanitarian cause.

WE regret to record the following deaths:—On November 20, DAVID ROBERTSON, the Naturalist of Cumbrae, of whom a longer notice will appear in our next; ARTHUR DOWSETT, President of the Reading Natural History Society, and the owner of valuable ornithological and entomological collections, on November 6; Dr. G. W. CHILD, lecturer on botany in St. George's Hospital Medical School, on December 1; the mycologist and florist. TH. KING, in Fochabers, N.B., aged 62; W. F. AINSWORTH, explorer, well-known for his researches in Asia Minor; Dr. J. A. MOLONEY, African explorer, aged 38, at Surbiton; J. W. RETGERS, known for his work on Isomorphism, on August 9, aged 39; Dr. E. WENZEL, Associate Professor of Anatomy at Leipzig, on October 25, aged 56; Dr. FRITZ WESTHOFF, zoological assistant at the Kgl. Akademie, Münster; in Munich, J. FESER, Professor of Pharmacology in the Veterinary College, aged 56; Professor E. VON WOLFF, author of the celebrated work "Landwirtschaftliche Fütterungslehre," and for forty-two years Professor of Agricultural Chemistry at Hohenheim, aged 78; Dr. A. H. POST, author of "Grundriss der ethnologischen Jurisprudenz," at Bremen; Dr. E. BAUMANN, Professor of Physiological Chemistry in the University of Freiburg i. B., noted for his researches on metabolism and cystin, and the discoverer of iodine in the thyroid gland, on November 2, aged 49.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

The following appointments are announced:—Lord Reay, to be President of University College, in the place of the late Sir John Erichsen; L. E. Shore, to be Lecturer in Physiology at Cambridge, in place of Dr. A. S. Lea, and A. Eichholz, to be additional Demonstrator in Physiology; Dr. Hugo de Vries, to be Director of the Botanic Garden in Amsterdam, in place of Dr. Oudemans; Dr. R. H. Saltet, to be Professor of Hygiene at Amsterdam; Dr. C. Julin, to be Professor of Anatomy at Liège; Dr. A. Möller, of Idstein, to be Lecturer on Botany in the Forestry Academy at Eberswald; Dr. J. Blaas, of Innsbruck University, to be Assistant Professor of Geology and Palæontology; Dr. Karl Möbius, Professor of Zoology in Berlin University, to succeed Professor Beyrich as Director of the Natural History Museum there, and Dr. W. Dames, to be Director of the Geological Department; Dr. H. Traube, to be Professor of Mineralogy, and Dr. O. Seeliger, to be Professor of Zoology, in Berlin University; Dr. K. Mez, to be Professor of Botany in Breslau University; Dr. Emil Schmidt, to be Professor of Anthropology in Leipzig University; Professor S. L. Schenck, to be Professor of Embryology at Vienna; Dr. T. Beer, to be Privatdocent in Comparative Anatomy at Vienna; Drs. B. Issatschenko and M. Grimm, of St. Petersburg University Botanical Laboratory, to be Assistants in the State Laboratory of Agricultural Bacteriology; Dr. Wladislaw Rothert, to be extraordinary Professor of Botany at Kazan University; J. S. Wright, to be Lecturer in Botany at Indiana Medical College; H. M. Richards, to be Lecturer of Botany at Barnard College, New York City; E. V. Wilcox, of Cambridge, Mass., to be Professor of Biology in Montana State College of Agriculture; Dr. Seitaro Goto, to be Professor of Botany in the First High School at Tokyo.

Sir Joseph Lister and Professor Michael Foster have been elected honorary members of the Asiatic Society of Bengal, in place of Huxley and Pasteur.

Professor Albert Heim, geologist, of Zurich, has been elected a foreign member of the Royal Society.

Mr. Michel Levy has been elected a member of the Section of Mineralogy of the Paris Academy of Sciences, in succession to the late G. A. Daubrée.

Professor O. Brefeld, the eminent fungologist, of Münster, has been made Geheim-Regierungs-Rath.

Dr. Müller, Director of the Königsberg Zoological Garden, has retired. Dr. H. G. Hallier has resigned his position at the Buitenzorg Botanical Garden and is returning to Germany.

The Hayden Memorial Prize for 1896 has been awarded by the Philadelphia Academy to Professor G. Capellini, of Bologna.

A statue of Charles Darwin, presented by Professor Poulton, is to be erected in the court of Oxford University Museum, which is also to be adorned by a bust of Sir Henry Acland.

A wing for scientific purposes is to be added to the buildings of Aberdeen University at a cost of about £10,000, which will be defrayed by the city in return for land given by the University Court.

A SCHOOL of science has been founded at Madrid, supported by the state. It comprises twenty-seven professorships, of which *L'Anthropologie* mentions the following, with the professors appointed to occupy them:—Physiological Psychology, Professor Simmaro; Origin of the Castilian Language, M. Pidal; Introduction to Sociology, Azcarate; Structure and Activity of the Nervous System, Ramon y Cajal; Anthropology of Spain, M. Anton; General Evolution of the organic and inorganic kingdoms, Cortarar.

A BACTERIOLOGICAL laboratory in connection with the veterinary department of Pennsylvania University has been established under the directorship of Dr. M. P. Ravenal to investigate the diseases of poultry and cattle.

THE visitors to the Huddersfield Economic Museum during the past year numbered 878, we learn from the report published in its organ, the *Naturalists' Journal*, for December. Mr. Mosley is a little disappointed with the lack of support, both to the Museum and the *Journal*, but is hopeful of the future. The Museum will not be open on Sundays, but visitors can be admitted if notice be given beforehand.

WE learn from the *Halifax Naturalist* that the Museum of Halifax (Yorkshire) is being removed to new quarters at Belle Vue. The borough of Todmorden has received from the local Botanical Society the gift of its library, and botanical, geological, and mineralogical collections.

AN ethnographical museum is to be founded at Budapest, of which the objects exhibited in the ethnographical section of the Millennial Exhibition are to form the nucleus.

DR. L. SERRURIER, Director of the Ethnographic Museum at Leyden, has resigned, because the state refuses to rebuild the museum.

THE balance over from the local fund, after paying the expenses of the British Association meeting this year, is about £980, which is to be invested, and the interest devoted to furthering the scientific work of the Liverpool Marine Biology Committee.

THE following are among the forthcoming afternoon lectures at the Royal Institution:—On Tuesdays, from January 19 to April 6, Professor A. D. Waller, on Animal Electricity; Professor H. A. Miers, three lectures on Some Secrets of Crystals, on January 21, 28, and February 4; Dr. J. W. Gregory, three lectures on the Problems of Arctic Geology, February 11, 18, 25; Professor W. Boyd Dawkins, three lectures on the Relation of Geology to History, on March 25, April 1, 8. The Friday evening meetings, for members and friends only, include Professor J. Milne, on Recent Advances in Seismology, February 12; Lieut.-Col. C. R. Conder, on Palestine Exploration, February 26; S. Bidwell, on Some Curiosities of Vision, March 5; Sir W. Turner on Early Man in Scotland, March 26.

AT the request of Professor Meldola and Mr. T. V. Holmes, president and secretary, respectively, of the Corresponding Societies Committee of the British Association, Professor G. S. Boulger, on December 12, delivered an interesting and suggestive address, before the Essex Field Club, on the "Federation Ideal for Natural History Societies, with Special Reference to the Eastern Counties." He dwelt on the scientific importance of the organisation of all the natural history societies, and pointed out the benefits that would accrue from joint work, mutual help, and joint publication, and the need of more individual workers. Communications from various local societies of East Anglia were laid before the meeting, and a lively discussion followed. Those interested in the subject may be referred to papers entitled "What shall we do with our Local Societies?" by Professor G. S. Boulger, (NAT. SCI., September 1896), and "The Organisation of Local Science," by G. Abbott (NAT. SCI., October, 1896).

THE German Fisheries Association offers a prize of 600 marks for the best essay on the history and biology of *Leptomitus lacteus*, with special reference to its appearance and disappearance in impure water. Papers must be sent in before May 1 next, to Mr. Weigelt, 90–91 Zimmerstrasse, Berlin, S.W.

AN interesting lecture on "Flight, Natural and Artificial," was delivered by Dr. G. H. Bryan, F.R.S., in the Imperial Institute on November 30. The advantage of a screw propeller over a a flapping method was illustrated by some little models, which were thrown up, and floated in a very bird-like manner. Lantern slides of various old and new machines were exhibited, and Mr. P. S. Pilcher's machine, which was displayed in the hall, was explained by the inventor.

DR. JOHN MURRAY, of the "Challenger," was on November 30 presented by the contributors to the Report with a handsome morocco album, containing all their portraits. The dedication plate was designed by Mr. Walter Crane.

ALTHOUGH the Russian universities are still closed to women, a medical school is to be founded for them in St. Petersburg.

THE Röntgen rays applied to palæontology, in the shape of skiagrams of fossils embedded in Chalk from near Rheims, were recently exhibited to the Paris Biological Society by Mr. Lemoin. The disputants over *Archæopteryx*, to whom we suggested this method of settling their differences, have not yet communicated their results to us.

THE quadrigentenary of Vasco da Gama's discovery of the sea-route to India, which was to have been celebrated at Lisbon in 1897, has been postponed till May, 1898, that being the month on which the explorer reached Calcutta 400 years before.

THE eleventh German Geographical Congress will be held at Jena on April 21, 22 and 23, when the subjects discussed will include biological geography and polar investigations.

AN international congress of experimental and therapeutic hypnotism is to be held in Paris in 1890.

IN consequence of unforeseen delays, it has been found desirable to extend the time allowed to competitors for the Welby Prize (announced in our last number) till January 1st, 1898. Professor Emile Boirac, Paris, is to be the French member of the Committee of Award.

MEXICO now ranks among the progressive nations who have legalised the use of the metric system. The Liverpool Chamber of Commerce, too, has unanimously passed a resolution in favour of adopting this system, and urging the necessity of bringing in a Bill for that purpose.

WITH reference to Dr. Ohlin's account of the Swedish Expedition to Tierra del Fuego, which appeared in NATURAL SCIENCE for September last, it is of interest to note that the first results of the Hamburger Magalhaessischen Sammelreise have just appeared. Lieferung I. is published by the Natural History Museum of Hamburg, and contains a general account of the expedition, by Dr. Wilhelm Michaelsen, and a particular account of the Vermes by different authors.

Professor D. G. Elliot says, with regard to his expèdition into Somaliland, that he has obtained a very extensive collection, chiefly of the large mammals, probably the most complete ever brought out of any country by one party. No fewer than fifty-eight cases and barrels were shipped direct from Aden. After this we need not remind our readers that the destination of the barrels was Chicago. He also obtained over 300 specimens of birds, fish, insects, and reptiles: this is not much.

Mr. Bastard, who is exploring in Africa, has been prevented by the trouble in Madagascar from penetrating into the interior of that island. He has, however, made good collections of fossils, also anthropological measurements and photographs.

Mr. Voillot has returned from a voyage to Haute Mamberé; he brings with him ten Baya skulls and an interesting ethnographic collection. Another valuable anthropological collection is that brought back from Russian Asia by Mr. E. Blanc.

Of Mr. Alexander Whyte's explorations in the Karonga Mountains in Central Africa, we learn that the results include 6,000 dried specimens of plants, 5,000 land-shells, 3,000 insects, numerous mammals, reptiles, geological collections, and so on. The climate was far from healthy, and several members of the party suffered from cold and sickness.

The *Daily Chronicle* of December 5, gives some interesting news of the French Hourst Expedition to the country behind the Niger Coast Protectorate; from it we learn that, in spite of difficulties, the results have been very valuable, and that the whole party returned well after an absence of three years. The war-songs of most of the tribes have been taken down by the phonograph, silhouettes of native heads and many photographs were made; and the geological and geographical surveys are also very complete.

The proposed Belgian Antarctic expedition, headed by A. de Gerlache, is to start on board the "Belgica," in the middle of next July. A laboratory is being constructed on deck for the scientific members of the expedition, who include a geological chemist, Mr. Archowsky; a meteorologist, Mr. Danco; a doctor and naturalist, Mr. Taguin; and Mr. Racovitza, who will conduct the dredging operations. The work of the expedition will be carried on so far as possible on the same lines as that of the "Challenger" expedition.

Professor Penzig, of Genoa, editor of *Malpighia*, has undertaken a botanical expedition to Buitenzorg, Singapore, and Ceylon.

Dr. Grünling, Curator of the Mineralogical Collection in Munich, has gone to Ceylon on an exploring expedition.

An agricultural experiment station, which will undertake very useful work, has been founded at Usambara, in German East Africa.

On Thursday, December 17, almost exactly at 5.30 a.m., an earthquake shock, severer than has been felt for many years, affected almost the whole of England and Wales. The exact position of the seismic focus has not yet been determined, but Hereford seems to have suffered most, for here a woman died of fright, the pinnacles of St. Nicholas' Church fell, and a pinnacle of the cathedral was damaged. Premonitory shocks were felt at various places from about 3 a.m., and a few after-shocks were also experienced.

CORRESPONDENCE.

Geologists and Specialists.

It is not surprising that some geologists should have protested against your note on "Pavingstone Palæontology," because its language was not very happy and did not seem calculated to smooth the way for bringing geologists and zoologists into closer touch with one another, which it now seems was your real intention.

It is satisfactory, however, to find that you are able to respond to the letter of "Stratigraphical Geologist" by giving a preliminary list of specialists who are willing, under certain conditions, to examine and name fossils collected by field geologists. I have long felt that there ought to be closer scientific relations between the specialist in the museum and the geological worker in the field. The specialisation of our science has now reached a point when the one set of workers cannot achieve satisfactory results without the assistance of the other set. I heartily endorse the remarks made by Mr. Marr in that portion of his presidential address which you quoted in your November number (p. 285), and I would remind you that they included some good advice to the specialists, who must be careful not to take too comprehensive views of species, and must remember that they too have something to learn out of doors. As Mr. Marr says, "the specialist who remains in his museum examining the collections amassed by the labours of others, and never notes the mode of occurrence of fossils in the strata will perhaps soon be extinct." By all means let us join hands, and remember that we can each help the other, and that we each have much yet to learn from Nature.

Coming now to the question of publishing a list of specialists, I think you are quite right in maintaining the desirability of making it more widely known that there are so many specialists who are ready to help any *bonâ fide* worker by naming fossils for him. I have received much kind assistance from several of the gentlemen on your list, but there are others to whom I have not hitherto ventured to apply for assistance because I had no assurance that they would be willing to give it.

I am sure there are men belonging to natural history societies and field clubs in various parts of the country who are quite capable of working-out local geological details, but who are deterred from trying because they do not know how to get their fossils named; there are doubtless others who make the endeavour, and who publish lists of fossils which are not as accurate as they might be.

If, therefore, any of the palæontologists to whom you have written have withheld their names because they think it is unnecessary to give them, I can assure them from personal experience that they are mistaken.

I may likewise point out that there is also such a man as a stratigraphical specialist, a student of one particular set of rocks, and that he may sometimes act as a useful intermediary. For instance, I am quite willing to examine collections made from any Upper Cretaceous strata, provided localities and horizons are indicated, and to name the fossils to the best of my ability. Any species which I could not determine with certainty might then be forwarded to specialists either by me or by the collector himself.

I trust you will shortly be able to publish a longer list of names, and I shall hope to avail myself of such assistance still more fully in the future than I have done in the past.

A. J. Jukes-Browne.

[We beg to thank the six friends who have expressed their willingness to have their names added to our list of specialists. We intend to publish a supplement to the list next month.—Ed. Nat. Sci.]

ORANG-OUTANG OR ORANG-OUTAN.

DR. ARTHUR KEITH remarks in his valuable paper on Anthropoid Apes, vol. ix., p. 316, note, "Temminck gives Orang-Outan as the correct spelling; Sal. Müller, who was familiar with the Malay language, rendered it Orang-Oetan, but Orang-Outang is the form in most common use." If Orang-Outang really is the form still in most common use, it only proves how difficult it is to remove an inaccuracy which has once crept into literature. *Orang utan* means "man of the forest," *Orang utang* would mean "a man who has debts." As far back as 1877 (*Mitth. Zool. Mus.*, II., p. 225, note) I showed this to be the case, and, more recently (*Abh. Mus. Dresden*, 1894–5, No. 14, p. 6, note) I recurred to the question with the following words, which I beg permission to quote translated: "With what difficulty even small faults get out of books, is proved by the spelling of the word Orang utan. Mostly one still finds Orang utang, though Müller and Schlegel so early as the year 1840 drew attention to the falseness of this spelling (*Verh. Nat. gesch. overz. bez.*, p. 11); later, in 1866, Gratiolet and Alix censured it (*N. Arch. Mus. Paris*, II., p. 4, note 1). In 1877 I remarked thereon (*l.c.*), and recently Jentink recurred to the incorrect 'g' (*Notes Leyden Mus.*, 1895, p. 17, note)." "Oe" in outan is the Dutch spelling; "ou," the English and French; and "u," the German, according to the pronunciation of the respective languages.

R. Zoological Museum, Dresden, A. B. MEYER.
9 *Dec.*, 1896.

AUSTRALIAN PICTOGRAPHS.

WITH reference to our Note on this subject (vol. ix., p. 80), in which we called attention to discrepancies between the descriptions and the drawings published by Mr. R. L. Jack, that gentleman informs us that his descriptions are accurate, but that his faithful copies of the drawings were lithographed by a careless draughtsman, whose errors Mr. Jack had no opportunity of rectifying.

DR. WHEELTON HIND'S "MONOGRAPH ON *Carbonicola, etc.*"

IT is with repugnance and regret that we publish the following letters from Dr. Wheelton Hind and Mr. W. F. Holroyd. We have urged both of these writers to expunge the personalities that they have permitted themselves to substitute for argument. Since, however, both of them seem unable to appreciate our objection, and insist on the publication of this or nothing, we have finally decided to follow our practice of allowing free voice to either side. Therefore, after due warning, the letters are printed *word for word* as their authors finally sent them to us.

We could not ask our Reviewer to reply to letters of this nature in other terms than those chosen by him; but in defence of ourselves against Dr. Hind's fina paragraph, we may point to his own assertion that Mr. Bolton informed him last summer that he thought of writing a review of this Monograph. For the rest, we leave it to any of our readers that have a liking for this kind of thing, to contrast the statements now made by Messrs. Hind and Holroyd with the Monograph itself, and with the remarks of our Reviewer.—ED. NAT. SCI.

IN your number of Natural Science for Dec. 1896 there appears a review of my Monograph on Carbonicola, Anthracomya and Naiadites, the nature of which from more points than one makes it necessary for me to make some remarks thereon; not I hope that I for one moment object to any fair and honest criticism, which the work doubtless calls for, being the production of a mortal.

The reviewer objects to the substitution of the generic term *Carbonicola* M'Coy, for *Anthracosia* King, a replacement which I deeply regretted, but according to the present rules of nomenclature I had no choice in the matter. Unfortunately in endeavouring to show grounds for the retention of *Anthracosia* the reviewer suppresses one portion of the evidence, and invents another. Briefly the position is as follows.

Sowerby, in 1829, describes Min. Conch. 2 coal measure shells as *Unio acutus*, and

U. subconstricta. J. de C. Sowerby in 1840 described *U. robusta* Geol. Coalbrookdale. In 1844, Prof. King proposed the name "*Anthracosia* for a group of Unionidæ characteristic of the coal measures" without figures or description as the reviewer says a preliminary notice of the worst sort. In Dec. 1855 M'Coy published the 3rd fasciculus of his work on the Synopsis of the Classification of British palæozoic rocks with a Diagnosis of *Carbonicola* to which genus he referred the U. acutus, U. subconstricta, and U. robusta, of the Sowerbys and U. turgeda, Brown. A mistake was made in describing the genus as possessing a posterior lateral tooth. King on receiving a copy of this work immediately published in Jany 1855 (Ann. Mag. Nat Hist 1856) a description of *Anthracosia* and in a postscript dated Dec. 27, 1855, says, "My friend evidently thinks it synonymous with Anthracosia which he admits being aware I intended describing; However, if the genus *Carbonicola* possess the characters diagnosed by Prof. M'Coy, it is clearly not the same as my *Anthracosia* which does not possess lateral teeth." There can thus be no doubt as to the priority of Carbonicola for one mistaken character in a description cannot invalidate, but the reviewer drawing on his own fertile imagination says "M'Coy failed to understand the genus he described and his diagnosis could only have been drawn up from specimens of other genera from a younger formation." His accusation is completely wrong for M'Coy correctly referred the four species mentioned above quite correctly to his genus, where as King described a new specific type and only referred one of Brown's many forms to his genus. It turns out too that the form of hinge described by King obtains only in the species of C. aquilina, another of J. de C. Sowerby's types. I did suggest that the lateral tooth was inferred from the close affinity Carbonicola has to Unio, but there is not the slightest foundation for the reviewer's statement; as it will appear later, the reviewer knew of this change and the reasons for it long before the work was in type and he wrote me that though he regretted the change, it seemed to be enevitable.

My reviewer seems very troubled that species and varieties are not defined with mathematical accuracy. This is of course the resultant of his long application to museum work, but he would go further for in a letter dated Nov. 20, 96, he writes me "Your definition of species is very faulty but for this I blame the Palæontographical. They should establish a set of rules for the guidance of workers so that all who consult their publications may know what view is taken of specific distinction as a whole. As it is each man is a law unto himself and the modern palæontologist stands on one side and scoffs." It is what I imagine even you, Sir, would call "high falutin," but it argues the possession of a mind more adapted to mathematical formulæ than to Biology. On one hand, he demands mathematically defined species and on the other, grumbles that the way species seem to pass into each other is not better shown.

Another point objected to is the invention of species on one or two examples, and I cordially agree with him, but this is a matter of personal observation and it is the easiest and cheapest form of criticism to judge work in the absence of specimens. It is at times necessary to describe unique examples, but it should be done with great caution. In two of the cases the reviewer will be glad to learn that other specimens have turned up which I think justify my views. In the case of the figures of Anthracomya dolobrata the reviewer objects that the type differs from the other shells referred to the species. Here he is not making allowance for the imperfections of the type and consequently his arguments are valueless. He also suggests that C. subconstricta is the internal cast of C. robusta, but he has not noticed that Sowerby's type is a testiferous example. Several of the other figures of this species are those from which M'Coy redescribed this species and are in the Woodwardian collection Cambridge. One more example of the reviewer's methods will suffice. He says "the specimens 6 & 7 Pl ix. are taken as types of C. aquilina, but they might equally serve as elongated forms of C. acuta." It is evident to me that he has not yet arrived at the fundamental differences between my descriptions of these two species, but it so happens that the two shells in question were brought to my house by the reviewer himself and that it was largely on his suggestion that they were referred to C. aquilina. Reviewers should really be careful in little transactions of this kind or the value of their remarks is liable to be heavily discounted.

The reviewer finds it difficult to appreciate the differences between Anthracomya and Naiadites, and grumbles that I do not contrast them. I really did not think it necessary to contrast genera of two different Families. He quotes a remark I make about the difficulty of distinguishing compressed forms of one species when the periostracum only is left and would make out that I apply it to the whole genus. Finally the reviewer remarks that the shells "are not described with uniformity and that it is therefore difficult to compare a couple of descriptions." Surely this is pure imagination on his part, for on rereading them I can only find one example of a fault of this description i.e. in the case of C. Polmontensis.

After inveighing in several places upon the absence of indications of varietal forms, and the passage of species towards each other, he finds fault with the excessive number of figures, which are numerous simply to demonstrate the facts which the reviewer supposes absent. He grumbles that I have in any way allowed horizonal distribution to come into the determination of Species but the answer is that in the same bed the intermediate forms if any are always present but in beds separated by many feet of strata this sort of evidence is necessarily absent.

On p. 36, I find that I specially thank Mr. H. Bolton for "looking over my specimens, MS. and proof; and for several observations which have been of great service to me in the preparation of the section on *Carbonicola*." Surely the grave faults which he now finds in my work should, in common courtesy, have been pointed out before publication, in the present case he forgets that every criticism he makes is an attack either on his own accuracy of observation or on his character as a proof-reader and adviser. Is there no etiquette in a matter of this sort, or is it permissible to pretend to afford every help possible in the production of a work, but at the same time to make reservations on which an attempt may be made to pull the whole work to pieces at some future date?

Stoke-on-Trent, Dec. 3, 96. WHEELTON HIND.

YOUR last issue contains a review by "H.B." of Dr. Hind's recently published Monograph of Carbonicola, Anthracomya and Naiadites." Your critic is evidently very inconsistent inasmuch as he points out, in the same sentence "signs of great haste" and yet considers the monograph as a "valuable contribution." If there is one thing more noticeable than another, in reading over the "Monog" it is that the author has been most careful and deliberate in dealing with what everyone who knows anything of the subject, considers a most difficult group to work. Evidence of this is given in the careful and elaborate study and delineation of the interiors.

Let us examine some of the so called omissions and commissions alleged by the reviewer, and first, the replacement of name "Anthracosia" by that of "Carbonicola." Dr. Hind's reasons page 39, vol. xlviii., are quite sufficient and conclusive to authorize him to drop "Anthracosia" and retain "Carbonicola." "McCoy" did describe the genus, even if he gave a rather erroneous diagnosis. "King" neither described nor figured. When, a year later, "King" described his Anthracosia, he gave the hinge and teeth of C. aquilina, a specimen which is not characteristic of a genus but only of a species.

The demands by H. B. for absolute fixity of a species and variety are absurd, inasmuch as the Carbonicola are so variable, and therefore we agree with Dr. Hind that it is better to take as types those specimens which are most typical, in a certain set from the same bed. It is further charged by "H. B." that no gradation of species is sufficiently recognized, but if so, why has Dr. Hind given such numerous figures —thirty-six for example of C. aquilina, Again, why does "H. B." think there are too many figures when he so clearly recognises the necessity of comparing a large series of specimens.

Another charge is that Dr. Hind fails to distinguish what constitutes a species. Will "H. B." kindly define what he thinks, exactly constitutes a species. ? With regard to A. dolobrata and A. Adamsii, I have seen both specimens myself and consider Dr. Hind quite correct in his naming of them. Again—species are said to have been founded upon one or two specimens. This is true but, the said species have received due consideration, and such naming is sometimes quite unavoidable, besides which, the matter has been fully justified in two cases by the finding since of other specimens. Further justification for naming single specimens is afforded, when they are so clearly differentiated from well known types, as in A. Lanceolata, page 105, vol. xlix. With regard to C. Rugosa and C. robusta, even if they should ultimately be proved to be of the same species, it would be obviously unfair to delete the species in the absence of perfect specimens.

As " H. B." ought to know, there is great difference between Anthracomya and Naiadites, there being nothing in common between them in perfect examples. Dr. Hind is therefore misquoted when he is said " to admit a very close resemblance in both crushed and perfect examples." He was speaking of the similarity of crushed forms of a single species, viz., A. lœvis var. Scotica.

It is a mistake of H. B.'s to say the various items of description are not in same order, on the whole, the diagnosis of the species are made with extreme care, and in proper order. Lastly reference must be made to " H. B.'s " previous knowledge of all he has revised.

On page 36 *Pal. Soc: Monog.* vol. xlviii we find statement that he intended himself originally to work up portion of the subject, and had already in M.S. a few pages of his paper. It also seems that he looked over the specimens selected for figuring, the M.S.S, and " proofs," of Dr. Hinds' Monog. Why then does he speak as he does of C. acuta, C. robusta, C. aquilina, &c. ? Why did he reserve his criticism until after publication. Was it chagrin at the successful outcome of another's work ?

Had " H. B." been as critically acute as he fancies himself to be, and as honestly critical as he ought to be, he would have been able to see, instead of " signs of great haste " implying careless work, signs of great painstaking, of persistent and accurate investigation, of unwearied research, and of the consummate skill of the " true " specialist with reference to the study and description of these difficult groups of Pelecypods.

W. F. HOLROYD, F.G.S.

I HAVE been favoured with the sight of the above letters regarding my review of Dr. Hind's Monograph. The review was signed only by initials. From these to jump to conclusions as to the authorship, and to publish, without permission, portions of private correspondence and garbled versions of our conversation, as Dr. Hind has done, is a course of action which must excuse me from entering into any discussion with him. Mr. Holroyd's position in this matter is like his arguments—not very clear; but to you and your readers I have only to say that I am ready to stand by everything written in my review.

HERBERT BOLTON.

The Manchester Museum, Owens College.

NOTICE.

TO CONTRIBUTORS.—*All communications to be addressed to the* EDITOR *of* NATURAL SCIENCE, *at* 22 ST. ANDREW STREET, HOLBORN CIRCUS, LONDON, E.C. *Correspondence and notes intended for any particular month should be sent in not later than the* 10*th of the preceding month.*

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NOTES AND COMMENTS.

"—ologists" *versus* "Collectors."

CONTRIBUTORS to our contemporary, *The Entomologist*, have lately been discussing the somewhat academic question whether one who does "not make his collecting entirely subservient to the elucidating of scientific problems "is or is not" worthy of the name of Entomologist." The answer obviously depends on what one means by "entomologist." If entomology is, as its derivation implies, the "science of insects," then an entomologist must be "a scientific student of insects"; and one who collects insects for the sake of turning an honest penny, or of putting them in picture-frames over the mantel-piece, or of quietly gloating over their beauty as a miser over his gold, or even—still as a miser—of making a larger collection than his neighbours,—such a one may be an excellent father or a model parishioner, but is assuredly no entomologist.

The discussion, however, has not been altogether barren. The fact has been insisted on, just as we have of late been forced to insist on it, that a collector need not necessarily be possessed of great erudition to enable him to do honest and useful work. The field-naturalist and the amateur are in a far better position for doing valuable work than is the average professional scientific man—tied to a museum workroom or a lecturer's table. But, unfortunately, the collector in so many cases does not do good work: he collects without observing; he cares for the name, not for the nature, of his newest find. It is the collector to whom the museum and laboratory naturalists look for valuable and essential information concerning the material with which they have to deal; but when they so often look in vain, can we wonder at an occasional grumble?

We have been accused of late in various quarters, by individuals and by some publications whose opinion we value, of attacking systematists, of attacking field-geologists, of attacking amateurs, of attacking state-paid officials, of attacking collectors, of attacking philosophers, of attacking morphologists. We know of no ground for these accusations. What NATURAL SCIENCE is founded to attack is *bad work*, what it has always laboured to defend and promote is *good work*. The day on which this Review ceases to distribute praise and blame, wrongly perhaps, but without fear or favour, will be the day on which it ceases to exist.

THE SCIENTIFIC ARRANGEMENT OF INSECT COLLECTIONS.

ANOTHER good result of the discussion in *The Entomologist* has been the appearance in the October number of that journal of a paper by Mr. W. Harcourt Bath, entitled "Should the formation and arrangement of a collection of insects be made subservient to the elucidation of scientific problems?" For his answer the author describes the plan of his own collection. The two subjects to which he rigidly confines his study are Distribution and Variation, with special reference to the Rhopalocera, European and Exotic. These subjects can be studied side by side, and the formation and arrangement of the collection made with reference to the two at the same time. At present attention is confined to the two zoological sub-regions in which Europe is included, and these are divided further into provinces and sub-provinces, which again are divided vertically into climatological zones. Mr. Bath's aim is to obtain a series of every species possible from each of these subdivisions, the series to be first typical, and then illustrative of variation and seasonal dimorphism. For the elaborate and ingenious method of arrangement and labelling employed, we must refer our readers to *The Entomologist*; and we need only quote the author's words, "it affords me much pleasure, from an intellectual point of view, which I did not dream of when I contented myself with being a 'mere collector.'"

The general question raised by Mr. Harcourt Bath is of such wide and practical interest that we hope room may be found in our contemporary for further discussion of it. It is clear that specimens of any kind, arranged in some logical order, are more likely to elucidate problems than those arranged on no scientific plan; moreover, the superiority of specimens to elaborate descriptions, even to tabulated statements, is apparent at a glance. Evidence of the value of Mr. Bath's method is afforded by his paper in the November number of the same journal "On the vertical distribution and derivation of the Rhopalocera in the Pyrenees." Of course each individual collector will have his own predilections, problems, and arrangement; the more diverse their points of view, the better. But when the collections of public museums come to be considered, the case is altered. We do not mean that they should have no scientific arrangement; but it seems to us that in each case the arrangement must primarily be systematic. At the British Museum the Lepidoptera are arranged according to a classification based on the latest views as to their genealogy. Here, however, we enter on a wide problem that might well be discussed at this year's meeting of the Museums Association; for it is to be at Oxford, where Professor Poulton and his energetic helpers can give a practical demonstration on the Hope collection.

The Kind of Work we Want.

Considering the strange misconception that our views have met in certain quarters, we almost despair of being able to explain what kind of work it is that we really should like to see. But in this very subject of Entomology, here is one of our best observers of nature, who has come in the nick of time to help us out of our quandary. In the well-filled January number of *Knowledge* is a paper "On describing and drawing insects" by Mr. F. Enock, whose admirable advice we recommend *mutatis mutandis* to others than would-be entomologists. Keep a *daily* journal of observations upon insect life, noting down in black and white every fact connected with the economy and habits of such insects as you are studying, or as pass under your observation. "Owing to the irregular methods which some authors adopt in their descriptions of insects [and other organisms], the task of naming is made more difficult. No two insects are described in the same order. Head, thorax, abdomen, legs and wings, in one; then in the next some other plan, making comparison of parts most confusing." To obviate this, draw up, and place beside you when describing, a definite order of description. "Whenever possible, make out your descriptions from *living* insects, when so much more *purpose* can be seen in the various parts, many of which contract and dry up out of sight in a dead specimen. I often think that much of the dryness or dulness of descriptive entomology is attributable to the fact that it has been made from lifeless creatures . . . The *pose*, too, of the antennæ, head, and whole creature can only be observed in a living insect; the mysterious light in the eyes and the ever-changing prismatic colours on the gauze-like wings, all can be seen, and all appeal to our susceptibilities." Make an *exact* drawing under an inch objective, not a freehand drawing or one in which you can put in any of your own ideas. "All drawings should be carefully inked in and the scale marked under each."

If everyone worked in this intelligent and accurate manner, half our occupation would be gone.

The Earthquake.

Whether we consider its disturbed area or the damage inflicted by it on buildings, the earthquake of December 17 will rank as one of the most important ever recorded in these islands. In Northumberland and Durham, several persons believe that they felt it; but, even if these two counties eventually should have to be excluded, the disturbed area embraces all the rest of England, the Isle of Wight, the Isle of Man, the whole of Wales, and overlaps the eastern counties of Ireland. The exact extent is at present undetermined, but it is clear that in this respect hardly any known British earthquake can surpass it.

The damage to property, though in no place of great consequence, is far from inconsiderable, being spread over rather a wide area. The city of Hereford appears to have suffered most. Several of the pinnacles of St. Martin's Church were broken, aud one of the pinnacles of the Cathedral was displaced. The chimneys of many houses were thrown down, and the walls of others cracked. At Gloucester a few chimneys were overthrown, and reports of similar slight damage come from Worcester, Dursley, Ross, Cinderford, and other places in the surrounding country.

The strongest shock occurred about 5.32 a.m., but it was preceded and followed by several slighter ones, of which unfortunately there are very few observations, owing to the early hours at which they occurred.

Professor Milne states that two of his delicate seismometers in the Isle of Wight show slight but certain traces of the earthquake, the movements continuing for one or two hours. At the Kew Observatory, a faint record is exhibited by the declination curve, and a more distinct one by the horizontal force curve; but the magnetographs at Greenwich and Stonyhurst were not affected. All of these places, however, lie within the disturbed area, and no record has apparently been obtained from any instruments outside it; the nearest one capable of affording any evidence is the horizontal pendulum erected a few years ago at Strassburg.

It seems very probable, as suggested by Dr. C. Davison, in *Nature* (vol. lv., p. 179), that the recent shocks may be connected with an earthquake which occurred about thirty-three years ago, on October 6th, 1863. The latter was carefully studied by Mr. E. J. Lowe, F.R.S., and was but little inferior in intensity and extent of disturbed area to the shock of December 17th. The part of the country where it was most severely felt is bounded by a line passing a short distance from Worcester, Leominster, Brecon, Newport, Monmouth, and Ross. It "was felt throughout Wales and the central counties of England, extending north as far as Doncaster, Huddersfield, and Clitheroe; east to Market Rasen, Peterborough, and Bedford; south to London, Dorchester and Plymouth; and in the west crossing St. George's Channel to Dublin and Wexford:" though it was also perceptible at several isolated places beyond these limits, such as Lancaster, Ulverston, Harrogate, Malton, Scarborough, Bury St. Edmund's, Brighton, and the Isle of Wight.

Dr. Davison is investigating our latest earthquake, and would be glad to have details of any observations sent to him at 373 Gillott Road, Birmingham.

Professor Bonney on Alpine Geology.

The history of geology has been described as a succession of nightmares. Thanks to the extreme difference of the methods employed in the different branches of the science, the experts on one part of the subject, if exclusively specialists, are often quite unable to understand the work of their colleagues in another. The crystallographic formulæ of the petrologist are usually as unintelligible to the palæontologist as a page of Chinese. Hence theories proposed by men of eminence in geology often become articles in the orthodox creed of that science simply by the subscription of men who are quite unable to check the methods or reasoning on which the theories are based. Thirty years ago a plague of theories was fastened on the shoulders of geology by that Old Man of the Sea, Eozoon, owing to the advocacy of biologists who regarded chrysolite as the same thing as chrysotile. Some years later came the great "schist question." According to what was once the orthodox school, schists were rocks produced by the alteration of sedimentary rocks belonging to various ages, Palæozoic, Mesozoic, and even Tertiary. When schists were found sandwiched between fossiliferous rocks they were all regarded as a series of beds of the same age, of which one layer has been altered and the other left. Accordingly the present school of geologists has grown up saddled with a load of theories proposed to explain away the eccentricities of "selective metamorphism." These theories have been especially disastrous to geological work in mountain areas. Instances quoted in support of the fashionable view from Nubian deserts or from South American jungles, or based only on single traverses, of complex mountain groups, carried no weight. The theory rested in the main on the " Mesozoic schists " of the Alps. All the leading Swiss geologists accepted the theory ; they coloured the official maps of the Swiss survey in accordance with it, and taught it in the bulky monographs that accompanied those maps. It thus had an immense influence on the geological thought of the rest of Europe. When sceptics doubted they were referred to the *Beiträge zur Geologischen Karte der Schweiz*, and if they adduced facts inconsistent with the views there promulgated, they were told that it was so much the worse for the facts.

During the last few years, there has come a complete change of view as to the age of the Alpine schists, and a consequent change in the interpretation of the whole geology of the Alps. The geologist to whose teaching this revolution is mainly due is, unquestionably, Professor T. G. Bonney, who on New Year's Day gave the Geologists' Association, London, a summary of the results of his 27 visits to the Alps. This lecture will no doubt shortly be published by the Association, and our object here is to direct attention to what is sure to be a clear and reliable outline of the fundamental facts of Alpine geology. It was not to be expected that the Swiss geologists would immediately admit the overthrow of the theory, which was once almost universally accepted from faith in their work. It was clear from remarks made at the conclusion of Professor Bonney's lecture that some English geologists have not appreciated how utterly the theory of the Mesozoic age of the crystalline schists of the St. Gothard area has been demolished by Professor Bonney's paper of 1890. To that paper no serious reply has ever been attempted. In fact, from the long list of literature on the St. Gothard area, published in the "Livret Guide" prepared by the Swiss geologists for the Geological Congress of 1894, that paper was carefully excluded. The failure of the Swiss geologists to accept the challenge there made to them to substantiate the accuracy of their observations, has relieved geology from an incubus of error, for release from which Professsor Bonney deserves the thanks of all geologists.

Schistosity and Slaty Cleavage.

Mr. G. F. Becker's paper on this intricate subject in a recent number of the *Journal of Geology* (vol. iv., pp. 429-448) is a lucid non-technical presentation of his theory of these rock-structures, as more fully elaborated in an earlier memoir. Of the different kinds of deformation to which rock-masses may be subjected, the author considers specially two distinct types of strain, which he names "pure shear" and "scission," though in general the effects of the two must be associated. In the former the directions of the planes along which flowing movement takes place are continually changing; in the latter one of the two directions of flowing is fixed. The author further considers the effect of viscosity as superposed on that of elastic resistance and as modifying the results. He submits that the resistance of the rock to rupture must necessarily be modified along those planes which have, during the process of deformation, been planes of flow; and he decides on experimental grounds that they will be planes of weakness rather than of increased strength, or, in other words, they will be planes of schistosity or cleavage.

On this hypothesis, pure shear would give rise to structures parallel not to a definite plane, but to planes lying between two limiting planes, making a certain angle with one another which would increase with the amount of shear; and there would be two sets of such planes, equally inclined to the direction of maximum compression. In the other ideal case, that of pure scission, one set of planes would be strictly parallel, while the other set would range through an angle twice as great as in the former case for a like amount of deformation. The author considers, however, that, owing to viscosity, the latter set would usually not become effective. The argument here is not convincing, for if viscous resistance be proportional to the *rate* of shearing (as it is in a homogeneous substance), and if this rate be extremely slow (as is usually presumed), this part of the resistance may be a vanishing quantity at every point of time.

We incline to think that any investigation which treats rock-masses as homogeneous substances must of necessity be unsatisfactory. The commonly accepted theory of the cleavage-structure has for its starting-point the heterogeneous structure of the rock affected. Differing as to postulates, Mr. Becker's theory also differs in its conclusions from the commonly received one. He finds that the structures induced must stand at acute angles to the causative force, instead of being perpendicular to it. This is very difficult to reconcile with the evidence presented by rock-masses in the field. It is also inconsistent with the evidence of the deformed concretions, fossils and the like, in numerous slates, which give us an actual picture of the strain-ellipsoid; in these cases the cleavage plane is always perpendicular to the least axis of the ellipsoid. While the paper is in itself of much interest, it may be regretted that the author has not made a fuller comparison between his theoretical conclusions and the facts of observation.

The Classification of Precambrian Rocks.

There has always been a tendency for men to classify natural facts, not according to knowledge, but according to ignorance, just as we even now continue to define the genera of living beings by the breaks in the chain, or the periods of geology by the gaps in the record. It is as a consequence of this human frailty that the history of geological science shows us a continued attempt to separate certain of the oldest rocks from their successors on the ground of the absence from them of fossils, and to label them Azoic or some equivalent name. As our knowledge grows the limits of this primitive group of rocks become more restricted; so that from having included all strata below the Devonian, it now includes only comparatively few of the ancient crystalline rocks. But the want of logic in this division is shown not only by continual fresh discoveries of fossils, but by the assumption, nowadays universal, that the oldest animals known to us were preceded by a long array of ancestors. Acting, no doubt, on this conviction, at a recent meeting of the Geological Society of Stockholm, Dr. J. J. Sederholm, head of the Geological Survey of Finland, pointed out how difficult it was to draw a true line of demarcation between the strictly preorganic or azoic rocks and those that possibly were once fossiliferous. He would designate the whole basal complex as "Archæan," without reference to contained organisms, separating from the succeeding "Archæozoic" on the ground of its having been affected by certain earth-movements of earlier date than the deposition of the latter rocks. The Archæozoic rocks again, for the Scandinavian region at least, Dr. Sederholm would divide into the older, "Jatulic," which have been involved in certain great mountain-folds, and the younger, "Jotnic," which are later than these folds.

Telegony Again.

Some time ago in our own columns (Natural Science, vol. iii., p. 436), Mr. Frank Finn brought together all recorded cases he could find, of which the most probable explanation appeared to be the occurrence of telegony. The collation was interesting and useful; but the problem was left very much in its previous condition. The majority of breeders are assured that the influence of an earlier mate is frequently seen in the progeny of a mother to a second mate; they take the greatest care to avoid accidents of this kind, believing that a pure-bred dam is spoiled for stock purposes if she has been crossed by a mongrel or hybridised. On the other hand many great breeders, and, perhaps, the majority of scientific naturalists, decline to admit the existence of any evidence for telegony, while some even deny the possibility of its occurrence. It is, of course, impossible to prove a negative, but the scientifically conducted experiments for which all naturalists must wish, have not yet been made on any satisfactory scale. Much is to be hoped from Professor Cossar Ewart's experimental stud. As most of our readers are aware, Professor Ewart succeeded last year in effecting a cross between a zebra stallion and Mulatto, a West Highland pony. The hybrid is strongly marked with stripes, although these differ from the stripes of the sire. Since the experiments are to be continued for some time, definite results may be looked for. The first interesting event that is expected is the birth of another foal from the same dam, the progeny of a horse. If this occur without mishap, telegonic influence of the zebra will be looked for.

In the meantime, Mr. Karl Pearson has presented to the Royal Society an investigation into telegony from another point of view. It occurred to him that if a telegonic influence existed, there might be a closer correlation between the younger children of a family and their father, and a lesser correlation between the younger children and their mother, than existed in the case of the other children. In fact, it might be an accumulating influence on the body of the mother by the father. With this possibility in view, he examined a large series of family statistics, and found that so far as stature was concerned, the result was negative: that there was "no evidence whatever of a steady telegonic influence of the male upon the female among mankind."

A third recent discussion of telegony, is that already reported by us as having occurred at the meeting of the Zoological Society on November 27th, when Mr. Chalmers Mitchell exhibited on behalf of Mr. Oswald H. Latter, a fox-terrier puppy whose sire was a fox-terrier and whose dam was a fox-terrier that had thrown a litter to a dachshund in the preceding season. There was no question that the puppy was a disgrace to well-bred parents, but Sir Everett Millais, who himself has conducted a large series of experiments, was convinced that it was a case of reversion to the beagle strain, which is part of the history of fox-terriers, and refused to see in it any relation to dachshunds. Obviously, the difficulties of getting evidence that shall exclude reversion, are very great in strains so sophisticated as those of modern dogs; but in so doubtful a subject, the investigation of every possible case is advisable, and we are glad to learn that Mr. Latter has set going further experiments on the subject.

The Function of the Intercostal Muscles.

The precise action of the intercostal muscles has been the subject of much dispute amongst physiologists. The simple and diagrammatic arrangement of hinged rods connected by crossed indiarubber bands, which Huxley's "Elementary Physiology" has rendered familiar to us from our youth up, has been recognised as a somewhat incomplete representation of the complex movements of the ribs in respiration. Some writers have declared their belief that the intercostal muscles play no important part in that act, and indeed so recently as 1894 Weidenfeld, as the result of experiments on dogs, maintained that they remained completely passive even in extreme dyspnœa. It is, however, admitted by almost all physiologists that the external intercostals and the intercartilaginous portions of the internal intercostals are inspiratory in function, acting synchronously with the diaphragm, though Masoin and Du Bois-Reymond found that, in the case of the intercartilaginei, this was only the case in forced inspiration. The action of the main mass of the internal intercostals, on the other hand, has been much less certainly recognised; they have been claimed as inspiratory muscles, though the opinion has been gaining ground that they are expiratory in function. Martin and Hartwell have in recent years maintained this, and indeed the fact that they contract alternately with the diaphragm makes it highly probable.

During the past year Messrs. K. Bergendal and P. Bergman published in the *Skandinavischen Archiv für Physiologie* (vol. 7, pp. 178–185) the results of a series of experiments carried on by them in Stockholm, on the action of the intercostals in rabbits, dogs, and cats. These seem conclusively to show that, in these animals at least, respiration can be carried on by the intercostal muscles alone, after the extirpation of all other respiratory muscles except the triangularis sterni, and that while the external intercostals and the intercartilaginous portions of the internal intercostals are inspiratory in their action, the interosseous portions of the latter are no less clearly expiratory. The experiments were carried out on narcotised animals, and tracheotomy was performed as a preliminary in case artificial respiration became needful during the course of the experiment. On exposing the external intercostal muscles and severing them from their lower attachments to the ribs, they were seen to contract visibly during inspiration, and the same was true of the intercartilaginei.

On isolating, after Rosenthal's method, a section of the chest-wall, including the second to the fifth ribs, on one side, so that it was completely separated from the ribs above and below and from the sternum in front, and on dividing all the muscles connecting this section with the vertebral column, so that the only muscles which could act on it were the intercostals, it could be seen that the ribs in the isolated section were strongly raised at each inspiration, the movement ceasing when the intercostal nerves were divided. Further, when the external intercostals were divided in this section, leaving the internal intercostals intact, the ribs were passive in inspiration, but were drawn down in expiration ; this movement ceased when the internal intercostals were also divided. In a still more elaborate series of experiments the authors succeeded, in cats, in extirpating the entire musculature of the chest-wall with the exception of the intercostals and the triangularis sterni ; even the intercartilaginei were severed. The phrenic nerves were divided. Pneumothorax occurred in two of the experiments ; in two it was successfully avoided. In all cases active respiratory movements occurred, brought about solely by the action of the intercostal muscles, and it was clearly manifest that the external intercostals raised the ribs, while expiration was not merely a passive recoil, but was produced by an active contraction of the internal intercostals. In one case the animal breathed quietly and peacefully for ten minutes after section of the phrenic nerves.

These very interesting and complete experiments should set at rest the question of the action of the intercostal muscles during forced inspiration and dyspnœa. It is perhaps not absolutely safe to argue from cats and rabbits to man, but the disposition of the muscles is substantially similar in man, and no good reason can be shown in this case for not so arguing. It may not be quite safe to interpret the phenomena of quiet respiration under normal conditions by the light of the facts observed during the dyspnœa of such severe and abnormal conditions as have been above described. But it is incredible that muscles which have one definite action in dyspnœa should have a different action in calm breathing, and there is at least a strong presumption that their action is similar in character, though less in degree. And so, after years of wandering, we come back to the old familiar hinged rods and rubber bands, glad to find that, after all, they were very near the truth.

A New Giant-Bird.

Some years ago it was announced by Professor Stirling that a number of skeletons of *Diprotodon* and other extinct marsupials had been discovered exposed on the surface of the ground at Lake Callabonna in South Australia, and that the remains of a large extinct struthious bird occurred associated with these mammals. Further details of this remarkable find have been awaited with some im-patience, and it is only within the last few months that there has appeared a paper by Stirling and Zietz (Preliminary notes on *Genyornis Newtoni*: a new genus and species of Fossil Struthious Bird found at Lake Callabonna, South Australia: *Trans. Roy. Soc., South Australia*, vol. xx.), which gives a brief account of this interesting bird. It is extremely unfortunate that no figures are given, since in the case of bird bones mere description and measurements convey little idea of the structure of the specimens.

It appears, however, that the femur and tibia of this bird are very similar in size and proportions to those of the Elephant-footed Moa (*Pachyornis*), but that the metatarsus is longer and more slender than in that bird. The inner toe was slender, while the middle and outer ones were short and flattened, their ungual phalanges being particularly small, short, and flat; one very peculiar character is the presence of only four instead of the usual five, phalanges in the outer toe. The sternum is said to resemble that of the emu, and the wing, though greatly reduced, was complete. The skull is nearly a foot long, and the mandible remarkable for the depth of its rami and the wide angle they include between them.

The authors, in conclusion, state that *Genyornis* probably lived in marshy places, and fed on herbage like the emu, which in many respects it resembled. It is to be hoped that further description and figures of this interesting new form will be forthcoming before long.

Monkeys in Madagascar.

One of the most important results of Dr. Forsyth Major's travels in Madagascar has been the discovery of the remains of an extinct monkey-like animal in the superficial deposits of that island. Dr. Major has briefly described and figured his find in the *Geological Magazine* for October, the relics consisting of portions of a skull and lower jaw, and has called the animal *Nesopithecus*, believing it to be a true monkey, indicating a new family of the Anthropoidea, "intermediate in some respects between the South American *Cebidæ* and the Old World *Cercopithecidæ*, besides presenting characters of its own." Mr. Lydekker, writing in *Nature*, November 26th, however, rather doubts its being an anthropoid, and suggests it may be "an offshoot from the original stock which connected the Monkeys with the Lemurs."

The discovery obviously is one of the highest importance, and we shall be interested to read Dr. Major's full description and examine his figures and conclusions when they appear. Till then we offer no opinion.

The Range of the Nummulite.

The already wide range of the genus *Nummulites* has been still further extended. In a short note to the *Geological Magazine*

(November) Mr. R. B. Newton has recorded the occurrence of this genus in rocks from Gazaland, South East Africa. The specimens come "from a large development of flesh-coloured limestone," but further specimens and evidence are desirable. We have now, therefore, suggestions of a much more extended range of the Nummulitic limestones of Eocene age, viz.:—England to China, England to South East Africa, China to South Australia, in the Old World; the West Indies and United States in the New World. In vertical range the genus is first found in Carboniferous rocks; there is a record from Cretaceous, but the main development of the genus was in the Eocene period, after which it seems to have died out.

Rank and Quality, and the Royal Society in 1778.

In the correspondence of William Legge, second Earl of Dartmouth (*Hist. MSS. Comm. 15th Report*, App., Part 1.) appear two letters touching the Presidency of the Royal Society, which contain matter of sufficient interest for quotation here. The first was written by Dr. Charles Morton and dated "British Museum, 25 Sept. 1772" and is quoted as follows:—"By direction of Mr. Barrow, President of the Royal Society, and many of the members, he writes to ask Lord Dartmouth to allow himself to be Proposed as President. The office includes a Commissionership of the Longitude, a Trusteeship of the British Museum, and the first Visitorship of the Royal Observatory."

The second is from Nevil Maskelyne and is dated "Royal Observatory at Greenwich, 22 Sept. 1778." It reads:—Your lordship is doubtless apprised, were it only by the public prints, that Sir John Pringle means to resign the office of president of the Royal Society on the Anniversary election at St. Andrew's day next. I can add, that, about a fortnight ago, he declared that resolution at a council which he had summoned on purpose, without any person being then mentioned either by himself or any other member of the council, as eligible to succeed him. It was, doubtless, his intention not merely to pay a compliment to the council, but rather to give the members an opportunity of enquiring after a proper person to fill the President's chair, and to make an opening for some person of distinguished rank and character, like your Lordship, to offer their service to the Royal Society on the day of their Anniversary election. There is no person, who publicly declares himself as aspiring to the Chair; so that it is entirely open; and from conversations which I have had with several fellows of the Society on the propriety, and even necessity, of prevailing on some nobleman to honour us by accepting the office of President, it appears to me that they are waiting for such an event, which would at the same time do most honour to the Society, and be most advantageous to science; for, without rank in the chair, the Society itself will not be so much respected and obtain that *éclat*, either at home or abroad which so useful an institution deserves; nor will the great end of the Society, the promotion of science, be carried on with so much vigour, since it is well known, that without the protection of the great, the arts and sciences will always fade and languish. Your Lordship, I flatter myself, will not be surprised, after what I have advanced, not without good grounds, of the sentiments of a great part of the Royal Society, at my declaring myself one of many who earnestly wish your Lordship would do us the honour to give us leave to mention your name to the fellows of the Society at large, and canvass them, in order to your nomination to be elected President of the Royal Society on November 30th next. At present (although I have mentioned your Lordship's name at a distance, to many fellows of the Society, and found it always received with the utmost respect and attention), yet, out of regard to your Lordship's delicacy, I have only apprised a very few gentlemen of this address to your Lordship, viz., Mr. Daines Barrington, Dr. Shepherd, Mr. Walsh, Dr. Warren, and Mr. Wegg, Vice-President and Treasurer of the Society. We are fully satisfied, that if your Lordship would but permit us to name you to our Brethren of the Society, even without your Lordship appearing at all in it yourself, if you did not choose it, that there would not be the least difficulty or obstacle to the gratification of our wishes, and the fixing your Lordship in the President's chair on the day of the election. We are the more induced to indulge our hopes that your Lordship will honour us by acceding to our wishes, from the consideration that your Lordship once actually accepted the office of President before Sir John Pringle was nominated, and only declined afterwards on account of the engagements of a busy office which your Lordship then held—that of Secretary of State for the American Department, which however having long since resigned, your Lordship is now at leisure to accept an honourable office with some share of business. We expect, in about a twelvemonth, to take possession of the new and magnificent apartments which his Majesty has been pleased to give us in the new erection on the old site of Somerset House, which will be more agreeable to your Lordship, as well as to the Society, than our present incommodious crowded house; and where we may expect to have our meetings attended by more persons of rank and quality than heretofore. If the usual hours of the meetings of the Society should be less convenient to your Lordship, they might be altered either to a later hour, or to one before dinner, and where it might not be convenient to your Lordship to attend at all, the chair might be taken for the time by a Vice-President, who is always of the President's nomination."

It is interesting to note that the chief bait held out to the proposed President is the "rank and quality" at the meetings; and the declaration of Maskelyne, one of the foremost men of his time, that science then depended, as art always does, on the patronage of the great.

Some American Botany.

The seventh annual report of the Missouri Botanical Garden which has just reached us contains, besides information about the gardens, several scientific papers. In fact, the garden-report ends at page 24; the rest of the book (which contains 209 pages) being occupied with papers dealing with the botany of the United States, a short address on the value of a study of botany, and a catalogue of a collection of books on pre-Linnean botany, the generous gift of Dr. Lewis Sturtevant. The plates are, as usual in these reports, excellent. There are no less than sixty-six to illustrate the three scientific papers, and also half-a-dozen depicting various plants of interest which have flowered during the year. Among these we note the *Victoria regia*. As the flowers of this great water-lily expand in the twilight the garden was on several occasions kept open in the evening to enable those who wished, to witness this interesting function. Professor Trelease, the director, contributes an account of the *Juglandaceæ* of the United States, in which special attention is drawn to such characters of the fruit, twig, bark, and bud as will be helpful in field studies. So well-marked are some of these characters that in the case of the hickory "most of the species are more readily known in their winter condition than during the period of flowering." The same is true of the walnuts. Some excellent photographs of portions of the trunks of different species give a good idea of the appearance of the bark, which shows marked differences according to the species. Miss Mulford contributes a descriptive revision of the agaves of the United States. Most of the agaves have their home in arid deserts of Mexico, Central America, and the south-western portions of the States. Owing to the rarity with which they bloom under cultivation they have earned the popular name of "Century plant." In their own country they have been used by man from time immemorial. The Aztecs showed their appreciation by reverencing the plant as a god. Saddle-cloths, sacks, and ropes have been made from the tough fibres, and Miss Mulford quotes, on Humboldt's authority, "a bridge at Quito, having a span of 130 feet, made of ropes of agave fibre four inches in diameter." The softer parts supply food and drink. Pulqué, a universal drink in Mexico, is made by allowing the sweet sap of the best-known species, *Agave americana*, to ferment in vats of raw hide. This species blooms at ten years of age. When it is about to send up its large flowering-stalk an enormous flow of sap takes place towards the central bud to meet the necessary expenditure. The Mexicans and Indians cut out the bud and surrounding leaves and collect the liquor, which flows freely from the wound, in a long cylindrical gourd. "Some plants produce an average of two gallons a day, and will keep up the supply for months." The leaf-spine with attached fibre serves as a ready-threaded needle; the strong flowering-stalks make handles for lances, poles for fishing, walls for houses; and it is said that the juice mixed with wall-plaster is used as an insecticide to keep away the destructive white ant. Its spiny leaves render it a very effective hedge-plant, and it has been extensively introduced for this purpose in the Mediterranean region.

A third paper is a short account of native ligulate Wolffias, small fresh-water plants closely allied to our duck-weed, by C. H. Thompson.

The Occurrence of Marine Organisms.

Many of the organisms commonly found in the plankton of the sea around the British Coast exhibit remarkable variations in their relative abundance at particular localities from year to year, but little is known as to the extent and causes of such variations. As a number of naturalists make use of the tow-net at many places round the coast, especially during the summer, much valuable information would be obtained, if in all cases records were kept of the presence or absence of a limited number of the commoner species, and these records subsequently brought together.

The Director of the Marine Biological Association of the United Kingdom has taken this subject in hand, and desires to tabulate the results before January 31st, 1898. The following is a list of the forms upon which information is desired, and it is followed by the nature of the information asked for:—*Halosphaera viridis*; *Noctiluca miliaris*; *Aurelia aurita* (including *Ephyrae*); *Agalmopsis*; *Muggiæa atlantica*; *Hormiphora plumosa*; *Beroe*; *Tomopteris*; *Anomalocera patersoni*; *Doliolum*; *Salpa*. Where the generic name only is given in this list, the specific name of the specimens taken should be added. Should any doubt exist, preserved specimens should be kept. In making a record the following should be stated:—Date; Hour; Locality, (with as much accuracy as possible); Depth, (depth of water, and maximum depth at which net has been worked); Quantity, (0, absent; 1, few only; 2, moderately plentiful; 3, exceptionally abundant). Observations on the temperature of the sea, and notes on wind, tide, &c., will also be of value. Records should be sent in before January 31st, 1898, or forwarded from time to time to the Director, Marine Biological Association, Plymouth.

The Problem of a Parasitic Copepod.

Let the impatient reader purchase 500 specimens of the Norway Lobster. Mr. J. Stewart Thompson (*Proc. Phys. Soc. Edinburgh*, vol. xiii) offers him the hope of discovering upon these as many as four specimens of a curious parasite. In the inevitable 'preliminary notice' he introduces us to an unnamed, unfigured, dimorphic (but in the female sex almost shapeless) animal, which he has found parasitic on the vas deferens of *Nephrops norwegicus*. He believed it to be a Copepod of the family Lernæopodidæ. In the members of that group no peculiarity of situation, no oddity of form, can any longer excite surprise. What really surprises is that the Demonstrator of Zoology in the New School of Medicine at Edinburgh should have been at the pains to prepare figures of his new species and yet publish his paper without them, that he should have evidence of its affinities which he is confessedly holding back, and that he should spoil the interest of his completed story by first forcing it upon the student in a rough unpolished sketch. It is satisfactory to learn that in both sexes of the new crustacean there is a 'niching on the dorsal surface,' though there is no such word as 'niching' in Webster's Dictionary. To explain the position of the parasite 'on the walls of the vas deferens' Mr. Thompson suggests the possibility 'that it entered the male reproductive duct as a larval form.' Whether the adults were found outside the duct or within it is nowhere explicitly stated.

The Hinge in Bivalve Molluscs.

We last referred to Dr. F. Bernard's notes on the development and morphology of the hinges in bivalve Mollusca in our December number (vol. ix., p. 358); the third instalment has now appeared (*Bull. Soc. Géol. France*, Sér. iii., tom. xxiv., pp. 412–49: figs.). In it the Anisomyaria, or such shells as the mussel, scallop and oyster, and their allies, are dealt with. In all these the prodissoconch begins with a crenulated margin, as in *Nucula* and *Arca*, the true teeth forming later. Only in the Spondylidæ, however, do these attain any conspicuous development; whilst in *Ostrea* they never develop at all, the denticles sometimes visible in its adult shell corresponding to the crenulations in the embryo. In the Mytilidæ the secondary development of the ligament extends backward, and swamps all trace of posterior teeth, whilst the anterior ones are represented merely by irregular thickenings, which are scarcely, if at all, functional. *Dreissensia*, which has been placed now with the Mytilidæ, and now relegated elsewhere, shows no affinity with that group so far as the morphology of its hinge is concerned; but of this genus we are to hear more anon.

Species Manufacture.

We are extremely glad to see that G. Dollfus, following in the wake of G. Coutagne, raises his voice (*Feuille Jeunes Naturalistes*, Nov., 1896), in protest against the wanton and pernicious multiplication of so-called species indulged in by certain of his malacological brethren, especially in the neighbourhood of Lyons.

The needed reform could only come from within, and we heartily welcome these signs of its advent. We gather further from the article that Mr. Coutagne proposes to bring out a genuine catalogue of the non-marine Mollusca of France, but we more than echo Mr. Dollfus' mild protest against Mr. Coutagne's apparent intention to disregard the rules of priority and to coin new names for his species. It is just this assertion of the "almighty ego" that has been at the bottom of all the mischief in nomenclature which it is his object in part to undo, and that, if adhered to, will deprive his catalogue of almost all its value.

I.

Worked Flints from the Cromer Forest Bed.

FOR the benefit of those not familiar with the geology of the Cromer district, I may, perhaps, be allowed to give the following particulars:—The valley to the west of Cromer, towards Sherringham, is still cloaked with a palæolithic gravel, which sometimes attains a thickness of eight or ten feet, and forms the summit of the cliffs. It is of light-brown colour generally, and implements of well-known palæolithic forms have been found in it for the last twenty years. They are well made, well finished, highly patinated, and only very lightly iron-stained, of an even uniform light-brown colour; calling strongly to mind the general facies of the Stanton Downham palæoliths. Flint flakes are also found in the gravel; and occasionally an implement gets washed out and falls upon the beach, where naturally it gets knocked about. But let the abrasive and destructive beach-action be what it may, the implements still retain their type and colour, and worked flints from this bed can readily be distinguished from those derived from the underlying deposits.

Below this gravel come the fantastically twisted and folded Glacial beds, appropriately termed the contorted drift, now sweeping in a low, long graceful wave, such as one would see in mid-ocean on the surface of the Atlantic, and now, amidst countless undulations of indescribable paths, trending almost vertically for from thirty to forty feet; then perhaps descending just as quickly, embracing a column of white or reddish brown material, which a lens reveals to be the old Crag or Cromer Forest Bed torn up in this fell swoop, push, or slide, and crushed to powder. In other places the contained fossils are less crushed, and frequently fragments are left of sufficient size for specific determination, especially when the torn-up mass is large, as some of them are; indeed, in the case of the great chalk islands, some of them are from two to three hundred yards long, and are now quarried for lime burning. But size and weight appear to have been nothing to the forces which operated during the formation of these remarkable deposits, as these huge masses of chalk were picked up, rolled over, and stuck on end, apparently as easily as the fragments of Forest Bed were carried up to the top of the cliff. Some amount of care is therefore necessary in reading the beds exposed in these cliffs. But all the abnormally violent action had ceased before the incoming of the comparatively peaceful conditions, under which the palæolithic gravels were deposited, since their deposition has never disturbed the underlying beds. The glacial deposits, on the other hand, may contain anything older than themselves, down to and including the Chalk.

Below the Glacial beds is the Forest Bed series. Into a full description of this we need scarcely enter here, suffice to observe that every one now knows this is not a Forest Bed at all, but was deposited in the estuary of a river, which during its history—probably like many others—fluctuated in salinity. When we first meet it, it is fresh, then a N.E. subsidence asserts itself and estuarine conditions obtain, but only for a while, and the last we see of this interesting link with the Pliocene period is that its waters are running fresh. During many parts of its life, at least, it was a powerful stream, sometimes cutting through the underlying Weybourn Crag. The latter, when present, rests upon the Chalk. Both in the Weybourn Crag and the Forest Bed, when they rest upon an impermeable sub-stratum, there is often developed a very heavy iron pan. One of these occurs in the Forest Bed, and is distinguished from the rest by the number of large mammalian bones it has preserved, from which fact it has long been known as the Elephant Bed. There is another important feature about this bed which perhaps has not been publicly noticed, or at any rate I do not know that any one has drawn attention to the fact, although many have noticed it, and that is, its property of imparting to the contained fossils an amethystine tinge. This colour in silica is attributed to manganese, but as there is so much of this metal in most gravels without this coloration, it may be supposed that in the form of manganese dioxide, in which it occurs in ordinary river deposits, it does not so colour ; but that as another oxide it may ; and possibly the original dioxide of this pan may have been so altered, and thus colours not only the bones, but the contained flints. When the iron is the more active, we get the flints of a brown colour, with various amethystine tinges; when the reverse is the case, we get the flint assuming a beautiful black, with the same amethystine hue pervading. This staining process is often very deceptive; at first sight, a flint appears quite fresh, but the moment the edges are abraded the fact is revealed that the flint is very much altered. As might be expected, there is every intervening stage of alteration present, but all are characterised by this amethystine tinge. This peculiarity may at first appear unimportant, but when we remember that it is characteristic of the bed under consideration, it becomes of very great importance, as when present it enables one to fix the horizon of a flint or bone after it has been washed out of its original matrix.

To come at last to the main subject of this paper. Recently in working this "Elephant Bed" for bones, I was delighted, after breaking away some eighteen inches of the iron pan, to see sticking out what appeared to be an unmistakable worked flint. I called my wife to see it *in situ*, before it was removed, since I fully realised the opposition that would be raised by a certain section of anthropologists to a Forest Bed implement. However, I left sufficient "pan" upon it to prove its original position.

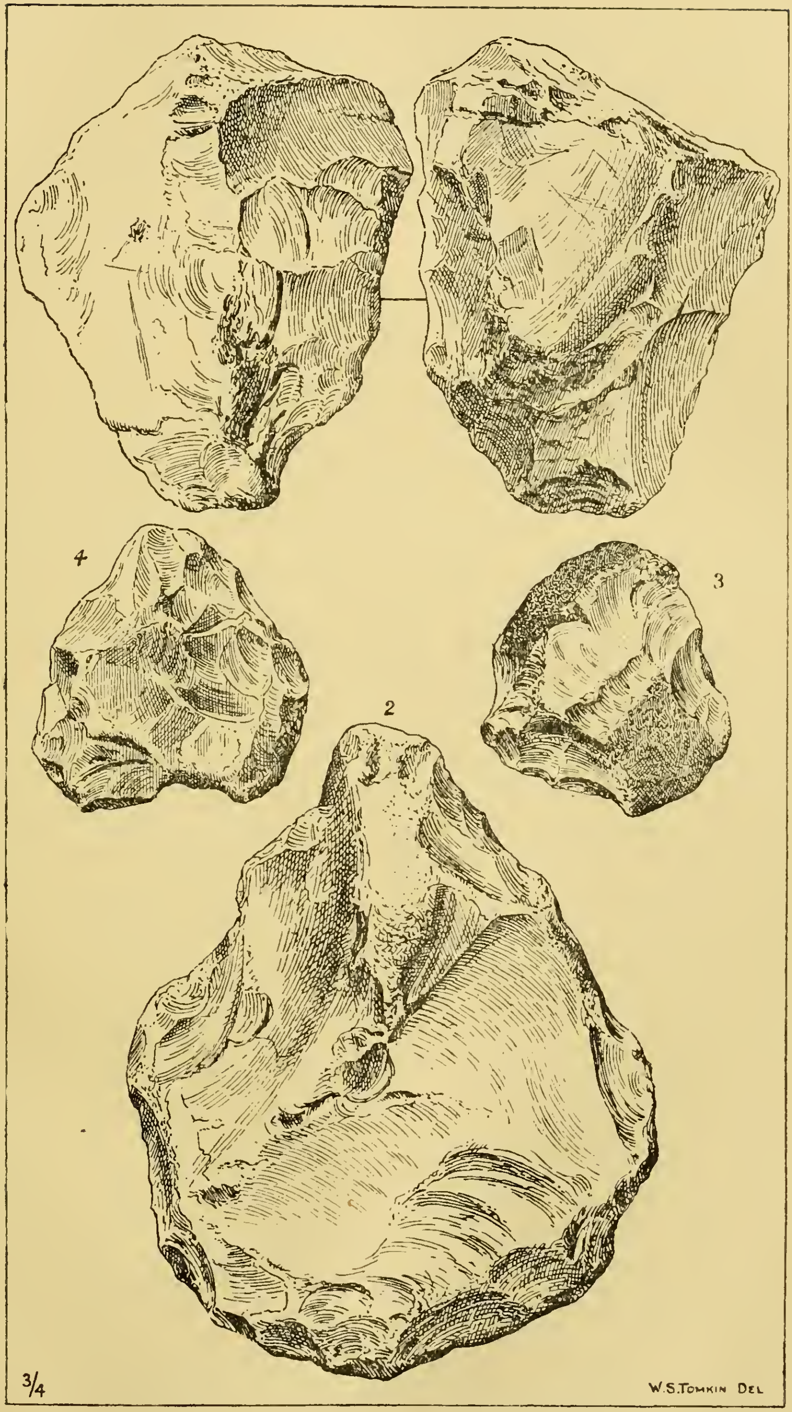
By reference to Fig. 1, it will be seen that this flaked flint presents the outline of the hand-pick type of implement, a type that has come down to palæolithic times; many collectors, like myself, will be able to pair it from the Thames and other valleys. It is generally supposed that these were held in the hand, before the process of hafting had been discovered. There is a broad flaked face opposite the point, and the shape fits the hand in a most suggestive manner. It will be seen that whatever part "starchy fissure" or natural forces might have played in operating on the original flint nodule after it left the Chalk, there are only very few facets that do not show the well marked conchoidal ripplings, so characteristic of man's work; and practically whenever possible there are the characteristic pits of concussion, from which the bulbed flakes were removed when the tool was brought into its present outline; and in the last side operated upon these are quite parallel for more than three-fourths of its length, and even turn the corner in the same zone.

These are points of inestimable importance: often a flint has been made to simulate an implement in outline through the removal of pieces or flakes by natural agencies—colloid fissure, prismatic fissure, frost-fracture, vicissitudes of gravel-making, etc.—associated with other facets which, from their conchoidal fracture, we might attribute to man, and thus hastily give him credit for an implement he never made. In such specimens, the naturally removed flakes clearly post-date those supposed to have been removed by man, truncating or transversely bisecting the original flake-facet, removing all traces of the pit of concussion, which must have originally existed, perhaps several inches beyond the present periphery. It is certain that whatever may be the outline presented by such a flint, it manifests no marks of design, and cannot be regarded as the work of man. In an artificially-worked flint, the last flakes taken off which bring the implement into the desired shape should truncate or remove the pits of concussion from the earlier made facets, and carry with them their own pits as evidence of the part they played. I am fully aware that this is a most exacting test, and that by its application many a specimen, at present cherished, would be rejected; but it errs on the side of preventing us accepting anything as human, which may be the result of combined natural forces. Tested by these uncompromising rules, the specimens here described, from the Forest Bed, come out unscathed.

The point of the implement shown in Fig. 1, and the edges that would be exposed if used in the manner suggested, show signs of abrasion, such as would be produced in active service; and when we see that these edges are obtuse, and that of all the others—some of which are quite acute—none show any such marks, may we not fairly consider this the rational explanation of the facts presented by this interesting specimen? I am fully aware of the fact that implements ought to be seen before one passes an opinion upon them; and I am equally conscious that before one can determine the possibilities of natural forces, and distinguish them from man's work, one should have years of actual and practical experience—not so much in rummaging amongst second-hand collections, nor even in visiting pits and buying implements from workmen, but in making them oneself. When one understands so much of the working of flint as to be able to set out and make an implement of any pattern and style of work desired, and also is able to tell a modern forgery at sight, then one is qualified to say what man or nature can or cannot do. In the eye of such a one as this, I am certain that the present specimen would improve upon acquaintance.

Fig. 3 shows a worked bulbed flake from the same bed as no. 1. It is one of the purple-black specimens. Only a very small portion of the original surface remains. The removed flakes in all cases were detached by sharp well-directed blows administered from the bulb-face, and frequently took a clean sharp direction quite across the flint, and when physically possible always show conchoidal ripplings and pits of concussion. It is this secondary flaking which gives rise to the present outline: only a very small portion of the original unworked surface of the flint remains. This specimen shows a well-marked *éraillure*, the meaning and importance of which must be explained at greater length.

I have tried some thousands of experiments of fixing flints and pitching round pebbles at them, and thus removing flakes, also by special suspension arrangements I am able to administer any number of blows at any particular spot with various degrees of force; but I have never yet been able to produce this scar in any way in which it may be conceived nature worked. In another set of experiments I have placed the flints, fixed and otherwise, at the bottom of a long inclined trough, and have let the stones slide down and strike the flint, or have put heavy weights behind them in a screw-jack action, both directly upon the flint and with the interposition of a loose pebble, but all with the same negative result. On the other hand, when I have tried to make a clean chop off a mineral scores of times I have been annoyed by a characteristic kick, giving rise to the *éraillure*. I explain it to myself in this manner:—In an ordinary blow one just brings the hammer upon the object, and is regardless of the rebound, which generally initiates the return motion, and thus is unrecognised; but when one wants to hit in a certain place, in a definite direction, there is an unconscious concentration of all muscular power to make the blow fall at that particular spot, and even keep the hammer there, and this voluntary muscular opposition offered to the uprise of the striker forces it back, occasioning a secondary but light blow. This can also be well seen and heard when one attempts to drive a nail in an awkward place by a series of slow



WORKED FLINTS FROM THE CROMER FOREST BED.

Three-fourths natural size.

deliberate blows, each of which will be followed by a second involuntary tap. It is this tap which removes the small flake from the bulb of percussion, and produces the well-known *érailluve.* This, therefore, is characteristic of an intentionally directed blow. Upon submitting specimen no. 3 to Mr. J. Allen Brown, F.G.S., he at once noticed this inestimable hallmark. All experiments thus point to the *érailluve* as being altogether more important than a mere bulb-of-percussion, and, so far as we are aware, may be taken as a proof of man's work, as it can easily be seen among flakes intentionally removed from a block, but so far as is known under no other circumstances.

Fig. 4 shows another example of the purple-black variety; but instead of being a chipped flake, it is worked all over, no portion of the original flint remaining unworked. The similarity to no. 3 in the flint, in the characteristic amethystine hue and in the work, is evident at a glance, and becomes more certain upon examination. The specimen was not obtained embedded in the pan, but at its outcrop on the foreshore below high-water mark, near the pan which yielded nos. 1 and 3, *Elephas meridionalis*, etc. No one who has seen the two and passed an opinion doubts for a moment that they both came from the same bed.

Fig. 2 depicts a very interesting implement. Like many of the very early plateau specimens, and the palæoliths, it is a naturally split flint and worked from one side only; the flaking is generally of a very bold and decisive character, although the edges show lighter but equally firm work, unaccompanied by contusion, thereby differing from the results of gravel-making, or beach-action, which invariably show the effect of undirected battering action. This implement was also out of the matrix when found, but its staining is so characteristic as to leave no doubt in my mind that it came out of the bed with which it was found associated.

I also discovered other specimens, some of which would doubtless strike some authorities as even more unmistakably human than those here figured, while the collection I was able to make of the stained and weathered flints from this bed would enable one to feel little doubt in deciding whether or no nos. 4 and 2 came from the Forest Bed. Although I have here referred only to the pan, I may say that I obtained a number of small bulbed and bulbed-and-pitted flakes from another part of the bed, and I am certain that patient and diligent research in these beds would be rewarded with specimens that would satisfy the greatest sceptic procurable. The above were all found to the west of East Runton. Mr. Clement Reid, F.G.S., writes me:—"I have always considered that if implements were found in the Forest Bed, it would be at Runton, although up to the present I have been unable to find any."

In conclusion, I may say that I have submitted these worked flints to a number of the first experts of the day, who have accorded them unqualified acceptance as being man's work. Sir John

Evans, F.R.S., who, as everybody knows, exercises with regard to such novelties a scientific caution that some might call extreme, and who has sometimes called himself the St. Thomas of anthropologists, writes:—"No. 4 may or may not be artificial, and the same may be said of no. 3, with even more probability of its being made by man."

Bearing past history in mind, and the reception which has been accorded to these specimens, the unquestionable evidences they present of being artificially worked, the unmistakable positions from which they were obtained, and the conditions under which some of them were found in the matrix, are we not justified in admitting the existence of man in Britain in the Forest Bed period?

W. J. Lewis Abbott.

Sevenoaks, Kent.

II.

Are the Arthropoda a Natural Group?

IT was Von Siebold who, in 1845, formed the Insecta of Linnæus, or the Condylopoda of Latreille, into a primary division of the Animal Kingdom, which he called Arthropoda. For thirty years this was considered a natural group with close relationship between the four classes which composed it, and in 1869 Fritz Müller said that there could scarcely be a doubt that all these classes were branches of a common stem. The pedigree was traced as follows. The primitive thysanuriform insect and the primitive myriopod were supposed to have had a common hexapod terrestrial ancestor which was, probably, a descendant of an aquatic zoëa-form crustacean. The Crustacea and the Arachnida were traced to a primitive phyllopod, which appears to have descended from an unsegmented nauplius.

After the publication of Professor Moseley's papers on *Peripatus capensis*, the Malacopoda (De Blainville), or the Onychophora (Grube) were taken out of the Vermes and admitted, by most zoologists, as a fifth class of the Arthropoda. Professor Huxley, in his paper on the Classification of Animals (1876), says that *Peripatus* is a low and primitive arthropod, and thus affords evidence of the highest significance as to the relations of the Annelida with the Arthropoda. But if *Peripatus* is a primitive arthropod, it can only be a primitive tracheate arthropod and not a primitive branchiate arthropod. To find the common stock of *Peripatus* and the Crustacea, we must go back to the earliest forms of worms; and the characters which were formerly supposed to shew a close relationship between the Crustacea and the Insecta—including hollow-jointed limbs, exuviation of the skin, and the structure of the compound eyes—cannot be due to inheritance, but must have been evolved independently by what is called convergence of characters.

If this is correct, the Arthropoda must either be divided into two primary divisions of the Animal Kingdom, each equal to the Mollusca; or they must be united with the worms in Macleay's primary division of Annulosa; for I suppose it will be allowed that, in a natural classification, a group should include the common ancestors of each member. If, however, the older conception of the origin of insects be correct, then *Peripatus* cannot belong to the Arthropoda—for certainly it is not a retrograde myriopod—but must be looked upon as a polychæte worm specially modified for a terrestrial existence.

I offer no opinion myself on the subject, but should like to know how others view the situation.

Christchurch, N.Z. F. W. Hutton.

No zoologist can be astonished that the genealogical trees of the animal kingdom, which have sprung up so plentifully during the last few decades, should require serious pruning. I take it therefore that Mr. Hutton's object is to ascertain how those who have made a special study of Arthropoda propose to arrange their genealogies at the present moment. Experience shows that the less one knows about a group, the easier is it to dash off its family tree, while, conversely, the more one knows, the more reluctant are the branches to group themselves in any satisfactory manner. Thus the genealogy of the Arthropoda will not be finally established until each of its component groups has received far more attention than has yet been devoted to it—that is, with this special object in view.

In giving my own suggested ancestry of the Arthropoda, I should state at the outset, that it is based upon special studies of only two of the groups concerned, the Crustacea and the Arachnida. The detailed arguments on which the following summaries of my conclusions are based, will be found in my "Comparative Morphology of the Galeodidæ" (*Trans. Linn. Soc., London*, 1896.) and in the other papers there cited, and further, in a summary now in the press for the March number of *Science Progress*.

(1). There is no difference of opinion in referring all the Arthropoda back to Chaetopod Annelids, but inasmuch as there is no reason to believe that the Arthropoda are the only derivatives from Chaetopoda, I should not count these latter also as Arthropoda. They are only the soil out of which the Arthropoda developed along a definite line, viz: the specialisation and structural modification of the parapodia for the purposes of prehension and locomotion.

(2). The lines along which these specialisations and modifications of the parapodia have developed for these purposes are, so far as my researches go, not the same in any two groups of Arthropoda, hence each group has to be deduced separately. The Arthropoda thus cannot be arranged in any single genealogical tree.

(3). The Crustacea. Regarding *Apus* and the Trilobita as the earliest and most annelidan Crustacea, we find that their peculiar specialisation was primarily the bending round of the prostomium ventrally to form a great labrum under which a certain number of parapodia could function as jaws within and around the mouth-aperture, one or more of the anterior pairs being left free to function as sensory organs. The parapodia posterior to the mouth-organs seem ventrally to have repeated the chewing functions of the jaws, while dorsally they developed into complicated swimming plates, retaining the original parapodial gill. The development of walking legs was secondary as is shown clearly by the Trilobita, which retained the *Apus*-like swimming plates under the pygidium. The Crustacea are the only well established group which became arthropods while retaining an aquatic life.

(4). The Arachnida. The common ancestor of the arachnids was a chaetopod in which the two first pairs of parapodia were thrown forward, two above and two below the mouth, the first pair as seizing limbs, the second pair primarily as sensory, and only secondarily as seizing organs; the prostomium and labium were squeezed between these to form a beak for sucking the blood of victims crushed out by the above-mentioned limbs. This liquid food-supply, pumped in with force by the œsophagus, as can be shown in detail, modified the whole of the rest of the organisation. The locomotion necessary for catching living prey was highly specialised, four pairs of parapodia behind the two grouped round the mouth developing into long legs, while the posterior region became a great distensible sac with its limbs aborted.

(5). The Insecta. The primitive insect I believe to have been worm-like, with a first pair of parapodia developed into antennæ, and the three following pairs forced forward as jaws to work in an anterior mouth-aperture; the following pairs were originally but little developed for locomotion, being used rather for clinging to the leaves and stalks on which the insect fed. The herbivorous caterpillar represents, I believe, a true stage, and probably one of the earliest stages, in the development of the Insecta from their chaetopod ancestors.

(6). The Myriopoda. These have two distinct types of mouth-organs; if these two different modifications of parapodia cannot be deduced the one from the other, then I should be compelled to regard the Chilopoda and Diplopoda as two distinct derivations from Chaetopoda. There would surely be nothing astonishing in the fact that two (or, including *Peripatus*, three) different descendants of the Chaetopoda, on taking to a land life, retained the whole series of parapodia, *i.e.*, behind the mouth-organs, for locomotion.

(7). *Peripatus*. I regard this as a feebly developed arthropodan derivative of the Chaetopoda, an arthropod in making, and rightly classed with the rest though in no true sense an ancestral form of any of the foregoing.

(8). The Pycnogonida. Whether these are to be deduced from any of the above or are again distinct specialisations, I feel unable even to guess; they remain the most enigmatical of the Arthropoda.

Two further remarks on these brief notes on the phylogeny of the Arthropoda :—

(9). I have endeavoured elsewhere to show in detail how the distribution not only of glands but also of the tracheæ of the different arthropods may be fully and simply explained by deducing them all (? the coxal glands) from the bristle-sacs of the original chaetopodan parapodia.

(10). All these new forms of life arising out of that highly plastic form, the Chaetopod Annelid, were, I believe, due not to the selection of small chance congenital variations but to definite responses to the environment. In each case, the adoption of a new method of feeding gave rise, slowly but surely, to a new organisation.[1] All subsequent resemblances in the descendants of these ancestral forms are purely convergent.

HENRY M. BERNARD.

Babington Road, Streatham.

IT seems to me that the striking common structural characters of the classes of animals generally comprised in the Arthropoda oblige us to retain that phylum, at least until phylogenetic speculations regarding its dual origin stand on more certain ground. A primary division of the arthropods into branchiate and tracheate forms seems inadmissible, when we can trace the relationship between the water-breathing king-crab and the mites or harvestmen breathing by means of air-tubes. The morphological and embryological evidence, at present available, appears to point to many-limbed animals with a number of similar segments as the primitive arthropods—the ancestors of crustaceans, arachnids, diplopods, chilopods, and insects. It is likely enough that the stock which gave rise to the three last-named classes had adopted terrestrial life at a very early period. But this stock must have been of a more decidedly arthropodous character than our living *Peripatus*, which represents an offshoot at a much lower point in the evolution of the phylum. The superficial resemblance of *Peripatus* to a myriopod cannot blind us, for instance, to the fact that its antennæ are pre-oral, while those of insects and chilopods are primitively post-oral. And Mr. Sedgwick has lately stated his opinion that it stands about half-way between arthropods and annelids. If we recognise its excessively primitive nature, it may be most convenient to retain it in the arthropod phylum, remembering that its tracheæ—a necessary adaptation to a terrestrial life—do not require us to regard it as approximate to the immediate ancestors of insects and centipedes.

Dublin. GEO. H. CARPENTER.

May I point out that, first and foremost I am as little inclined to agree with the view put forward by F. Müller, and adopted by so many authors, that the nauplius represents the starting point and ancestral form of the Crustacea, as I am to agree with Haeckel's strongly formulated interpretation of the importance of the zoëa in the derivation of the Malacostraca. Twenty years ago in my "Untersuchungen zur Erforschung der Genealogischen Grundlage der Crustaceensystems" I sought to show that the zoëa, so extremely variable in form, has no phylogenetic significance, but only represents

[1] See *Nature*, vol. l., p. 546, 1894.

a larval form, secondarily arisen, into the organisation of which many later acquired characters have been inserted so as to alter it in various ways.

To apply this view to the explanation of the nauplius was a natural consequence. A few years later I myself came to this conclusion, having been caught and held fast by the alluring teaching of F. Müller, which Haeckel had also enthusiastically taken up; but when Hatschek had drawn, from an unbiassed study of the development of the annelids, his conclusion that the nauplius represented a changed trochophore larva, I also tried to point out in my paper "Neue Beiträge Zur Morphologie dei Crustaceen" (*Arbeiten der Zool. Inst. Wien*, 1886), that the nauplius must be looked upon as an altered larval form of the annelid stock with accelerated arthropod characters. "If we cannot distinguish the genetic relations between arthropods and annelids in respect of metamerism of the body, of the similar processes of separation of the segments at the hinder section of the body of a growing animal, and of the general agreement in internal structure, especially that of the brain and ganglion chain, of the segmental organs (*Peripatus*), shell glands, antennary glands, limbs and parapodia, then we have to imagine as the ancestral form of the Protostraca (Archiphyllopoda) an organism like a many-limbed annelid with incipient arthropod characters, especially in the structure of the extremities" (*l.c.* pp. 93, 94).

If we view the nauplius form from such a standpoint, the dilemma which Hutton insists upon with regard to the origin of the Onychophora falls to the ground, and there is no reason to disallow the probability of all classes of Arthropoda having descended from annelids. No one can deny that the Onychophora represent the class nearest related to the annelids. They will therefore correspond to a branch which became cut off from the common stem quite early, even before any important reduction in the segmental organs took place, and acquired in connection with a land-habitat their peculiar tracheal respiration, differing from that of the insects and myriopods. From this stem the ancestral form of the hypothetical Protostraca or Archiphyllopoda may also have arisen. In fact, we have to think of it as having had some such form as I tried to construct at that time ("Crust. Syst.," *l.c.* p. 100). Arnold Lang, in his excellent text-book of Comparative Anatomy (1891), has drawn a more complete picture of this supposed stem-form, which corresponds to advances in knowledge since the time at which I wrote. I can express myself as agreeing in all essential points with this scheme, as regards both the internal structure and external form.

That the Myriopoda and Insecta which are nearly related to one another, and therefore are sometimes put together in one arthropod class, as Antennata, have the same origin as the Onychophora, appears to me as little admissible as the erroneous view which derives them from an aquatic zoëa. More rightly should the Antennata be looked upon as a third group of arthropods, derived from a special branch of the chief stem.

Similarly the Arachnoidea must hold the position of a fourth arthropod group. As is well known, different views have been taken as to the relations of the Arachnoidea. According to those authors who make a fundamental difference between gill- and tracheal respiration, they form, in connection with the insects, myriopods, and Onychophora, a sub-phylum of the Arthropoda, viz., Tracheata. The second sub-phylum is that of the Branchiata, which includes, besides the Crustacea, the Gigantostraca, Trilobita, and Xiphosura (Limulus). On the other hand, a second school of authors holds the Arachnoidea to be related to the three last-named groups, and classifies them together under the head Palæostraca. This conception, which I hold to be the right one, depends on the remarkable agreement shown by the segmentation of the body and morphological structure of the Palæostraca and the number and position of their limbs with what obtains in the scorpions; and it is furthermore strengthened by the similarities observed between the internal structure of *Limulus* and *Scorpio* (endoskeleton, coxal glands, structure and position of gill- and lung-books). The highly organised Arachnoidea would therefore stand nearest to the oldest and most original forms of this group of arthropods, and have developed from water-living Branchiata into terrestrial Tracheata. As regards the transformation of the gill-plates carried in chambers on the ventral surface into the lamellæ of the so-called lungs or tracheal chambers, it seems to me that McLeod has given a likely explanation.

But now the question comes,—are we to consider this fourth arthropod group as an independent branch of the arthropod stem, or are we to derive it from the Protostraca? The latter view would commend itself if we could succeed in discovering a nauplius-like larval form in the young stages of the Palæostraca. Of course the early stages of *Limulus* have been compared to trilobites, and on the other hand the oldest and most primitive trilobite (*Protaspis*), the larval nature of which cannot well be doubted, has been described as the forerunner of the crustacean nauplius and called Protonauplius (Beecher). Nevertheless, satisfactory proofs of this very interesting point are wanting, especially the presence of limbs, the structure and position of which are not known. So long as the necessary proofs are in abeyance, we must attach greater probability to the first of our suppositions, and thereby look upon the Arachnoidea and their predecessors the Palæostraca as a group arisen independently and having no connection with the Crustacea.

It is not necessary for me to emphasize the fact that in my discussion I have not dealt with proved facts, but only with views which claim a right to be heard, in deciding the relationship of the classes of Arthropoda from a standpoint based on the theory of descent.

Vienna. C. Claus.

I THANK you for your letter with the enclosed communication by Professor Hutton. But it is impossible for me, in only a few days, to work out so good an answer as I could wish, besides, for the present I am much engaged with other work.

During more than ten years I have spent much time preparing a paper on the structure and morphology of the skeleton, etc., in all orders of Crustacea and Myriopoda, and some orders of Insecta. A kind of preliminary note has been published in the *Zoologischer Anzeiger* for 1893 (translated in *Ann. & Mag. Nat. Hist.*, 6 ser., vol. xii., pp. 417–34), and I hope that the final paper will be published in a few years. Before this publication I do not wish to enter into any further discussion on this subject, as I believe it will be better to be able to refer readers to the full representation of the numerous details in the said work.

In this short answer I cannot make use of the modern terminology, since I dislike ancestor-hunting and pedigrees. It seems to me that too many authors take special pleasure in the construction of new "trees," and I must confess that I am not able to recognise the

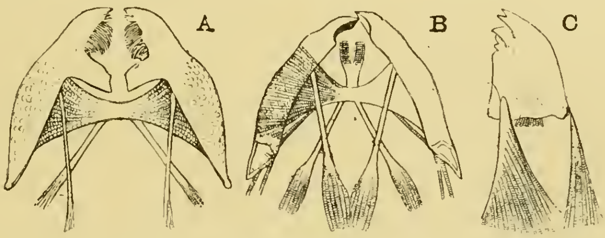


FIG. 1.—MANDIBLES WITH THEIR MUSCLES SEEN FROM BELOW.

A. *Diastylis goodsiri*, Bell. B. *Japyx solifugus*, Hal. (after Meinert, 1865). C. *Blatta* (*Periplaneta*) *Americana*, Fabr.

utility of these interesting forests. As to the Arthropoda, new facts, thorough-going studies of the adult animals are exceedingly wanted; further speculations on the facts at present generally known will, in my opinion, be of no value.

And now some remarks on the question itself. The lower malacostracous Crustacea and the Thysanura are, in my opinion, more closely related to each other than hitherto recognised. Here I shall produce but some few facts. If good figures of the mandibles with their muscles of *Nebalia*, *Diastylis* (belonging to the Cumacea) and *Blatta*, as types for lower Malacostraca and for the Orthoptera, were laid before a zoologist, and then figures of the same parts of two Thysanura, such as *Machilis* and *Japyx*, were shown him without telling him the names of these two animals, he would say without hesitation that the mandibles of the Thysanura belonged to Crustacea, and not at all to Insecta. The three accompanying figures (Fig. 1) will, I hope, illustrate this statement, and the following explanation will be satisfactory for this purpose. In *Diastylis* and *Japyx* we find a common median, free, internal plate with enormous muscles radiating into the elongated lateral cavities of the mandibles; and the two other pairs of muscles, with the long tendons, also are very characteristic (two small pairs of muscles issuing from the superior margin of the mandibles are not visible). In *Blatta* the cavity of the mandible turns backward, no median free plate exists, and of muscles we find three: an enormous *musculus adductor* from the inner edge, a *musculus abductor* from the outer edge, and a short oblique muscle from the interior wall of the mandible. It may be remarked that *Blatta*, which has been investigated by numerous authors, is without good reason generally considered as being of low organisation; the distance from *Blatta* to such low forms as *Japyx* and *Machilis* is, in reality, rather great, at least as great as between *Blatta* and Coleoptera.

Lubbock makes mention of a second pair of maxillæ in the Thysanuran family Collembola; the limbs he has seen are the *first* pair of maxillæ, as they originate in advance of the second well-known pair on both sides of hypopharynx; in *Japyx* this first pair possesses a well-developed, three-jointed palp. Thus, in the Thysanura we observe four pairs of mouth-limbs, homologous with the four pairs in Isopoda and Amphipoda; the conformity exists to such a degree that in *Machilis* and the Malacostraca the two lobes on the second maxilla originate from the *second* and the third joints, while the lobes on the first maxilla in the lower Malacostraca are produced from the *first* and the third joints; finally, in the Amphipoda the fourth pair, the maxillipeds, has one or two of the joints coalesced in the median line, thus forming a kind of lower lip with palps—as the fourth pair, the labium, in *Machilis*, etc. (These and numerous other facts are mentioned in my preliminary note of 1893.)

Not very far distant from the Thysanura we find *Scolopendrella*, which forms a transition to the Chilopoda and the Diplopoda, the distance between these three orders being much shorter than that between, say, Phyllopoda, Isopoda, and Copepoda.

The Crustacea, the Insecta, and the Myriopoda, are, in my opinion, more closely related to each other than are the Arachnida to any of them—not to mention the Pantopoda, with good reason now generally accepted as a separate class.

It is now, I think, generally admitted that the larval stages: nauplius, zoëa, larvæ of insects, etc., have been very much over-estimated in judging the relationships between the different classes of Arthropoda and the relation of the whole series to lower animals. In Korschelt and Heider's well-known "Lehrbuch" (vol. i., p. 904), we find a similar view.

The well-known characters for the four old Classes of Arthropoda: the jointed legs (in all forms not much degraded by parasitism), the arrangement and the quality of the muscles, the exuviation of the skin, the eyes, etc., are, in my opinion, good evidence of their rather close relationship to each other.

Peripatus I have never examined, but, so far as I can see, it stands apart from all Arthropoda and must be removed from the series. Korschelt and Heider have placed it among these animals, in front of the Myriopoda, and have put together all the essential characters known in 1890; but this same chapter of their work shows very plainly the enormous differences between *Peripatus* and the five classes of Arthropoda, with the structure of which I am tolerably acquainted through studies during many years. Such features as the quality and arrangement of muscles, the series of nephridia, the structure of the mouth and the "limbs," the eyes, and the generative organs with their ducts seem to be sufficient. Furthermore, it will be admitted that *Peripatus* has nothing to do with the Crustacea, and since *Scolopendrella* and the Thysanura show a closer relation to the lower Malacostraca than hitherto recognised, while the Myriopoda are divergent, specialised forms (for instance, the "gnathochilarium" is, in spite of embryology, formed by the coalescence of two pairs of mouth-limbs), I think that no place for *Peripatus* will be found among the Arthropoda. I am most inclined to look upon this animal as a "worm specially modified for a terrestrial existence," and it must, in all probability, take the rank of a separate class between the multitude of very different forms together composing the "worms" (such as Annulata, Chaetognatha, Sipunculida, Nemertina, etc.) whose mutual relationships are still in part rather obscure.

H. J. HANSEN.

Copenhagen.

THE conclusion of F. W. Hutton as to the position of *Peripatus*, expresses the view which I also hold, and have expressed in *Jenaische Zeitschrift*, Bd. xxx., p. 67, 1896.

In a treatise entitled "Die Entwickelung der sog. Lungen bei den Arachniden, &c.," (*Zeitschr. wiss. Zool.* Bd. lviii.: See also *Muzeum Lemberg*, 1896), in connection with the discovery that, in *Trochosa*, the so-called lung passes into tracheal branches, while in other spiders and in scorpions it is sac-like and thus must actually be regarded as a sac-trachea which does not develop further, I have shown that the view that the Arachnida are to be derived from Crustacea, whose respiratory appendages sank beneath the surface of the body and thus led to the formation of arachnidan lungs, cannot be maintained. On the contrary, anatomy and ontogeny both show that the arthropodan appendages, together with the gills, are to be deduced from the lungs.

It is self-evident that the Arthropoda are descended from those lower worm-like animals which, in adaptation to terrestrial life, developed integumental depressions, *i.e.*, sac-tracheæ, for the purpose of breathing air; the possibility that this adaptation was of special utility to animals dwelling on the shore and subject to the recurrence of the tides is by no means denied. Exposure to air and sun brought about, for protection, the hardening of the integument, while locomotion gave rise to annulation. In this condition of the body, the animal could subsist in a damp, slimy medium. It is justifiable to assume that limbs were developed in adaptation to life on dry land, where the skin would suffer from friction and the respiratory organs from exposure to dust. Simple lateral sac-tracheæ (Fig. 2 A*a*) carried on respiration also during the rainy season: more highly developed tracheæ (lungs) with folded walls (Fig. 2 A*b*), especially when muscles were associated with them, afforded the animal the means of closing the sacs, concomitantly with the development of the first rudiments of the locomotory organs by the evagination of the folds (Fig. 2 B).

Ontogeny teaches us that the limbs, as a rule, do not begin to form simultaneously along the trunk, but develop from before backward. The anterior portion of the body thus became adapted for life on land sooner than the posterior portion: it is therefore not surprising that, as stronger subsequently developed musculature

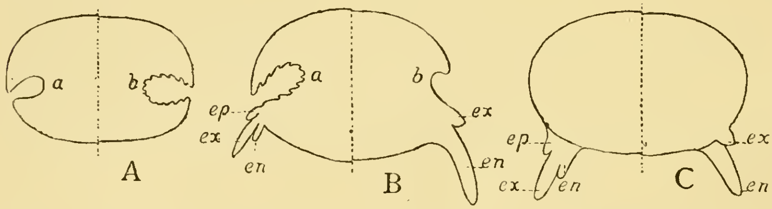


FIG. 2.—COMPARATIVE DIAGRAMS ILLUSTRATING EVOLUTION OF LUNGS AND LIMBS.

A—Combined diagram. *a*, simple sac-trachea (lung); *b*, sac-trachea provided with folds.

B—Combined diagram. *a*, a folded sac-trachea; *b*, the remains of the so-called lung; *e*, epipodite; *ex*, exopodite; *en*, endopodite.

C—Combined diagram. *ep*, epipodite; *ex*, exopodite; *en*, endopodite. (In *Trochosa singoriensis*, Laxm.)

appeared at the anterior part of the body, the respiratory organs vanished, but left behind the limbs as vestiges, while, in the posterior part, the respiratory organs persisted in a high state of development. Whereas, if the whole body had become simultaneously adapted to land life, or else one part shortly after the other, it would have retained the original worm-type (Myriopoda).

But how have the Crustacea arisen? Their structure, considered anatomically and ontogenetically, leaves no doubt of their racial relationship to the other Arthropoda. The annulation of the exoskeleton and the development of the limbs must, in them, have been determined by the same causes as in the other Arthropoda. The Crustacea, as Simroth has so conclusively argued in his "Entstehung der Landthiere," 1891, were originally terrestrial, their limbs serving for locomotion on land. They may have been compelled by the search for food, to take to an aquatic life. The exo-, endo-, and epi-podite developed from lung lamellæ for increase of the respiratory surface. The gills are the homologues of the lung lamellæ, since it has been established by Kingsley's interesting researches (*Quart. Journ. Micr. Sci.*, 1885) that the limbs of *Limulus* are protruded from a depression; and in *Ligia*, *Oniscus*, and other Crustacea,[1] similar invaginations have been observed near the limbs. In all groups of Arthropoda, indeed, in the embryo, limbs are to be found which reveal the primitive type[2] (Fig. 2 C). Since, therefore, the Crustacea became aquatic, it is evident that the depressions (sac-tracheæ) functioning as respiratory organs must not only have ceased to develop further, but must have degenerated, and the absence of tracheæ in this class is thus accounted for.

The parapodia and gills of the annelids are due to the same evolutionary factors. The annulation of the annelid body and the hardening of the integument at any rate testify to the fact that they also are of terrestrial origin. The original sac-like respiratory organs have disappeared, the parapodia and the branchiæ remaining behind as vestiges.

While referring the reader for details to the original works on the subject, I think that in thus deducing arthropod limbs from lungs I have disposed of the assumed derivation of the Tracheata from the Branchiata, and have come nearer to the true origin of the Arthropoda, more especially of *Peripatus*.

It is, however, difficult to arrive at definite conclusions as to the systematic position of the last-named arthropod. We are as yet only on the threshold of a great building in one corner of which truth dwells. The assumption that *Peripatus* is most nearly related to the most highly developed annelid, and Hutton's conclusion that the former should be directly classed with the latter are not without justification. It is certain that *Peripatus* cannot possibly be derived from the Crustacea. I incline to the view that *Peripatus* is a secondarily adapted land animal. In the first process of adaptation, it acquired gills and parapodia-like truncated limbs, which, owing to aquatic life, could not develop further. The animal, in this process, lost its respiratory organs. Later, by gradual *re-adaptation*, it re-acquired breathing organs in the form of trachea-like invaginations of the integument, sometimes scattered all over the body. This view gains in probability from the fact that other arthropods (*Oniscus*, *Birgus latro*) have become readapted to land life.

When we consider the origin of the limbs and the gills, as well as of that of the parapodia and gills of the annelids, we arrive at the conclusion that water and land have from the first shaped these animals, and that they are all to be derived from worm-like ancestors. In this case, *Peripatus* represents a transition form between the Arthropoda and the highest annelids, being connected with the former

[1] cf. *Zeitschr. fur wiss. Zool.*, Bd. lviii., pp. 54–78; *Muzeum Lemberg*, 1896; *Zool, Anz.*, Nos. 455 and 473.

[2] cf. also *Zool. Anz.* Nos. 363 and 364, and especially No. 392, where a case is cited of a limb possessing an appendage as well as the endo-, exo- and epi-podite.

by the structure of the limbs, especially of the jaws, and of the tracheæ, and with the latter by the slight development of the limbs. It was merely a freak of nature, that the conditions which influenced the formation of the body of *Peripatus* were different from those that affected other arthropods. After careful consideration of the comparative anatomy and ontogeny of these animals, the type "Arthropod" appears to me, if we omit their primitive ancestors, not sufficiently comprehensive, and I recognise the advisability of accepting Cuvier's type Articulata, divided into Arthropoda and Annelida, all the more since this type could, by means of the Trochophore, be united with the Mollusca in a common genealogical tree.

A. JAWOROWSKI.[1]

Lemberg.

THE questions raised by Captain Hutton should have more space for their adequate discussion than NATURAL SCIENCE can afford. I will therefore give my views upon the subject in outline only, and would refer the reader for more details and for a tolerable bibliography to my paper on the Classification of the Arthropoda (*American Naturalist*, vol. xxviii., 1894, Tuft's College Studies, No. 1). Some thirteen years ago (*Amer. Nat.*, vol. xvii., 1883) I was troubled with almost the same questions as those bothering my antipodean colleague, and now, although my line of argument would be much different from that then adopted, my views are essentially unchanged. It is interesting to note that von Kennel, in his "Lehrbuch der Zoologie" (1893) and in other papers, has adopted similar views. His researches upon *Peripatus* entitle his opinions to consideration.

The first question, that in regard to the importance of the nauplius, can be briefly answered. I think that most morphologists at present regard it as an adaptive condition introduced early in the phylum and not as representing an ancestral condition. Any other view leads to inextricable difficulties. The nauplius, I think, can safely be ignored in any discussion of the inter-relationships of the Arthropoda.

Next comes the question concerning *Peripatus*. Has it that importance in phylogeny which it seemed it must have when Moseley's investigations were first published? Is it an arthropod? Is it an annelid? Or is it a representative of a distinct group? As my views upon these points are somewhat at variance with those held by many morphologists, I may be permitted to say a few words concerning them.

Peripatus differs from all arthropods in several features. It has functional nephridia, repeated metamerically in most of the segments of the body; while so far as I am aware no true arthropod has more than two of these organs. Striped muscle is found nowhere in

[1] Kindly translated from the German by Mrs. H. M. Bernard.

Peripatus, except in connection with the jaws. The antennæ are outgrowths from the primitively pre-oral region, and hence cannot be compared with the antennæ of Insecta or either pair of antennæ in Crustacea. The ciliation of the nephridia and alimentary canal is without parallel in the Arthropoda. The external cuticular skeleton is weakly developed and the legs show no true jointing. Then, too, the eyes are unlike those of any true arthropod, but are strikingly like those of the polychæte *Autolytus*, Fig. 3. It is further to be remarked that in *Autolytus* there are two pairs of eyes, the second pair occupying the position of the problematical structures labelled *d* by Balfour (*Quart. Journ. Micr. Sci.*, xxiii. pl. xviii., fig. 22). The muscular nature of the pharynx is not arthropodan but resembles rather that of the Chætopoda.

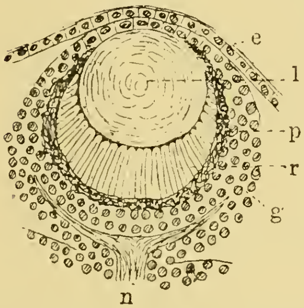


Fig. 3. Section of eye of Annelid, *Autolytus*. *e*, epidermis; *g*, ganglion layer; *l*, lens; *n*, optic nerve; *p*, pigment layer; *r*, rods. Compare with eye of *Peripatus* as figured by Balfour, *Quart. Journ. Micr. Sci.*, xxiii., pl. xviii., fig. 22, 1883.

On the other hand the features relied upon for associating *Peripatus* with the (tracheate) Arthropoda are the following:—the presence of tracheæ; but we must remember that at least two other kinds of tracheæ occur in the Arthropoda which could in no way be derived from those of *Peripatus*, and further, that the resemblances between the tracheæ of *Peripatus* and those of Hexapoda are, aside from the physiological one, extremely slight. In fact the evidence, so far as tracheæ are concerned, points to convergence rather than to community of descent. In *Peripatus* a pair of nephridia is set apart exclusively for reproduction purposes, a condition which recalls the structures in both Hexapoda and Crustacea; but we must not forget that similar relations occur among certain annelids as well as in other groups. Of more importance are the facts that the dorsal vessel, with its several ostia, is enclosed in a pericardial chamber, while the circulation is lacunar and the cœlom is much more reduced than in most annelids. To these might be added the claws terminating the legs and the character of the jaws, apparently appendicular in nature. We must, however, remember that so far as our knowledge goes, we are doing no great violence to morphology when we compare the feet of *Peripatus* with the appendages of a syllid worm, or when we compare their terminal claws to chætopod setæ. In short, I may say that there is a pretty large amount of doubt as to whether the Malacopoda are Arthropoda.

Now, leaving *Peripatus* and the nauplius out of the question, we are met with the greater problem:—Is the group Arthropoda a valid one? Or is it rather an assemblage of convergent forms with no common ancestor nearer than the annelids?

Omitting all considerations of minor groups like the Tardigrada, Linguatulida and Pycnogonida, I think that recent students have shown that there are three large groups among the forms commonly considered as Arthropoda. The first of these, the Branchiata, includes the Crustacea and Arachnida. The arguments for this association have been so often presented that I need not rehearse them here. For the second group the name Insecta or Antennata may be employed. It includes the Hexapoda and the Chilopoda. Both Pocock and myself have repeatedly pointed out that there is no valid group of Myriopoda. The Chilopoda are decidedly like the Hexapoda in their broader anatomical features, while the Diplopoda[1] differ so totally from these that they may form our third group. Concerning these Diplopoda, however, so little is known that they may be left out of consideration in what follows.

For convenience of contrast of the characters of the Branchiata and Antennata I employ parallel columns.

BRANCHIATA.	ANTENNATA.
Respiration by gills (or lungs or tracheæ derived from gills) arising in connection with appendages.	Respiration by tracheæ (modified glands ?) never connected in origin with the appendages.
Functional nephridia in somites, two, five, or both.	No functional nephridia.
Genital ducts (modified nephridia) opening near the middle of the body.	Genital ducts (modified nephridia) opening near the end of the body.
Malpighian tubes (when present) entodermal.	Malpighian tubes ectodermal.
Stomodaeum long.	Stomodaeum short.
"Head" indefinite, ranging great y in limits even in the same group.	Head distinct, consisting of pre-cephalic lobes and five segments (as indicated by the neuromeres).

These are, it seems to me, points of fundamental importance, and I can hardly see how one can reconcile them without going back to the annelid ancestor. Certain it is that the two groups must have become differentiated at a time when nephridia were present in all segments, when these served as the only excretory organs, when there was no head comprised of coalesced somites, when there were no specialised respiratory organs apart from the general body-surface or possibly portions of the appendages, and when no appendages were specialised as jaws to the exclusion of others. No known form outside the annelids meets these requirements; and these facts, it seems to me, go far towards showing that the group Arthropoda does not form a true phylum, and that the similarities which are often advanced to support such a view are the results of convergence, many of them explicable as homoplastic.

J. S. KINGSLEY.

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[1] F. C. Kenyon has recently shown that *Pauropus* is an aberrant dipiopod: "The Morphology and Classification of the Pauropoda," *Tufts College Studies*, No. 4, 1895.

Mr. F. W. Hutton's letter opens a question which has engaged my attention for some time, and I think that his fears for the Arthropoda are fully justified by the facts. I have long felt that, while the term Arthropoda is a very convenient one as expressing a certain kind of structural modification, it does not imply a phylogenetic relationship among the forms included in it. In short, the Arthropoda had not an arthropod common ancestor, and jointed legs cannot be regarded as an absolute criterion of relationship.

The position of *Peripatus* is closely associated with this question. Its claims to be included among the Arthropoda are too well known for it to be necessary to recapitulate them here, though we shall see that one of them—the presence of tracheæ—is of doubtful value. If we admit *Peripatus* as a primitive arthropod—an offshoot not far removed from the ancestral stem—the ancestral stem can only be that of the Myriopoda and Insecta. It is quite impossible to insert *Peripatus* into the ancestry of the Crustacea, and it is equally impossible to find a place for it in any line passing from the Crustacea to the Insecta. The Arachnida must also be excluded from this series, as any attempt to derive the Arachnida from a tracheate ancestry leads only to confusion worse confounded. Even if we leave the probable ancestry of the terrestrial arachnids from aquatic forms, related to *Limulus* and the eurypterids, out of the question, we find that the lower and more primitive members of the class are in every case provided with a respiratory apparatus of peculiar and characteristic form, which must, in this case, be secondary and developed from tracheæ. This being the case not only with the lower orders, such as Scorpionidea and Pedipalpi, but the lower members of the Araneina having also no tracheæ, any attempt to construct a phylogenetic tree starting from tracheate forms ends in failure. We must therefore admit that tracheæ have been developed twice along two independent lines, and this, it may be noted, weakens the claims of *Peripatus* somewhat.

The arachnid question is, however, somewhat of a side issue, as the Crustacea alone are sufficient to show us that, if we admit *Peripatus*, we must break up the Arthropoda and depose the jointed leg from its present high place. It must be regarded as a product of convergent evolution or homoplasy. Another important point in this connection is noted by Mr. Hutton, namely, that the compound eyes of Insecta and Crustacea are in this case also a product of convergent evolution. The resemblance between them is very close, but I hope soon to be in a position to show that it is only superficial.

If, on the other hand, we take advantage of the weakening of the claims of *Peripatus*, due to the necessarily double, and therefore, possibly threefold origin of tracheæ, and cast it out from among the Arthropoda, I do not think that we gain much. By doing so we necessarily seriously weaken the absolute position of the jointed leg; for, though not elaborately articulated, the legs of *Peripatus* approach sufficiently near the arthropod type to shew us the possibility of convergent evolution acting so as to produce jointed legs along different lines. The remaining classes of the Arthropoda are, further, by no means easily arranged. The Crustacea must be regarded as standing on their own base down to the lowest forms, and possibly, if the nauplius has any phylogenetic significance, down to much lower forms than exist at present. The whole series of Crustacea, however, give us no point from which the other Arthropoda can be derived. The characteristic specialization of the crustacean head resulting in two pairs of preoral appendages and three pairs of masticatory ones, would have to be undone to arrive at any of the other Arthropoda. It is of course possible here as elsewhere to imagine a hypothetical ancestral form with jointed legs from which the various classes can be derived, but is it necessary? Is the jointed leg a structure of such profound significance that we should let it trample on all other considerations? The gulf between a parapodium and such a leg is not so wide and deep as it appears at first sight, and must have been crossed more than once whichever position we assign to *Peripatus*. The change is one of obvious advantage in many ways, and given the formation of an exoskeleton, is almost inevitable. Further, the legs of the different classes have no points in common, except the one of being jointed. The legs of the Crustacea are traceable throughout to modifications of the one well-marked biramous type persisting through all kinds of functional changes, but no trace of this type, except a few extreme variations which might be used to prove anything, is seen in the other classes. Similarly, the legs of Arachnida and Insecta have very few points of resemblance. The number of joints, the relation of the anterior appendages to the mouth, the presence of antennæ (if these are to be considered as homologous with the other appendages) in the Insecta and Myriopoda, all afford marked points of difference.

One important point of morphological resemblance remains however, *i.e.*, the cœlom. The universal disappearance of this structure in Arthropoda wants explaining, and, if one is prepared to drop *Peripatus*, is an argument of no little weight for the phylogenetic unity of the group. If we retain *Peripatus*, its importance vanishes, since, as I have said, it is impossible to consider *Peripatus* as ancestral to the whole group, and it follows that the cœlom was suppressed independently in the different classes. It seems possible that this reduction of the cœlom may be directly associated with the acquisition of jointed legs, and of a body formed of a series of inflexible rings, the consequent enormous muscular development tending to encroach upon it. The excretory function usually associated with the cœlom being partly taken over by the formation of the cuticle, would permit of this encroachment on the cœlom, and would account for the reduced number of nephridia. The whole question of the cœlom in the Arthropoda is, however, wrapped in profound obscurity.

In conclusion, once one fairly faces the Arthropod question, the "Phylum" seems to disintegrate into a number of separate groups. Three assemblages seem quite natural, (1) the Crustacea, (2) the Arachnida, and (3) the Tracheata Antennata (Insecta, Myriopoda, and very probably *Peripatus*). The relations between these are yet to be determined, and the common ancestor is almost certainly to be sought for among animals which are not arthropod in structure.

MALCOLM LAURIE.

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THE question raised by Professor Hutton seems to be twofold: (1) Are the Arthropoda mono- or poly-phyletic? (2) If polyphyletic, is it justifiable to speak of them comprehensively under one title? Touching the first question: it is generally admitted that the key to arthropod morphology is to be sought amongst the chætopod worms. It is also admitted, I take it, that among the arthropods those which stand nearest to the chætopods are the Prototracheata (*Peripatus*), on the one hand, and the Phyllopod Crustacea (*Apus*), on the other; also that *Apus* is the most primitive type of the great aquatic branchiate division, the Crustacea; and that *Peripatus*, if associated with any classes at all, must be looked upon as most nearly related to the ancestral type of the terrestrial tracheate division, comprising insects, centipedes, and millipedes.

This conclusion leads to the inquiry whether the *Peripatus*-like ancestor is derivable from the *Apus*-like ancestor or *vice versâ*; or whether the two are traceable to a common hypothetical form, possessing the characteristic arthropod features, and thus standing apart from the Chætopoda. The answer that is given to these questions will be the answer to Professor Hutton's query concerning the plurality of origin of the Arthropoda.

To my mind the answer must be in the negative in each case; whence it follows that we must go back as far as the Chætopoda to find a connecting link between the Tracheata and Branchiata. Moreover, it appears to me that the same result will be reached if we select any other crustacean instead of *Apus* as the primitive type, and if we discard *Peripatus* as the ancestor of the Tracheata, and choose some other form, real or imaginary, to take its place.

It should be borne in mind, however, that there is no connecting link either between *Peripatus* and the Chætopoda, or between *Apus* and the Chætopoda. The two forms seem, in fact, to have travelled from the chætopod starting-point along parallel roads; and being furnished with the same material in the way of parapodia, not to mention other organs, they have utilised them in the same way in response to similar needs, converting those in the neighbourhood of the head into feelers and jaws, and others farther back into more perfect organs for progression. It thus becomes possible, owing to the disappearance of the intermediate stages, to give a diagnosis of the primitive Crustacean, and of the primitive Tracheate, which will serve to show how, in company with the more specialised forms usually associated with them, they may be distinguished from all known bristle-footed worms. This is one of the reasons why, in spite of my belief in the plural origin of the classes signified, the retention of the term "Arthropoda" commends itself to me as expedient. Another is, that, owing largely to our ignorance of that special branch of morphology which deals with the nerve-segments of the head (cerebral neuromeres) and the homology of the cephalic appendages, we are not yet in a position to decide as to the number of sections into which the old group should be severed.

Although the question of the classification of the Arthropoda is somewhat outside the limits of the present inquiry, I should like to be permitted to add that, in my opinion, the following main sections may be recognised :—I. PROTOTRACHEATA or MALACOPODA, for *Peripatus*, which occupies an isolated position, though most nearly related, in my opinion, to the Chilopoda of the section Opisthogoneata. II. TRACHEATA, with the subdivisions: A, PROGONEATA (Classes: Diplopoda, Pauropoda, Symphyla); B, OPISTHOGONEATA (Classes: Chilopoda, Hexapoda). III. BRANCHIATA or ACERATA, including: A, CRUSTACEA; B, GIGANTOSTRACA (Trilobita, Eurypterida, Xiphosura); C, ARACHNIDA; D, PANTOPODA (Pycnogonida).

The terms Branchiata and Acerata seem to me to be equally open to objection as applied to the third assemblage; but for the present they may pass. The Pantopoda are here included, because their affinities appear, on the whole, to be near the Arachnida. The latter are placed near the Gigantostraca, because, in my opinion, the close resemblance between the Eurypterida, not to mention *Limulus*, and the scorpions is inexplicable on the theory of convergence during descent under totally different conditions of life from forms so remote from both as the Chætopoda; though this is what my friend, Mr. Bernard, would have us believe. As for the Linguatulina and Tardigrada, the evidence that they belong to the Arthropoda seems to me far from conclusive.

R. I. POCOCK.

British Museum (Nat. Hist.).

IT may be convenient to mention that the first author to whom Professor Hutton refers is, not Philipp Franz von Siebold of Japanese celebrity, but Carl Theodor Ernst von Siebold, who established the Arthropoda in the Text-book of Comparative Anatomy, published by himself and H. Stannius 1845–1848. The second volume appeared in 1845, but the first, for which Von Siebold is responsible, not till 1848. It is this latter which is occupied by the invertebrate animals. Among them the Arthropoda are the fifth principal group, containing Crustacea, Arachnida, Insecta, the Myriopoda being included under the Crustacea. Fritz Müller's opinion that the four classes, Crustacea, Insecta, Myriopoda and Arachnida, are all branches of a common stem, is given in the long and interesting foot-note which he contributed to Dallas's translation of his "Für Darwin" in 1869. If this opinion be correct, "it is evident," he says, "that the water-inhabiting and water-breathing Crustacea must be regarded as the original stem from which the other terrestrial classes, with their tracheal respiration, have branched off." The tracing of the pedigree must be sought elsewhere, for in the conclusion of "Facts for Darwin," after glancing at a suggested connection between the Rotatoria and Crustacea, Fritz Müller says, "But I can see nothing certain. Even towards the nearer provinces of the Myriopoda and Arachnida I can find no bridge. For the Insecta alone, the development of the Malacostraca may perhaps present a point of union."

It is obvious that if the primitive arthropod is to be regarded as anything like a crustacean nauplius, *Peripatus* cannot be a primitive arthropod. Still it might be an arthropod, without being a primitive one. Primitive and tracheate seem to be scarcely compatible terms. Among the Crustacea some of the terrestrial Isopoda have appendages furnished with tracheæ. It would be very difficult to believe that in the Isopoda such a character was primitive and not a late acquisition. Considering the remoteness of the geological time at which we find the classes of Arthropoda already distinct from one another, there are but dim hopes of our ever positively discovering the point at which their lines of ancestry meet. The "probable antiquity" which Mr. R. I. Pocock (*Journ. Linn. Soc. Zool.* vol. xxiv., p. 518, 1894) attributes to *Peripatus*, *Peripatoides*, and *Peripatopsis* may be indefinitely great, but hitherto it has told us nothing of the form from which they started on their peripatoidal career. We cannot tell how many stages have been made in advance, nor how many backward. Although this anomalous group has only been seriously studied for a short time, there are already three names for it at our disposal, Malacopoda, Onychophora, and Prototracheata, of which the last seems the least suitable because it implies a yet unproved hypothesis. But there is surely no urgent necessity to decide whether the group belongs to one or other of the primary divisions of the animal kingdom as hitherto accepted. Whether it be left standing on the outskirts of the Vermes or of the Arthropoda, in the modern activity of research there is always a chance that fresh light will be thrown on its affinities, and its position then assigned on reasonable grounds, and not by premature guessing.

T. R. R. Stebbing.

Tunbridge Wells.

Summary.

Though the opinions as to the constitution of the Arthropoda, advanced by the specialists who have kindly replied for us to Captain Hutton, are as diverse as possible upon many points of taxonomy, there is tolerable unanimity touching the isolated position of *Peripatus*. Even on this subject, however, there are gradations of opinion, from that held by Mr. Stebbing, who would wait for further evidence before expelling the Malacopoda from the rest of the Arthropoda, to that of Dr. Hansen, who boldly maintains that *Peripatus* is nothing but a specialised chætopod. Mr. Carpenter and Professor Claus, on the other hand, regard it as a type that early diverged from a common arthropod stock, structurally in advance of the grade of organisation characteristic of the higher Annelida. These two authors therefore regard the Arthropoda as a homogeneous, monophyletic assemblage, while Messrs. Kingsley, Laurie, Pocock, and Bernard, who hold that there is no common ancestor for *Peripatus* and, say, the Crustacea nearer than the Chætopoda, are compelled to conclude that the "sub-kingdom" is polyphyletic. This also appears to be the view of Dr. Jaworowski, and also, as we have good reason to suppose, of Dr. D. Sharp. The balance of opinion seems therefore to be in favour of a plural origin for the Arthropoda.

Concerning the relationship of the various classes it is noteworthy that Messrs. Claus, Carpenter, Bernard, and Kingsley admit no affinity between *Peripatus* and the Tracheata Antennata, while Pocock and Laurie lean to the opinion that the two may be related, an opinion which was long ago tacitly expressed by Lankester when he established for them his group Ceratophora.

Again, Mr. Bernard seems to stand alone in his view as to the independent annelid origin of the Arachnida, Messrs. Claus, Kingsley, Pocock, Laurie, and Carpenter placing this class in close proximity to the Xiphosura, as was originally done by Lankester. But, while Pocock, Bernard, and Claus consider the Xiphosura to be the lineal descendants of the Trilobita, Professor Kingsley places the latter alongside the Crustacea and the Xiphosura at a distance under the Arachnida. This tangle of opinion is still further increased by the association of the Trilobita with the Crustacea by both Mr. Bernard and Mr. Pocock, and the separation of the two as of independent origin by Professor Claus. Mr. Laurie preserves a judicious, but provoking silence on the topic of the Trilobita.

Most of our contributors speak of the Myriopoda as a recognisable and natural assemblage; but while Messrs. Pocock and Kingsley repudiate the notion and classify the Chilopoda with the Hexap da, Kingsley does not admit the affinity that Pocock thinks traceable between the Diplopoda and the two classes now mentioned.

The opinions of Drs. Hansen and Jaworowski, though mentioned in connection with *Peripatus*, differ so entirely from those of the other contributors as to be of necessity treated apart. Hansen, who, perhaps wisely, has nothing to say concerning descent, regards the Arachnida and Pantopoda as isolated classes of the Arthropoda, and the Crustacea, Myriopoda, and Insecta as closely allied, this unique conclusion being based upon the structural resemblances in the gnathite known as the mandible, which is assumed to be homologous in those classes. Lastly, Jaworowski declares it to be a self-evident fact that the Arthropoda are descended from worm-like ancestors possessing sac-like pouches for breathing air. The opinion of the majority, however, is decidedly in favour of their descent from aquatic non-tracheate organisms.

Captain Hutton deserves the thanks of zoologists for having elicited the opinions of so many well qualified to speak on this interesting question. But we should like to be present when he next lectures to his students on this subject, to hear how he has managed to reduce these varied views to dogmatic order.

III.

Have Fish a Memory?

A CHAPTER IN THE EVOLUTION OF THE BRAIN.

FEW contrasts are more striking than that between the nebulous metaphysical psychology of former generations and the modern development of that science on physiological lines. It would now appear that comparative anatomy is likely to prove of inestimable value in its further advance; and for this, science is largely indebted to Professor Ludwig Edinger of Frankfurt, whose labours in this field, extending now over many years, have already gone far to establish a veritable science of comparative psychology. Professor Edinger asks us to publish an appeal for information on the subject of the existence of memory in fishes. The importance of this question, perhaps at first sight not very apparent, is clearly shown in the light of an address which he has published in the *Allgemeine Medicinische Central Zeitung* for 1896 (Nos. 79 and 80) on the Development of the Brain-tracts in the Animal Series, which lies before us, and a short account of which will not only prove of interest to our readers, but may help to stimulate a response to the question propounded by its author.

His contention is briefly this. No hard and fast line exists between the mental capacities of the lowest and highest vertebrates. We ought to be able to trace the various transitional stages, and by the light of comparative anatomy and histology to assign their true psychological value to the different nervous structures which make their appearance in ascending the animal scale. But to do this we require much new and detailed information as to the mental capacities of various groups of animals; and in collecting this it must not be taken for granted that apparently deliberate and purposeful actions are in reality due to conscious volition as we understand it in the higher animals. Professor Edinger denies that in lower organisms the lower nerve-centres, which alone are present, can fulfil the same functions as the higher centres, which are first to be found higher up in the animal scale. He regards the appearances of conscious volition shown by such invertebrates as a sea-anemone or an earthworm as purely reflex: it is the observer who is apt to employ too "anthropopsychic" a standard in their interpretation. It has been demonstrated by Retzius that the ventral nerve-cord of an earthworm contains a complete and elaborate reflex mechanism:—afferent sensory fibres which, after sub-division, are connected with the dendritic processes of the great ganglion-cells, whence arise the motor nerves, and also nerve-cells which seem from the distribution of their processes fitted to fulfil an associative function between the neuro-muscular apparatus of adjacent segments or metameres. A precisely similar fundamental mechanism is present in the spinal cord of all vertebrates from the lowest to the highest, and this mechanism must alone suffice for all the various and often purposeful movements which a decapitated frog, duck, or rabbit can perform, and indeed for the whole range of capacities possessed by *Amphioxus*. A direct influence of the brain upon the affairs of the cord does not exist in the lowest vertebrates, though already in sharks association-tracts connect the cerebellum and mid-brain with the cord. In passing upwards, such association-tracts develop more and more, till in mammals we find the pyramidal tracts connecting the cord with the cerebral cortex itself. At the upper end of the cord, where the cranial nerves arise, the association-tracts are immensely developed, and this, with the size and importance of the nerve-nuclei there situated, gives rise to the medulla oblongata.

The cerebellum first appears in cyclostomes (the lamprey and hag-fish), but its development depends chiefly on physiological causes: great swimmers have great cerebella, *e.g.*, sharks; and swimming reptiles have one twice as big as have reptiles which live on land. The lateral lobes are comparatively late acquisitions.

Next to the spinal cord, the mid-brain is the most constant in the whole animal series: in all embryos, and permanently in lower vertebrates it is a large and conspicuous organ, and it receives the main part of the terminations of the optic nerve, as well as the terminations of most of the sensory association-tracts from the cord. In animals with large eyes, *e.g.*, bony fishes and birds, it has an unusual development.

The fore-brain consists essentially of an olfactory apparatus, a basal ganglion, and a pallium. The olfactory apparatus is always basal, and has already reached a high grade of development in fishes, though in different classes it varies with the mode of life, being almost absent in birds. The function of the corpus striatum, though as yet unknown, is manifestly important. It is the pallium which is the important region from the point of view of psychical development: the evolution of other parts of the brain is insignificant in comparison with that here to be seen, ending in the colossal cerebral hemispheres of man. For simple movements, for the primary appreciation of sensory impressions, a simple apparatus serves everywhere alike, but higher associative mental activity first becomes possible with the development of this part of the fore-brain. In earliest rudiment, it exists as a thin plate of epithelium in selachians, ganoids and teleosteans. In amphibians it is definitely of nervous texture, and in reptiles it first takes on the form of a distinct cerebral cortex. The primitive cortex seems to have demonstrable relations with the olfactory apparatus only: it is merely a centre for olfactory associations. The differences between a fish and a reptile must be that the latter is able to store, associate and compare its olfactory impressions—the former, not. In passing up the animal scale new centres are added, one at a time, and so piece by piece the whole cerebral cortex is built up. It is for the future to trace the details of the process, but already we can follow the "installation," as it were, of the cortical visual field. In fishes, in young animals, in a new-born child, the retinal fibres end in the roof of the mid-brain. In the second month, as the child begins to acquire visual experience, fibres develop from the mid-brain to the occipital region of the cerebrum, and other anatomical connections quickly arise between this cortical visual field and other regions of the brain. The child now begins not merely to see, but to perceive and correlate visual impressions. This visual tract from the mid-brain to the cortex is wholly absent in lower vertebrates. Fishes are readily deceived by the angler; even a hungry frog or snake may fail to see its prey unless the latter moves, or is detected by smell. But birds have this tract well developed, and they clearly perceive and store impressions. It takes a good scarecrow to deceive a sparrow. Just as the lower vertebrate depends on smell for the supply of its requirements, so the bird relies on vision. It is of course amongst mammals that the cerebral cortex acquires its chief complexity, and that the manifold development of association tracts gives rise to the white substance of the hemispheres.

The bearings of Professor Edinger's question will now be more clearly grasped. The animal kingdom got on for a long while without a cerebral cortex: What capacities are possible to the lower primary centres unaided? What, in the domain of a smell, can a reptile accomplish more than a fish? What, in the domain of vision, can a bird do which a reptile cannot? What new faculties has a mammal associated with the new cortical areas which it possesses? Even in man we require accurate topographical studies of individual brains in relation to individual gifts and faculties. It is evident that in the answers to such questions as these, psychology may attain a far more secure basis than any which has yet been provided for it.

The particular case put by Dr. Edinger is this: There is a general opinion, that fish have some sort of memory, that they can recognize people, know how to find or to avoid places, where they have formerly had some experience; that fish, which have once escaped the rod, know the bait, etc. It is highly desirable that all observations of this kind should be collected in the interest of comparative psychology. The reason is that till now we believed the function of memory to depend on the action of the brain-cortex. All experience in man and in the higher animals has led to that conclusion. But it has in recent years been proved that fish have no brain-cortex at all; they are the only existing vertebrates without one. Now, if we could prove beyond the possibility of doubt, that fish really have a memory, that they gain experience and can make use of it, then we should have to give up the usually accepted opinion that memory has its seat in the brain-cortex. Therefore, it is extremely important to have an entirely new collection of observations. Dr. Edinger begs all, who are in any way interested in fish, and especially anglers and naturalists, kindly to send an account of any observations they may have made to him at 20 Gaertnerweg, Frankfurt a. M. He requests them particularly to take nothing as a well known fact. The smallest observation may prove of the greatest importance.

IV.

Additions to our List of Palæontological Specialists.

IN our December number (vol. ix., p. 361) we published a list of specialists willing, under the conditions there stated, to determine fossils for stratigraphers. Our first duty now is to thank our contemporaries for the favourable terms in which they have spoken of our action, and various correspondents whose commendations we have unfortunately no space to publish. Better, however, than these kind words, is the fact that a slight prejudice which a few had at first against the scheme seems to be disappearing, so that we can now request our readers to add the following names to the former list.

ARTHROPODS—		
Ostracods .. / Phyllopods ..	From any horizon	T. Rupert Jones.
Eurypterids .. / Limuloids .. / Scorpions or other Arachnids.	,, ,, ...	Malcolm Laurie.
Trilobites	Silurian	Philip Lake.
COELENTERATES—		
Graptolites	From any horizon ..	Gertrude L. Elles. / Ethel M. R. Wood.
FORAMINIFERA	,, ,, ...	T. Rupert Jones.
PLANTS	Newer Tertiary and Quaternary	Clement Reid.
,, ...	Palæozoic (if internal structure is preserved) ..	D. H. Scott.

Our readers are also asked to correct the name of one of the genera in which Miss Donald is specially interested, from *Aelisina* to *Aclisina*.

Mr. E. J. Garwood, Secretary to the British Association Committee appointed for the study of life-zones in the British Carboniferous rocks, asks us to call attention to the Report of that committee. This we have already done more than once. We may, however, add that the report contains a list of eleven palæontologists asked to co-operate with the committee "to identify such fossils as may be submitted to them." We did not allude to this list for the following reasons: first, we received authoritative information that some of the naturalists mentioned had declined the proposals of the committee in whole or part; secondly, four of their number definitely refused to allow their names to appear in our list, fearing to be overwhelmed with frivolous applications, a danger we have done our utmost to guard against; thirdly, the names of four of these gentlemen are in our list; lastly, Mr. Garwood's reply only reached us on December 13th. We are still unable, for reasons indicated, to quote this list of Mr. Garwood, but we have pleasure in referring our readers to it, either in the *Geological Magazine*, November, 1896, p. 520, or in the forthcoming *Report* of the British Association.

We should also recommend geologists who do not find their wants provided for in our list, to refer to the annual volume of the Palæontographical Society, where they will find a goodly list of monographs in course of preparation or publication, the authors of which *ought* to be glad of material.

The *Naturalist* does us the honour of making the following criticism:—"We may point out serious omissions in these conditions, and we think it ought to be made clear that in return for the full acknowledgment to be given of the specialists' assistance, the specialist himself should undertake to give a correspondingly full acknowledgment to the sender of the material on every occasion of publishing notes on the specimens, and also that the right of priority in publication is also to be reserved to the sender." With the former condition we fully agree, but as regards the latter we must point out that it would lead to a danger we are doing our best to fight against, namely, the preliminary publication of imperfect descriptions, or even of hosts of *nomina nuda*. Of course we take it for granted that our specialist friends have still some sense of honour left, and will not rob the stratigrapher of all his new facts and conclusions.

SOME NEW BOOKS.

The British Museum Catalogue of Madrepores.

Catalogue of the Madreporarian Corals in the British Museum (Natural History). Vol. ii. The genus Turbinaria. The genus Astræopora. By Henry M. Bernard. Pp. iv., 106, plates xxxiii. Published by order of the Trustees. London, 1896. Price 18s.

Following upon the late Mr. George Brook's monographic catalogue of the genus *Madrepora*, comes the present volume, dealing with two genera of perforate corals, together including a far smaller number of species than the single genus *Madrepora*. Mr. Bernard has proved a worthy successor to Mr. Brook, and in compiling this catalogue has followed with tolerable closeness the lines laid down by his predecessor. The diagnoses of both genera and species have been framed with evident care, while the illustrations are numerous and very good of their kind. The collotype reproductions of photographs, introduced with such success by Mr. Brook, have been used in illustration of the entire specimens and show the habit of the colonies very well. Mr. Bernard, however, has improved upon the method of his predecessor, by introducing a number of lithographs showing details of corallites: these drawings, which illustrate the characters used to diagnose the different species, are of great value, as may well be understood by anybody who has attempted to determine the species of a coral from verbal diagnoses alone.

Every student of corals knows that they are protean in their variety, and that there may often be found, on one and the same colony, calicles of diverse and perplexing forms. This difficulty is emphasized by Mr. Bernard, who gives an account of the variations of each component of a calicle, and shows that there is no single character to be relied upon as a sure mark of specific distinctness. Finally, he complains of the restraint of the Linnean system, and prophesies a time when we shall have recourse to some other conception than that of a species. Such a complaint is scarcely in keeping with the progress of our ideas, for there are few nowadays who regard a species as necessarily fixed and invariable, or as varying only within very narrow limits. All Anthozoa present the most perplexing gradations of character, and the Zoantharia do not afford the same easy means of identification as do the Alcyonaria, for there are no spicules of characteristic and constant form. In point of fact, Mr. Bernard has in his arrangement expressed very well the modern conception of a species. He has selected certain leading features (in this case the habit of growth is most relied upon), and he has arranged his collection in groups about selected individual specimens chosen as types. Each group constitutes a species. The species, as thus arranged, will have a greater or less value according to the skill with which the author chooses his type-specimens, and his instinctive aptitude for associating with them those forms which most truly resemble them. By "most truly" we mean in the greatest number of particulars.

In selecting the habit of growth, that is, the mode of budding, as the basis of his classification, Mr. Bernard has done the best that a man can do who relies on external characters; still, he seems but little satisfied with his choice, and has a decided hankering after the calicular diagnoses of Milne-Edwards and Haime. His method, indeed, leaves something to be desired, as will be shown further on, but it would not be improved by reversion to a method which extended trial has shown to be unsatisfactory. It strikes one that he is not consistent when he says (p. 20) "that the form of the corallum is not altogether independent of the form of the individual calicle, the corallum itself being but an aggregate of individual calicles." He has shown just before that the characters of calicles may vary according to their positions on the corallum, so that the corallum would appear to determine the calicle rather than the calicle the corallum. The growth of the corallum, again, is modified by local conditions, and the growth of the corallum modifies the calicles. In short, corals are extremely sensitive to the influence of environment, and are highly plastic. Hence the difficulties of classification.

In order to account for certain structural features, Mr. Bernard has called into being a new influence, that of the gravitation of the nutrient fluid in the cœnenchyme. He would have it that the nutrient fluid runs downwards, is most abundantly supplied to the lowest portions of the colony, and that excessive growth takes place there, leading to immersion of the calicles. This is not quite a happy conception: the region of most active growth is in the *uppermost* portions of a normal crateriform colony, and where growth is greatest the supply of nutriment must also be greatest. The gravitation theory is quite inapplicable to numerous acrogenous forms of perforate corals, *e.g.*, *Madrepora*, and the phenomena which the theory is invented to explain are better accounted for on other grounds, viz., that in the older parts of the colony such growth as takes place is limited, both in amount and direction; it can only result in the filling up of the cavities or in a thickening of the mass; growth in the latter direction leads to immersion of the calicles.

It may be doubted, too, whether Mr. Bernard has formed sufficiently accurate ideas as to the morphological value of the parts of the corallum. Turbinarians, according to him, are purely cœnenchymatous corals, and he speaks of the skeleton of the polyp as being built up entirely of the septa and their *synapticular* connections. Surely the use of the term "synapticular" is unwarrantable; and the references to cœnenchyme and theca, or the absence of theca, betray a want of precise conceptions respecting these structures. Those who are acquainted with synapticulæ as they occur in the Fungidæ or in *Stephanophyllia formosissima* will not hesitate to deny their homology with the trabecular pieces which unite the septa in a perforate coral such as *Turbinaria*. Compare again Von Koch's account of the development of the skeleton of *Astroides calycularis*, Dr. Fowler's description of the anatomy of *Rhodopsammia parallela*, his description of *Turbinaria mesenterina*, and a section made through any species of *Madrepora*. In *Astroides* and *Rhodopsammia* one speaks of a theca formed by union at many points of the peripheral ends of the septa; the theca is porous, permeated by canals, which communicate with the cœlentera of the polyps. In *Madrepora* and *Turbinaria* the exsert portions of the calicles have a structure to all intents and purposes identical with that of *Rhodopsammia* and *Astroides*. The cœnenchyme is but an extension of the perforated theca; it is permeated in the same manner by a canal system; and the characters of the corallum, as seen in section, are indistinguishable from those of *Rhodopsammia*. In fact, the porose theca has become much extended; buds have been formed from its canals; and these buds have become new polyps, each with its own theca, which has extended in a like manner and given rise to new buds. Cœnenchyme is but a name for the spongy tissue intervening between the *cavities* of the polyps. If each cavity has its own spongy wall, this wall is called a theca, if the spongy walls are fused to form a mass, the mass is called cœnenchyme. The names are hallowed by use; yet they do but signify two phases of one and the same structure. To say, as Mr. Bernard does, that the Turbinarians are *athecalia*, is to reject all those concepts of the nature and development of a "theca" that have been formed as the result of the labours of Von Koch, Fowler, and others.

Such loose conceptions as those here criticised result from too exclusive attention to external features, to the neglect of the study of the evidence afforded by sections. For this Mr. Bernard can hardly be blamed. The system of the British Museum demands diagnoses founded on external characters: considerations of internal structure find no place in its catalogues. But the external characters are the expression of an internal structure; and if the latter is not thoroughly studied and mastered, the external features are apt to be wrongly interpreted. Forms like corals, whose superficial features vary, as has been shown, with every change of circumstance, cannot be truly classified except as the result of a close study of the intimate structure which remains fairly uniform through all those changes impressed upon the colony by the action of external influences.

G. C. Bourne.

The Craft of the Taxidermist.

Artistic and Scientific Taxidermy and Modelling. By Montagu Browne, 22 plates and 11 text figures, 8vo. Pp. xii., 463. London: Adam and Charles Black, 1896. Price £1 1s.

It would seem that brighter days are in store for the long-neglected art of taxidermy. To the present time its existence has depended largely upon the whims and caprices of a small minority of the general public, whose influence on the whole has perhaps been the reverse of beneficial. That the case should have been otherwise was hardly to be expected, so long as the end and aim was no more ambitious than the preservation of trophies of the chase or defunct pets, which were entrusted to men who were as devoid of all artistic sense of form and colour, as they were ignorant of the merest rudiments of anatomy. The last few years, however, have witnessed a decided change for the better. This may be traced to the zeal of private collectors like the late Mr. Booth, of Brighton, and to the emancipation of museums from the fetters of tradition, and their re-organisation on a scientific basis. So long as the museum remained a species of "Old Curiosity Shop," so long was taxidermy doomed to "live desiring without hope."

Mr. Montagu Browne has ever been a champion of the new *régime*, and has given a proof of his earnestness by his contributions to practical taxidermy. His last volume now before us is decidedly his best, aud represents matured thought and experience. As curator of the Leicester Museum, he has ever contended that taxidermy should receive official recognition, and that the museum should be its official home. Who will say him Nay?

In the space of some 400 pages Mr. Browne conducts us through the whole domain of taxidermy and modelling. The history of the origin and progress of taxidermy is compressed into some 18 pages.

Then follow chapters on methods of preservation and modelling compositions, collecting Vertebrata and Invertebrata, the skinning and setting up of mammals, birds, and fishes, etc., by various methods, the modelling of flowers, foliage, etc., and the arrangement of animals in groups in natural surroundings.

These chapters are, on the whole, all that could be desired; here and there, however, we would venture to differ from the author. For instance, we are far from sharing his enthusiasm for bichromate of potash as a preservative fluid. Throughout the book methylated spirit is described as methyl-alcohol, which is, of course, not correct. The formulæ for modelling waxes will be found very useful, but No. 63 might be still further improved by doubling the amount of plaster; the ochre may be omitted at discretion.

The directions for "setting up" mammals by means of wooden models or mannikins will be particularly welcome. It is, however, a matter for real regret that these are not supplemented by a few illustrations from the author's previous works, as well as by one or two after Hornaday. This method is overshadowed in the present volume by a system of "casting from the actual subjects and their reproduction in paper." In the hands of a master of the craft this process may be made to work successfully, but he must be a master, or a very wooden effect is certain to result. Once the model is made, alteration is almost out of the question. We think this latter method should not be attempted, if at all, until after the 'mannikin' has been thoroughly mastered.

No one who has ever tried the system of skinning birds under the wing will ever again revert to the ancient method of removing the body through the breast. The latter system is dismissed in the present volume as utterly bad, whereas in the author's former books, this method received the chief share of attention. Perhaps the tyro would have been better advised if he had been bidden to choose such a bird as a starling or gull, instead of a pigeon, for his first lesson, since from the reviewer's experience, the pigeon is one of the most difficult birds to undertake. It is hard to skin, and hard to mount.

On p. 194 Mr. Browne treats of the method of setting up birds on "scientific principles." The method in question is the same as that adopted for large mammals, namely, the employment of a paper model made from a cast of the body after removal from the skin, which is then to be stretched over it. Experience may gain it a place in our affections, but certainly we are not at all disposed in favour of it. Later, a process is explained by means of which the whole skeleton is retained and made to form a framework for the support of the skin. This, as an object lesson for demonstrating the relation of the bones, and the way in which the skeleton determines the outer form, is admirable, and nothing could be more instructive or helpful to a beginner or one ignorant of anatomy. We commend it to the notice of the professional "bird-stuffer," who, as a rule, is about as ignorant of these matters as can well be. "Many specimens have been set up by this method, which is so easy of accomplishment, and gives such fine results, that in the Leicester Museum, in future, all mammals and birds, when not modelled, will be executed by this process." As Mr. Browne points out, there is nothing new in this; it has been tried and discarded, and as a matter of every-day practice will be rejected now, and it is best so. This method, as well as that of paper models, is a dangerous one, and inimical to the progress of taxidermy. It would scarcely be possible to invent a system which demanded less of the operator. There should be no necessity for such shifts and props if the operator is, as Mr. Browne would have him, an artist, and familiar at least with surface anatomy. What sculptor would think of modelling clay upon a skeleton, when making the preliminary model for some statue?

All will agree with the author in his contention that no "taxidermist ever has mounted, or ever will mount, a fish, amphibian, reptile, or cetacean even decently, or with more than a remote approximation to nature by any of the resources of their art known to them." Consequently, we must resort to plaster-of-Paris models, full details concerning the making of which will be found in this book. We are also offered a choice between reproduction by means of layers of paper, or paper-pulp worked into the plaster mould and lifted out when dry. With both these methods Mr. Browne claims to have achieved great success. Yet a third method—that of models in modelling glue. All these are new, and we await the test of time with some curiosity. Certainly a model in glue of a grass snake which is now exhibited in the Leicester Museum leaves nothing to be desired. Many of the invertebrates, *e.g.*, cephalopods, have been subjected to the glue model test.

The chapter devoted to casting and modelling from natural foliage is very fully and lucidly handled, and will prove invaluable to many.

The concluding pages of the book deal with the formation of "Pictorial groups of Mammals, Birds, etc." Here we are treated to a charming word-picture of a Lincolnshire marsh in midsummer, which, however, jars sadly with the accompanying cold-blooded account of the slaughter of the poor unsuspecting birds who had made this quiet and beautiful spot a temporary home.

In conclusion we may add that the illustrations are for the most part extremely good and to the point. In spite of the one or two drawbacks to which we have drawn attention, the work is, and indeed will long remain, the very best and most authoritative treatise we possess on practical taxidermy.

W.P.P.

Reproduction in Algæ and Fungi.

Die Bedingungen der Fortpflanzung bei einigen Algen und Pilzen. Von Dr. Georg Klebs. 8vo. Pp. xviii., 543, with 3 plates and 15 figs. in text. Jena: G. Fischer, 1896. Price 18 marks.

We have before us the results of very numerous observations and experiments forming the "special portion" of a general work on the physiology of reproduction in lower organisms. The second part, which will appear later, will be based on the results detailed in the first, as well as on facts already known. We heartily commend this plan of work. When a man has filled five hundred pages of text with an account of his own observations, his general conclusions will demand a certain respect and consideration. Whether they ultimately prove to be right or wrong is another question. So far, the author has put before us in a plain straightforward way the conditions which accompany or seem to govern reproduction, both sexual and asexual, in some common species of fresh-water Algæ and Fungi. It is open to anyone who is interested in this most interesting of subjects to repeat and verify or disprove his observations. *Vaucheria*, *Hydrodictyon*, *Protosiphon*, *Botrydium*, *Spirogyra*, *Œdogonium*, *Ulothrix*, *Conferva*, *Chlamydomonas*, and a few others represent the Algæ, *Eurotium repens* and *Mucor racemosus* are the objects of observation among Fungi.

A *resumé* of some of the work on *Vaucheria* will give an idea of the whole. Let us take the asexual reproduction by means of zoospores in *V. repens* and *V. clavata*. The relations to this process of the following factors are considered in order, namely, (1) nutrition, (2) moisture, (3) light, (4) temperature, (5) chemical nature of the environment, (6) oxygen, and (7) currents of water. In the case of nutrition it was found that starvation, by growing in the dark, thereby stopping formation of carbohydrate, after a time prevented zoospore-formation, though growth still continued. Similarly, plants cultivated for a month in distilled water, though exposed to the light, grew slowly, but could not be induced to form zoospores. In the second case the alga was grown for some days in damp air. Under such conditions there arise from the threads which lie on the substratum, numerous branches, which, when surrounded by water, produce zoosporangia at their ends. In damp air the sporangia were never formed. The addition of water acts as a stimulus, and the transition must accordingly be sudden; if gradual there is no result. By exclusion of light, formation of zoospores was induced often after twenty-four hours only. Care must of course be taken that the exclusion of light is not of long enough duration to cause starvation. Experiments with different coloured solutions indicate that shutting off the yellow-red rays is especially favorable to zoospore-formation. Klebs has previously shown that the process in question ceases below 3° C. and above 26° C. He also finds that temperatures between 10° C. and 20° C. are equally favourable to zoospore-formation, so that within these limits temperature ceases to be a factor in any given experiment. Increase of the amount of oxygen in the water in which the plant is growing does not lead to production of zoospores. By transposing individuals from streaming into stagnant water, rapid formation of zoospores was induced; probably as a result of a diminution of the amount of dissolved oxygen. These few remarks give but little idea of the numerous observations occupying 80 pages of the text. For details, and for an account of the remainder, we refer our readers to the book itself, to which physiologists in general, and serious workers at Algæ and Fungi in particular, will, we feel assured, give a hearty welcome.

Names at the Zoo.

List of the Vertebrated Animals now or lately Living in the Gardens of the Zoological Society of London. Ninth edition (by P. L. Sclater). 8vo. Pp. xvi., 724, with 70 woodcuts. London: Printed for the Society, 1896. Price 4s. 6d.

The last edition of this valuable list appeared no less than thirteen years ago, so that the handsome new volume contains the records of twelve years (1883–95), arranged, as usual, in systematic order. The names of species recorded in the previous lists are also added, with references, so that the present work is practically an index to all the species of vertebrated animals received alive by the Zoological Society of London during the past thirty-four years. As regards the names used, Dr. Sclater has endeavoured to make as few changes as possible, "not being one of those advocates of priority who seek to carry out that useful principle without reference to grammar or common sense." Nevertheless, so far as the new series of British Museum Catalogues has advanced, he has adopted the various "revisions" of nomenclature without any question. This does not seem to us quite consistent—merely to use "revised terms" where they are ready to hand and need no seeking—but it at any rate displays great good sense to follow implicitly some standard work of reference, when such exists and is universally accessible. It is, perhaps, too much to expect that the publications of our National Museum shall set a universal and permanent fashion in zoological nomenclature, but some consummation of this kind is devoutly to be wished.

New Serials.

The *Monatschrift für Psychiatrie und Neurologie*, edited by Professor Wernicke, of Breslau, and Professor Ziehen, of Jena, and published at Berlin, began with the present year.

We learn from the *Daily News* that the Imperial Natural History Society of St. Petersburg intends to publish a flora, first of European Russia, and afterwards of Russia in Asia and the Caucasus.

Nature announces, though without stating the price or place of publication, the appearance of a new German weekly journal, devoted to pure and applied science, to literature and to art, *Die Umschau*, edited by Dr. J. H. Bechhold. The first number contains, among other articles, one on ethnology by Dr. Max Buchner.

Messrs. Schleicher Frères, of Paris, announce "L'Année Biologique : Comptes rendus annuels des travaux de Biologie Générale," published under the direction of Professor Yves Delage, with the assistance of a committee, to whom Dr. G. Poirault is secretary. The first volume deals with the literature of 1895, and is in large 8vo. We have not yet seen a copy.

Middlesex Hospital is starting a journal of its own.

Scraps from Serials.

The *American Journal of Science* for January prints a letter by Dr. Webb, of St. Augustine, Florida, stating that the body of an immense octopus, 18 feet in length and 10 feet in breadth, has been washed ashore near that city. The arms no longer remain. Mr. A. E. Verrill, commenting on this, suggests a weight of four to five tons for the body and head, and states that the dimensions are larger than those of any of the well-authenticated Newfoundland specimens. He thinks that it may be a species of *Architeuthis*.

Mr. J. Collinson, of Wolsingham, Co. Durham, a well-known crusader against cruelty to animals, writes to the *Farmer's Gazette* for January 2, praising the refusal of the British Produce Supply Association to trade in larks, which are valuable friends to the gardener and farmer. The lapwing, too, as he quotes from the *Gardening World*, although of priceless value to agriculturalists on account of the vermin it destroys, is subject to ruthless persecution in spring by those who want its eggs for varnish or for food, in autumn, "by people who foolishly imagine they are eating something of a dignified and gamey nature."

The *Zoologist* for January is no longer edited by Mr. J. E. Harting, its editor of the last twenty years, who has been forced to retire in consequence of the pressure of official duties and other engagements. A worthy successor has been found for him in Mr. W. L. Distant.

The *Westminster Review* for January contains a curious article by Dr. Olindo Malagodi on the Psychology of Anarchist Conspiracies, which is of a reassuring nature, inasmuch as he contends that Anarchists are dreamers rather than fanatics. "It is the hyperæsthesia of their personality which forces them into their conflict with society. . . . Anarchists will continue to live in a small way on small deeds." We do not quite know why this *Review* criticises the Smithsonian Institution for its manner of publication. It is not correct to say that "in order to obtain its Reports application must be made to members of Congress." No *bonâ fide* worker who applies direct to the secretary is likely to meet with refusal.

With its January number the *Photogram* enlarges its monthly issue to 32 pp., and introduces improvements in paper and printing. The present number contains an article by John Mills on spectrum photography for beginners; but even when the *Photogram* contains nothing that comes within the scope of NATURAL SCIENCE, it remains of interest and value.

The *Engineering News* for November 10, contains an account by John S. Denis of the photographic methods used in the Canadian Topographic Survey. This method is especially valuable in broken and difficult country.

Some results of the expedition organised in 1891 by Professor F. W. Putnam to the ancient city of Copan in northern Honduras, are given by C. C. Willoughby, of the Peabody Museum at Harvard, in the *Scientific American* for December 26, 1896. The article is illustrated with an admirable photograph of the amphitheatre with the temples in the background. The age of these massive and magnificent ruins cannot be certainly decided; they are, says Mr. Willoughby, unquestionably prehistoric, and the builders of this city belonged to the same civilisation as the constructors of the temples and pyramids of Yucatan (see NATURAL SCIENCE, vol. viii., p. 159, March, 1896).

We have received the Final Report of the British Association Committee on the Marine Zoology, Botany, and Geology of the Irish Sea. It consists for the most part of a list of the species recorded from that area. The Committee do not regard their work as finished, but feel that it will in future be carried on at Port Erin by the Liverpool Marine Biology Committee. We do not know whether this report is really published or not. It bears no imprint or date other than "Section D.—Liverpool, 1896," and no price or publisher's name is marked on it. Neither does it contain any reference to the Report of the British Association, of which the volume for 1896 is not yet issued. We should like to ask Professor Herdman, who is responsible for this Report, whether he considers such conduct to be consistent with the resolutions unanimously passed by the British Association Committee on Zoological Bibliography and Publication, of which we see that he is a member.

We have received the *Journal* of the Institute of Jamaica for July 1896, which was issued on November 10th. It contains the following contributions to the fauna and flora of Jamaica:—Dragonflies, by G. H. Carpenter; Lizards, Scorpions and Myriapods, by J. E. Duerden, and the Melastomaceæ by W. Fawcett. We also find notes by H. L. Clark on the life-history of *Synapta vivipara*, a holothurian concerning which very little has hitherto been known; on the marine zoology of Kingston Harbour by J. E. Duerden, who also contributes an interesting note on phases in Jamaican natural history, dealing with the remarkable change in the fauna that took place after the introduction to the island of the mongoose in 1872. Originally brought to destroy the rats, themselves introduced by man, the mongoose also destroyed the majority of the snakes, lizards, turtles, and birds of the island, thus leading to an excess of many injurious insects, so that in 1882 a very bad state of affairs was chronicled. Now, however, it appears that in all parts of the island the mongoose is not so plentiful as formerly, and is beginning to be attacked by those very ticks which it has permitted to multiply. The balance of nature is therefore being restored. The lizards, snakes, and birds are reappearing, and "the maximum influence both for good and for evil, of *Herpestes mungo*, is passing away in Jamaica."

Boletim do Museu Paraense contains in the 4th part of the first volume, a striking plate of the water-plant *Mourera fluviatilis*, one of the Podostemaceæ, illustrating a contribution to the botanical geography of Guiana, by Dr. Jacques Huber. Herein also F. Katzer announces the discovery by Dr. João Coelho of Silurian rocks, largely composed of sponge-spicules and containing graptolites, in the Maecuru valley. Such fossils had not previously been found in Brazil.

The *Cheltenham Examiner* has lately been publishing a series of articles on Cheltenham as a health resort, in which we are as much surprised as pleased to find one entitled "The Great Divide," a term which the writer here applies to the water-shed between the Severn and the Thames. He proceeds in thoroughly scientific, no less than popular fashion, to show how this has gradually moved backwards from the west, so that the Cheltenham area, instead of draining into the Severn, formerly supplied the Thames. The Chelt now flows westward, but there is evidence of a former river flowing along the same valley, but in an easterly direction, draining the westerly extension of the Cotteswolds, since worn away.

Cedric Chivers' "New Book List," the first volume of which has just closed, is a useful idea well carried out. But it will not be of much help to scientific men till someone of more technical knowledge is placed on its staff. A zoologist, looking at the subject index of no. 12, would infer that no zoological works had been published during December last; and yet the list itself contains Newton's "Dictionary of Birds, Part IV.," Cunningham's "Natural History of the marketable marine Fishes of the British Isles," Grant's "Handbook to the Game Birds, Vol. II.," "The Cambridge Natural History, Vol. II.," and five or six others that ought to have been referred to under zoology. So also Bailey's "Survival of the Unlike" is not alluded to under botany.

Other Literature Received.

Fabrica Oris Dipterorum, H. J. Hansen: Copenhagen, Reitzel. Die Leitfossilien, E. Koken: Leipzig, Tauchnitz. Bibliography of more important Contributions to American Economic Entomology, pt. v., S. Henshaw: U. S. Dept. Agric. Traité de Zoologie Concrète, Y. Delage and E. Hérouard, vol. i. La Cellule et les Protozoaires: Paris, Schleicher. An Account of the Crustacea of Norway, vol. ii,, G. O. Sars: Bergen Mus. Catalogue of African Plants collected by Dr. F. Welwitsch in 1853–61, pt. i, W. P. Hiern: Brit. Mus. (Nat. Hist.) The Survival of the Unlike, L. H. Bailey: New York, Macmillan. A Dictionary of Birds, pt. iv., A. Newton: Black.

Undescribed Shrew of Genus *Sorex*, C. F. Batchelder; Rev. Squirrels of N. America; Skunks of Genus *Mephitis* of E.N. America; New Mammals from Indian Territory and Missouri, O. Bangs; *Romerolagus nelsoni*, n.g. and sp. of Rabbit, C. H. Merriam; *Proc. Biol. Soc. Washington.* Entwickelung d. sogen. Lungen bei den Arachniden, A. Jaworowski: *Zeitschr. f. wiss. Zoologie.* Entwickelung des Spinnapparates bei Trochosa &c., A. Jaworowski: *Zeitschr. f. Naturwiss.* Instruction in Natural History in the Jardin des Plantes, Paris, C. Earle: *Science.* On the Bio-Geography of Mexico. &c., T. Townsend: *Trans. Texas Ac. Sci.* N. American Oligochæta, F. Smith: *Bull. Illinois State Lab. Nat. Hist.* Human Relics in Drift of Ohio, E. W. Claypole: *Amer. Geol.* On the Teeth of Mazodus; On the Structure of some Palæozoic Spines from Ohio, E. W. Claypole: *Proc. Amer. Micr. Soc.* Insects affecting the Cotton Plant, L. O. Howard: *Bull. U.S. Dept. Agric.*

Nature, December 17, 24, 31, January 7, 14. Nature Notes, Jan. Science Gossip, Dec., Jan. Naturalist, December, January. Westminster Review, January. Victorian Nat., Nov. Knowledge, January, Photogram, January. Irish Naturalist, January. Scott. Geogr. Magazine, January. Revue Scientifique, December 12, 19, 26, January 2, 9, 16. Feuille des jeunes Naturalistes, January. L'Anthropologie, November–December. Naturen, January. Naturæ Novitates, Nov. (22), Dec. (24). Literary Digest, December 12, 19, 26, January 2, 9. American Journ. Science, January. Science, December 11, 18, 25, January 1, 8. Amer. Nat., December. Amer. Geol., December. Botanical Gazette, December. Illinois Wesleyan Mag., October, November. Psychological Review, January. De Gids, January.

OBITUARY.

EMIL DU BOIS-REYMOND.

BORN NOVEMBER 7, 1818. DIED DECEMBER 26, 1896.

IN Professor du Bois-Reymond science has lost not only a great physiologist, but the veritable founder of one of the most important branches of physiology. Though not a German by descent he was born in Berlin, lived there nearly all his life, and was absolutely identified with its University and Academy of Sciences.

His father was originally a watchmaker in Neufchâtel, but had moved to Berlin, where he obtained a very good position. His mother was of Huguenot extraction. He began his education at the French College in Berlin, and passed thence to the College of Neufchâtel. But at eighteen he matriculated at the University of Berlin in the philosophical faculty, and after a year settled down to those studies which led up to his life's work—his medical curriculum lasting from 1837 to 1843. Johannes Müller—the grandfather of modern physiology, if such men as Ludwig and Du Bois-Reymond may be regarded as its fathers—then occupied the chair of physiology at Berlin, and to his influence some of the greatness of Du Bois-Reymond's future career may justly be ascribed. Matteucci's essay on the electrical phenomena of animals appeared in 1840, and at Müller's suggestion, Du Bois-Reymond undertook to repeat the observations which Matteucci had made. Thus began the work to which his life was afterwards devoted, and he chose the subject "Quæ apud veteres de piscibus electricis exstant argumenta" for his thesis for the degree of Doctor in Medicine. He became Assistant at the Anatomical Institute in 1844, and Privat-docent in 1846. The electrical manifestations of muscle and nerve, to the investigation of which he now applied himself with untiring ardour, constituted an unworked field, and Du Bois-Reymond was well fitted to explore it. His energy was unbounded, his mechanical skill as an experimenter unrivalled, and he displayed the greatest ingenuity in devising the requisite apparatus for his researches. To this day the Du Bois-Reymond induction coil and key and the non-polarizable electrodes which he invented are amongst the earliest apparatus with which the student of the physiology of muscle and nerve is confronted on commencing laboratory work. His discoveries soon brought him into a prominent position among physiological workers, and the publication of his "Researches on Animal Electricity" in 1848 and 1849 established his reputation. He strongly advocated the view that the phenomena of animal life were explicable on physical and chemical principles. In 1858, having already been Extraordinary Professor for some two years, he succeeded his great master as Professor of Physiology in the Berlin University, and held that post till his death —a potent influence for good upon all those who came under his influence, and the founder of an important school of physiology. In 1867 he was appointed Honorary Secretary of the Berlin Academy of Sciences, and this post he very worthily occupied, for he was no mere specialist in his own subject, but a man of wide culture and attainments, as his writings on other subjects testify. The magnificent Physiological Institute, which was built in Berlin after the war with France and which is one of the most complete and perfect in existence, was planned by Du Bois-Reymond, and here it was his satisfaction to witness the accomplishment of much admirable work by the younger men, Baumann, Fritsch, Kronecker, Goltz and others, who followed in his footsteps.

In person, he was of a medium height, but of strong frame, and with piercing eyes and a full beard. He possessed remarkable powers as an orator, and was master of a clear and lucid style in lecturing. Not only were his physiological lectures highly appreciated by students, but the weekly popular lectures on general subjects which he used to deliver at the University attracted very large audiences. As Secretary to the Academy of Sciences he delivered orations on the occasion of the death of any of the members, which will long be remembered. He was a man of the utmost frankness and sincerity and always stated his convictions in clear and emphatic language: though taking no direct part in politics, he had the reputation of being a Radical. Among his general writings may be mentioned the celebrated "Essay on the Limits of Natural Knowledge," essays on Physiological Teaching, on University Organisation, and on Civilisation and Natural Science. He wrote also on Leibnitz and on Voltaire in their relation to Natural Science.

In the domain of physiology Du Bois-Reymond remains for all time as the great founder of our knowledge of the electrical phenomena of muscle and nerve. As the direct result of this he played a foremost part in breaking up the vitalist doctrines which had previously held sway in physiology. Of all the bodily functions, muscular and nervous energy were held to be the most clearly "vital" in their nature, and when it was demonstrated that they were associated with and conditioned by definite and measurable physical and chemical changes, vitalism, or at least the old vitalism of our forefathers, received its death-blow. Du Bois-Reymond, indeed, would have nothing to do with the modern revivals of vitalism which have been propounded in recent years by Rindfleisch and others, though at the same time, as his essays shew, he by no means took up the narrow position that every problem must be regarded only from the physical and experimental side. His greatness as a physiologist depended very largely on his exquisite skill and ingenuity as an experimenter. Some of his theories and views have been challenged, notably by Hermann, and it is pretty generally admitted that his "currents of rests" are rather to be explained as "currents of injury." Nevertheless, the great bulk of the new facts which he discovered in his own special sphere remains not only uncontroverted but confirmed by subsequent experimentation, and if some of the theories which he built on them have required modification, this can in no way detract from the greatness of his work as a pioneer in the field. In Du Bois-Reymond has passed away a true father of modern physiology, and one whose influence will be felt so long as the science shall endure.

DAVID ROBERTSON.

Born (Nov. 28, O.S.), December 10, 1806. Died November 20, 1896.

WHEN David Robertson was three weeks old he lost his father. His mother was left with three little children and no external resources. At the age of eight the 'wee Davie' became a cowherd. At twenty-four, having had experience of many and varied employments, he took up the fancy that, instead of being a farm labourer, he might as well be a medical man. He was on the point of accomplishing this heroic resolve, when, just as he was ready to take his degree, his course was again diverted. He became a tradesman, a merchant, a man of substance, and eventually, at about the age of fifty-four, he relinquished the lucrative paths of commerce in favour of an ever-growing and engrossing devotion to the study of natural history. Thenceforward the observation of nature, with a special leaning to aquatic zoology and botany, became the business and the pleasure of his life. His researches were carried on in the most unselfish spirit. The keynote of his work was helpfulness. He might be diffident and over-anxious in publishing papers of his own, though these were numerous and excellent, but he was ever eager, confident, and enthusiastic when encouraging others, when showing hospitality to men of science, when searching for needed specimens, or when discovering and making known some 'happy hunting ground.' Fortunately, his wife, who survives him, was in full sympathy with his tastes and pursuits, and not only a sympathetic companion but a zealous and highly capable fellow-worker in the same field of science.

During the latter years of his prolonged and honoured life, Robertson's heart was much set on securing the permanence of the Cumbrae Station for marine studies. In a letter to the present writer, dated December 3, 1895, he says:—

"You will, I am sure, be pleased to hear that the prospects of the

Biological Station at Millport are continuing to improve. Dr. Reid, whom you saw at Fern Bank, has lately contributed £500 towards a new stone building for the Station. I do not remember whether I told you that the visitors to the Station last summer were over 6,000! and there have been a great many workers at the Station during early spring and the summer months, who all give excellent testimonials expressing their opinion that no better situation than Millport could be selected for a biological station. The purity of the water and the varied character of the bottom of the neighbouring sea, the richness of animal and vegetable life, the absence of strong currents, and well sheltered bays, make boating safe and pleasant, and where dredging is concerned it is more expeditious and agreeable, and not the least [advantage is] the large fleet of boats, and there is no place in the United Kingdom where the hire is so cheap—three pence per hour for a boat with oars.

"I have been reading the life of Darwin, by his son. He speaks often of his blessed pigeons and his pet pangenesis, and I fear my discourses will come to have an uncontrollable tendency to drift on to the Biological Station of Millport."

This summer he had the satisfaction of turning the first sod for the erection of the new building. In its completion and prosperous employment for the furtherance of science, Robertson will have a memorial more to his own liking than a monument in Westminster Abbey.

Besides being a Fellow of the Linnean Society and a Fellow of the Geological Societies of Glasgow and London, he received distinctions that were none of his own seeking, and that took his modest nature by surprise. At fifty-nine he found himself an honorary Fellow of the Imperial Royal Zoologico-Botanical Society of Vienna. The Natural History Society of Glasgow chose him for its President when he was eighty-one. At eighty-eight he was made an honorary D. C. L. of the University of Glasgow—a fit and graceful crowning in his native city, among friends who knew and loved him, of his memorable career.

T. R. R. S.

THOMAS GWYN EMPEY ELGER, whose excellent handbook, "The Moon," we reviewed in August, 1895 (vol. vii., p. 138), died at Bedford on January 9th, at the age of 59. Besides being an astronomer of repute, he was the chief authority on Bedfordshire archæology, and his collection of implements, from palæolithic to Norman times, found in the gravels of the Ouse Valley, was one of value. He was intimately connected with the Library and the Literary and Scientific Institute of Bedford, also with the Archæological and Natural History Societies of the county. In his profession of civil engineer, Mr. Elger had laid down railways in Denmark, and between King's Cross and Edgware Road, London, and had been engaged in many similar works of importance.

JOHANN AUGUSTE STRENG, the celebrated German mineralogist and chemist, died at Giessen, on January 9, aged 67. He was assistant to Bunsen at Breslau, and in 1853 became teacher of Chemistry at Clausthal. From 1867 till 1895 he occupied the Chair of Mineralogy in Giessen University. He made microscopical investigations of rocks from Minnesota and worked on rocks from Harz and the Nahe, also, among other things, on felspars, and series of phosphates.

We regret to record the deaths of:—On November 4, Lieutenant E. D. YOUNG, R.N., commander of the Livingstone Search Expedition, and discoverer of the Livingstone range of mountains, aged 55; VIVIEN DE ST. MARTIN, renowned for his researches in ancient geography, at Paris, aged 95; DR. STRAUSS, Professor of Pathology at the Paris Medical School, noted for his writings on tuberculosis and cholera, aged 51; on October 6, in Passy, the entomologist Professor A. HÉNON, aged 74; Captain LUCAND, of Autun, an esteemed mycologist; on December 17, at Munich, Dr. JOSEPH V. GERLACH, formerly Professor of Anatomy at Erlangen, aged 76; on October 7, F. BENSELER, Inspector at Vienna University Botanical Garden, aged 65; Dr. LUIGI CALORI, Professor of Anatomy in Bologna University; Dr. R. ZOJA, who has done some work in experimental embryology, with his younger brother Alfonso, on September 26, at Nigazzo, by an accident; Dr. P. BINET, Deputy Professor of Therapeutics in Geneva University, and author of many works on the action of drugs, aged 41; Dr. LEONARD J. SANFORD, formerly Professor of Anatomy and Physiology in the Medical Department of Yale University, who died at Newhaven, on December 12, 1896, aged 64; F. HAZSLINSZKY, mycologist, described as the Nestor of Hungarian botanists, at Eperies, on November 19; on December 12, at Copenhagen, Professor ASGER STADFELDT, aged 66; F. SACCARDO, of Avellino, an authority on vine diseases, and a writer on lichenology, on October 6, aged 27; on October 15, in St. Petersburg, the Director of the Botanical Garden, Professor A. BATALIN; BARON N. DELLINGSHAUSEN, naturalist, at Riga; Dr. F. MORAWITZ, founder of the Entomological Society of St. Petersburg; Dr. MODEST GALANIN, editor of the St. Petersburg *Journal of Public Hygiene*; HORATIO HALE, the anthropologist, of Canada, on December 29; Dr. W. H. ROSS, formerly Professor of Anatomy in Mobile Medical College, Alabama.

Naturæ Novitates announces the deaths of: H. J. POSSELT, Conchologist at the Zoological Museum, Copenhagen, on July 20; A. NEGRI, Privatdocent in palæontology and geology at Padua, on December 11; S. SCHOLZ-ROGOZINSKI, African explorer, in Paris, aged 35.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

At the Smithsonian Institution F. W. True succeeds the late G. Brown Goode as Assistant Secretary and Director of the National Museum, while Richard Rathbun becomes Assistant in Charge, in place of the late W. C. Winlock. E. Büchner, to be Director of the Zoological Museum of St. Petersburg Academy of Sciences, in place of T. D. Pleske, resigned; Augusta Årnbäck, to be Amanuensis to the Professor of Zoology at Stockholm University; Professor M. J. Elrod, of Illinois Wesleyan University, to be Professor of Biology in the Montana State University; Clarence W. Perley, to be Assistant in Biology at the Massachusetts Institute of Technology; W. E. Castle, to be Instructor in Biology in Knox College, Galesburg, Ill.; Miss Knight, to be Professor of Anatomy and Pathology to the Lhudiana Medical School, India; G. Hedren, to be Amanuensis at the Polyclinic of Upsala University; J. H. Burkell, late Assistant Curator of the Herbarium, Cambridge University, to be an Assistant in the Kew Herbarium; A. Pestalozzi, to be Assistant in the Zürich Botanical Museum; Ernest Clark, Secretary of the Royal Agricultural Society, to be first Gilbey Lecturer in the History and Economics of Agriculture, Cambridge: Dr. Edler, of Göttingen, to be Associate Professor of Agriculture, at Jena; Professor Blass, to be full Professor of Geology at Innsbrück; Dr. Szadeczky, to be Associate Professor of Geology at Klausenberg; F. B. Peck, of Amherst College, Mass., to be Associate Professor of Geology and Palæontology at Lafayette College, Easton, Pa. During the absence of Professor H. O. Lindgren, of Lund, on parliamentary duties, Professor C. M. Fürst will lecture in Anatomy, and Dr. N. E. Wadstein on anatomy and histology; during Professor S. A. B. Lundgren's absence from the same university, the teaching in geology will be conducted by Dr. J. C. Moberg.

The only New Year's honour in which the British public has shown much interest has been the raising to the peerage of Sir Joseph Lister, who will, we are glad to learn, be still known under his own famous name, as Lord Lister.

The Royal Order of the Prussian Crown of the second class has been given by the Emperor of Germany to Dr. Roux, whose co-discoverer of the antidiphtheritic serum, Dr. Behring, has been invested with the Grand Order of the Crown of Italy. Other honours are the gifts by the Czar of the Cross of the Commander of the Order of St. Anne to Mr. Gérard, Director of the Paris Municipal Laboratory; the Cross of St. Stanislas, to Dr. Bordas, Sub-Director; and the same to Dr. Bertillon, Director of the Anthropometric Service.

We learn that Professor Otto M. Torell, head of the Swedish Geological Survey, is seeking retirement on a pension.

The Walsingham Gold Medal of Cambridge University has been awarded to W. McDougall, of St. John's College and St. Thomas's Hospital, for original researches in physiology.

R. W. T. Günther has been elected to an Official Fellowship at Magdalen College, Oxford, as tutor in natural science.

THE late Dr. H. F. Antells left to the University of Helsingfors the sum of £40,000, the income from which will be applied to travelling scholarships and scientific expeditions.

ALFRED NOBEL, the discoverer of dynamite, has, after making a few personal bequests, left to Science as his residuary legatee, a fortune of some £2,000,000. We give a translation of the passage in his will relating to this :—" This is set apart as a prize for those who, during each preceding year, have done the greatest service to mankind. The income is to be divided into five equal parts as follows :—a part to him who made the most important discovery or invention in physics ; a part to him who made the most important chemical discovery or improvement ; a part to him who made the most important discovery in the domains of physiology or medicine; a part to him who produced the most remarkable work in the field of imaginative literature ; and a part to him who worked most or best in uniting the nations and in doing away with or diminishing standing armies, as well as in founding or spreading peace congresses. The prizes for physics and chemistry are to be awarded by the Swedish Academy of Science ; for physiological or medical works by the Carolinska Institut of Stockholm ; for literature by the Academy of Stockholm ; and for the pioneers of peace by the Norwegian parliament. It is my express will that in the award of the prizes no regard shall be paid to nationality, so that the most worthy shall receive the prize, whether he is a Scandinavian or not." To criticise this more than princely gift is ungracious, but we cannot help thinking that prizes of this nature do not encourage honest and profitable work so much as do the endowment of institutions or the foundation of research-scholarsh ps.

BARON HIRSCH bequeathed 2,000,000 francs to the Pasteur Institute, which will be enlarged by chemical and biological laboratories.

THE Sedgwick Memorial Museum of Geology at Cambridge is to be built on he ground lately belonging to Downing College. We understand that the plans (published in NATURAL SCIENCE, vol. iii., p. 451, Dec., 1893) will, in consequence, have to be altered.

A NEW departure has been made this winter at the Science and Art Museum, Dublin, in a series of Museum demonstrations, undertaken by members of the staff and other helpers. Two demonstrations a week have been given through December and January, natural history alternating with art subjects. The difficulty of exhibiting small museum-specimens to a large audience led to a restriction of the number of tickets issued for each occasion to thirty, or at most fifty. It is satisfactory to record that there was a large demand for tickets, and that the audiences seemed thoroughly interested with the explanations of the objects.

ON January 11 a gas explosion in the Public Buildings at Penzance considerably shattered the Geological Museum. Dunipace House, near Larbert, the residence of Mr. J. A. Harvie Brown, has been destroyed by fire, including the museum, which contained one of the finest private collections of stuffed birds, skins, and eggs in the country.

THE Report of the now municipal Museum of Bristol, presented in December, 1896, shows that the repair of the premises has been continued, and that a number of the cases on the ground floor have been renovated and their contents better displayed. In this way the anthropological department has been set in order, to the gratification of the public ; the rock collection has been rearranged and newly labelled. A nearly-perfect skeleton of the moa, presented by Mrs. Tuckett, has been set up in the geological department. We have already noticed the students' geological collection of about 1,000 named specimens that has been fitted up. An herbarium of European plants, with accompanying literature, has been presented by Mr. W. G.

Smith. The attendance during two years was 160,000, of whom no less than 4,225 visited the museum on Whit Monday, 1896. In the library an important move ha been the collection in a separate room of all books and pamphlets connected with Bristol, their number now reaching 2,129. It is also satisfactory to learn that the excellent penny guide to the museum by Mr. E. Wilson meets with a ready sale. We have, indeed, already received a copy of the seventh edition, issued September 1896. It has a view of the museum as frontispiece.

The East Wing of the American Museum of Natural History at New York was opened on November 30, and excellent illustrations are contained in *Science* of December 11. With the West Wing now in course of erection, the museum will have a total floor-space of 148,258 square feet. The twenty-seventh annual report states that a deficit of nearly £1,600 was met by special subscription from the trustees. The collections have been largely augmented from the various expeditions sent out. Two new halls have been opened, one of Ethnology and the other of Vertebrate Palæontology. The collection in the latter, up to the summit of the Oligocene, must be indeed unique if it contains, as asserted, with few exceptions, more or less complete remains of every genus and species known. The south side is occupied by the Perissodactyla, Rhinoceroses, and horses; the north by the Mesozoic animals, the meseutherian Amblypoda and Creodonta, the Tillodontia, Rodentia, Insectivora, and Carnivora. In the ethnological department the material is placed in geographical order; where the collections are sufficiently complete the following subjects are illustrated: Physical types, relation of man to nature, industries, dress, trade, social organisation, and religion.

The annual *Report* of the Auckland Institute and Museum for 1895-6 shows that the revenue of the museum is likely to be increased, through some of the Crown lands from which its income is derived having been taken up under mining leases. The museum appears to suffer from the want of a taxidermist, which prevented its acceptance of such important specimens as an adult leathery turtle and a fine example of the true sun-fish, *Lampris luna*. The Ethnology Hall has received from the Chief Hami Te Waewae the gift of a carved post or "tiki" from Ota Kanini Pa, Kaipara, a fortress well known in ancient Maori history. It will be remembered that some time ago Little Barrier Island was set aside as a preserve for the rarer members of the New Zealand fauna. The *Report* states that there are persistent rumours as to the slaughtering of the birds for sale, and in consequence of this all Maoris have been removed from the island.

A Botanical Institute in connection with the Garden has been established at Münster, Westphalia.

The Castle Garden Aquarium at New York was opened on December 10, with a collection of about 100 species of fish, which will be increased by importations.

American botanists have taken up very eagerly the suggestion made by Professor D. T. MacDougal that a tropical laboratory for botanical work should be founded by them. The situation proposed is either the East Coast of Mexico or an island near the Caribbean Sea.

The Belgian Government has instituted, in the International Exhibition at Brussels in 1897, a Scientific Section. This will be devoted to Physics and Mathematics and the Natural Sciences, with a department for Bibliography, a subject much cultivated by our Belgian colleagues at present. The president of the section will be General J. de Tilley, while Mr. Eugene van Overloop will act as

Commissary for the Government; their secretaries will be Messrs. G. Gilson and E. van den Broeck. Mr. A. Houzeau de Lehaie will be president of the Geological and Geographical Section; J. B. Carnoy of the Biological; Dr. E. Houzé of the Anthropological; and Dr. F. van der Haeghen of the Bibliographical Section. A large number of prizes are offered for best collections and answers to certain questions, full particulars of which can be obtained by writing to one of the secretaries.

THE "Allgemeinen Gartenbau-Austellung" at Hamburg will hold an independent scientific exhibition between May 28, 1897, and the end of September. There will be shown: (1) Diseases of cultivated plants due to mechanical, atmospheric, or soil conditions; (2) animal and vegetable enemies of plants; (3) ditto, friends of plants; (4) variations and abnormalities in plants; (5) comparative manurings of pot-plants; (6) wild ancestors of cultivated plants; (7) living, exotic, useful plants in pots; (8) a selection of the same preserved; (9) selection of plants and plant-structures arranged on morphological and biological lines; (10) the results of scientific pollinisation experiments; (11) scientific aids to gardening. The programme will be sent to interested applicants by Dr. R. Kraepelin, Naturhist. Museum, Hamburg.

THE Committee of the International Geographical Congress, held in London in 1895, has recently sent to the various geographical societies resolutions urging the importance and desirability of: (1) Antarctic exploration; (2) a geographical bibliography, compiled by various states; (3) a topographical survey of Africa; (4) a map of the earth on a scale of 1 : 1,000,000, with the meridian of Greenwich and metric measurements; (5) the continuance of physical investigations lately made in the Baltic, North Sea, and North Atlantic; (6) an international system of seismographic stations; (7) agreement between the various geographical societies as to the spelling of foreign names; (8) the printing on all geographical maps henceforward the date of their publication. Further, they request the opinion of the societies as to the application of the decimal system to the measurement of times and of angles.

THE Geological Society of London has made its awards for this year as follows:—Wollaston Medal, W. H. Hudleston; Murchison Medal and part of fund, H. B. Woodward; Lyell Medal and part of fund, G. J. Hinde; Bigsby Medal, Clement Reid; Wollaston fund, F. A. Bather; balance of Murchison fund, S. S. Buckman; balance of Lyell fund, Joseph Lomas and W. J. Lewis Abbott.

THE Surveyors' Institution is building itself a new home in Great George Street, Westminster, from plans by Mr. A. Waterhouse. The second floor of the building is devoted to the museum of forestry, which is being greatly elaborated by the Institution.

WE learn from the *Revue Scientifique*, and why we should be left to learn it from this we do not quite know, that a mycological society is being formed in England under the presidency of Mr. G. Massee. Excursions of a week's duration will be taken each year, and the first, beginning on the third Monday in September, 1897, will be to the Sherwood Forest.

THE naturalists of Manchester are turning their attention to interesting points in the history of their societies. The Literary and Philosophical Society has published as complete a list as possible of its members since its foundation more than a century ago. The President of the Geological Society of the same city (Mr. Mark Stirrup), now sends us a copy of his address on the early history of that body. Next to the Yorkshire Geological and Polytechnic Society, this is the oldest provincial geological society in England, having been founded in 1838. During its long career it has not only continued to be a flourishing organisation for the promotion of research, both in pure science and in the application of geology to mining, but it has also contributed very largely to the fine collection of geological specimens in the Manchester Museum, now in the Owens College, of which it provided the nucleus many years ago.

MR. R. L. JACK sends us an unpaged (? unpublished) extract from vol. xii., *Proc. Roy. Soc. Queensland*, announcing the discovery by Mr. J. Coghlan, of two undoubted specimens of *Orthoceras* in the Cairns Range of Western Queensland. This appears to prove the extension of Ordovician rocks from S. Australia. Hitherto the oldest fossiliferous rocks known in Queensland have been the Burdekin Beds of Middle Devonian age.

WE understand that important results have been obtained by Mr. J. E. S. Moore, who has been investigating the fauna of Lake Tanganyika. His collections seem to show that it cannot be very long, geologically speaking, since this body of water, now fresh, formed a part of the sea.

CAPTAIN BOTTEGO has reached Lake Rudolf, and is on his way back to Mombasa. Mr. A. H. Neumann, who has returned to England also from Lake Rudolf, followed the eastern shore into the Randile country. He will present some of his spoils to the Brisish Museum (Nat. Hist.).

G. KOLB, a Bavarian explorer, has, during the past two years, made two journeys in the country between Kilimanjaro and Kenia in Africa. On his way to the latter he passed the Gunga Lake, situated in a crater and surrounded by steep walls; though 6,500 feet above sea-level, it is inhabited by hippopotami. Attempting the ascent of Kenia from that side, he found a large oval plain, thinly covered with grass; at one end was a glacier brook, round which the temperature dropped at night to 9·5° Fahr.

THE following details about French explorers are gleaned from *L'Anthropologie.* Mr. Clozel, Administrator of the French Possessions on the Ivory Coast, is endeavouring to make valuable ethnographic and geological collections. Important results are expected from two such enthusiastic explorers as Messrs. Bonnel de Mézières and de Béhagle, who are starting for Central Africa. Mr. Bonin has returned to Tonkin from the south-western provinces of China, whence he brings much material and many facts of ethnographical and anthropological nature. On their way from Turkestan to Siberia, Mr. Chaffanjon and his party have gathered large collections of the fauna and flora, and accumulated much information regarding ethnography and geography. In Siberia, too, Baron de Baye has been carrying on his archæological and ethnographical studies. Mr. E. Blanc, who has been to Nijni-Novgorod, is bringing back rich scientific collections. Mr. Raoul, official Colonial chemist, is starting on Government business for Borneo, where he hopes to carry on scientific studies. The Hourst expedition, whose return was noted in our last, has proved the navigability of the Niger from Bammako to the sea.

REUTER states that Messrs. Oloufsen and Philipsen, two Danish officers, have returned from portions of the Pamirs hitherto unexplored by Europeans with rich collections for the Natural History Museum, Copenhagen, over 300 photographs of places and racial types, and valuable meteorological observations, some from 14,000 feet above sea-level. Some of the tribes met with were of diminutive stature, their domestic animals being small in proportion; they are fire-worshippers, have no money, and buy wives for five or six cows, or fifteen sheep, apiece.

THE University of Washington at Seattle sent a natural history expedition, consisting of H. H. Hindshaw, Mrs. Hindshaw, and T. C. D. Kincaid to the Snake River, and its results are given in the *American Naturalist* for December. The sandhills of Douglas Co. are, in part, covered with a flow of basaltic lava, and form the water-bearing rock of Eastern Washington, known as the John Day bed. This contains many mammalian bones, large collections of which, as well as of insects and Indian relics were obtained, and are now placed in the museum of the University.

THE *Daily Chronicle* of January 18 announces that on January 14, Zurbriggen, the famous Swiss guide attached to Fitzgerald's Andes expedition, reached the summit of Aconcagua, over 24,000 feet above sea-level. Fitzgerald reached the arête at 23,000 feet.

According to the *Daily Chronicle*, preparations for the Austro-German Antarctic Expedition have begun. Julius von Payer is taking a leading part. A preparatory expedition will start from Melbourne next summer with the object of wintering at Victoria Land, and making preliminary investigations. Dr. Meves (Stuttgart) is entrusted with the geological and geographical researches, and Dr. Schöner with those relating to botany and zoology. The final expedition is to be fitted out with two ships, which will sail as far as the meridian of Kerguelen Islands. One is to winter there to serve as a refuge for the crew if the principal ship founders. The latter is to advance as far as the Antarctic magnetic pole, which has hitherto been fixed only by means of a calculation.

KING LEOPOLD of Belgium offers a prize of £1,000 for the best essay sent in before the 1st of July next on the meteorological, hydrological, and geological conditions of Equatorial Africa, from a sanitary point of view. The chief points to be discussed are the mode of life, work, food, clothing, dwelling, most favourable to health, the conditions affecting the life of Europeans in the Congo basin, and tropical diseases and their treatment.

MR. ALFRED SHARPE, the Acting Commissioner in British Central Africa, has issued regulations with regard to the new game preserve on the Shire River. A written permission must be obtained before shooting or trapping animals within a prescribed area, licences may be refused, and breaches of the regulations punished.

ON Monday, December 28, a large bog near Rathmore, about twelve miles from Killarney, situated on a table-land about 776 feet above sea-level, burst its bonds, and flowed, in the form of black, liquid mud, over a large part of the surrounding country, destroying several habitations and lives. The course followed that of the Ownacree river. The escape of the bog had no connection with any earthquake shock, but seems to have been due to recent heavy rains, which loosened the lower layers of the bog, and caused rifts in the upper layers.

WITH reference to the plague now raging in Bombay, the *Bombay Gazette* states that the medical officer, Mr. Hankin, ascribes its diffusion chiefly to rats and ants. Infected rats deposit the disease-germs on floors over which they pass. When they die their bodies are eaten by ants, which absorb the bacilli and deposit them in cracks, and especially near water. It is therefore not enough to remove patients from their homes: it is the healthy that should be removed and carefully segregated.

FROM a letter in the *Shooting Times* of January 9 we learn that anthrax has broken out among the elks in the forest of Ibenhorst, near the Kurisches Haff, in the north of Germany. This, says the correspondent, R. Zeitler, is a heavy blow for the stock of elk in Germany, since about forty elks perished in Lithuania last spring in consequence of the high water and of frost. More than usual, also, were shot in the autumn. To preserve at least a small stock in East Prussia, it has been proposed that no elk should be killed during the next two years, that in future no cow or calf should be shot at any time, and that the male elk should be shot only during a four week's season.

CORRESPONDENCE.

"A Plague o' both your Houses!"

Under this appropriate [?] heading Natural Science (vol. x., p. 4) makes the statement:—"Our need still is for facts . . . we would suggest that the present is not the time for speculation on the merits or failings of Natural Selection. . . . A truce to library papers for ten or twenty years! . . . Perhaps, at the end of the suggested period, it may be possible to formulate important results based on experimental evidence, say, for instance, whether variation is indefinite or definite."

As you say that you were "urged to the above remarks" by my paper:—"Does Natural Selection play any part in the Origin of Species among Plants," perhaps I may venture to say that I *have* done, and that, too, for *twenty* years, precisely what you now advise me to begin to do! As you say "no new experiments were advanced" by me, I can only presume that you have not done me the honour to read my book on "The Origin of Plant Structures," in which plenty of experiments are recorded.

I would also like to observe that it is no question of "speculation" or of pitting one *theory* against another, for the conviction that *definite adaptation* is all-sufficient to account for the origin of species, is forced upon me by the accumulation of positive facts, which speak for themselves. No *theorising* is required at all. The origin of species by means of natural selection is still, as I pointed out, an unverified *deduction*; whereas adaptation by definite variations is proved, not only by a vast accumulation of parallel series of facts, *i.e.*, by *induction*, but has been verified abundantly by experiments. If this be not sufficient to convince Darwinians, I venture to think that nothing ever will. Conversely, until they can prove the universal existence of *indefinite variations*, The Origin of Species by Means of Natural Selection must remain a baseless hypothesis.

George Henslow.

[We have not only read but reviewed Mr. Henslow's book (vol. viii., p. 201), nay, more, we recommended it. We implied in the Note of which Mr. Henslow complains, that in his paper at the Linnean he advanced no new experiments. If he did adduce any fresh experiments conducted by himself since February, 1895, we will admit our error, and shall be pleased to publish the details of them.—Ed., Nat. Sci.]

Natural Science for March, 1896, alluded to Dr. J. D. F. Gilchrist's method of killing *Aplysia* in an expanded condition, by cocaine. In May, 1896, Mr. H. Hanna wrote to us that a 1 per cent. solution of chromic acid was "simply perfect" for the purpose. Dr. Gilchrist now writes to us from Cape Town as follows:—"I have, at Naples and elsewhere, repeatedly used a 1 or 2 per cent. solution of chromic acid—the method at present in use at Naples—and, after many trials with this and other methods, have come to the conclusion that the method I recommend is the most satisfactory."

NOTICE.

To Contributors.—*All communications to be addressed to the* Editor *of* Natural Science, *at* 22 St. Andrew Street, Holborn Circus, London, E.C. *Correspondence and notes intended for any particular month should be sent in not later than the* 10*th of the preceding month.*

To the Trade.—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.*

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NOTES AND COMMENTS.

Franz Josef Land and its Explorers.

In reply to our note entitled "The Little Trip of the Fram," Mr. Montefiore Brice has favoured us with a lengthy and characteristic epistle, to the details of which we reply elsewhere. We are, however, sure that Mr. Brice would wish us to give equal prominence with our original note to a disclaimer of any wish to attack his friend Mr. Jackson, either for the work he is doing, or for sentences in his private correspondence. The person we felt called on to criticise was Mr. A. Montefiore Brice. According to Mr. Jackson's letter, the true position, by latitude and longitude, of Nansen's winter hut was as far from Nansen's idea of its position as Paris is from Prague. What was Mr. Brice's object in publishing that paragraph? Many people thought he meant to insinuate that Nansen's latitudes and longitudes could not be relied on. And we objected to discredit being cast on Nansen by quotations from the private letters of "a man who is," as Mr. Brice insists, "still at work in the Arctic Regions, and unable to reply for himself" to the questions that are being asked.

No amount of effusive laudation in other parts of Mr. Brice's paper could prevent this damaging inference; and while we are only too pleased to learn from him that this inevitable conclusion was incorrect, we cannot exonerate the Council of the Royal Geographical Society from publishing these incompletely proved statements, which might so easily have been laid hold of by would-be slanderers of Nansen. Preliminary notices of this kind are as objectionable in Geography as they are in Zoology.

Mr. Brice quotes the president of the Geographical Society as saying that "the value of such a survey" as Mr. Jackson's "can hardly be exaggerated." But Sir Clements, it will be noted, used the future tense; and when at some future time we have Mr. Jackson's results properly put before us, we shall be better able to appraise their precise value. For the present, we need only compare them with the magnificent opportunity offered him for obtaining information of great geographical value. What geographers wanted to know about Franz Josef Land, was its extension to the north and to the east, and whether Wilczek Land was a small island or the western edge of a great land-area. This was the main problem that we hoped to see solved by the expedition so generously equipped and supported by Mr. Harmsworth. But the moment Nansen's map was flashed on to the screen at the Albert Hall, it was clear that it was he who had solved the problem, showing Wilczek Land to be a small island, and the Dove Glacier non-existent. This he has done as a parergon, in the course of doing what most people consider a good deal more than "only extending our knowledge a few degrees in one direction." To place the revision of local topographical details and of their nomenclature by the side of this, surely is (*pace* Sir Clements Markham) gross exaggeration.

As for Mr. Brice's facile sneers at anonymous attacks, we may point out that anonymity does not carry with it irresponsibility; the Editors of this Review accept the fullest responsibility for these editorial Notes, and communications will always find them at the address given on the wrapper and last page of each number.

The Learned Society Bore.

In the good old days when the various branches of natural history were less specialised than now, and when everyone worthy of the name of naturalist could take an intelligent interest in the whole subject, the meetings of societies were a real and distinct incentive to progress. Members could appeciate most of the discussions, and they returned to their own special studies with suggestions from the results of kindred lines of research. Now, however, the whole condition of affairs has changed. Modern work in the various departments of biology and geology is so highly technical and special, that it is safe to say in most subjects not more than a score of investigators in the whole world can appreciate every bearing of each new contribution. Catholic societies, like the Royal Society, and the similarly named societies of Edinburgh and Dublin, the Literary and Philosophical Societies of Manchester, Liverpool, and Birmingham, and the Linnean Society of London, include among even their most distinguished members many who have not the faintest idea of the meaning and methods of the researches of many of their equally eminent fellow-members. No mind can now cover the whole field beyond the general elementary principles.

Under these changed circumstances, there are those who ask whether the meetings of such societies are any longer of value, beyond the casual conversations in the tea-room and friendly consultations in the library. Authors, indeed, are coming to care little for the meetings that they bore, and regard the societies merely as publishers less mean than most. When a specialist addresses an audience of which not more than one or two members can appreciate a single sentence, is it not a waste of precious time for the others, and would they not be advancing science more effectually by spending a quiet hour instead in their own studies? The answer that would be given by many Fellows of the Linnean Society of London may be inferred from the way in which their meeting-room has been emptied by abstruse verbosity on more than one occasion of late.

On a recent occasion of this kind an author succeeded in practically emptying the room, leaving scarcely any Fellows beyond the officers and four members of council who felt it their duty to remain. The "reading" began abruptly with some thanks to botanists who had assisted the author. No reference whatever was made to the precise bearing of the new contribution on previous research in the same direction. No attempt was made to explain, to the Fellows who were not specialists on the group dealt with, the nature of the material, and yet no specialists were present. As the author proceeded, he picked out a casual sentence at irregular intervals from his systematic diagnoses, often not finishing the reading of the same even so far as the punctuation-mark; and the monotony of this procedure was only interrupted by an occasional rummage through a pile of dried specimens dumped down in absolute chaos on the table. Some of these the author explained, in a lucid interval, had been lent by the Director of Kew Gardens. We only hope that by some strange accident they escaped the pulverisation which to an onlooker seemed inevitable. On the board was arranged a series of diagrams, without the faintest indication as to what they represented, and the author only made a confused and incoherent reference to a small proportion of them. A map, duly exhibited to illustrate the distribution of the species, was not even referred to.

We have devoted so much space to this circumstantial recital, not because the instance was an isolated one calling for censure, but because the frequency of such performances requires that some vigorous and emphatic protest should be made, if our societies are to retain their old prestige and influence in promoting the communion of workers one with another. Nor have we mentioned the Linnean Society because matters are worse there than elsewhere; but surely if a paper is worthy of presenting to any society with such honorable traditions, the mode of presentation to the meeting is worth half-an-hour's preliminary consideration and arrangement. An author might at least have his illustrations in correct order for reference; he might explain the object of his communication and how it bears on the progress of that department of science to which it relates. He might deign to give his fellow-naturalists in other fields an outline of his methods that would be at least intelligible to them. If he is not prepared to do this, let him give his communication immediate burial in some special magazine which none but the specialists closely concerned are likely to see. The most technical contribution can, we maintain, be made the subject of an intelligent brief address, if only its author will take a little pains; and we emphatically assert that it is the duty of the officers of all our Societies to prevent such disrespectful treatment of the Fellows or Members in meeting assembled, as that to which we have referred.

SIR HENRY HOWORTH.

A RECENT lively discussion in the House of Commons had at least one result of importance to scientific men: that philosophical and judicial critic, Mr. T. Healy, decided that Sir H. H. Howorth, though not much of a dynamiter, was "a distinguished geologist." This induced us to reconsider a few of the geological writings of our occasional correspondent. Two papers especially, recently published in the *Geological Magazine*, had not yet been otherwise disposed of. These discuss the Chalky Boulder-Clay of Eastern England, and the sands and gravels associated therewith, and their author absolutely repudiates the idea that any of these deposits are at all the result of ice-action. Such an opinion is unpopular with English geologists, and it is possible that scant notice will be taken of Sir Henry's summary of the objections to the orthodox explanation. Now, Sir Henry Howorth's geological work may be divided into two parts, one analytic and critical; the other constructive. The former attacks the theory of those whom he calls the "ultra-glacialists"; the latter enunciates a rival theory of his own. As a destructive critic Sir Henry is at his best; and his criticisms would appear to be so unanswerable that glacial geologists prefer to ignore them, and simply to ridicule the alternative theory that he proposes. Unfortunately for these folk, the papers before us contain no allusion to catastrophic deluges, but only a summary of objections to the view that in Pleistocene times a great ice-sheet deluged Eastern England and deposited the Chalky Boulder-Clay as a glacial moraine. There are remarks in these, as in other writings of our erudite humorist, that had been better omitted, for they weaken his case by exaggeration, and rouse opposition by over-emphasised heresy. But these clear statements of fact appear to us fatal to the ice-sheet theory: they show it to be inconsistent with the distribution and composition of the supposed glacial beds, and with the form and nature of the included pebbles and rock-fragments. Moreover, the extracts quoted from the writings of those who support the theory show that their classification of the East Anglian drifts is based on arbitrary distinctions. These papers by Sir Henry Howorth should be widely read, and ought to clear away much extravagant speculation.

THE ORGANISATION OF SCIENCE IN THE UNITED STATES.

Two important articles dealing with this subject have recently appeared in *Science*. The first, which is an editorial, appeared in the number for January 8, and urges a step that has been somewhat seriously considered for at least twenty years, namely, the severance of the National Museum from the Smithsonian Institution, or at least the appointment of a director of the museum, who shall be other than the Assistant-Secretary of the Institution. The Smithsonian Institution, founded for "the increase and diffusion of knowledge among men," is not so much an American as a world institution, endowed by an Englishman for whom the Government of the United States acts as executor. The museum, on the other hand, as its name implies, is a national affair of the United States, having no logical connection with the Smithsonian, and constituting no organic part of it. While the Museum is now well established, and can readily be made directly responsible to Congress, the Smithsonian is hampered in its work by the necessity of applying to the Government for appropriations, and by the burden of administrative duties, which were never contemplated in its original scheme. A more personal reason is that a man like George Brown Goode, capable of serving both as a museum officer, and as Assistant-Secretary, and ultimately Secretary, of the great organisation, is not so easily found. The present situation, therefore, occurring as it does at the close of fifty years of the Institution's life, affords an excellent opportunity for reconsidering the relations of the Institution and the Museum.

The second article to which we would draw attention is by the Assistant-Secretary of Agriculture, Chas. W. Dabney, Jr.; it was prepared at the suggestion of the Hon. Gardiner G. Hubbard, and is in *Science* for January 15. It urges the formation of a national department of science, and points out how enormously the official scientific work of the United States is at present duplicated; for instance, "there are four hydrographic offices in as many departments, viz., the Hydrographic Office of the Navy, a similar office in the Coast Survey of the Treasury Department, the Division of Hydrography in the Geological Survey (Interior Department), which measures the rivers of the arid regions, and the Weather Bureau, which measures rivers and studies lake-currents." "In addition, the Fish Commission measures the waters of fishing-grounds and rivers, the Engineer Corps of the Army measures the Mississippi River, the lakes and harbors." A complete list of the scientific agencies is given in the article. "The only way" says Mr. Dabney, "to avoid duplication and waste of time and money, and to secure the proper coordination and cooperation, is to first bring all these bureaus together in one of the existing departments, or in a new department. When these bureaus have been thus brought together under the direction of one Secretary or executive head, the reorganisation will be comparatively easy. It should take place naturally and gradually in the course of ordinary business." "The policy should be to transfer the different scientific bureaus or surveys to one department, as opportunity offers, or as the Secretaries now having charge of them find it expedient to recommend it."

The great danger that all scientific institutions of this kind in America have to fight against, is the meddling of politicians. A real advance was made not long ago, when all those employed in the scientific bureaus were put in the classified service, corresponding to the Civil Service of Britain, and were thus no longer changed with the change of governments. Still there is always room for the enterprising Congressman, and there may be some to think that the National Museum is better off when responsible to an enlightened body like the Smithsonian Institution, than it would be when under the thumb of unsympathetic politicians. A great scientific department, too, would bring with it the inevitable red-tape; and it is doubtful whether the production of scientific work would be facilitated by a subdivision of the department according to means rather than ends, which is what Dr. Dabney seems to suggest. No one but a cheese-paring politician or a Treasury clerk would wish to see more offices like those of the Government Printers, the Stationery Office, or the Office of Works.

Geographical Distribution in America.

The boundaries of the life-areas of southern North America are discussed by Mr. C. H. Tyler Townsend in a paper "On the Bio-Geography of Mexico, Texas, New Mexico, and Arizona," lately contributed to the Texas Academy of Science. Mr. Townsend endeavours to lay down more precisely the limits of the regions and sub-regions marked out by Dr. Merriam. For this purpose he uses the distribution of plants as well as of animals. The Transition and even the Boreal Zone can be traced southwards in the higher mountain-ranges into Texas and Mexico, and at a lower level Upper Sonoran elements in the fauna and flora were noticed. The Neotropical belt is believed by Mr. Townsend to stretch considerably farther to the north along the west coast of Mexico than along the east; and he attributes this to the cold northerly winds which prevail at certain seasons on the Gulf coast, but never on the Pacific shore.

At the end of his paper Mr. Townsend gives a "Synopsis of the Life Divisions of America." He recognises five Regions, which he terms Boreal, Neotemperate, Neotropical, Austrotemperate, and South Boreal, the "Austrotemperate" including Chili and the Pampas, while the "South Boreal" is a circumpolar region to which Patagonia is transferred. What countries of the Eastern Hemisphere are included in it we are not told. This attempt to raise Patagonia, even with New Zealand perhaps thrown in, to the rank of a zoological region comparable to the Neotropical will hardly commend itself to students of the distribution, especially when the new region is saddled with so peculiar a name as "South (!) Boreal." And those naturalists who are inclined to follow Dr. Merriam in considering southern North America as worthy of regional rank will prefer the established name Sonoran to such a barbarous hybrid as "Neotemperate." We regret that Dr.

Merriam himself should have proposed "Austral" to supersede "Sonoran." To a resident in New York or Washington, "Austral" may be an appropriate term enough to indicate the region in question; but we owe so much to the American students of geographical distribution that we regret the more they do not seem to remember that their terminology should be appropriate for world-wide use.

A Remarkable Rodent.

In 1894, Mr. E. W. Nelson obtained from Mount Popocatepetl in Mexico, at and above a height of 3,000 metres, specimens of a small, short-eared, tailless rodent, which lives in the large saccaton grass there in considerable numbers, having habits and coloration like those of the common meadow mice of the mountains, with whose runways its own are intermingled. This rodent has lately been described by Dr. C. Hart Merriam under the name of *Romerolagus nelsoni* (*Proc. Biol. Soc. Washington*, x., pp. 169–174, Dec. 29, 1896), and has been referred by him to the Leporidæ. Its progression, however, is on all fours, in which respect, as well as in some important morphological features, it departs from the true leporine type. Although some Oriental species, of which Dr. Merriam is possibly not aware, show similar lack of agreement with the hitherto received diagnosis of the Leporidæ, yet, were it not for the high authority of Dr. Merriam, we should incline to place this Popocatepetl rodent with the Lagomyidæ. The following characters are admittedly those of that family: complete clavicles, short hind legs, absence of an external tail, and short ears. The skull, it is true, is said to be "much as in *Lepus* (subgenus *Sylvilagus*)." Nevertheless it departs from the leporine type in the depression of the brain-case, the posterior elongation of the jugals, the small size and absence anteriorly of the supraorbital processes — all, as before, characters in which it approaches the skull of Lagomyidæ. But, in the regrettable absence of information as to the dentition, specialists must for the present content themselves with recognising the form as one of unusual interest.

To the name given we have three distinct objections. First, we seem to recollect that the catalogue of the Mexican exhibit at the Chicago Exhibition contained the reproduction of a photograph of this same rodent, with the name "Lepus diazi, sp. nov," assigned to it by Ferrari-Perez. This has unfortunately been overlooked by Dr. Merriam. Next, we cannot approve of this combination of a modern personal name with a Greek noun: words like *Romerolagus*, *Leedsichthys*, *Agassizocrinus*, *Lichtensteinipicus*, *Uroskinnera*, and *Cookilaria* are the scorn of the cultured and the laughing-stock of the vulgar. Lastly, if this "new and remarkable" animal really has "an interest quite apart from that which attaches to most new discoveries," is it not a pity that the names proposed for it should be of an interest so limited and purely personal?

Palæontological Peccadilloes.

We are glad to find that American scientific men are beginning to protest against the curious methods of some of their fellow-countrymen. In the October number of the *Illinois Wesleyan Magazine*, Professor M. J. Elrod protests against some of the palæontological work published by the Indiana Geological Survey. "Diligent search," says he, "has failed to reveal a single palæontological specimen as belonging to the state of Indiana. The collectors have the pleasure of gathering them, attach their own names or have them attached, write descriptions, and the state foots the bill. So far as one is able to observe from reading these reports, no means whatever are taken to preserve these outside of private collections, and the hundreds of fossils described may at any time be carted out of the state, sold, or bartered away, and no one be the wiser. Yet we complain because legislators are not more liberal towards these scientific surveys! Evidently there are two sides to the question. We cannot wonder at the astonishment of our friends across the water at such proceedings. The wonder is that they have not long since been stopped."

Professor Elrod also protests against the absurd employment as specific names of the names of obscure individuals and villages. "The bugbear of science," says he, "is its array of names, often meaningless. When proper names are used they are still more meaningless." Certainly, when we find Professor Gorby's appended to thirteen different species in one volume, together with such monstrosities as "Kokomoensis" and "Boonevillensis," it is time, as Professor Elrod says, "to call a halt." As he points out, it is not only the name-givers that are to blame, but also the eponymous individuals whose "not objecting is generally taken as meaning that such a course is acceptable."

Strata as diagnostic of Species.

There was once a man named William Smith who wrote a book called "Strata Identified by Organised Fossils." Hence he was known to his contemporaries as "Strata Smith," and to his successors as "the father of British geology." Since his day the principles laid down by him in England, and independently and almost synchronously by Cuvier and Alex. Brongniart in France, have been applied in greater and greater detail, so that there are now workers who, like Lapworth and Buckman, work out the succession of the fossils inch by inch through the stratified rocks, and find certain species characteristic of definite horizons over large areas. This minuteness of detail is achieved not merely by careful work in the field, but by accurate discrimination of species in the study. Lesser features are now recognised as diagnostic, and two forms that an older generation would have lumped together are now recognised, if not as distinct species, at least as successive mutations of a species. This separation of mutations is akin to the detailed systematic work of the modern zoologist, who finds himself able to recognise geographical races and physiological varieties. But we must beware of reversing the process. One antelope is not to be separated from another merely because it lives in another part of the country; nor are two bees to bear different names only because they have a fancy for different flowers. Similarly one ammonite must not be labelled with a new name merely because it was found two inches or even two yards above one previously known. We know perfectly well that some forms of life can be subjected to varying conditions, or can live through many ages without becoming modified in structure. Moreover, to regard a parallel of latitude, or a particular stratum, as in itself a diagnostic character of a species, would be to upset the methods of stratigraphical geology, and the conclusions based on a study of geographical distribution. It would be an obvious logical fallacy, akin to putting the cart before the horse.

We have been induced to put these well-worn truths very clearly, because we have recently stumbled on two instances of the opposite, or shall we say the topsy-turvy, method. One instance was the peculiar remark of Dr. Wheelton Hind, criticised by our reviewer on p. 386 of vol. ix. To this author, however, we have no wish to allude further. It is more serious when we find a solid worker and acute writer like Mr. G. Dollfus, patronising the same heresy. In the *Revue Critique de Paléozoologie*, i, p. 40, when discussing the "Monograph of the Foraminifera of the Crag" by T. Rupert Jones and others, he writes: "Si la Paléontologie conserve sous un même nom spécifique des formes de tous les étages et de tous les pays, elle perd toute sa valeur . . . . elle crée pour les foraminifères une survie extraordinaire, toute spéciale, qui est une surprise et une anomalie dans le règne animal." Now Mr. Dollfus may believe that the Crag species of Foraminifera do differ in form or structure from their ancestors and descendants. But Messrs. Jones, Burrows, Sherborn, Millett, Holland, and Chapman believe they do not; while on the one hand they hold that local and temporary dwarfing is readily produced by lowering of temperature or diminution of food-supply, on the other hand, they believe that the Foraminifera are a group the majority of whose representatives have been subject to precisely similar conditions from very early times; they believe that they have an enormous range, and in that fact they see nothing extraordinary. Holding these views, are they really to give new names to their specimens merely because they come from the Crag and not from the London Clay? If Mr. Dollfus does not mean this, he has not expressed himself with the lucidity of his nation. If he does mean it, we can only suppose that he is a "special creation" man, a pre-Darwinian, and anti-Lamarckian, who has come out of the eighteenth century on the Time-machine of Mr. H. G. Wells, and doesn't know how to reverse its action.

The Meaning of a Species.

In the *Botanical Gazette* for December, 1896 (vol. xxii., pp. 454-462), Professor L. H. Bailey has a suggestive article on "The Philosophy of Species-making." Discarding Cuvier's conception of a species as "the reunion of individuals descended from one another, or from common parents, or from such as resemble them as closely as they resemble each other," no less than the older conception of a species as a unit of creation, he attempts to show that a species is a purely subjective idea, and consequently defines it as: "The unit in classification, designating an assemblage of organisms which, in the judgment of any writer, is so marked and so homogeneous that it can be conveniently spoken of as one thing."

This way of regarding a species involves inevitable consequences. First, extreme splitting: "I look with favour upon the tendency to make specific names for forms which have heretofore been regarded as well marked varieties." Second, arbitrariness: "Species are judgments," said Asa Gray; "I would not for a moment make it a test of a species that there should be no intergradient forms," says his pupil. Third, the exaltation of the species-spotter, the name-lover, the museum mind: "Species-division will be useful in proportion as it is founded upon obvious and easily ascertained attributes"; "it is not necessary, or even desirable, that we should search for obscure or anatomical characters with which to separate them. These characters belong to anatomy, physiology, embryology, and the like, not to taxonomy." No "physiological species" for Professor Bailey!

We quote this article because the views of a clever and experienced man on such a subject are always of interest. But we cannot agree to this separation of taxonomy from the other branches of biology, nor can we hold that expediency is everything. The last time we quoted an American botanist at length we were accused by an eminent though anonymous geologist of "endorsing" pernicious opinions, insulting to our supporters. Professor Bailey's views would perhaps please our critic; we must therefore explain that we do not endorse them.

A Pine and its Fungus.

In the same number of the *Botanical Gazette*, Mr. B. T. Galloway describes the action of a rust-fungus occurring on the leaves of the scrub or Jersey Pine (*Pinus Virginiana*) and causing the plant to cast its leaves. This result is known to follow the attacks of a number of fungi, and in this particular case Mr. Galloway has made an interesting series of observations on the course of events in the leaves affected. During the winter the leaves of the pines, in common with other evergreens, change colour, the dark green giving place to orange as the season advances. With the approach of spring the normal colour is resumed, growing brighter as conditions become more favourable. In early May those affected by the fungus (a species of *Coleosporium*) may be recognised at some little distance by their pale yellow leaves and general thinness of growth. The colour is due to the effects of the fungus, which is confined almost entirely to or near the tips of the needles formed the previous season. The thinness of growth is owing to the lack of leaves, all except those of the previous year's growth having fallen prematurely.

The writer finds that the fungus is propagated by sporidia which appear on diseased leaves at the same time as the new leaves. The sporidia develop only in wet weather, and are washed or drop from those on which they are borne on to the young needles just showing their tips. The latter show no signs of infection for two or three months, and it requires twelve months to complete the development of the fungus. During a large part of this time it does not seriously interfere with the functions of its host-leaf. As soon, however, as it ruptures the tissues, loss of water is increased to about one-fifth above normal; the reserve water in the cells is gradually used up, loss of turgidity, and other physiological changes follow, resulting in the gradual death and casting of the leaves.

Experiments on the rate of loss of water from diseased and healthy parts of the same leaf show that, before the fungus has ruptured the cortical tissue, transpiration is less from the infected areas. This is found to be due to the permanent closing of the stomata, and may result in keeping the diseased parts alive longer than the healthy when a leaf or branch is removed from the tree.

Drunk and Incapable.

In a recent number of the *Journal of Botany*, J. L. Williams describes various observations made in North Wales on the intoxication of humble-bees by the honey of certain composite plants, and the relation of their consequent movements to the pollination of the flower. The bees were seen greedily sucking the honey from heads of flowers of *Scabiosa* and *Centaurea*, after which they were incapable of flying away, and rolled helplessly about in the flower-head, or fell to the ground. On recovering from their debauch, after a few minutes they immediately flew to another plant of the same species and began afresh. During their struggles the bees became covered with pollen, and on visiting a second plant were very effectual agents in impregnating its flowers with pollen from the one previously visited. Mr. Williams suggests that this method of cross-pollination may in time become normal. Hitherto its occurrence has been uncertain, and was only observed a second time after an interval of five years, when the writer was unable to obtain sufficient honey for analysis, so that the cause of intoxication is unknown. Whether or no advantages may accrue to the plant from thus 'drugging' the nectar which it offers to insect visitors, the comparative frequency of such effects cannot be denied. Nor are they confined to members of families like *Compositæ* and *Dipsaceæ*, where the flowers are crowded into a head, and where alone there seems any chance of profit to the plant. On a warm sunny afternoon humble-bees may be found in large numbers drunk and incapable on tobacco plants; and we remember once counting as many as thirteen on one plant in the Botanic Garden at Cambridge.

East African Lepidoptera.

Dr. W. J. Holland has lately issued (*Proc. U.S. Nat. Mus.*, vol. xviii., pp. 741–767) a list of Lepidoptera collected by Messrs. Chanler and von Höhnel in the neighbourhood of Mount Kenia. While some of the species have a wide range over the whole Ethiopian Region, a much larger proportion show South African affinities, though there is no special relationship with the fauna of West Africa. So far as regards Lepidoptera, Dr. Holland is inclined to unite the South and East African sub-regions suggested for birds by Dr. Bowdler Sharpe (Natural Science, August, 1893.)

The Great Carcinologist of Norway.

To praise too much is invidious; in appreciating the carcinological work of G. O. Sars, it would also be difficult. In the amazing abundance and variety of his work, the clearness and accuracy of his descriptions, the dexterity and fulness of the accompanying illustrations, whoever may compete with him on one head or another, in all combined it may safely be said that he stands unrivalled. Within the tolerably well-marked confines of the crustacean group there is room for so much diversity of form that men who are recognised as specialists in regard to one division may be almost absolutely ignorant in regard to others. There is probably not a single one on which Professor Sars could not speak with authority; there are very few which he has not already handled with a master's touch. Though the Cumacea have in a sense been known for a century and a quarter, and even brought under the light of science more than half a century ago by the sagacious Kröyer, still it is to Sars that we owe the chief expansion of this still limited and curious order, and the clear understanding of its morphology. What he has done with pen and pencil would fully account for the time of an ordinary man, but Sars seeks his specimens not only in the cabinets of museums, but also in the depths and shallows of the ocean itself, securing thereby the advantage of seeing the creatures in the postures and colours of life. In many instances he has successfully studied the remarkable changes of form which some crustaceans undergo between leaving the egg and attaining to maturity. Not content with searching the fresh waters of his own country, we find him further engaged at home in raising Crustacea from the dried mud of other lands, and by this means able to comment on the life-history, and give figures and descriptions, of many interesting Entomostraca from Australia, Algeria, New Zealand, and the Cape of Good Hope. While still occupied with the first volume (1890–1895) of his "Crustacea of Norway," comprising the Amphipoda, a standard work by itself sufficient to establish a solid reputation, he boldly undertook for the Imperial Academy of Sciences of St. Petersburg another large book on the Crustacea of the Caspian Sea (1893–1896).

Now is commenced the second volume of the "Crustacea of Norway," the first two parts consisting of 40 pages and 16 plates. These are concerned with the Apseudidæ and Tanaidæ, and of their value no better indication can be given than the incidental remark of a learned English colleague, who writes, "I have had the first number of Sars' Isopoda, and shall now try to work up the Tanaidæ I have on hand." Be it remembered that some of the species are less than a tenth or than a twentieth of an inch long, and then it will be understood why even experts gladly wait to see limbs and mouth-organs boldly and clearly figured by Sars before beginning to sort and name their own collections.

Just to keep up our reputation as destructive critics, we may point out that Professor Sars' keenness of vision for minute details enables or persuades him sometimes to distinguish genera and species which eyes more dim or more careless would willingly have done without. While giving almost everything else that could be desired, he whittles down synonymy nearly to the vanishing point. Doubtless this is owing in part to considerations of space, and in part to a humane desire to leave something, however humble, for others to do. One other feature may, by some of our most constant readers, be thought a more glaring fault than the rest. Those whose palates long for the acid of sharp criticism must go elsewhere than to Sars' writings, for his works will probably be searched in vain for one biting word about his fellow-workers.

It is highly honourable to the Bergen Museum to have undertaken the responsibility of issuing the "Crustacea of Norway," finding, as its notice says, "that, in the interest of zoological science, the further publication of this most valuable work ought not to be delayed." It is a national work, in the sense of being an honour to the nation which produces it, but Norwegian Crustacea are far from being confined to Norway, and, were it otherwise, the treatment of them by Sars would still make his volumes indispensable to every student of the group. This and all the important works of his later years have been given to the world in very satisfactory English. It would be pleasant to know what England has done for him, how many of our learned societies have enrolled him as a foreign member, and, of the millions of men and women here who are enthusiastic over the exploits of his brother-in-law, Nansen, how many individual persons have ever heard the name or given a thought to the labours of the brilliant and indefatigable naturalist, Georg Ossian Sars.

AN ANCIENT ARACHNID.

A WELCOME contribution to our knowledge of fossil Arachnida is a short "Note on Eophrynus," by Messrs. F. T. Howard and T. H. Thomas, published last year in the *Transactions* of the Cardiff Naturalists' Society (vol. xxviii.). The specimen forming the subject of the paper, and identified as *Eophrynus carbonis*, Woodw., was discovered in a coal-pit at Pentre in the Rhondda Valley. Unfortunately it consists merely of the lower side of the abdomen, no traces of the cephalothorax or appendages, about which there is still much to be learnt, being visible. Many interesting points of structure have, however, been preserved, and these have been clearly reproduced in the enlarged figures on the plate accompanying the paper.

For the following comment we are indebted to Mr. R. I. Pocock. "The authors with enviable confidence describe as stigmata five pairs of slits piercing the front margin of segments 2–6. If their interpretation be correct, the fact is one of far higher morphological importance than they seem to suspect. For no arachnid, recent or fossil, is known to possess more than four pairs of abdominal stigmata; and the recent species of Opiliones, the order to which *Eophrynus* and its allies, *Kreischeria* and *Anthracomartus*, appear to belong, are characterised by the presence of but a single pair placed upon the first free sternal plate of the abdomen. These considerations, coupled with the fact that *Eophrynus*, in spite of its great antiquity, is a specialized and not a primitive type, would make one inclined to suspend judgment as to the respiratory nature of the orifices in question.

"As for the name that Messrs. Howard and Thomas have assigned to Mr. O'Connor's specimen, I venture to think, though after all this is a small matter, that it is probably erroneous with regard both to genus and species. In the first place the structural differences between the abdomen of this fossil and that of *Eophrynus prestwichii* are too great, one would think, to permit the species in which they occur being rightly referred to the same genus; and in the second place Dr. Woodward's figure of the abdomen of *Eophrynus carbonis* represents this region as considerably longer than wide, whereas in the example from Pentre the abdomen, unless greatly expanded by crushing, must have been a good deal wider than long."

Caution in creating new genera and species is no doubt highly commendable, and no one will accuse palæontologists as a class with reluctance in projecting new names into literature. But in cases like the present where the characters of the fossil are clearly expressed, a wrong determination is apt to lead one's fellow-students into error.

PREHISTORIC MAN.

MR. JOHN WARD has collected together in the *Reliquary and Illustrated Archæologist* a compendious account of the exploration of Rain's Cave, near Brassington, in Derbyshire. The excavation was of a thoroughly systematic nature and yielded an abundance of prehistoric remains, but nothing of an earlier date than neolithic scrapers and other implements, and bones of *Urus* and the wolf. The early discovery of a skeleton showed that the cave had finally been used as a cemetery.

In the same work there is a note on Carn Brea, in Cornwall. Some of these hut circles have recently yielded scrapers, arrow and spear heads, a diallage celt, and a small stone evidently used as a muller for grinding grain.

Has the Lamprey a Supra-renal Body?

We have received an interesting paper by W. E. Collinge and Swale Vincent on the structures in the cyclostome fishes hitherto regarded as the supra-renal bodies (*Anatomischer Anzeiger*, xii., pp. 232-241). The authors have made a most careful survey of the earlier literature on the subject, and the historical summary which they give should prove of great value to all students of ichthyotomy. The results of their investigations go to show that supra-renal bodies proper are not present in cyclostomes; that they first appear in the true fishes and acquire an increased importance in air-breathing vertebrates. They consider that the supra-renals are not degenerate structures "with a past," but that they have been independently evolved to fulfil certain functions which it remains for future physiologists to demonstrate. Whether anatomists generally will agree with the authors that there is not the homology between the pronephros of cyclostomes and the supra-renal bodies of gnathostomes that was maintained originally by Rathke and more recently by Weldon, time alone will show. Deeply rooted notions, whether based on incontestable observations or not, require very convincing arguments to overthrow them, and these latter are not furnished in the paper under consideration.

Sergestes.

An important contribution to our taxonomic knowledge of the marine Crustacea was made to the Zoological Society on December 1st, when the Rev. T. R. R. Stebbing communicated a paper by Dr. H. J. Hansen, of Copenhagen, on the species of the genus *Sergestes*. Dealing with the sixty hitherto described species, the author succeeded in reducing them to twenty, showing that previous observers had, from insufficient material, described as new, males and females, and young and adult stages of the same forms. His researches went so far as to render it very probable that there are trustworthy characters by which adults can be distinguished in future from immature forms, and it is to be hoped that the confusion that has arisen in the past will not be found a difficulty to future zoologists.

The Eggs of the Nautilus.

Our readers, who doubtless remember Dr. Arthur Willey's fascinating article "In the Home of the Nautilus" (Natural Science, vol. vi., p. 405, June, 1895) will join with us in congratulating him warmly on the success that he has at last gained. Professor Ray Lankester wrote thus to the *Times* (Feb. 8th) :—". . . . Dr. Willey proceeded first to Ralum, in New Britain, where he spent a year trapping the nautilus in seventy fathoms of water and dredging in vain for its eggs. He then tried a station on the coast of New Guinea, where he was nearly drowned by the capsize of his small craft. After passing through New Caledonia he arrived last summer in Lifu, one of the Loyalty Islands, where nautilus can be captured in three fathoms depth only. Here he constructed a large submarine cage in which he kept specimens of nautilus, feeding them daily. On December 5th last his patient endeavours were rewarded. Some of the nautili had spawned in the cage, and thenceforward he was able to obtain abundant samples of the eggs. Each egg is as large as a grape, and is deposited separately by the mother nautilus. At present we have received but few furthur details from Dr. Willey, but he has doubtless by this time obtained the young in all stages of growth."

Evidence for the South African Committee.

We strongly commend to the gentlemen now appointed to enquire into South African affairs, a paper entitled "The Witwatersrand and the Revolt of the Uitlanders," written by Dr. George F. Becker, and reprinted from the *National Geographic Magazine* (Washington) for November, 1896. Dr. Becker, who is a well-known member of the United States Geological Survey, was in Johannesburg shortly after the Jameson Raid. He gives here a lucid and fair-minded account of the causes that led to that disaster, and of some of the occurrences at the time. He happened to be in a position favourable for obtaining information, and this he has sifted with scientific caution. We have rarely read an account of any impassioned dramatic struggle, even of one in a far-off antiquity, so sympathetic and yet so free from prejudice.

I.

The Problem of Instinct.[1]

THERE is probably no subject in the whole range of biology, the study of which has been so universally neglected as Instinct. Both scientific and popular writers continually refer to it as if its nature and limitations were matter of common knowledge, and its facts so well established as to be almost above criticism. Yet when we ask how it is known that certain actions of man or animals are due to instinct and not to experience or imitation, we find an almost total absence of accurate observation or experiment, while hardly two writers are agreed as to the exact meaning of the term. It is only within the last quarter of a century that a few biologists have made any careful experiments on the phenomena presented by the actions of the higher animals, under such conditions as entirely to exclude the agency of imitation or of parental guidance; and although these experiments are as yet quite insufficient in quantity and far too limited in scope, having regard to the wide field covered by the actions and behaviour usually considered to be instinctive, yet the results reached are already very interesting, and are sufficient to show us that we need not despair of a complete solution of the problem, at all events as regards the higher animals.

One of the first English observers to attack the problem on the experimental method was the late Mr. Douglas Spalding, who in 1873, in *Macmillan's Magazine*, described a number of experiments on young chicks and ducklings, carefully blinded for the first few hours after birth. His conclusions were, that these young birds not only showed intuitive powers of walking, scratching, and pecking, but also possessed intuitive knowledge—or acted as if they possessed such knowledge—of various kinds. He asserted that they were afraid of bees and of the cry of a hawk, and that they intuitively knew the meaning of a hen's call-note and danger-signal when heard for the first time. These results were opposed to Mr. Spalding's preconceived ideas, and were therefore the more readily accepted, and have been frequently quoted as settling this point—the possession of instinctive knowledge as well as the power of co-ordinated movements of various kinds. Now, Professor Lloyd Morgan has repeated all

[1] HABIT AND INSTINCT, by C. Lloyd Morgan, F.G.S. Pp. 351. London: Edward Arnold, 1896. Price 16s.

these experiments many times and with a considerable variety of species, and, while confirming many of Mr. Spalding's observations and conclusions, has shown that those here referred to are erroneous. More important still, he has shown exactly where and why the conclusions arrived at are erroneous, and has thus afforded most valuable guidance to future experimenters in this interesting enquiry. Some examples of these corrections are the following.

Mr. Spalding noticed a difference in the behaviour of young chicks to flies and to bees, and concluded that they "gave evidence of instinctive fear of these sting-bearing insects." This, if true, would be very important, since it would show an intuitive perception of the dangerous character of a special insect, and if of one, presumably of all common dangerous or hurtful objects, antecedent to experience. But the whole series of observations made by Professor Morgan himself, as well as those made by other good observers, shows that they have no such perception of the qualities of objects. They pick up stones as well as grain, bits of red worsted as well as worms, gaudy-coloured inedible caterpillars as well as those which are edible. They do not recognise water till they have felt it, and they do not know that water is drinkable till contact with the beak sets up the nervous and muscular reactions of drinking. By a series of careful experiments Professor Lloyd Morgan shows that young chicks have no fear of bees as bees, but merely because they are large and unusual. They are equally suspicious of a large fly or beetle, and, though eating small worms greedily, are afraid of a large one. And when the chicks were a few days old, and were no longer afraid of large flies, they showed no fear even of wasps, when presented to them for the first time.

Very similar is the correction of Spalding's statements that his young chicks gave the danger-signal when a hawk was flying high overhead, and that a young turkey showed equal alarm when a young hawk, kept in a cupboard, uttered a shrill sound; whence he concluded that the fear of birds of prey, whether seen or heard, was instinctive. For it is now shown that *any* strange object or *any* unusual sound causes exactly similar alarm when first seen or heard by any kind of young birds, and Mr. Hudson, of La Plata fame, came to a similar conclusion. Other cases which have been thought to prove instinctive dread of enemies in various young animals are shown to be explicable on similar principles; any sight, or sound, or smell, very different from what they have been accustomed to, alarms them, and they learn what are really dangerous either through the actions of their parents or by their own personal experience.

But, though young birds and mammals do not possess instincts which enable them to discriminate between objects that may be useful and those that may be hurtful to them, they often possess the most wonderful acuteness of the senses and powers of co-ordinated muscular action, which enable them rapidly to acquire the knowledge that is essential to them. The young chick only a few hours out of the shell walks and runs, and is able to pick up small objects in its beak, some being rejected and others swallowed. The young duck swims when put into the water, or when it accidentally walks into it, but it has no instinctive desire for it, and does not, as is often stated, run to it from a distance. Young dippers dive perfectly the first time they reach the water, and young swallows fly with great precision, avoiding obstacles almost as readily as do the old birds. With such congenital powers, and with an instinctive fear or suspicion of everything that is strange to them, they learn with marvellous rapidity; and having once found that a particular object is disagreeable or unfit for food they rarely require a second lesson, and thus in a few days accumulate a stock of experience, which, unless the process has been closely watched, may easily be set down to instinct.

About one-third part of Professor Lloyd Morgan's work is devoted to such experiments and observations on young birds and mammals as have now been indicated, and the amount of new and varied information here brought together is sufficiently large to form the basis of sound reasoning on the nature and limitations of the faculties involved; and perhaps no living biologist is better fitted to do this successfully than the author. In the series of chapters headed: "The Relation of Consciousness to Instinctive Behaviour," "Intelligence and the Acquisition of Habits," "Imitation," and "The Emotions in Relation to Instinct," we have a careful and interesting study of the physiological and psychological aspects of the facts that have been laid before us; a study which is in the highest degree instructive, and which will serve to guide future students of the subject both as to the interpretation of the facts already established, and as to the observations most needed for the elucidation of matters which are still unsettled. These chapters, however, are hardly suited for illustration or summary, and we will therefore pass on to those which deal with the alleged instincts of adult animals, and with some of the most disputed questions which now divide biologists; but, before doing so, it will be well to quote the author's definition of instinct, as well as the conclusions he has reached as to its nature.

At the end of the first chapter, which gives a popular sketch of the facts which demand explanation or verification, Professor Morgan says:—

> "We may now sum up what has been advanced in the foregoing discussion, and say that, from the biological point of view, instincts are congenital, adaptive, and co-ordinated activities of relative complexity, and involving the behaviour of the organism as a whole. They are not characteristic of individuals as such, but are similarly performed by all like members of the same more or less restricted group, under circumstances which are either of frequent recurrence or are vitally essential to the continuance of the race. . . . They are to be distinguished from habits which owe their definiteness to individual acquisition and the repetition of individual performance."

And after having described the various actions of young birds antecedent to experience, our author summarises some of the conclusions to be drawn from the observations as follows :—

"1. That which is congenitally definite as instinctive behaviour is essentially a motor response or train of motor responses. Mr. Herbert Spencer's description of instinct as compound reflex action is thus justified.

"2. These often show very accurate and nicely-adjusted congenital or hereditary co-ordinations.

"3. They are evoked by stimuli, the general type of which is fairly definite, and may, in some cases, be in response to particular objects. Of the latter possibility we have, however, but little satisfactory evidence.

"4. There does not seem to be any convincing evidence of inherited ideas or knowledge (as the term is popularly used); that is to say, the facts can be equally well explained on the view that what is inherited is of the nature of an organic response.

"5. Association of ideas is strong, and is rapidly formed as the result of individual acquisition.

"6. Acquired definiteness is built, through association, on the foundation of congenital responses, which are modified, under experience, to meet new circumstances.

"7. Acquired definiteness may pass, through frequent repetition, into more or less stereotyped habit."

Having thus given the author's standpoint and his main conclusions from a body of well-observed facts, we will pass on to his treatment of those activities of adult animals which are generally classed as instinctive.

In the chapter on "Some Habits and Instincts of the Pairing Season," the songs, dances, and displays of plumage by birds are described, and Professor Lloyd Morgan seems inclined to the conclusion that their great development indicates the action of that form of sexual selection termed "preferential mating." Some additional observations are quoted which support this view, but the final conclusion is that—"in all these matters further and fuller evidence from direct observation is to be desired."

The next chapter is on "Nest-building, Incubation, and Migration," and affords much matter for careful consideration. As to nest-building, I have always urged that careful experiments are required before we can accept as instinctive the building of a peculiar type of nest by each species of bird; and we find a few such recent experiments now adduced. But when we remember how such a careful experimenter as Spalding was led to wrong conclusions through not varying his experiments sufficiently, the few experiments yet made on nest-building under conditions by no means rigid and with results not described in sufficient detail, can hardly be accepted as settling the question. This is one of the problems that can only be finally settled by experiments tried on a large scale and with every precaution, and the results preserved for comparison and study; and if ever an experimental biological farm is established this subject of nidification would form one of its most interesting and comparatively easy enquiries.

Passing over a very interesting discussion as to the habits of the cuckoo and their probable origin, and one hardly less interesting on the habit of the lapwing and many other birds of simulating injury to distract attention from nest or young, we pass on to the broader and more important subject of migration, which, however, is rather briefly treated. The evidence now accumulated seems to justify Professor Lloyd Morgan's conclusion, that while the migratory impulse is innate, yet, " the element of traditional guidance may be effectual, in the migration stream as a whole, in some way that we have hitherto been unable to observe." The chief obstacle to this view consists in the well known observations of Herr Gätke at Heligoland, that, during the autumn migration, in the case of the great majority of species, the young birds migrate earliest and alone, the adults following considerably later. But admitting, as everyone must, the accuracy of Herr Gätke's observations, does the conclusion necessarily follow? He himself assures us that the birds which rest on or pass within sight of Heligoland only form a fraction of the whole of the migrating hordes, most of them travelling by night at great altitudes, and very few passing within sight of the island, and of these few only, perhaps, one in ten thousand stopping to rest. The fact that young birds of many species are the first to visit Heligoland every year without exception, may possibly be explained by the fact that, while the older birds which lead the way travel high and go on without stopping, a large number of the young fly lower, and being either fatigued by the long unaccustomed flight or attracted by the sight of the land, descend to this elevated and fertile islet for rest and food. The late Mr. Seebohm, whose extensive journeys to Siberia and in various parts of Europe for the purpose of collecting and studying birds rendered him an authority on this subject, gives the early migration of young birds on the authority of the Heligoland observers, and does not support it by any observation of his own in the northern regions from which so many of the migrants come. In America, although some writers state that young birds migrate first in autumn, Mr. C. Hart Merriam, of the Department of Agriculture, tells us that this notion is " contrary to the experience of most leading American Ornithologists and to the information collected by the Committee on Migration of the American Ornithologists Union." [1] But if we reject the conclusion based upon the Heligoland facts as not necessarily following from them, we shall find that there is not much difficulty in forming a theory which accounts for the main phenomena, and the outlines of such a theory have been very well expressed by Mr. Seebohm himself at the end of the chapter on *Migration* in his " Geographical Distribution of the Charadriidæ," in the following passage :—

[1] Report on Bird Migration in the Mississippi Valley, by W. W. Cooke, p. 13, Footnote.

"The assemblage of migratory birds in large flocks, which in many cases wait for a favourable wind before they venture to cross wide stretches of sea, and consequently start altogether as soon as the weather is suitable, and arrive on the other side in enormous numbers or rushes; the keen sight of birds and their extraordinary memory for locality; the great variety of routes chosen, and the pertinacity with which each species keeps to its own route—these and many other facts all point in one direction. The desire to migrate is a hereditary impulse, to which the descendants of migratory birds are subject in spring and autumn, which has acquired a force almost, if not quite, as irresistible as the hereditary impulse to breed in the spring. On the other hand, the routes of migration have to be learned by individual experience. The theory that the knowledge of when and where to migrate is a mysterious gift of nature, the miraculous quality of which is attempted to be concealed under the semi-scientific term of *instinct*, is no longer tenable."

The views here expressed appear to me to harmonise well with the general conclusions as to the nature and limitations of instinct arrived at by Professor Lloyd Morgan, and they are enforced by some considerations which writers on this subject usually overlook. The numerous recorded facts of birds returning year after year to build in the same spots as in the preceding year, indicate that most of the spring migrants are old birds. Not only is this the general belief of observers, but it is rendered probable by the known longevity of most birds, and the obvious circumstance that those which have escaped the dangers of the double migration on the first occasion will be more likely to escape in each succeeding year, so long as health and strength continue. The fact that the breeding population of birds in any country does not increase year by year, but, though there are considerable fluctuations, remains on the average constant, proves that there must be an enormous destruction of the young birds, which certainly amount in number to several times as many as the old ones, and it seems probable that this destruction takes place during the two annual migrations, and more especially during the first one in autumn, when the young birds have had no practice in long continued flight and no experience of the dangers of the sea. If the birds of more than one season live on the average only four or five years more, it follows that only a very small percentage of the enormous annual progeny of young birds can survive to take their place. Hence it may well be that all those countless myriads of birds of the year that visit Heligoland are among the failures which, if they leave the island, perish in the waters. We know that enormous numbers *must* perish during each year, and where so likely as during that first attempt to traverse the North Sea? This is rendered almost certain by the recently issued Report of the British Association Committee on Bird Migration, in which it is stated that at the various periods of the great autumnal rushes at Heligoland, when countless thousands of birds pass over that island, no corresponding influx has been noticed on our east coasts during the four successive years that the two records have been compared. Either then, these myriads of birds passed southward to Holland or flew out to sea and were mostly lost. As Herr Gätke does not mention any corresponding flights along the coasts of Holland and Belgium it is to be presumed that they have not been noticed, and we are almost forced to the conclusion that the greater part of these young birds, whose immense numbers at Heligoland excite so much astonishment, are really failures, and form a portion of those which are annually eliminated by the severe test of migration; and if this be so, much of the marvel supposed to attach to the successful migration of young birds disappears, since such "successful migration," except in the case of a small percentage, does not occur.

There is one alleged instinct of great popular interest which Mr. Lloyd Morgan does not deal with—the means by which many of our domestic animals, especially cats, dogs, and horses, find their way home under circumstances which seem to preclude any direct guidance by the senses. Narratives of the most marvellous character have been published, but, unfortunately, no systematic experiments appear to have been made, except a few by the late Mr. Romanes. These showed that a dog could exactly track its master's footsteps by scent, and it was shown that the scent was derived from his shoes, which must of course be full of perspiration and other emanations from the skin, because when he wore new shoes or those of another person his track could not be followed. In *Nature*, vol. vii., there is a considerable discussion of this subject, and many remarkable cases are narrated by various correspondents, but in none are all the data given for arriving at a rational conclusion on the question. I then wrote summarizing the discussion (*Nature*, vol. viii., p. 65), and suggested a series of experiments, which would alone give us any real information. My suggestion was that a dog whose antecedents were known should be taken by a circuitous route by rail and road to some spot where he had never been before, and should be there handed to some person he did not know, who should carry him a short distance, and on releasing him should keep him in sight, either on foot or horseback, till the animal returned home, noting carefully every movement and action. A moderate number of such experiments would settle the question of instinct or sense-observation, and it is to be hoped, now that a more intelligent interest is taken in the subject, such experiments will be made. I may add that, theoretically, any instinct of direction is almost inconceivable, because quadrupeds in a state of nature do not require such an instinct. They learn, step by step, the surroundings of their birth-place, extending their range, perhaps, year by year, but never requiring to go back over a country they have not previously traversed. In their case ordinary memory, assisted by very acute senses, would be all they would need. In the case of domestic animals returning home, we find that the recorded time elapsed varies from a few hours to several days or weeks, even when the distance can be easily traversed in a day. This shows that an instinctive sense of direction cannot always be the agency employed; and, as in no one case is the exact route of the animal's return known, the assertion so often made, that a special sense of direction is required to explain the facts, cannot be justified.

Two very interesting chapters, near the end of the volume, are those entitled "Are Acquired Characters Inherited," and "Modification and Variation." In the first of these the bearing of the whole of the phenomena of instinct on the vexed question of inheritance is pointed out, and the conclusion is reached, that although there is little or no satisfactory evidence of the transmission of acquired modifications—that is of habits, or their effects on the organism, as opposed to instincts, yet there are many curious facts which seem to indicate some connection between congenital and acquired characters. What this connection is, the chapter on Modification and Variation attempts to show.

Modification of the individual by the environment, whether in the direction of structure or of habits, is universal and of considerable amount, and it is almost always, under the conditions, a beneficial modification. But every kind of beneficial modification is also being constantly effected through variation and natural selection, so that the beautifully perfect adaptations we see in nature are the result of a double process, being partly congenital, partly acquired. Acquired modification thus helps on congenital change by giving time for the necessary variations in many directions to be selected, and we have here another answer to the supposed difficulty as to the necessity of many coincident variations in order to bring about any effective advance of the organism. In one year favourable variations of one kind are selected and individual modifications in other directions enable them to be utilised; in Professor Lloyd Morgan's words—"Modification *as such* is not inherited, but is the condition under which congenital variations are favoured and given time to get a hold on the organism, and are thus enabled by degrees to reach the fully adaptive level." The same result will be produced by Professor Weismann's recent suggestion of "germinal selection," so that it now appears as if all the theoretical objections to the "adequacy of natural selection" have been theoretically answered.

Biologists owe a debt of gratitude to Professor Lloyd Morgan for this most interesting and suggestive volume. It exhibits all the clearness and philosophical acumen which characterise the writings of the author, and although in his desire to be impartial he has sometimes suggested difficulties which are more apparent than real, yet the work is on the whole an admirable introduction to the study of a most important and fascinating branch of biology, now for the first time based upon a substantial foundation of carefully observed facts and logical induction from them.

ALFRED R. WALLACE.

II.

Fresh-water Biological Stations: Europe's Example.

MR. SCOURFIELD'S plea for a fresh-water biological station in England (NAT. SCI., vol. x., p. 17, Jan., 1897) gave me much pleasure. England is very late in making this proposal, and all biologists working on lakes have often asked why the English lake-district remains unexplored in this respect.

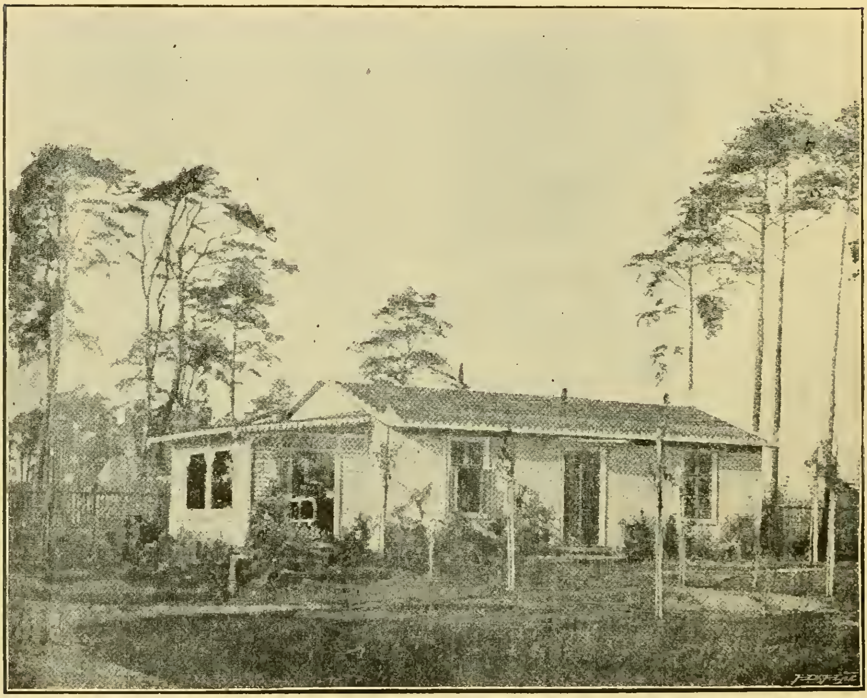
Every day fresh-water biology assumes greater importance, and in a country so far from the sea as Bohemia, we naturally came to the conclusion that the study of our fresh waters was our first duty. Last summer I delivered a course of lectures drawing attention to the variety and interest of the life-groups in more than thirty kinds of Bohemian fresh waters, each with its own special fauna and flora: springs, mountain brooks, mountain rivers, rivers of the plain, back-waters of large rivers, ponds, lakes, bogs, small pools with *Apus*, snow-tarns with *Branchipus*, etc. Each of these kinds of water varies in its own fauna with the seasons of the year, and also from year to year according as rain and sunshine also vary. Here is work for a century.

Besides America, as you yourselves have mentioned, Russia also is beginning to support the biological study of fresh water. A fixed laboratory is to be established in connection with an agricultural institution, while both moveable and smaller fixed stations will be founded in different parts of Russia. The sum of 73,000 roubles has been placed at their disposal. Even if these institutions were almost to confine their attention to fish, biology would gain much.

The sum of £500, which is suggested as enough to start such a biological station in England, is none too much. The building for our moveable station was presented to the committee by a friend of mine, and cost him £70. With all its internal fittings, it now has a value of £200, yet everything is very humble, and the want of better instruments strongly felt. The annual working expenses of three investigators amount to £40, their work itself being given freely. Nevertheless, we have just finished the examination of two lakes in the Böhmerwald, the report on them being now in the press; and the station has been transferred to Podiebrad in the middle of Bohemia, for the investigation of the river Elbe. At the fixed station, I may add, we have examined the plankton of the pond near Biechovic every fortnight, paying special attention to infusorians and rotifers.

Last summer I visited two biological stations in Germany, and published in *Mittheilungen des Oesterreichischen Fischerei-Vereines* (No. 65, Wien, 1896), a paper entitled, "Eine Studienreise in Angelegenheiten der Fischerei." As the information may assist your proposal, and is not likely to be accessible to your readers, I venture on an extract or two.

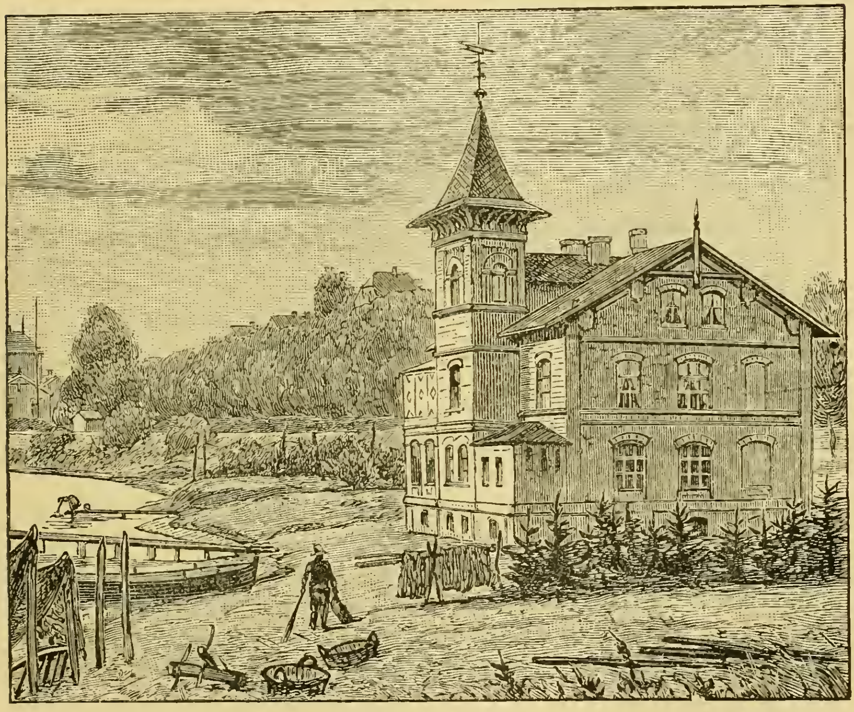
The biological and fishery station on the Müggelsee near Berlin is close to the great water-works of Friedrichshagen, built at a cost of £1,000,000 to supply the capital with water. These waterworks



THE BIOLOGICAL AND FISHERY STATION ON THE MÜGGELSEE.

supply the station gratis with a definite quantity of water; not, however, with enough for the experimental basin, to which water is to be pumped from the lake by a motor next year.

"Professor Frenzel was waiting for us, and was kind enough to explain the arrangement of the station in detail. On the sandy, slightly shelving shore of the lake, in a fir-wood, stands a small one-storeyed cottage with four chambers, two workrooms, an aquarium-room, and the servant's lodging. The workrooms have each about the size of our moveable stations, but suffice for the work of an unassuming naturalist. The tanks in the aquarium are of cement, rendered harmless, when finished, by glowing charcoal; many smaller ones are of glass. The experiment-basins in front of the buildings were for the most part empty, since the Government supply was not enough to fill them all. A petroleum-boat is at the service of the station. During the last three years this institution has been granted 27,806 marks from the German Fisheries Union. A detailed report on the arrangement and work carried on was published by Dr. Frenzel in the *Zeitschrift für Fischerei*, Heft I, 1895; separate copies of it may be obtained. Of late, importance has been attached to attracting young people, who come here from the Agricultural College in Berlin, and from the University, and obtain an insight into the fauna of the lake.



THE BIOLOGICAL STATION ON THE PLÖNERSEE.

"The Müggelsee Station is undoubtedly a good beginning, and I received the impression that Germany was ready to do much more for the study of fresh-water biology, so soon as the results there obtained should receive universal recognition.

"The Plönersee lies in a delightfully picturesque district, reached in about two hours from Lübeck, and on its shore, in the town of Plön itself, is the biological station, where for the first time in Germany the scientific investigation of fresh water found a decent shelter. I was kindly received by the Director, Dr. Zacharias, and in the short time at my disposal I obtained, through his guidance, an idea of the arrangements of the station. The building contains a number of fairly large rooms, easily accessible from a corridor between them. Besides the laboratory of the Director, and of an assistant, there is a larger apartment for researchers visiting the station, then a room for the nets, and a library. On the ground-floor are aquaria, ventilated by a petroleum motor of two-horse power.

"The catches are exactly measured, and then counted as individuals, after Professor Hensen's method,—a very tiresome and lengthy proceeding. For our work in Bohemia, where we have to do with shallow water, the quality of which and the plankton vary suddenly and often, this method is not to be recommended; moreover, we obtain sufficient knowledge of the distribution and composition of the plankton by horizontal dredgings of 100 metres, going metre by metre.

"The results of Dr. Zacharias and others working at the laboratory are published in the *Forschungsbericht*, of which three volumes have appeared up till now.

"The establishment of the institution, as well as its direction, are still of a provisional character, which hardly conduces to prosperity. When we consider the happy progress that has been made in the investigation of fresh water by the institution of the three stations in Plön, on the Müggelsee, and in Trachenberg, it is strange that opinions as to their value should be so diverse. People underestimate the problems set before such institutions, and forget that the individuals who are to cope with them, must first be discovered, that they must make systematic studies throughout many years, and above all, that they should be placed in a position untroubled by anxiety as to their future, and not compelled to supplement by other undertakings the insufficiency of their material situation."

I hope that you who in England are supporting this movement for a fresh-water station, will be able to convince people of the great practical, as well as scientific value, of such an institution, and that we on the Continent may soon hear that your wealthy country has done her duty by fresh-water biology.

Anton Fritsch.

Prague.

III.

The Light-Sensations of Eyeless Animals.

THE fact that certain skins are sensitive to light has long been known. Most of us are familiar with the case of the earthworms, whose sudden withdrawal into their holes on the approach of a lighted candle was observed and described by Darwin. The earthworms have no eyes, and there is no other apparent explanation of this reaction but that the skin itself is sensitive to the light-rays. It is also well known that this is not the case with all skins—our own, for instance—and that, in the vast majority of animals, there is only one organ, viz., the eye, which is sensitive to variations of light intensity.

The assumption is justifiable that if we could only explain the mechanism of this diffuse light-sensation of the skin, usually called, not very happily, the " dermatoptic " function, we should be in a fair way towards understanding the mechanism of the exquisite sensitiveness of our eyes. It is, indeed, open to anyone to assume that this diffuse sensitiveness was once universally present, but was lost on the development of eyes, these specialised organs rendering the less efficient function unnecessary; and that eyes first arose merely as localised areas, in which this sensitiveness to light was specially concentrated.

It is quite possible that this may have been the case: it is a fair hypothesis; but, on the other hand, the facts seem to show that the dermatoptic function is not necessarily a primitive condition retained, but may also be secondarily acquired. It is found, for instance, in isolated members of different animal groups, and nearly always in adaptation to some special manner of life; this favours the idea that it has been secondarily developed. The ancestors of the earthworm, for instance, were probably more free-living chætopods, and, as such, almost certainly possessed eyes, which were lost in the earthworms owing to their burrowing manner of life. I know of no record of any modern chætopod possessing the dermatoptic function.

With regard to the snails, which possess both eyes and, to a slight degree, the diffuse sense, shrinking at a passing shadow even when the eye-bearing tentacles are cut off, we may, on first thoughts, suppose the diffuse function either to have persisted along with the specialised, but not very highly developed, eyes, or to have been secondarily acquired. But the fact that the nearly related slugs show no such sensitiveness to light seems to indicate that the second hypothesis is more likely to be correct. On the other hand, in the case of blinded newts, which select the darker places in which to lurk, as if their skins were in some way sensitive to light, this might be thought more likely to be the primitive function retained, otherwise it is not easy to see why it should have been required in addition to eyes. The reported sensitiveness to light of the blind cave-dweller, *Proteus anguineus*, is a quite intelligible adaptation to the needs of the animal.

Here, then, we are brought face to face with a very fascinating problem. The skins of animals may possess a sensitiveness to light which must in some way be correlated with the development of eyes. Have we any clue which will explain the facts, and also the connection between the diffuse light-sensation and the specialised light-sensation of our eyes? All who are interested in this—and who is not?—will naturally welcome any book dealing specially with this subject. Probably all that can be said about it from the generally accepted point of view can be found in Dr. Willibald Nagel's 120 pages, published by Gustáv Fischer, of Jena, under the title of "Der Lichtsinn augenloser Tiere." Excellent as Dr. Nagel's treatment of the subject is, there is, as we shall see later on, a factor in the problem which Dr. Nagel is not alone in too lightly dismissing, though due attention to it would have brought him nearer to some positive and satisfactory conclusion.

The first part of the work is a reprint of a lecture under the somewhat startling title of "Seeing without Eyes" (Sehen ohne Augen). The second part contains a description of experiments made by the author in the Naples Aquarium and elsewhere on the reactions, chiefly of molluscs, to sudden positive or negative variations of light-intensity. The third part consists of five short appendices or notes, expanding certain subjects which had been too briefly dealt with in the lecture; and the volume concludes with a useful bibliography of treatises on the physiology of sensation in general, and of sight in particular.

In addition to the better known examples of the dermatoptic function above mentioned, all of which are cited in Dr. Nagel's book, we have the phenomena especially investigated and described by the author himself. These require a brief notice.

First of all, then, the author established beyond doubt that, apart from the stimulus of very bright lights, it is the sudden changes in the light-intensity to which skins endowed with the dermatoptic function react, and further, that some animals react to sudden increase of light, and others to sudden diminution. That the oyster, for instance, should react to a sudden diminution of light had been denied by Rawitz, more or less on theoretical grounds. The denial and the objections are conclusively met by the author, the former experimentally, by showing that if a number of oysters kept in a vessel are found open, they will shut simultaneously the moment a dark object comes between them and the light. While the shutting of a single oyster on the passing of a shadow might be a mere coincidence, the simultaneous clapping to of a number of individuals excludes this possibility.

The animal that gave the best reactions among the molluscs was *Psammobia vespertina*, a bivalve which, as the name implies, lives in sand. The siphons of this animal protrude from the sand as delicate, whitish, transparent tubes, without a particle of pigment. If these siphons are suddenly illuminated, care being taken to avoid any mechanical stimulation, they contract. It is important to note however, that there is a short latent period of about a second. The brighter the light, the greater the contraction.

If a number of *Psammobia* are placed in a glass with plenty of sand and carried into the direct sunlight, they bury themselves instantaneously. If there is no sand in the glass, they flit about in the water until exhausted. But while very bright light violently stimulates these animals, it was observed that, with moderate illumination, it was not the light itself, but the sudden change from less to greater which caused the reaction ; a moderate light appears to be that to which they are accustomed.

Having established the extreme sensitiveness of the siphons of *Psammobia* to sudden increases of light, the experiments were varied. Sudden diminutions were tried, but without other result than a slight lateral movement. *Psammobia* therefore belongs to the group which only responds to sudden increase of light, and this, it is found, contrasts with another mixed group of animals which only respond to sudden diminutions of light, and are not stimulated by its sudden increase. These two different and opposite kinds of reaction are called light-sensation and shadow-sensation.

Again varying the experiments, Dr. Nagel found that illumination with lights of different wave-lengths showed that all the visible spectrum except the reds led to reaction. This effect is interesting and important, and we shall have to refer to it again.

It is not necessary to follow all the experiments, or to repeat a list of the recorded cases referred to by the author. The sudden closing of the sessile brachiopod *Thecidium*, on the passing of a shadow, is not given. I mention it here as a further indication of the secondary acquirement of the dermatoptic function. This animal has a method of life not unlike that of the oyster, although there can certainly be no close genetic relationship between them. In both cases the animals are exposed when the valves are open, and in both cases the lids clap to on the passing of a shadow.

A generalisation of interest to biologists must be noted in passing. All the animals which responded to the sudden diminution of light were such as inhabited strong shells, as in the cases just mentioned, or lived in tubes into which they could withdraw, their times of exposure being when feeding. The significance of this the author, no doubt correctly, points out. Warning of the approach of an enemy would in most cases be first given by its shadow. Hence this "Schattenempfindung" is most probably protective. On the other hand, the sensitiveness to light-increase is most prominent in animals which live in sand or mud, from which they occasionally and, as it were, shyly emerge. Their sensitiveness to light serves to keep them near their retreats. *Amphioxus* is a good example of this. It lives in sand, and is violently agitated by light if it cannot bury itself. It was originally thought that the sensitiveness to light of this animal might be due to the pigment spot at the anterior end of the nerve cord, but "headless" specimens are equally shy of light. This appears to limit the seat of the sensation to the skin.

These very variations of the function, in more or less evident adaptation to the biological needs of the animals, are, it seems to me, a further indication of its secondary origin, and we are led to believe that, if need be, any skin could acquire the capacity of reacting, either to sudden increases or to sudden diminutions of light-intensity if its life-condition required it. I say we are led to believe this simply because so many different animals have developed it, and quite apart from any theory as to the physiological principles involved; we shall return to these latter later on.

We noted above that Rawitz was rather inclined to doubt the existence of a "Schattenempfindung," his reason being, that if light were a stimulus, the negation of light could not be so at the same time. Dr. Nagel discusses the point in one of his appendices. He rightly points out that the fallacy in this argument lies in supposing that the light-waves as such are felt, *i.e.*, give rise to some corresponding vibrations in the nerves. This is certainly not the case, and all we can rightly assume is that certain potential energies exist within the living substance, and that these are brought into play by the action of light, the resulting molecular movements having no necessary likeness to light-waves. From this point of view, changes in the molecular or atomic movements within the living substance, so far as we can see, could be produced as well by sudden illumination as by sudden shadows. Such changes are all, then, that we require for the production of the sensation. Further, Dr. Nagel's answer to Rawitz might have been still more complete if he had pointed out that the sudden diminution of light most certainly causes a distinct sensation with us. A sudden shadow always startles us. In view of the justifiable correlation between the specialised organ for recording variations in light-intensity, and the skin possessed of the dermatoptic function, there seems to be no reason why, in such essential matters, we cannot assume that what is true of the one will be true of the other also. Indeed, as above noted, Dr. Nagel has experimentally established this shadow-sensation in oysters.

After having established the facts concerning the sensitiveness to light of the skin in certain animals, the next question that arises is: Are there any specific sense-organs functionally associated with this sensation? Its answer, contained in Appendix 3, p. 98, is: no such organs can be found. The skin is richly provided with simple sensory cells, and the author is driven to assume either that there are subtle differences, not evident in their microscopic appearances, between these cells, some being tactile, others sensitive to light and so on, or that one and the same apparently simple sensory cell may serve for different sensations. Dr. Nagel is an advocate of the latter hypothesis. The infusorian is a simple cell and is capable apparently of very diverse sensations; at least it reacts to many different stimuli. And, again, any cell has at least two different and opposite reactions, viz., expansion and contraction, and these, with their degrees, might make it possible for one and the same cell to be the end-organ of more than one sensation.

We may then briefly sum up Dr. Nagel's conclusions. The whole skin is a possible retina; cells sensitive to light are there, and the nerves connecting these cells with the central nervous system; all that is wanted to form an eye proper is a dioptric apparatus, *i.e.*, primarily, a lens to throw an image on the skin. Distinct vision of the external world would result as soon as there were a sufficient number of the sensory cells to give a connected translation of the image with its various lights and shades into sensation.

Beyond these suggestions as to the probable explanation of this curious function and of its relation to specialised vision, Dr. Nagel does not go, and indeed it does not seem possible to go further along recognised lines. As above stated, however, Dr. Nagel follows in the beaten track, and practically neglects one important factor in the problem. I refer to the pigment.

The complete absence of the pigment in the siphons of *Psammobia* already noted is especially emphasised by Dr. Nagel, as tending to strengthen the ordinary view that pigment is not necessary to sight, a fact which was already known from the absence of pigment in the eyes of albinos. Nevertheless, Dr. Nagel, who may be taken to be expressing the generally accepted doctrine, is obliged to admit that the pigment has some very intimate association with light-sensation. It is almost univerally present in eyes, being absent only in albinos, which must be considered as abnormal (Missbildungen). What this relationship is no one has hitherto been able to explain. And on this subject Dr. Nagel confines himself to the oft repeated suggestion, that the pigment forms sheaths round the sensory cells. In this way, the dazzling of the light is prevented, and each end organ is illuminated by itself, the distinctness of the image being thus assured. I should not, of course, wish to deny that this ensheathing of the rods and cones for the isolation of the end-organs is one of the functions of the pigment. There are certain facts, however, which render it somewhat doubtful. The pigment in the eye, for instance, is very granular, and granules, especially if they are, comparatively speaking, large, are not the best materials for a light-proof sheath. And again, the more perfect the dioptric apparatus, the less necessity is there for this ensheathing of the nerve endings, inasmuch as the rays are already arranged in definite small-angled pencils, whose directions practically coincide with the long axes of the nerve endings. Yet we do not find that pigment is less developed in the highest eyes. Indeed, intense pigment is found even in the eyes of mammals whose skins are pure white in every other part of the body.

We have then, it appears to me, to look elsewhere for the chief function of the pigment, the universal presence of which in normal eyes makes it highly probable that that function is one of supreme importance in the physiology of sight.

Let us see first what are some of the recognised effects of light on the skin. One well-known effect is that which is called sunburning. This browning of the skin is due to the movement under the influence of light of pigmented granules between and into the cells of the skin. In the case of the Vertebrata a certain amount of pigmented matter is found in the cutis, and this seems to travel outwards towards the light. Whether the light helps in any way to produce more pigment, perhaps by increasing the metabolism, is unknown. Certain it is that pigment frequently appears where specially active metabolism incidental to growth is going on. One might mention as examples the case of the regrowing of the lizard's tail and the rapid multiplication of cells in certain cancerous growths; in both these cases, pigmented matter is very abundantly produced.

Our special point here, however, is that this pigmented matter invades the epidermis under the action of light. It forces its way between the innermost cylindrical cells and eventually reaches the outermost cells where, excepting when very abundant, it disappears, becoming incorporated, with loss of colour, in the dying horny cells which form the outermost protection of our body. So active is this process, indeed, that if the light contains a large proportion of rays from the violet end of the spectrum, the sunburning may be very pronounced; excessive horn-cells are produced and the outermost peel off.

Here, then, we have a very distinct effect, involving the movement of granules between and within the epithelial cells, taking place in the skin under the influence of light. It is surely not difficult to believe that this process might, if required, come under the cognizance of the ordinary sensory cells of the skin. Judging from what we now know of the origin of gustatory and auditory sensory areas out of epithelial cells, which indirectly stimulate the nerve fibres in contact with them, there is surely no insuperable obstacle to our believing that other epithelial cells, stimulated by the movement of pigmented granules set in motion by the action of light either within their bodies or between them, also indirectly and mechanically stimulate the indifferent nerve cells or fibres in contact with them. It seems to me clear that the specialisation of this process might give rise both to the dermatoptic function and to eyes.

In the first place, it must be noted that the retinal cells, in the great majority of eyes, are modified epithelial cells. As such it is quite possible that they are not themselves directly continuous with nerve fibrils; the nerve fibrils may be merely associated with the retinal cells. It would then be these latter that are stimulated by light, and they in their turn stimulate the associated nerve fibrils. This possibility is admitted in the last edition of Quain's Anatomy, vol. iii., pt. iii., p. 155; 1894. It is now generally acknowledged, as there noted, that this indirect stimulation of the nerves by modified epithelial cells obtains in the specific organs of touch, taste and hearing. It has not, however, been established either for sight or smell, although, as above stated, there are reasons for believing that it holds good for the former. In view of this indirect stimulation of the nerves in three out of our five senses, much of the discussion as to the possibility of undifferentiated sensory cells responding to different stimuli is, at least in the present instance, not quite up to date. At any rate it seems to me that the simpler problem should come first; what are the changes produced in the epithelium by light? Now it is well known that the epithelial cells are influenced in a striking way by light, viz., in the movements within and between them of the pigmented granules. Cannot this fact supply us with a clue to the understanding of the sense of light, whether diffuse or concentrated in eyes?

When we turn to the eye, we find that one of the few effects of light, about which we have no doubt, is that the pigmented granules move backwards and forwards in enormous quantities between the prolonged outer ends, the rods and cones, of the retinal cells. This process must surely be regarded as a specialisation of the ordinary invasion of the epidermis by pigmented matter under the action of light. For not only is the eye a specialised portion of the epidermis, but in both the eye and in the epidermis the pigment travels outwards under the action of light. It seems to me indeed impossible to avoid the conclusion that eyes are only specialised organs for the utilisation of these movements of the pigment under the action of light in the interests of sensation. For if the stimulation of epithelial cells by waves of sound can be communicated to the associated nerves, awakening the sensation called hearing, there is no reason that I can see why the stimulation of the retinal cells by the movement of granules against their surfaces or within their bodies should not also be communicated to the nerves, awakening the sensation called sight.

As I have elsewhere maintained, eyes arising by the stimulation of the ordinary sensory nerves of the skin indirectly by the epithelial cells, which are themselves directly stimulated by the movements of granules within or in contact with them, would naturally appear at those spots which are most frequently and brilliantly illuminated. It would be to such parts that the pigment would be most drawn by the light. That is not all. The very excess of pigmented matter brought to the surface by the light would supply the material for a dioptric apparatus. (See further *Ann. Mag. Nat. Hist.*, Feb. 7th, 1896, and *Quart. Journ. Micro. Sci.*, Dec., 1896.)

Armed with this clue to the understanding of light-sensations supplied us by the pigment, we turn to those cases in which no pigmented matter can be detected, *e.g.*, in the eyes of albinos and in the semi-transparent siphons of *Psammobia*, both of which are stimulated by light. The clue in no way fails us. The essential point involved in it was the movement of granules. While it is possible that deeply-pigmented granules move more actively under the action of light than unpigmented granules, there is no reason whatever to suppose that it is only the pigmented granules which are affected by the stimulus.

As a matter of fact, pigmented granules are of all shades of colour, from deep blackish brown to light reddish and yellowish browns. We have only to assume, then, that in the cases where no pigment can be detected, granules are nevertheless present, which are set in motion by the light, or, in the case of the shadow-sensation, brought to a standstill by the sudden withdrawal of the light, and that the epithelial cells stimulated by this motion or sudden cessation of motion in their turn stimulate the associated nerve-cells or fibrils. I may, indeed, be allowed to state in advance that, among many other retinas which I am now investigating, that of the albino rabbit shows clearly that this supposition is partly, if not entirely, correct. Not only is coarsely granular matter present in the chromatophoral cells in such eyes, but, further, this matter is frequently found massed high up between the retinal rods, just like the pigment granules in normal eyes. While I have no distinct proof that this matter finds its way between the rods as a direct effect of the action of light, it would be hazardous to deny the probability of this having been the case.

I think, then, it is abundantly clear that the pigment, in its broadest sense, meaning pigmented granules, cannot be dismissed in the way it usually is, as unessential to light-sensation, merely because in certain abnormal eyes there is no colouring matter. On the contrary, it seems to me that if, instead of turning away from the functions of pigment as too problematical to be worth considering, as Dr. Nagel has done in his little book, he had concentrated his attention upon them, his interesting facts and able discussion of them might have been carried a step or two further.

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IV.

The Literature of Fossil Ostracods.

THE common *Cypris* of our rivers, ponds and ditches, or the various genera, both freshwater and marine, belonging to the same class of animals, have had representatives or close allies in every geological period from the Cambrian downwards. It is the purpose of this sketch to bring before the readers of NATURAL SCIENCE a general idea of the amount of work already done on this group, and the probably great amount of ostracod material preserved in the rocks beneath us.

Up to the year 1855, valves of Ostracoda from Palæozoic rocks had been described here and there, with other fossils, but little serious attempt had been made to study any of the groups as a whole, or to classify such forms as had received the scanty technical attention given by the collectors of these fossils. In 1855 Professor Rupert Jones, then assistant secretary of the Geological Society, commenced a series of papers on the subject in the *Annals and Magazine of Natural History*, a series which has now reached number xxxii., and has provided material wherewith to study and classify the whole known group. In the elucidation of the various forms as they came to hand, Professor Jones had, from time to time, the invaluable assistance of Harvey B. Holl and James W. Kirkby, and these authors between them have published a vast amount of information on a most difficult group.

The Palæozoic Ostracoda present so many anomalies, that comparison with recent forms is often impossible, and deductions that were at first drawn have had to be abandoned as fresh material has come to hand. The singular protuberances found in the carapaces of the genus *Beyrichia*, with their hundred and one modifications, and the still more remarkable projecting spines of *Æchmina*, seem to baffle suggestion as to their use and meaning.

An illustrated account of some of these early forms appeared in 1869 in a paper, now very scarce, published by the Geologists' Association of London. This gives the student a very good idea of the various modifications undergone by the ostracodal carapace. In 1886 (*Quart. Journ. Geol. Soc.*) Jones and Kirkby gave a synopsis of all British "Carboniferous" forms, arranged to show their zonal and geographical distribution, and further showed that while the genera provided a more or less safe guide to stratigraphy, the species were fairly represented throughout the whole series. In a paper that has recently appeared (*Sci. Trans. R. Dublin Society*), Jones and Kirkby deal with the Carboniferous Ostracoda of Ireland, and in a table at the end of the work have given their geographical distribution in that island. Much, however, remains to be done before we can correlate the species with those of Britain and the Continent, and learn the true value of the forms, or their value to stratigraphy. A similar paper from these authors, dealing with the Silurian, Ordovician, and Cambrian forms, would be of much service, and would considerably advance the subject.

Notes on Devonian Ostracoda will be found scattered throughout the series of papers in the *Annals* above mentioned, and it might be worth some student's while to draw up for the whole Palæozoic series such a table as is suggested.

On Mesozoic forms many papers have appeared, the chief of which are those by Jones (*Quart. Journ. Geol. Soc.*, 1884), on the Richmond Well; by Jones and Sherborn (*Proc. Bath Field Club*, 1888), on the Fuller's Earth; by Jones (*Geol. Mag.*, 1878, p. 103, &c., and 1888, p. 534, &c., and *Quart. Journ. Geol. Soc.*, 1886), on the Purbeck and Wealden; also Jones and Hinde (*Palæont. Soc.*, 1890) on the Cretaceous in general. This last monograph was a second and improved edition, so to speak, of Professor Jones' original monograph of 1849, and, besides revising old work, brought the subject right up to date; a separate paper (*Geol. Mag.*, 1893) by Frederick Chapman and Sherborn, dealing with the Gault of Folkestone zone by zone, completed the history of the Cretaceous forms as known up to that time. Among these Mesozoic Ostracoda there is one single form which seems to be an absolutely safe zonal guide, and that is *Cypridea granulosa* (Sow.) the *Cypris fasciculata* of Edward Forbes. It has never been found anywhere but in beds of Middle Purbeck Age, and there it occurs abundantly. Those forms with which it is associated pass up or down into the other beds; more extended collecting, however, may possibly show this species to be more generally distributed.

The Cainozoic Ostracoda, partly treated by Jones in 1850 (*Annals*, ser. 2, vol. vi.), were monographed by him in 1857 (*Palæont. Soc.*), and a revision of this work by Jones and Sherborn appeared in 1889 in the same publication. There are, of course, various separate papers concerning the group, but the information was all collected together in the supplemental monograph of 1889, and references were given to previous literature. The Pleistocene forms received adequate recognition from Dr. G. S. Brady, Mr. Crosskey, and Mr. David Robertson in 1874 (*Palæont. Soc.*); and the recent world-wide species have been described by the first of these authors in the Challenger *Reports*, the *Transactions* of the Zoological Society, and elsewhere; and those of all Western Europe by G. S. Brady and A. M. Norman in 1896 (*Sci. Trans. R. Dublin Soc.*).

Enough has now been said to give a clue to the literature of the subject and to show the necessity for further work. The valves of Ostracoda are so common in certain beds as to rank among the rock-formers, and they present usually a delicacy and intricacy of ornament and hinge structure which admits of their easy differentiation. The smooth forms present many difficulties; but, with care, specimens can be found which show the hingements, and thus permit determination of the genus, though the determination of the species is much more difficult. What is really wanted is careful collecting from some well-known section, or series of sections, of any geological formation, and this would be of greater importance than the casual description of various species taken at random from many localities, though the latter is, of course, a very necessary work.

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V.

Human Evolution.

I.—According to Mr. H. G. Wells.

MR. WELLS' contribution to this discussion in the *Fortnightly Review* for October, 1896, though interesting, appears to me more ingenious than convincing; and his conclusion that "man is still, mentally, morally, and physically, what he was during the later Palæolithic Period," may be attacked on both *a priori* and *a posteriori* grounds.

Mr. Wells' contrast of slowly-breeding mankind with quickly-breeding rabbitkind is undeniable so far as it goes: but does it go very far? His argument depends on the assumption that the plasticity—the tendency to variation—of mankind is no greater than that of rabbits; whereas, if the plasticity of man exceed that of the rabbit more than the fertility of rabbits exceeds that of men, it is obvious that a large part of Mr. Wells' argument vanishes at once. Now, I do not intend to support this proposition; but the onus of proof lies upon Mr. Wells, and unless he can demonstrate the negative, he must abandon part of his argument.

As a matter of fact there is a large amount of evidence of a general character that Mr. Wells will find it very awkward to deal with; for it is one of the fundamental positions of evolutionists that domestication—deprival of natural habitat and natural environment—is one of the most powerful causes of variation. Darwin's writings are full of evidence upon this point; and it is well known to experimenters that the first step in artificial selection is to cultivate or domesticate a plant or animal, since the constitution of the species is thereby so profoundly affected, its normal equilibrium is so much disturbed, that marked varieties may be expected to arise; and until varieties arise, selection, of course, cannot enter upon the scene. Now, seeing that no animal has been so long or so completely removed from natural conditions as man, we should *a priori* expect to find this species more variable, and more unstable in constitution, than any other.

Another potent cause of variation is cross-breeding. To this also no animal has been so long and so thoroughly subjected as man—a fact that renders Mr. Wells' case even weaker. But both the *a priori* difficulty, and the *a posteriori* difficulty that the evolution of man physically is proof of his wonderful plasticity, Mr. Wells ignores, while bringing forward an *a priori* argument dependent on the unproved assumption that man's constitution is stable and not prone to great variability.

But, setting aside all the foregoing considerations, is Mr. Wells' argument unassailable? No! for his conclusion that each of us *at birth* is scarcely distinguishable, mentally and morally, from Palæolithic Man, necessarily implies, either:—(1) That we are at least equally indistinguishable from one another, and that all the huge differences between modern man and man (of the same race) are attributable to differences in training and nurture; or (2) that there were differences between new-born Palæolithic babies, quite as great, and—with some allowance for comparatively recent psychical selection—proportionately as frequent, as those obtaining to-day.

The mere acceptance of the former alternative as the corollary of Mr. Wells' argument disproves his thesis; for that corollary is contradicted by every man's daily experience. In the moral sphere, everybody recognises that some children are born naturally good, and, though with a minimum of moral training, become the pioneers of moral evolution; whilst others are born naturally vicious, and, in spite of the most careful training, inevitably go to the bad. Even in the same family, and under practically identical conditions of training, we may find both black sheep and white. Mr. Wells' argument from the Wolf-Boys is futile, since there is no proof that a boy with a strong inborn moral bent would sink to the same level as the recorded Wolf-Boys.

In the intellectual ('mental') sphere, to accept this corollary would land us in results so obviously absurd that merely to mention them is sufficient; for it has become a truism that genius is inborn, and that no training, or want of training, can eradicate the inborn differences between a genius and a dunce.

The acceptance of the former alternative being thus fatal to Mr. Wells' argument, let us see what follows from the second. Here I am willing to concede him something in the intellectual sphere; for it has long been my conviction that some intellectual geniuses at any rate must have appeared from time to time among primitive races, and that this 'great-man-theory' may explain such discoveries as the boomerang and the use of fire. But from this admission to the assumption set forth above is too long a stride to be taken without a good deal more evidence than we yet possess. Of course, Mr. Wells cannot afford to admit that the mental differences between Palæolithic babies were similar in kind, but different in degree, to those between modern babies; for that would be to invoke the very potency of selection which his article was written to disprove.

Moreover, there are still two very awkward facts for Mr. Wells to reckon with. First, is it not a fact that individuality is less marked the lower we descend in the scale of civilisation, and that savages are far more innately alike in every respect than are civilized men; just as there certainly seems to us to be far more inborn individuality, and far less dead-level, among dogs than among sheep and rabbits? Secondly, the very conclusion that Mr. Wells controverts, is a necessary deduction from Spencer's definition of evolution; for, in the mental sphere, increased heterogeneity means increased individuality and decreased 'all-alike-ness.' Mr. Wells will therefore find it necessary to disprove a good deal of "First Principles," in order to clear the way for his own argument.

In the moral sphere, the case is a good deal worse for Mr. Wells. All the same difficulties must be met; and in addition there are several others. First, we have no such presumptive evidence for moral geniuses among primitive man as for intellectual geniuses, so that Mr. Wells must make even larger assumptions. Secondly, a good many, who might concede Mr. Wells his primitive intellectual geniuses, probably would agree with me in thinking it far more unlikely, in view of what we know of sociology and psychology, that there were such marked moral geniuses. Lastly, there is a very damaging fact that Mr. Wells has probably entirely overlooked, one that is, perhaps, fatal to his whole argument. Morality, that is subjective morality (the emotional impulse), is entirely a factor of the emotions; and the emotions are simply the psychical side of, broadly speaking, visceral changes. How potent a factor in morality is this visceral change, may be realised if we reflect upon the profound emotional differences between men and women. Let Mr. Wells carefully study Havelock Ellis' "Man and Woman," and he will see how strong is the case against him, and how vast a difference in moral impulse may reasonably be attributed to a comparatively slight alteration in man's visceral physique.

But even did we waive every objection, and grant Mr. Wells the second alternative corollary from his argument, how would this benefit him? His whole drift is that nurture does everything, and Nature almost nothing, for man's advance. How then does it help him to assume that moral and intellectual geniuses were as characteristic "sports" among our Palæolithic ancestors as among ourselves? For, were this the fact, there were no need of Mr. Wells' alarmist article; and he would be only endorsing the oft repeated warning that we ought carefully to breed from our most gifted stocks, and strive to repress the great increase of the worthless.

Several of the arguments here only outlined are set forth more fully in chapter vii. of my "Towards Utopia" and in the *Free Review* for February, 1895. To Mr. Francis Galton's works on heredity I attach so great value that I venture strongly to commend their study to Mr. Wells and those interested in his argument.

In conclusion, I will only add that Mr. Wells would perhaps make out a far stronger case were he to invert his argument; and, instead of maintaining that, because we do not differ at birth from Palæolithic babies, therefore we are devoted to savagery unless saved by nurture, to urge that the progress already made proves our descent from a gifted stock innately destined to great things. He himself, curiously enough, admits racial differences among mankind, and he can find a mass of arguments in Galton's writings, in the results of recent archæological research, and, among other treatises, in Weismann's "Essay on the Musical Sense." I should be interested to see what Mr. Wells' ingenuity could effect with this inverted argument. After all, what we primarily need is ample discussion, from every point of view, of our anthropological data.

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II.—According to Mr. G. Archdall Reid.[1]

To choose a title that shall adequately cover, and no more than cover, the subject of which an author treats is certainly a difficult task. Mr. Reid has failed to perform it. His volume entitled "The Present Evolution of Man" contains a first portion, dealing with Organic Evolution, to which the title gives no clue, while only in the second part of the work is the subject really discussed. On the other hand his title indicates a vast subject of which he considers only a small part. Man's present evolution is dealt with under two subheadings, Physical and Mental: the former concerned only with man in reference to disease; the latter, with man in reference to indulgence in stimulants and narcotics. This is certainly a very partial treatment of a subject with such possibilities as the present evolution of Man.

Mr. Reid has somewhat mistaken the term "Evolution." In one case he says (p. 7) "the process of evolution has generally been in an upward direction;" but in other cases he uses the term evolution in opposition to "retrogression." The second use is at variance with the first; and it is incorrect. Evolution, the act of unfolding, is strictly applicable to all the phenomena connected with the succession of organic life, as it has unfolded, does, and will unfold itself.

This unfolding has been in the form sometimes of continued progression, sometimes of retrogression after progression, and frequently of progression in certain features combined with retrogression in others. "Evolution," therefore, does not, as Mr. Reid seems to think, imply only progress.

There is yet other misuse of terms, as in such sentences as these (p. 21): "In every species natural selection as a cause of evolution [progression], and atavism as a cause of retrogression, are constantly at

[1] "The Present Evolution of Man." By G. Archdall Reid. Pp. 370. London: Chapman and Hall, 1896. Price 7s. 6d

war. If the one force predominate we have evolution, if the other, retrogression; if the two forces balance one another, the species in which this occurs undergoes for a time neither evolution, nor retrogression." To speak of natural selection as a cause of progression, is certainly a debatable point; but atavism cannot be called a cause of retrogression. It is a phenomenon of retrogression; and it is certainly not a force. Atavism, indeed, is a much misunderstood term. Some writers have called the features of the normal infantile stages of ontogeny, such as monkey-habits of a child, atavic; which is clearly incorrect. Mr. Reid describes atavism as "a failure to recapitulate in the ontogeny the last stages of phylogeny—it is an arrest of development." (p. 47.) He thinks that he is in agreement with Herbert Spencer, who, however, defines it as a recurrence of ancestral traits. But particular facial features appearing in an individual, after having missed several generations, need not necessarily be arrest of development. That would leave a man of our race with a negroid type of nose, such as is common to all young children. It requires a more extended development than obtained in the immediate ancestors to reproduce that highly specialised type of nose, which attracts attention as a characteristic of certain families. Again, another phenomenon often called atavic is the return to ancestral features exhibited in senility, for instance, loss of teeth in man and increasing tendency to a hairy body. For such phenomena there has been proposed the term hypostrophy, which is a truer rendering of the facts as gathered from palæontology.

Rightly enough, Mr. Reid insists that natural and artificial selection are but terms for different exhibitions of one phenomenon, and that man himself is as subject to the laws of evolution as is any other organism. There are naturalists, who so far belie their name, that as soon as they consider man, and what man does, they assume super-natural agencies. Mr. Reid does not make this mistake. Man, indeed, is a most potent influence in modifying the environment of all species, his own included; but though alteration of the environment changes the manner of evolution, it does not stay its action. Man's reason has caused him to cultivate, and so give special forms to, certain species; but in the main his reason has made him the most formidable beast of prey that has ever appeared on the earth. Other animals have taken life to sustain their own. Man does more: he takes it ruthlessly for the gratification of his vanity, for the mere sake of destruction, or from religious zeal. Man's reason has profoundly altered the conditions of environment the world over; and it has been the cause of the greatest misery to the organic world, himself included.

To turn to the first part of Mr. Reid's work, dealing with organic evolution. This is discussed on the assumption of the non-inheritance of acquired characters. "Biologists generally," says the author, "are gradually veering round to this opinion." This may be doubted. Certainly palæontologists favour the Lamarckian teaching, modified by a doctrine of the appearance of characters gradually earlier in each generation, which is, in fact, a necessity of ontogenetic recapitulation. And it must be confessed that palæontologists have exceptional facilities for observation.

Mr. Reid speaks of "spontaneous variations, caused we know not how" (p. 12). True, we may not know how all variations are caused; but this is no reason for regarding any as spontaneous. It seems safer to suppose that all variations could, if our knowledge were sufficient, be traced to some cause in the environment. For instance, the crumpling of our toes is assumed to result from the wearing of boots. And the character is transmitted. Two of my children, born with straight-toed feet, which would not disgrace a savage, and kept bare-footed for this very observation, showed a crumpling in of the toes before boots were taken to at all.

There are other causes of variation. I have noted in my own children that a short period between one birth and another has unfavourably influenced the offspring; that change of scene by the mother has been favourable; that a vegetarian diet during gestation produced a very small child; that change back to a meat diet produced the largest child of the family; and lastly, visits to the Inventions Exhibition by the mother during the period of gestation resulted in a child which, unlike the others, has shown, since infancy, a remarkable mechanical proclivity. Now such variations might by a casual observer be called "inborn"; they would be classed as "spontaneous variations, arising we know not how."

Mr. Reid does believe in the transmission of acquired characters in the case of unicellular organisms (p. 134), and this, perhaps, explains the whole matter. The germ is a unicellular organism, and therefore it should be modifiable in accordance with its environment. Such environment would be different in the body of a sedentary clerk, and a hard-working agricultural labourer; and on this hypothesis the offspring in these cases would be different.

In the special section (Part II.) of his book, Mr. Reid first deals with man's evolution against disease. He says that man improves in the face of disease, that the more he is tried by it, the more he is able to resist, that races which have never known particular diseases, such as measles, are killed, like flies on the approach of winter, by what is to us a mild complaint. This looks remarkably like inheritance of acquired characters. Mr. Reid says it is due to elimination of the unfit. But that, if it were the sole cause, should leave a race better able to resist disease all round. This is not the fact; it is only the individual that is better equipped against such diseases as he has had particular struggles with. This seems as though he were influenced by each struggle, and as if his disease-resisting powers acquired, by use, greater strength of coping with those diseases of which they had knowledge, while not resisting those in which they could get no training; and these characters are transmitted to offspring.

In dealing with man's evolution against stimulants, the author takes a similar line of argument: that those races which have the most experience of alcohol are best able to resist it;—he regards this as due to elimination of the unfit. He gives statistics to show that abstainers live longer than non-abstainers; but they prove nothing; they certainly do not show that thereby abstainers become more numerous. Let us take a case: there are women who abstain from childbirth, others who do not abstain. It is certain that the abstainers live the longer, because the risks of childbirth are many, and result in the loss of many lives. Therefore, in course of time, by the elimination of non-abstainers, only abstainers should be left. It is a *reductio ad absurdum*; but it signifies that tables of comparative longevity do not affect the matter, unless it can also be shown that those who live longest produce most children. This does not follow in the least; and again it is highly probable that non-abstainers are more productive than abstainers. We have evidence on this point. "Total abstinence is more practised by the steadiest men in other respects, though it is also true that the fact of abstaining assists in promoting continence." (p. 322). On the other hand, alcohol certainly stimulates the sexual instinct. Agricultural labourers indulge very largely in drink; and at the same time, they have very considerable families.

Anyone who has paid attention to the population question, who knows the early marriage-rate of farm servants, and their large families, has necessarily come to the conclusion that they produce far more than their share of the next generation. This means that the professional classes must be largely recruited from time to time from those whose forbears were farm servants a few generations ago; and we know that such is a fact. We have no data in this country to show what does happen; but Karl Pearson has proved that in Copenhagen the artisan class beats the professional, and that "the population would appear to be ultimately reproducing itself from the artisan class" (Nat. Sci., vol. viii. No. 51, p. 325, May, 1896). In this country the surplus from the farm-labouring class of each generation joins the artisans. Professor Pearson's statement thus confirms our surmise, and it really means that the greater bulk of our population is being reproduced by those classes who indulge the most in alcohol. The temperate middle and professional class is known to marry latest of all, and it is by no means prolific. Such considerations cut the ground away from many of Mr. Reid's arguments concerning man's evolution in respect of alcohol.

It is strange that a work entitled "The Present Evolution ot Man" should take no account of the reproduction question. For instance, there is at the present day a sudden decrease in the birth-rate—stated recently for London as 15 per cent. below the decennial average, which itself has been growing less. This implies that the population is less by hundreds of thousands, and it undoubtedly corresponds with the rapidly-spreading knowledge of chemical means for checking fecundation. However beneficial a small family may be to the individual, it is not to the good of the race; in fact, all evolution shows that the good of the race and of the individual are antagonistic. The less fertile race will inevitably succumb to the more fertile, and this decrease of the birth-rate heralds its disappearance. It is remarkable that a book dealing with the evolution of Man says nothing of a factor more important than any of which it treats.

However, Mr. Reid's work does well to call attention to man's evolution in certain respects. He says it is a book for the general public, and to that public it may be commended. Unfortunately, the taste of the general public is for works on evolution that show bad science and worse argument, well spiced with theology, and Mr. Reid's book is certainly not of that kind: it leaves theology alone. One may dissent from some of his statements and many of his conclusions; still he has a right to the position he takes up, while his arguments deserve and require careful consideration. His attempt to arouse public attention is praiseworthy, and deserves success.

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VI.

Science in New Zealand.

A RECENT number of *The Press* of Christchurch, New Zealand, contains an interesting article by Professor Arthur Dendy, on the present position of science in New Zealand. It is very largely a lamentation over the apathy displayed by the members of a new community. "Our best and most promising University Students," says the Professor, "are obliged to leave us in order to find a market for their intellectual wares, and those few scientific men who have taken up their abode in this colony must still look mainly to Europe and America for an audience." The comparative isolation of the few centres of intellectual activity in the islands is also a great hindrance, since there can be no central library or museum readily accessible to all workers, and funds are not forthcoming to establish such at more than one point. The New Zealand Institute, as the representative scientific body of the colony, is thus seriously handicapped in its career of usefulness compared with the similar institutions in the other Australasian colonies. Instead of being one flourishing and influential society, with first-class accommodation for the meetings and for the library in the capital city, the Institute consists of a "Board of Governors," most of whom are nominated by the Government, and a "Manager," with a number of "incorporated," but practically independent, local Societies in Wellington, Auckland, Canterbury, Otago, Westland, Hawke's Bay, Southland, and Nelson.

It is, indeed, "to a certain extent, a Government institution, and as such receives an annual subsidy from Parliament of some £500, which just about pays the expenses of publication of the annual volume. The incorporated Societies secure the advantage of having their proceedings, including such papers as are deemed worthy by the authorities, published and distributed free of cost to themselves, so that the whole of the annual income of each is available for local purposes. That this is a great advantage cannot be doubted; but, at the same time, it is an arrangement which has, at any rate from the point of view of the Incorporated Societies, serious drawbacks, especially with regard to what may be called the Library Question. Out in these colonies, remote from all the great centres of intellectual activity, the scientific investigator is peculiarly dependent upon the literature of his subject for keeping himself abreast of the times, and it is therefore of the greatest importance that the Society to which he belongs should maintain its library in as efficient a state as possible, for in the great majority of cases he is not himself in a position to spend much money in the purchase of costly books. Most learned societies keep up their libraries very largely by means of the exchange of their own publications for those of similar institutions throughout the world. In this colony all the advantages of such a system of exchange are confined to Wellington, the headquarters of the Institute, and the local societies outside Wellington profit nothing thereby. It is true that a rule provides for the admission of the public to the use of the Museum and Library, subject to bye-laws to be framed by the Board, but this privilege can be of little value to any but residents in Wellington. The difficulty might, perhaps, be to a certain extent obviated by printing and circulating a catalogue of the Library, and making arrangements whereby members of the affiliated societies could have books posted to them."

"If the support given to the local branches of the Institute can be regarded as any indication of the degree of culture in the different centres of population in New Zealand, we may arrive at some interesting results from a comparison of the lists of membership. Thus we find Auckland first with 172 members, Wellington 144, Otago 94, Hawke's Bay 80, Canterbury 70, Westland 62, and Nelson 24, a result which certainly would not be expected by any person conversant with the higher educational institutions in these centres, and especially with the working of the University Colleges. The degree of popularity and success of the University Colleges appears to have no relation whatever to the prosperity (as judged by membership) of the local branch of the Institute. If, however, we make the comparison in a different manner, by observing the amount of original research emanating from each branch, I think the University centres may fairly claim to have done their share."

The most galling thought to an enthusiast like Professor Dendy is that, while he and his colleagues reside in an almost unique country for opportunities of scientific research, they are "almost forced to neglect them for want of encouragement." Meanwhile, "scientific men from Europe and America, often assisted by a grant from some learned society, hail with enthusiasm every chance of visiting the Australasian colonies, where they remain for a few months collecting, and then return home laden with spoil to work out their results at leisure and under the most favourable conditions." At the same time, even Professor Dendy himself admits that circumstances are improving. The Canterbury College, for instance, has lately exhibited noteworthy progress, and the Canterbury Museum is now one of the finest institutions of its kind in Australasia. Nor must we forget Dunedin, which has done so much for the progress of natural science in recent years. We have Professor Dendy's "man in the street" in this country, and no inconsiderable development of that relic of barbarism. Let us hope that culture in other quarters may soon as completely overpower him in the remotest colonies of Australasia as it has done in the older centres of civilisation.

SOME NEW BOOKS.

Worms, Rotifers, and Polyzoa.

The Cambridge Natural History, edited by S. F. Harmer and A. E. Shipley, vol. ii. Flatworms and Mesozoa, by F. W. Gamble; Nemertines, by Miss L. Sheldon; Threadworms and Sagitta, by A. E. Shipley; Rotifers, by Marcus Hartog; Polychæte Worms, by W. B. Benham; Earthworms and Leeches, by F. E. Beddard; Gephyrea and Phoronis, by A. E. Shipley; Polyzoa, by S. F. Harmer. 8vo. Pp. xii, 560, with numerous illustrations. London: Macmillan and Co., Ltd., 1896. Price 17s. nett.

This second volume of the Cambridge Natural History is certain to prove a most welcome addition to English zoological literature. It deals with a series of animal groups, all deeply interesting to the specialist in morphology; some important from their economic relations to other living things, others in their life-histories rivalling the marvels of fairy-tales. And the style in which they are here treated is also interesting: history and the early observations of the older writers lend their charm; accounts of habits and mode of occurrence, of life, in a word, from the cradle to the grave, are given in ample detail, accompanied by full references to modern and current literature. The whole is admirably illustrated: the figures, as may be seen from those kindly lent us by the publishers, well chosen, well executed, clear and of excellent size. British species are freely alluded to;

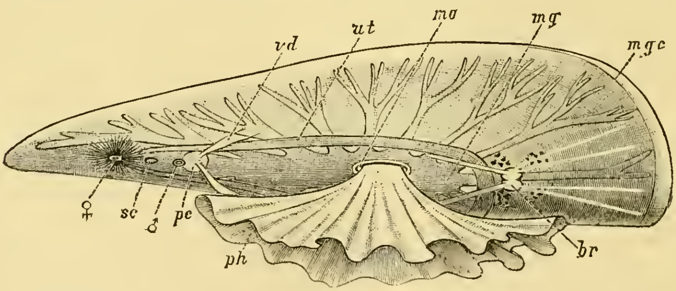

Fig. 1.—*Leptoplana tremellaris.* Example of a Polyclad Turbellarian. Three-quarter ventral view, ×6. *ph*, pharynx protruded from *mo*, mouth; *br*, brain; *mg*, stomach; *mgc*, marginal groove; *pe*, penis; *sc*, sucker; *ut*, uterus; *vd*, vas deferens; ♀ female genital aperture; ♂ male ditto.

indeed the account of the Polyzoa is avowedly based solely upon them. The work will appeal to the student working for examination, but it ought also to stimulate in the amateur naturalist an intelligent interest in animals too much neglected and ignored by him, usually because short yet comprehensive and not too technical accounts of their structure and classification are unavailable. The want is now charmingly supplied. Nowhere, save in monographs, will the lover of pond-life find a better description of the Rotifera, or the wanderer by the sea-shore of the Polyzoa. Indeed, help in relation to these sprightly beings is extended to include methods of preservation. The index, to crown all, is a thoroughly good and full one.

The volume opens with the flatworms or Platyhelminthes,—a phylum with three branches, Turbellaria, Trematoda, and Cestoda, organised on a similar and very distinct plan. The Turbellaria are dwelt on at some length as occupying "the lowest position in the whole group of worms," and "most closely allied to that great extinct

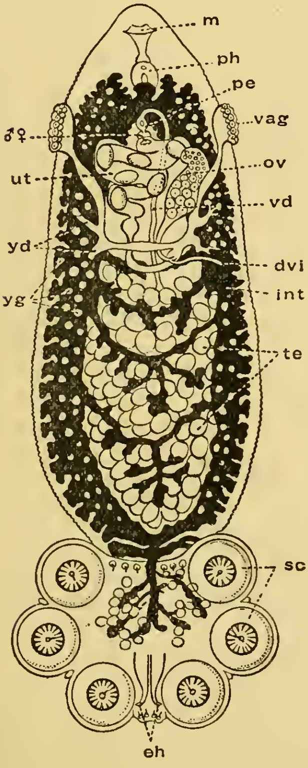

FIG. 2.—*Polystomum integerrimum*, a Trematode from a frog's bladder. Ventral view, ×8. *dvi*, ductus vitello-intestinalis; *eh*, hooks of sucking disc; *int.* intestine (all black); *m*, mouth; *ov*, ovary; *pe*, penis; *ph*, pharynx; *sc*, suckers; *te*, testes; *ut*, uterus with eggs; *vag*, left vagina; *vd*, vas deferens; *yd*, yolk-duct; *yg*, yolk-glands; ♀ ♂ common genital aperture.

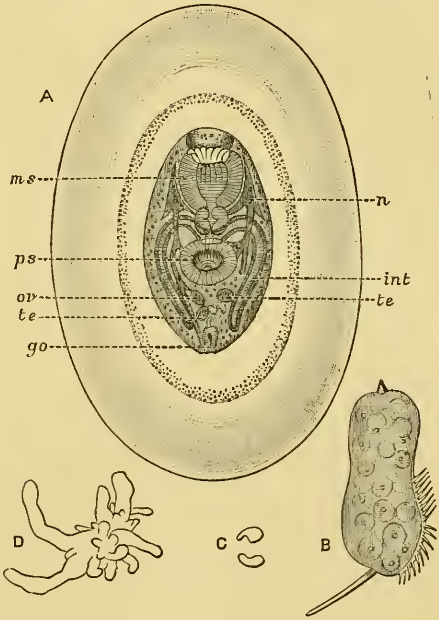

FIG. 3.—Life history of *Distomum macrostomum*, a Trematode. A, tailless cercaria from a *Leucochloridium*, × 40; B, larva as hatched from the egg, ×125; C, metamorphosed larva (sporocyst) that has pierced intestinal wall of *Succinea*; D, actively growing sporocyst, or *Leucochloridium*; *go*, genital aperture; *int*, intestine; *ms*, mouth sucker; *n*, nervous system; *ov*, ovary; *ps*, ventral sucker; *te*, testis.

group, from which they, the Nemertinea, Rotifera, and even the Annelids" appear to have been "derived." A full description of the Polyclad, *Leptoplana tremellaris* (fig. 1), with good figures, is given; and a no less good figure of the anatomy of the Triclad, *Planaria lactea*. The division of the Trematoda into Mono- and Di-genea is retained, though with a protest (p. 63). There is an excellent account of the Turbellarian-like Monogenetic family, the Temnocephalidæ, which has no British representative. Mr. Gamble believes in the existence, repeatedly denied, of a ductus vitello-intestinalis uniting uterus and intestine in *Polystomum* (fig. 2), and in its homology with the Laurer-Stieda canal of Digenea. The life-history of the Digenea is exemplified by that of *Distomum macrostomum* (fig. 3) and its sporocyst *Leucochloridium paradoxum*, which, by the way, has been found in two localities in Oxfordshire. The selection is unfortunate, for the sporocyst is one of the two most peculiar known, utterly unlike the ordinary form; and the cercaria is not typical. The anatomy and ordinary form of the sporocyst and cercaria are not described, and the redia is only cursorily mentioned. Yet sporocyst and redia are dimorphic forms of fluke, with a by no means simple structure. A useful table of life-histories is given, and a similar one for certain Cestoda, as well as a second table for the identification of the tape-worms inhabiting man and the domestic animals. Mr. Gamble evidently leans to the view that there is no alternation of generations in the life-history of the majority of tape-worms. Scolex and proglottides are parts of one individual. To the facts adduced it might be added that in *Ligula* and *Schistocephalus* it has been said that the vitellaria, their ductules, and the array of testes are continuous throughout the body; so too with the germaria of *Ligula*. In *Tænia echinococcus* the vesicular embryo multiplies *quâ* vesicle, a number of scolices arising from the secondary, etc., vesicles; though *T. cœnurus* rarely multiplies as a vesicle but always develops many scolices, the two species are regarded as "instances of an alternation of asexual generations in the larval state with a sexual adult state."

The Mesozoa of E. van Beneden are treated as an appendix to Platyhelminthes, as they are by other authorities. It may be conceded there is no proof of the Dicyemidæ and Orthonectidæ being forms of life intermediate between Protozoa and Metazoa: there is equally no proof that they are related to one another or that they are derived from platyhelminthic ancestors, simplified by an extreme degeneration due to parasitism. Before a sporocyst was transformed into a dicyemid, its structure must have been sublimed out of recognition.

In her account of Nemertinea, Miss Sheldon has been able to use the results of Bürger's recent monograph only in the modified classification and the account of the excretory organs with their terminal and platyhelminthic flame-cells. Nothing is said as to the nature of the blood-vessels, whether cœlomic or not. The land and fresh-water forms are enumerated, and in part described, together with the parasitic (?) leech-like *Malacobdella*. Detailed reference is made to the views of those who find features of alliance to the Chordata.

The old name Nemathelminthes unites the Nematoda (thread-worms), Nematomorpha (*Gordius*, *Nectonema*), and Acanthocephala. It is an echo of Platyhelminthes, and its use, therefore, is to be deprecated. Mr. Shipley does not base the collocation on phylogenetic grounds, but on convenience and a greater similarity between the three groups *inter se* as contrasted with other groups of animals. Camerano's argument as to the similarity of the "chief details in the fertilisation aud development of the egg" of *Gordius*, with the similar processes in Nematoda, does not outweigh considerations of anatomy; and if Kaiser's description of the nephridial organs of *Gigantorhynchus gigas* be correct, the Acanthocephala must certainly be severed from the two other groups. An unfortunate error has crept into the account of *Rhabdonema nigrovenosum*. The parasitic generation in the frog's lung is a female in structure, but, physiologically, a protandric hermaphrodite: it is not the case that no females occur in the alternate generation (p. 151), or that there is parthenogenesis (p. 161).

The Rotifera have always proved attractive to the non-professional naturalist. There are, however, certain difficulties in their anatomy, and these are clearly elucidated in Professor Hartog's account of the class. The varieties of trochal disc, the structure of the mastax, reproduction, and habits are fully dealt with. The classification is modified somewhat from Hudson and Gosse's, and the Professor broaches a new and interesting idea as to the zoological affinities of the class, bringing it "into close relationship with lower Platyhelminthes," especially the Pilidium larva; he rejects any affinity to Crustacea.

The four orders, Archiannelida, Polychæta, Myzostomaria, and Oligochæta, are ranged by Dr. Benham under the term Chætopoda: he deals with the first three. The remarkable genus *Dinophilus* is included, apparently with some reluctance, in the order first-named; *Saccocirrus* is omitted, and the interesting *Histriobdella homari* relegated to a foot-note. In the account of *Polygordius* no mention is made of the development of the excretory system, neither is its trochosphere figured; indeed, this important larval form is nowhere treated adequately in this volume.

Nereis pelagica (fig. 4) is described as a type of the Polychæta, but the description is too much interrupted by facts of general anatomy.

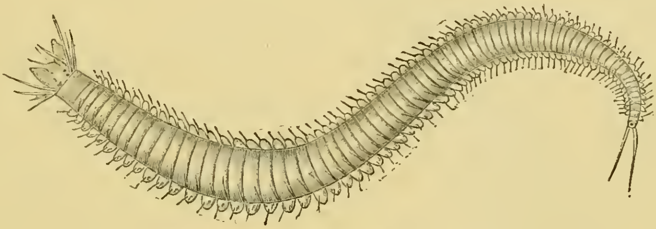

FIG. 4.—*Nereis pelagica*, nat. size.

The classification of the order adopted is the one proposed in 1894 by Dr. Benham; but any difficulties owing to its recent character are minimised as far as possible by the full account to which chapter xii. is devoted. Certain errors (*inter alia*) in general anatomy may be noted. The septa of the body are not so complete as to isolate the compartments from one another, and the parapodia are essentially hollow organs, facts not explicitly stated; prostomial tentacles are probably not restricted solely to the sub-order Nereidiformia, *e.g.*, according to Mayer, the tentacles of *Spio* may be prostomial; the modified chætæ of *Capitella* are confined to the male, and modified anterior chætæ occur in other Polychæta; all polycirrids are devoid of a vascular system, and, in *Capitella*, some, not all, of the cœlomic corpuscles are red; the siphon is found in some Eunicidæ as well as in *Capitelliformia*; the longitudinal nephridial duct of some Terebellidæ, *e.g.*, the common *Lanice conchilega*, is omitted; *Capitella* has one pair of gonoducts, other genera of the family more than one pair; *Haplobranchus* is not hermaphrodite. A whole chapter (chapter xi.) is devoted to a *résumé* of the natural history of the order.

Turning to the Oligochæta, the order is for once treated with complete justice, and by its monographer, Mr. Beddard; the Hirudinea are brought into intimate relation with it, the differences between the two being, as he says, neither so "numerous" nor so "important" as they are usually held to be. It may be noted that the stomatogastric nervous system of Oligochæta is often connected to the œsophageal commissures, and the chambers of the body are not completely isolated *inter se* as might be inferred from the language of p. 355. The spermatophore of the order is mentioned under Hirudinea (p. 402). Goehlich's observation on the nephridia, reproductive habits, and cocoon of the earthworm should have found a place. It is a pity Mr. Beddard did not figure *Chætogaster*, *Nais*, or *Tubifex* as common British types, or give details as to the fission of aquatic forms. The circulatory system, cœlomic spaces, and genitalia of Hirudinea are clearly explained. There is a brief reference to the

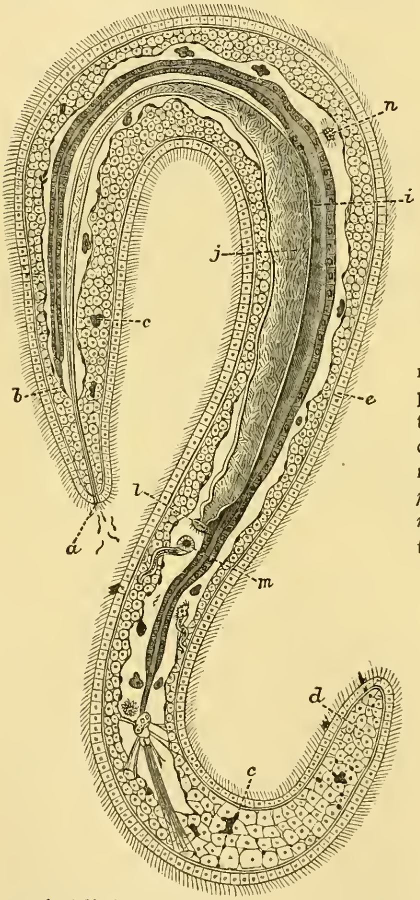

FIG. 5.—*Bonellia viridis*, a Gephyrean, adult male × 133. *a*, generative pore with spermatozoa issuing; *b*, anterior end of intestine (*i*); *c*, wandering cells containing chlorophyll; *d*, parenchymatous connective tissue; *e*, epidermis; *j*, vas deferens opening internally at *l*; *m*, "left nephridium" (Shipley, but see text); *n*, spermatozoa in body-cavity.

nephridial network of certain leeches. Mr. Beddard probably considers it, as in analogous instances among Oligochæta, to be a development from simpler conditions.

Sipunculus nudus is described as a type of the Gephyrea, and Mr. Shipley has given a welcome key to the many genera of the Sipunculoidea. An error should be noted in the description of the figure of the male *Bonellia* (fig. 5): the letter *m* refers to what is really the homologue of the left anal vesicle of the female; the homologue on the right side is also shown. The remarks on the affinities of the Gephyrea should be carefully studied. The same advice applies equally to the interesting genus *Phoronis*, of which an excellent description is given.

The Polyzoa conclude the volume. Mr. Harmer bases his account on British species solely, regarding them as amply representative, a statement which hardly applies to the Cyclostomata. The controversy as to the name of the class is alluded to; many English writers are certainly inclining to Ehrenberg's name Bryozoa. The Entoprocta are rightly retained as one of the two chief divisions of the class. The brown body of the Ectoprocta, the derivation of the avicularium and vibraculum, and the morphology of the larva are explained; and the view that the fresh-water Phylactolæmata are not primitive forms, as usually stated, is put convincingly. The references to the palæontology of the class are scanty: *e.g.*, Ulrich's work on American Palæozoic forms is passed over; Walford's results are accepted as trustworthy, whereas he seems to have mistaken accidental scratches and the like for structural features; existing families of Cyclostomata commence *before* the Jurassic period, even in British Silurian and American Carboniferous strata. There are two useful tables for the identification of British fresh-water and marine species respectively. As to the latter, the student should bear in mind the caution given on pp. 515–16 concerning the form of the colony, especially as to encrusting and erect forms. Note that *Epistomia*, and not *Notamia*, is the correct generic name for the species *bursaria* (p. 526).

Consideration of the whole volume suggests a few general remarks. There is a certain vagueness, sure to puzzle the non-professional student, in the use of the terms *body-cavity* and *cœlom*, and the connections in which they occasionally appear. Not that it is confined to this work; still, it could easily be avoided. The absence of brief definitions to the principal groups, such as are given in the companion volume on Arthropoda, is a misfortune. They certainly conduce to clearness of ideas, and are indispensable in certain stages of knowledge. Classificatory terms are also used in a somewhat haphazard fashion. For instance, the Platyhelminthes constitute a *phylum* with three *branches*, whilst the Nemathelminthes are not designated, and are divided into three *sub-orders*; the Rotifera and Polyzoa are *classes*; the Chætopoda are not designated, and are divided into four *orders*, one of which is the Oligochæta (p. 241); but the Oligochæta on p. 347 are termed a division of the *order* Chætopoda, whilst the Hirudinea, apparently co-ordinate with Oligochæta, are divided into two *sub-orders*. Such things ought not to be. Finally, it is remarkable how sparing is the use of the word "worm," whilst the term Vermes—the term under which the whole contents of the volume were at one time classified—is mentioned, and with a touch of scorn, but once. A prefatory introduction would have been a good thing: it might have explained many essential points and removed difficulties.

Seasonal Dimorphism.

Neue Versuche zum Saison-Dimorphismus der Schmetterlinge. Von Dr. August Weismann. Abdruck aus den *Zoologischen Jahrbüchern, Abth. f. Syst.* Bd. viii. Jena: Fischer, 1895.

New Experiments on the Seasonal Dimorphism of Lepidoptera. By Dr. August Weismann. (Translated from the German by W. E. Nicholson, F.E.S.) Reprinted from *The Entomologist*, Jan.–Aug., 1896.

Dr. Weismann's essay should be another reproof to those ill-informed persons who cry out on its author as a pure speculator. The misconception is natural; for his theories have reached everyone, even politicians, while the exact laborious work that has accompanied each theoretical advance has been of a character intelligible only to specialists. The essay now before us gives the results of a series of experiments conducted at intervals, since 1875, upon the conditions determining the seasonal and climatic variations of butterflies. The earliest results were translated into English by Professor Meldola, and appeared among the "Studies in the Theory of Descent" in 1882; the present essay has been well translated by Mr. W. E. Nicholson. The existence of these translations is particularly fortunate, as by them many English entomologists may be stimulated to assist in the important work of collecting additional experimental observations on these curious occurrences. Much good work of this kind is well within the opportunities of the ordinary or field collector, and would afford him nearly as much pleasure as the capture of rare duplicates for exchanges.

The general nature of the circumstances under investigation is as follows: many lepidoptera exhibit, apart from sexual dimorphism, a dimorphism related to climate or to seasons. Some European forms, for instance, have Polar varieties notably different in colour and markings. Some temperate forms have southern, semitropical varieties equally different. Many have two broods; a spring brood resulting from eggs or pupæ that have hibernated, and a different summer or autumn brood. The spring broods tend to resemble northern forms; the summer or autumn broods resemble southern forms. In Lamarckian days most of these differences were set down naïvely to the direct effect of heat or of cold. Detailed experiments show that the explanation is not so fortunately simple; and Professor Weismann devotes his pages to describing his experiments, correlating them with those of others, and discussing their interpretation from the point of view of bionomics and heredity.

In 1875 he assumed that it was to a certain extent obvious that seasonal dimorphism was the direct result of climatic influences—chiefly of heat and cold. He suggested even then the possibility that the seasonal changes were adaptive, like the seasonal changes of Arctic mammals, with the difference that the changes did not occur in one and the same individual, but alternately in individuals of different generations. However, the upper side of butterflies, which is not usually adaptive, varies as much as the adaptive under side in the two generations: moreover, by artificial subjection to a higher or lower temperature, the stamp of the winter form may be impressed on the summer brood, or *vice versâ*. It seemed plain that the measure of heat acting during the pupal period directly formed the species in one way or in the other.

Now, however, he is inclined to the view that there is *adaptive* seasonal dimorphism in addition to *direct* seasonal dimorphism. There are cases known, for instance, in which one seasonal form differs from another, not merely in colouring, but in the presence of complicated details of marking and colouring, such as ocelli; and it cannot be thought that heat or cold, drought or humidity, are the direct causes of these. Professor Weismann thinks that in the embryo there are present the germinal constituents for both forms: the seasonal or artificial change acts as the stimulus to liberate and bring into activity one or the other set. In any particular case it may be that both direct and adaptive dimorphism is present. Very many carefully devised experiments, and the fullest observation of the different forms, alive in their natural surroundings, are required for the discrimination. Professor Weismann discusses his own experiments and those of others so clearly and fully that anyone, with the taste and opportunity, who reads the pamphlet is like to busy himself at once with some contributions of his own to the problem. We congratulate the conductors of *The Entomologist* on placing before their readers so stimulating a piece of work.

The Common Drone-Fly.

The Natural History of *Eristalis Tenax* or The Drone-Fly. By George Bowdler Buckton. 8vo. Pp. iv., 91. With illustrations, 9 pls. London: Macmillan & Co., 1895. Price 8s. nett.

The drone-fly is among the largest of British Diptera, and fulfils one of the first requirements in the case of an animal selected as a type for scientific study, in that it is exceedingly common. The larva of the insect is of remarkable structure, and the fly itself is peculiarly interesting, especially on account of its deceptive resemblance to the hive-bee. It will therefore be understood that in the hands of a competent writer, the drone-fly might be made to render a real service, not only to dipterology, but to comparative entomology besides. Alas for our fond hopes! The "Natural History of *Eristalis tenax*" has yet to be written.

To deal satisfactorily with the natural history of even a single insect within the limits of some ninety pages of large type demands much careful study, not only of the insect itself, but also of other forms belonging to the same order, not to speak of an intimate acquaintance with the work of previous writers. We are not surprised when Mr. Buckton tells us that his "special attention to *Eristalis*" dates from "the summers of 1893 and 1894." The whole book, including the illustrations, bears unmistakable evidence of hasty work, lack of thoroughness, and want of grip, while disjointed remarks and hazy speculations, couched in high-flown phraseology, are too often made to do duty for a concise and suggestive statement of facts.

The pages devoted to Introduction and Classification contain so many errors that we can only allude to a few of the more glaring. On page 4 and elsewhere, the author perpetuates the time-honoured myth that the larvæ of *Volucella* prey upon those of wasps and hive-bees. The larvæ of *Volucella bombylans* and of other species of the same genus are very common in the nests of *Vespa germanica* and in those of humble bees, certain species of which *V. bombylans* closely resembles; but it is now known that they act as scavengers, feeding merely upon the refuse of the cells, and not upon the progeny of their hosts. (See Natural Science, vol. ii., p. 54). At the foot of page 6 (where, by the way, the Oestridæ are twice called *A*stridæ), we have a table purporting to show the systematic position of *Eristalis* according to Walker, in which, while the Nycteribidæ are allowed to represent the Eproboscidea, the much better known family Hippoboscidæ apparently belongs neither to this section nor to that which is still blest with a proboscis. Why are Prof. Brauer's researches upon the classification of Diptera, and Williston's "Synopsis of North American Syrphidæ"—the most important revision yet attempted of the family to which *Eristalis* belongs—completely ignored?

On the following page we are told that in predaceous Diptera the *labium* "operates like a piercer," and a little later that "the imagos of Diptera possess no mandibles." Statements of this sort simply take one's breath away, and in quoting them it is difficult to refrain from copious notes of exclamation. "*Eristalis arbustorum*, Linn." is quoted quite correctly on page 8 as the name of a distinct species, but on the following page it appears as a synonym of *Eristalis tenax*.

Mr. Buckton's description of the imago of *E. tenax* would certainly not enable anyone to recognise the fly, and he makes no mention of the two vertical stripes of hair on the eyes, which alone are sufficient to distinguish the species from any other member of the genus. In the description of *Eristalis arbustorum* on page 15, we are told that the eyes are "approximate at the vertex," although this species is no exception to the rule that in the majority of what we may call ordinary Diptera, the eyes in the female are wide apart.

The chapter on "Life History" is quite the best portion of the book, and we gladly recognise that the author has endeavoured to study the living insect. But again we find serious errors of commission and omission. *Pipunculus*, which belongs to a family nearly allied to the Syrphidæ, is called a *gnat* (p. 19); the author probably intended to write *Chironomus*; the larva of *Eristalis tenax* is said to possess eight pairs of false feet, and that of *E. arbustorum* seven (p. 21), which is exactly the reverse of what we were told seven pages previously. Moreover, considering the title of the book, we think we are justly entitled to complain that while nearly forty pages are devoted to anatomical details, the life-history of the insect (if we exclude four pages on the "Bugonia" myth later on) is dismissed in less than twelve. We fail to find any reference to the protective value, or otherwise, to the insect itself of its remarkable similarity to the hive-bee, and the author does not appear to have made any experiments upon the subject.

With regard to the anatomical portion of the book, the author certainly seems to have plied scalpel and dissecting-needle assiduously, but his observations are too often confused and superficial, he fails to grasp the really important points of his subject, and he manifests throughout a sadly imperfect acquaintance with the work of previous writers. In his account of the internal structure of the eye he makes no reference to Hickson, in dealing with the halteres and their function he omits to allude to Jousset de Bellesme's book upon the subject, and in his remarks on the nervous system he says nothing about Brauer's memoir. Moreover, we are apt to distrust the author's statements as to obscure points of internal anatomy when, with regard to well-known external details with which every entomologist is familiar, we find such flagrant misstatements as:—"The metathorax is . . . . . . principally represented by the scutellum." . . . . . . "The wings are affixed to the metanotum." The account of the structure of the proboscis, too (pp. 35–37) is quite unintelligible, and the figure on Plate V. merely serves to make confusion worse confounded.

The pages headed "Development of Eristalis" are found on examination not to refer to the ontogeny of the insect at all, but to consist for the most part of vague musings upon the ancestry of the Tracheata. The value of Mr. Buckton's reflections may be deduced from the following sentence, which apparently is intended to convey the author's last word upon the subject:—"Certain considerations would lead us to the belief that aerial came from aquatic forms; but from the incomplete state of our knowledge caused by want of material, generalization at present would appear to be very unsafe."

HORTICULTURE AND EVOLUTION.

THE SURVIVAL OF THE UNLIKE: a collection of evolution essays suggested by the study of domestic plants. By L. H. Bailey. 8vo. Pp. 516. New York: The Macmillan Co; 1896. Price 2 dols.

THESE reprinted essays, written primarily for horticulturists, form a valuable contribution to the study of evolution. The underlying motive of the collection is the emphasis placed upon unlikenesses and their survival because they are unlike. Moreover, it is not variation that requires explanation, but heredity, for "normally, or originally, unlike produces unlike."

The first conclusion of those who studied wild plants was that species were fixed entities, originally created as we now see them, and that, if they varied, it was within narrow limits. With cultivated varieties the systematist had nothing to do. *Brassica oleracea* might have innumerable varieties under cultivation, but in nature it was apparently a fixed type. Mr. Bailey, however, refuses to keep the botanist outside the garden-fence, and would consider the forms of *B. oleracea*—cabbage, cole-wort, cauliflower, and kohl-rabbi—as four sub-species. But, though these and other garden-types, such as parsnips and radishes, maintain persistently the characters impressed on them by the horticulturist, yet the many races of each are in a constant state of flux. Mr. Bailey, as a horticulturist, is so impressed by this universal change, that he considers a new character to be useful to a species simply because it makes it unlike its kin. But if it be a just inference that the varieties of the cabbage survive because they are "unlike," it is equally logical to conclude that the wild *B. oleracea* survives because it is "like." A study of plants under both conditions suggests that a constant environment cannot induce the plant to change, but that a plant which meets with "changed conditions of life" (Darwin) may vary to any extent in response to them. The differences, as Mr. Bailey puts it, "find the places of least conflict, and persist because they thrive best. . . . There are, therefore, as many species as there are unlike conditions in physical and environmental nature." This is only true if "species" stands for any degree of variation. "To Nature, perfect adaptation is the end; she knows nothing, *per se*, as species or as fixed types. Species were created by John Ray, not by the Lord; they were named by Linnæus, not by Adam."

The thesis of original variation is based on such individual variations of plants as were discussed in NATURAL SCIENCE, vol. vi., p. 385. "Inasmuch as no two individual organisms ever are or ever have been exactly alike, so far as we can determine, it seems to me to be the logical necessity to assume that like never did and never can produce like." "It is a more violent assumption to suppose that the first unspecialised plasma should exactly reproduce all its minor features than to suppose that it had no distinct hereditary power, and therefore, by the very nature of its constitution, could not exactly reproduce itself." This view is supported by the time it takes a breeder to fix any new character; five years is perhaps the average period for plants. Too much stress, however, must not be laid on individual variations, which really are nothing more than results of inexact growth and unequal distribution of nutriment, for no organism can be "absolute" in nature; and they do not give rise to varieties so long as the environment is unchanged. If a "species" or "form" be regarded irrespective of these individual variations, then it is true that "like produces like." Consider it as Mr. Bailey does, and it is equally true that "unlike produces unlike."

The facts of horticulture lead Mr. Bailey to summarise this part of his book thus:—"Unlikenesses in plants are (1) the expressions of the ever-changing environmental conditions in which plants grow, and of the incidental stimuli to which they are exposed; (2) the result of the force of mere growth; (3) the outcome of sexual mixing. They survive because they are unlike, and thereby enter fields of least competition."

The third essay, on "The Philosophy of Bud-variation," has already been noticed in NATURAL SCIENCE (vol. vii., p. 103). Briefly put, it maintains that new varieties and species can be bred from buds and cuttings just as well as from seeds, although in nature seeds have the advantage owing to their greater dispersal. These phenomena, according to Mr. Bailey, render Weismann's conception of the germ-plasm, not merely unnecessary, but untrue.

Much of the book is devoted to relating the evolution of various horticultural varieties, or even, as Mr. Bailey would call them, species. The main conclusions are brought together in the essay on "Experimental Evolution amongst Plants," which shows that many garden forms are as distinct from one another and from their ancestors as are the corresponding wild forms, and that they are therefore just as much entitled to be called species, for "form" is the only criterion of specific difference. To the demand for experimental evidence of evolution, our author replies,—"The horticulturist is one of the very few men whose distinct business and profession is evolution. He, of all other men, has the experimental proof that species come and go."

GEORGE HENSLOW.

THOMAS SAY AS PALÆONTOLOGIST.

THE fifth *Bulletin of American Palæontology*, issued on December 7, 1896, by Gilbert D. Harris, Ithaca, N.Y., at a price of $1.77 to non-subscribers, turns out to be, not the original monograph for which Professor Harris offered a prize some months ago, but "A Reprint of the palæontological writings of Thomas Say," a piece of work probably quite as valuable. Say's five careful papers were published from 1819 to 1825 in the first series of *Silliman's Journal* (now the *American Journal of Science*) and in the *Journal* of the Academy of Natural Sciences of Philadelphia. Both publications are now scarce. An appendix to this *Bulletin*, issued in January, 1897, reprints various footnotes contributed by Say to the "Account of an Expedition from Pittsburgh to the Rocky Mountains . . . under the Command of Major Stephen H. Long," Philadelphia, 1823. They describe, and in some cases name, fossils collected by the Expedition. It is to Say that we owe the first description of *Pentremites*, which, by the way, he invariably called *Pentremite*, and the foundation of the order Blastoidea; he also introduced *Caryocrinus* and *Exogyra*. The present reprint follows the originals "word for word, line for line, page for page, and plate for plate"; it seems to be intended as an exact reproduction, and therefore three misprints in the second article, "lamarck," "silecified," and "abrubtly," should have been left, as in the original. Surely it would not be much more expensive to reproduce the original text by photo-engraving, just as the plates have been copied. This would obviate all risk of error.

NEW SERIALS.

From Paris we receive No. 1 (Janvier, 1897) of *Revue Critique de Paléozoologie*, a quarterly published under the direction of Maurice Cossmann, 95 Rue de Maubeuge, with an annual subscription of six francs within the postal union. In this number Mr. Cossmann reviews general works and palæoconchology; J. Lambert, echinoderms; and G. F. Dollfus, Bryozoa, zoophytes, and Foraminifera. It is proposed to analyse the important palæontological works of each year, without respect to nationality, while indices of new names and of strata will be provided to each volume. Short rectifications of nomenclature from correspondents are asked for, but not original articles. The present notices are like those of the *Neues Jahrbuch für Mineralogie*: good abstracts without too much of the abstractor's personal views. An excellent model to follow.

The first number of the *Journal of School Geography*, a monthly of thirty-two pages, edited by Professor R. E. Dodge, of New York, assisted by Professor W. M. Davis, Dr. C. W. Hayes, Dr. H. B. Kümmell, Dr. F. M. McMurry, and Mr. R. de C. Ward, appeared in January. It is intended for teachers, but will interest yet others. Facts are desired, and wordiness eschewed. It contains articles, notes, and reviews. Geographers will do well to send one dollar, being a year's subscription, to 41 North Queen Street, Lancaster, Pa., U.S.A.

Masson et Cie., Paris, announce a new quarterly journal, *Archives d'Anatomie Microscopique*, to be edited by Professor L. F. Henneguy, under the direction of Professors E. G. Balbiani and L. Ranvier.

We have received Nos. 1 and 2 of *Die Umschau*, a weekly journal published by H. Bechhold, Frankfurt-a-M., at a yearly subscription of ten marks, single numbers forty pfennige. It contains short articles dealing with progress and activity in the whole round of pure and applied sciences, literature, and art. Among the contributors are mentioned M. Buchner, K. Busz, W. Flemming, R. H. Francé, O. Hamann, H. H. Hirsch, P. Jensen, R. Michael, A. Nestler, L. Reh, F. Römer, Fr. v. Wagner, and O. Zacharias. The second number contains a paper on cave-fauna by Dr. Hamann, being based on his book "Europäische Höhlenfauna," Jena, 1896.

Bibliography.

In connection with the *Bibliographia universalis*, *Il Policlinico*, an Italian medical review, will, from January of this year, publish regularly a bibliography of current Italian medical literature; the same will be done for Belgium by the *Journal médical*. The movement will also be aided by the *Bibliographia obstetrica-ginecologica italiana* for 1895.

The Geological Society of London has issued Part I. (A–La) of a "General Index to the first fifty volumes of its *Quarterly Journal*," compiled by Mr. L. L. Belinfante, the Assistant-Secretary. The conclusion is promised in August. It is arranged both as author and subject index in one alphabet, and the subject-headings provide a wealth of information in themselves. The care lavished on the preparation is shown by the numerous minor details that have been carefully noted and preserved, and whether one is on the hunt for "Bagshots" or obscure scraps of fossil reptiles, one's time will be saved by this valuable index. A slight fault is the omission of the authority for the new specific and generic names.

Mr. W. Rupert Jones' "Geological Literature added to the Geological Society's Library during the year ended December 31st, 1896," improves on the good qualities of previous issues. It seems a pity that this cannot be slightly extended in scope so as to make it a complete record of each year's geological literature. Publications not in the Society's library might be distinguished by some sign, a perpetual reminder to possible donors.

We have received the fifth and concluding part of the "Bibliography of American Economic Entomology" (down to 1888), issued by the U.S. Department of Agriculture. This comprises a list, under authors' names (L—Z), of papers and notes, and its utility is vastly increased by an index of subjects at the end. A large number of the contributions here recorded would not be inserted in any general zoological bibliography, and their collection in the present work is therefore of special value. The lists have been compiled, with assistance, by Mr. S. Henshaw. We are glad to see that it is intended to issue supplements, bringing the work up to date.

Various changes in editors have come in with this year. *La Nature* will be edited by Mr. H. Parvil in place of Mr. G. Tissandier. Professors J. S. Kingsley and C. O. Whitman leave the editorial board of the *American Naturalist*, the former being succeeded by Mr. F. C. Kenyon, of Philadelphia. The *Botanical Gazette* has blossomed out in a large number of associate editors representing various American and European scientific centres. Professor H. Marshall Ward is the English representative. Professor J. Seth joins the editors of the *Philosophical Review* (Cornell University). Dr. W. Sklarek is joined, as editor of the *Naturwissenschaftliche Rundschau*, by Drs. J. Bernstein, professor of physiology at Halle; W. Ebstein, professor of pathology at Göttingen; A. v. Koenen, professor of palæontology at Göttingen; V. Meyer, professor of chemistry at Heidelberg; and B. Schwalbe, professor of anatomy at Berlin.

Scraps from Serials.

While we in England are talking about Pliocene Man, and while his Burmese contemporary is to be strenuously defended in our pages by Dr. Fritz Noetling, our American cousins are still disputing over the remains of Man supposed to be in glacial drift. The last piece of important evidence has been sent us by Professor E. W. Claypole, who, in the *American Geologist* for November, 1896, describes a grooved greenstone axe of common aboriginal American type, said to have been found by Mr. Elmer E. Masterman in the tough blue clay below glacial gravel, near New London, Huron Co., Ohio. The specimen when found was considerably decomposed, perhaps by the action of sulphurous water. Other specimens have also been found in or about the same beds by Mr. Masterman. In the *American Naturalist* for January, however, Mr. H. C. Mercer points out that the intended conclusion can hardly be accepted until some professed archæologists shall have witnessed the actual finding of a similar implement *in situ*.

In *Knowledge* for February, Dr. W. F. Hume recalls the fact that specimens of diatoms collected by Nansen from the ice-floes between Iceland and Greenland, and found to belong to species hitherto known only from Bering Strait, as well as particles of minerals collected from the same place, and thought by Törnebohm to come from North Siberia, were among the evidences which caused Nansen to imagine the drift of the ice across the Arctic Ocean. The importance of little things has rarely received more striking proof. This number also contains an article by Harry F. Witherby, entitled "'Twixt Land and Sea," with three half-tone process illustrations, described as "engravings from original oil-paintings by 'A Son of the Marshes.'" Now a picture almost identical with one of these has been hung on our own study-walls for fifteen years, under the supposition that it was a copy of a painting by Wolf. The others also seem familiar. Who is the dupe?

Dr. Bashford Dean has put together a very interesting paper on the Public Aquaria of Europe from notes made during the travels of several years, His paper, which appears in *Appleton's Popular Science Monthly* (November, 1896), deals with Naples, Amsterdam, Plymouth, Paris, Berlin, while Brighton in particular is illustrated and generally described. He remarks that Brighton Aquarium is the most typical, if not the largest, of the newer aquaria of Europe. From Dr. Dean's account we do not gather that any other aquarium than that of Brighton condescends to the Music Hall entertainment combination.

Memorias de la Sociedad Antonio Alzate, vol. ix., nos. 9 and 10, contains papers by Dr. A. Dugès on the foot of monkeys and the skeletons of the bird and the tortoise, and one by Dr. R. E. Cicero on the medical habits and knowledge of animals.

Readers of Dr. Axel Ohlin's paper. "A Zoologist in Tierra del Fuego" (NAT. SCI., Sept., 1896) should be interested in an account of recent explorations in the Patagonian Andes, 41° S. lat., by Dr. Hans Steffen, in the *Scottish Geographical Magazine*, Feb., 1897.

Additional information concerning the huge octopus that we alluded to in our February number, p. 130, is given by Mr. A. E. Verrill in the February number of the *American Journal of Science*. It is a true octopus, with a body 21 feet long, and almost 7 feet wide. A stump of an arm still attached reaches the length of 36 feet, and was 10 inches in diameter where it was broken off distally. The parts cast ashore probably weigh at least six tons, and the total mass, when living, was probably twice that weight. The drawings of it will be published in the *American Naturalist*. Mr. Verrill names it *Octopus giganteus*, but considers that it may be allied to *Cirroteuthis*. To this number Mr. Verrill also contributes an interesting note on nocturnal and diurnal changes in the colours of certain fishes and of the squid (*Loligo*), with notes on their sleeping habits. The nocturnal colours are obviously protective, and the positions assumed by the fish while sleeping are often quaint and unexpected. In another note Mr. Verrill considers that the nocturnal coloration of various animals has been selected on account of its protective value. This number also begins the long-looked-for outline of a natural classification of the Trilobita, by Dr. C. E. Beecher.

In the *Geological Magazine* for February, Dr. J. W. Gregory discusses the fossils on which Dr. H. Hicks recently based his claim that the Morte Slates of North Devon were of Silurian age. He concludes, from examination of the specimens, that they belong not to the Silurian genera, to which the President of the Geological Society assigned them, but to characteristic Devonian genera. Whoever may ultimately prove right, it is clear that such obscure material as has hitherto been the sole reward of Dr. Hicks' laudable perseverance is not enough to warrant a re-arrangement of the stratigraphy.

FURTHER LITERATURE RECEIVED.

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OBITUARY.

HEINRICH GÄTKE.

Born May 19, 1813. Died January 1, 1897.

ORNITHOLOGISTS of all lands will hear of the death of Heinrich Gätke, of Heligoland, with much regret; at the same time it is a matter for congratulation that he lived long enough, with unimpaired faculties, to finish his remarkable work, *Die Vögelwarte Helgoland*, published in German at Brunswick in 1891, and an English edition and translation at Edinburgh in 1895. This, the full value and importance of which to students it is difficult to over-estimate, was the result of more than fifty years' experience and study in the best ornithological observatory on the west coast of Europe by one of the most careful and reliable of observers, one, too, whose labours were practically continuous day and night, for during his long life he was rarely absent from the island.

Gätke was borne in a small town of the Mark of Brandenburg. and it was his desire to become a marine painter that first induced him to visit Heligoland, and finally settle there. Subsequently, during the time of the English occupancy, he held an official position under the Governor. It was constant touch with nature when following his profession which first brought him into contact with that marvellous variety of bird-life, which year by year, with almost rhythmical precision, sweeps to and fro over the lone red rock in its grey setting of sea. I first made Mr. Gätke's acquaintance in 1874, on which occasion, and subsequently, I saw his splendid collections of birds and moths, the whole of which had been prepared and set up by himself, and placed in the well-lighted studio adjoining his house on the Oberland. The total number of species of birds observed on Heligoland during his life was 398, a most extraordinary number for so small a locality, representing, as they did, wanderers from many lands. Every yard, nay, nearly every foot, of the beautiful garden near his house was memorable for some rare capture or occurrence.

Gätke completed the German edition of his work on May 19, 1890, his 77th birthday; the English translation, revised and corrected by himself, in 1895; and very shortly after this he was struck down by partial paralysis. He just lived to cross the threshold of 1897, dying on January 1, in his 84th year.

When Gätke first commenced the study of birds, his guide and text-book was the two Naumanns' great work, *Naturgeschichte der Vögel Deutschlands*. Up to 1874 he had held little communication with English ornithologists. Since that time, however, many naturalists, attracted by the wonders of the island, went over; one result was that he was liberally supplied by Professor Newton, Mr. H. E. Dresser, and others, with all our best and most recent publications on birds.

For twenty-two years the writer carried on a regular correspondence. His last letter, conveying thanks for English reviews of his book forwarded, says, "Such a reception of my work proves indeed a great rest of happiness in the evening of my life."

He wrote as perfectly in English as in German, and his many letters, like his book, are full of beautifully expressed thoughts and word-pictures in connection with his favourite study.

John Cordeaux.

We regret to record the deaths of:—Sir Spencer Wells, M.D., surgeon to the Queen's household, and author of several important surgical works, especially on ovariotomy, on February 1, at Antibes near Nice, aged 79; Henry Boswell, an eminent student of mosses, especially those of Great Britain, at Headington, near Oxford (it is hoped that his large collection will be secured by the University, of which he was an honorary M.A.); Dr. Edward Ballard, F.R.S., a distinguished investigator of causes of disease, and promoter of sanitary reforms; R. Warner, horticulturist, aged 82; on October 3, in Manchester, Joseph Chappell, entomologist, aged 67; on September 11, in Paris, Dr. Emile Moreau, ichthyologist, aged 76; in Troyes (France), the mycologist, Major P. Briard; G. F. Schacht, who made improvements in the application of certain drugs to the treatment of disease, aged 73; Dr. H. von Nördlinger, formerly Professor of Forestry in Tübingen; at Blumenthal, on December 23, Captain E. Dallmann, Antarctic explorer, aged 66; on December 28, in Dantzig, the newly-appointed Director of the Bacteriological Institute, Dr. T. J. Lickfett, aged 49; on January 2, at Munich, Franz v. Baur, a well-known forestry botanist and Professor at the University, aged 66; on October 26, at Linz, Dr. A. Dürrnberger, specialist on Salices, Rosaceæ, and Hieraceæ, aged 59; on December 5, the botanist, Dr. Rudolph Raimann, Professor of Natural History at Vienna Commercial College, aged 33; on December 16, the Austrian Botanist, J. Ullepitsch, aged 68; on January 13, in Vienna, the well-known cave-explorer, F. Kraus, aged 63; the conchyliologist, Dr. A. L Brot, in Geneva, on August 30, aged 75; Dr. S. Trinchese, Professor of Comparative Anatomy and Embryology at Naples, and author of many biological works; in Verona, the herpetologist and conchyliologist, E. de Betta; J. H. Thiry, formerly Professor of Surgical Pathology in Brussels University; on January 9, at Rotterdam, the Director of the Zoological Garden, A. A. van Bemmelen; Dr. S. A. B. Lundgren, Professor of Geology in the University of Lund; at Copenhagen, on January 12, C. Bahnson, a noted ethnologist and Inspector of the Prehistoric Department of the National Museum, aged 42; Dr. Charles Heitzmann, histologist, of New York; Professor W. H. Pancoast, a Philadelphia surgeon; W. H. Ward, of Newark, N.J., a horticulturist; E. G. Lodeman, Instructor in Horticulture in Cornell University, on December, at Mexico.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Misled by an official notice from the Smithsonian Institution, we announced that Mr. F. W. True had been placed in charge of the National Museum, Washington. We now learn that Mr. C. D. Walcott, Director of the U. S. Geological Survey, has been appointed Acting Assistant Secretary of the Smithsonian, with duties confined to the charge of the National Museum. He will, however, only undertake these duties temporarily, without relinquishing his present work. The possibilities of the situation are alluded to in our Notes and Comments.

The following appointments are announced :—Dr. James Ward, well-known as a physiological psychologist, to be Professor of Mental Philosophy and Logic at Cambridge, where also Dr. E. Barclay-Smith is to be Senior Demonstrator, and F. C. Kempson, Junior Demonstrator, in the Department of Anatomy ; Francis E. Lloyd, of the Pacific University, Forest Grove, Oregon, to be Professor of Biology in the Teachers' College, New York ; Professor B. Hatschek, of Prague, to the chair of Zoology at Vienna University ; Dr. Ch. Julin, of Lüttich University, to be Professor of Comparative Anatomy ; Dr. J. Rückert to be Professor of Anatomy in Munich University ; Dr. C. J. Martin, to be provisionally the successor to Dr. G. B. Halford as Professor of Physiology in Sydney University ; Dr. Hans Lenk, to be Professor of Geology at Würzburg University ; Mr. W. Gardiner, Lecturer of Botany at Cambridge, to be Bursar at Clare College ; Dr. Alexander P. Anderson, to be Professor of Botany at Clemson College, S.C. ; Dr. A. Maggiora, to be Professor of Experimental Hygiene in Modena University, and Dr. A. Serafina, to hold the same position at Padua ; Dr. C. Wardell Stiles, as special commissioner for the U. S. A., to report on the parasitic diseases of seal life ; Mr. J. G. Luehmann, for nearly thirty years principal Assistant to the late Baron von Mueller, to be Curator of the Melbourne Herbarium ; Dr. G. v. Istvanffi, formerly Privatdocent at Buda-Pesth University, and Curator of the National Museum, to be on the staff of Klausenburg University ; J. J. Forrester, to be Director of the Royal Agricultural Society's Experiment Station in Woburn, England ; Dr. K. Müller, of Halle, the well-known bryologist, and whilom editor of *Natur*, Dr. E. Pringsheim, Privatdocent of Physics in Berlin University, Dr. Karl Eckstein, of Eberswald, Dr. Georg Fischer, of Bamberg, and Dr. Ludwig Plate, of Berlin, to receive the title of Professor. G. Dewalque, Professor of Physical Geography and Geology at Lüttich University, having reached the age-limit, is resigning his post.

The University of Catania proposes to construct a zoological laboratory on the Island of Cyclops, off the coast of Sicily, which has been presented to them by Signor Gravina.

Professor J. Chandra Bose, lecturing before the Indian Section of the Society of Arts on February 11, complained that the University of Calcutta was not properly equipped for scientific research, nor was its science-teaching properly organised.

The Government proposes to ask Parliament to provide funds for the frontage of the South Kensington Museum and for the electric light installation in the National Gallery, National Portrait Gallery, and the Natural History Museum.

Mr. H. W. Moffat has presented to the British Museum (Natural History) a skeleton of one of the ancient inhabitants of Zimbabwe, the authenticity of which was apparent not only from the peculiar formation of the skull, but from the characteristic gold ornaments still encircling its arms.

The *Echo* states that a unique bronze of a dancing bear is exhibited in the Egyptian Department of the British Museum. It is 7 inches in height, and appears to be of Theban workmanship. The bear was not a native of the Nile valley, and this is therefore intended for the Syrian bear, with which the Egyptians may have become acquainted through the campaigns of Thothmes III. and Rameses II.

WE deeply regret to learn that the prospects of improved accommodation for the valuable collections in Brighton Museum are as bad as ever. Even the Public Library seems to be a disgrace to the town, if we may judge from the mournful report of its Committee. One expects better things of "London-on-Sea." Turning to London, we are glad to see that the fine library of its University, known apparently to about 100 people, is to be rendered more accessible to graduates. Steps that will have a similar effect are contemplated by the Geologists' Association, London, which wishes to deposit its books in the Free Library of St. Martin's parish, of course on certain conditions. We hope, for the sake of both parties, that the negotiations will have a successful issue.

WE have already described the arrangement of the mineralogical and geological sections of the Royal Museum, Peel Park, Salford. We need, therefore, only refer to the appearance of an official handbook to these sections, compiled by Herbert Bolton, and illustrated with figures of fossils and with a less appropriate plate of an entrance corridor with antique statuary. This is the first of a series which it is proposed to issue, and costs 2d.

THE Botanical Museum at Weimar, erected and equipped at the expense of Professor C. Haussknecht, was opened on October 18th., on the occasion of the meeting of the Thuringian Botanical Union. This is destined to be the central station of the Union for researches in systematic botany.

JAMES LLOYD, author of the "Flore de l'Ouest de la France," who died at Nantes on May 10, 1896, has left to the town of Angers his fortune and scientific collections, stipulating that the herbarium should be preserved in its present state in a special room, that the library should also form a special library, placed in the same building as the herbarium, and that the Maire of Angers should select from three candidates proposed by the Botanical Society of France, a curator for both herbarium and library, who should receive a salary of at least 3,000 francs.

THE Council of the Royal Botanic Society has offered a site free of cost for the erection of a students' observatory in connection with London University, together with the use of a lecture-room.

AT the annual Meeting of the Geologists' Association of London, held on February 5th., Mr. E. T. Newton, the President, read a paper "on the evidence for the existence of Man in the Tertiary Period," evidence which he regarded as still inconclusive, despite the efforts of Mr. Lewis Abbott, as described in NATURAL SCIENCE.

THE following extract from Sir Joseph Prestwich's will has been communicated by Lady Prestwich to the Geological Society of France;—"I bequeath the sum of £500 to the Geological Society of France, at Paris, in consideration of the courtesy and friendly co-operation which I have always received from the geologists of that country." This comes into effect on the death of Lady Prestwich, and the income will suffice for a prize to be given every three years. Lady Prestwich will be glad of any letters of her husband's for a biography which she is compiling. These should be sent to her at Shoreham, near Sevenoaks, and will be returned as soon as copied.

At a meeting of the Hull Geological Society on January 7, Mr. T. Sheppard exhibited a skull from the Millhill gravels, near Brough. It differs somewhat from the typical brachycephalic skulls of the Bronze Age in being longer and narrower. The supraciliary ridges are prominent.

At a meeting of the Norfolk and Norwich Naturalists' Society, on January 26, the following papers were read :—T. Southwell, on Grassi's work on the development of the eel ; W. H. Tuck, on aculeate Hymenoptera from Tostock, Suffolk ; A. W. Preston, meteorological notes for 1896 ; and G. H Harris, on the Yarmouth herring-fishery of 1896, which was notable for a large catch, the total landed at Yarmouth being 19,252 lasts, and at Lowestoft 8,189 lasts. It was remarkable that this vast host should be "told," that is, literally counted, not in ones, but in fours or "warps." The fish were "told" into baskets or "mands" on deck, thirty-three "warps" or a "long tale" (or long tell) hundred go to a "mand." The "mand" was slid on a plank from deck to quay, carried across the road, and emptied into a larger basket, or "swill." Thirty "swills" went to the "last." A "last" was 13,200 herring, or 3,300 "warps." Mr. Harris suggests that the word "mand," now applied to the basket, was originally the word for the heaps or "mounds" of fish. The other words are clearly of Scandinavian origin—"varp" means a netful or draught of fish ; "tal" is a number ; "läst" is a ton ; "swill" is a very old English word for a herring-basket, probably connected with the Swedish "sill," herring. May it not therefore be that "mand" equals the Swedish "mängd," or multitude ?

The tenth annual Report of the Liverpool Marine Biology Committee is as interesting as the reports of previous years, and contains suggestive remarks by Professor W. A. Herdman on the relations of the Marine Biological Station at Port Erin to the general public, especially that of tender years. Experiments in fish-hatching were conducted on a modest scale, but not without success, at the station during the spring of 1896. Eight dredging expeditions were organised during the year, resulting in the capture of a number of new and interesting species ; amongst these is a large green Gephyrean worm, probably a new species of *Thalassema*, distinguished by the possession of a remarkable green pigment, not allied to hæmoglobin or chlorophyll, with a single broad absorption band in the red between C and D. One of the greatest needs of the station is a young chemist or physicist who would join the expeditions with the object of reporting upon the sea-water at the various localities, depths, and seasons. Other items of interest in this Report have already appeared in our news pages.

The programme of the Scarborough Field Naturalists' Society for 1897 shows that this body continues its good work of popularising science, and of utilising the work of its more enthusiastic though perhaps less trained members. On the latter point Mr. D. W. Bevan offered valuable advice in his presidential address on January 14.

We regret to learn from the *Revue Scientifique* that the Jardin d'Acclimatation, which was founded in 1858 to conduct experiments in concert with the Société d'Acclimatation on the introduction of foreign animals and plants into France, has now separated itself from the Société, and will form for itself a new society.

The Cape of Good Hope Government intends to investigate the marine fauna of the South African coast. A small biological station will probably be erected on False Bay, and equipped with a steam launch of 150 tons. Specialists who may wish to work up the material obtained are requested to communicate with Dr. J. D. F. Gilchrist, Agricultural Department, Cape Town. The conditions are as follows : Specimens will be forwarded as procured, and, on receipt of manuscript and drawings, each piece of work will be published without delay in a uniform style, so as to form ultimately a complete record of the Cape marine fauna. Authors' copies will be forwarded as soon as published, and a certain circulation will be guaranteed. No money remuneration is offered, but duplicate specimens may be retained by the authors. Unique specimens will be handed over to the South African Museum in Cape Town.

A Biological Survey of Alabama has been organised and put into operation. The Survey will be carried on under the auspices of the Alabama Polytechnic Institute by the specialists engaged at that institution in the various lines of biological investigation. Its object is to study in field and laboratory all plants and animals occurring in the State, and the various conditions affecting them. The work will be done systematically and thoroughly, aud all results published. In a region so interesting and little worked as this portion of the Southern United States, careful and extended research will be sure to yield results of the greatest value, and we hope that Alabama biologists will receive the support they deserve. Large quantities of material in all groups of plants, and of animals (especially insects) will be collected and properly prepared. In connection with the Survey there has been founded an Exchange Bureau, from which will be distributed all duplicate material. Any desiring to correspond relative to specimens, literature, or the work of the Survey, should address : Alabama Biological Survey, Auburn, Alabama.

Even the hardened scientific man, in all his pride of ignorance of the Greek language, history, and culture, may feel some thrill of interest in hearing that Professor Dörpfeld, in his excavations to the N.W. of the Areopagus, has discovered one of the potsherds by which Themistocles, the great and mean, was ostracised in 471 B.C. On it are impressed the words "Themistocles Phrearrios," the latter having reference to the deme in which the statesman was born.

In connection with the Essex County School of Horticulture, a botanic garden and demonstration ground is to be established on three acres of land acquired at Rainsford End, Chelmsford.

The *Scottish Geographical Magazine* states that an experimental station has been formed in Zanzibar for the study of the agricultural resources of East Africa, and the suitability of various crops for cultivation in that region.

G. Zenker, formerly Director of the Jaunde Station in the Cameroons, has set up in Bipinde a research station for the study of the fauna and flora of this district.

The Government of Dutch India has appropriated $6,000 for the erection of a research laboratory at the Buitenzorg Garden.

A scientific expedition to the Far East has been organised by the German Government, and will probably start on January 27 from Bremen on board the North German Lloyd s.s. "Sachsen."

A meteorological station, in connection with the German Antarctic expedition, is shortly to be established under the direction of Dr. Rudolph Mewes, in Victoria Land.

Dr. O. Nordenskiöld has sailed in the Chilian vessel "Magellanes" for the South Shetland Islands, where both coal and gold are said to occur.

Nature announces that J. Graham Kerr and J. S. Budgett, both of Cambridge, who started last August for Paraguay, have been successful in their quest for specimens of *Lepidosiren paradoxa*, and will shortly return with abundant material to work out.

Mr. C. G. Pringle has returned from his annual trip into the more unknown regions of Mexico with about 20,000 zoological and botanical specimens.

The Vienna Academy has sent Drs. H. Müller, Ghon, Albrecht, and Pöch to investigate the bubonic plague in India. Their expenses will be paid out of the Treitl Fund.

Reuter states that Dr. Koch has discovered that cattle can within a fortnight be rendered immune to rinderpest by a mixture of serum and virulent rinderpest blood.

CORRESPONDENCE.

Mr. Jackson's Map of Franz Josef Land.

A friend has just drawn my attention to an article which appeared in the January issue of Natural Science, and as this article forms an attack—an anonymous attack—on Mr. Jackson's work in Franz Josef Land, and criticises my report of that work in the *Geographical Journal* of December last, I must ask you to give my reply the same publicity you afforded the series of inaccurate statements which I shall now proceed to refute. I need scarcely add that I shall not comment on the animus or the singular taste of this anonymous attack on a man who is still at work in the Arctic Regions, and unable to reply for himself.

(1.) But it may clear the ground, in the first place, if I deal with the way in which you have sought to insinuate that I was belittling the magnificent work of Dr. Nansen. Nothing was or could be farther from my intention, and the following extracts from my paper will clearly show this.

Writing of Nansen's safe return on the "Windward," I said:—"The delight and the wonder of it were not confined to what we call 'geographical circles,' but commanded the attention of that world which honours the courage and applauds the prowess of the polar explorer—a world which is as wide as our globe, and only limited by the distribution of the human race."

Anxious also to show how my absent friend appreciated the great explorer's work, I quoted from a private letter of Mr. Jackson's as follows:—"I told him [Nansen] how intensely pleased I was to be the first person to congratulate him on his magnificent success."

And again:—"I have done my utmost to make him comfortable, and to give him a good time after his rough experiences. He has made a most extraordinary journey, which, for daring, is, in my opinion, absolutely unequalled in the annals of discovery, either in the Arctic or other Regions."

Further, you begin your article with "Well! we *have* made a mistake. . . . We thought he (Nansen) had achieved a magnificent success. But a paper by Mr. A. M. Brice in the December number of the Geographical Society's *Journal* shows up our error." You then quote a portion of a passage which I will, with your permission, quote more fully. I was pointing out how much more has to be done in the North Polar regions, and my words were these:—

"The journey of Nansen—memorable as it must remain—still leaves immense areas absolutely unknown. The drift of the *Fram*—full of marvel though it be—has only extended our knowledge a few degrees in one direction. [Her marvellous drift was roughly on a W.N.W. course from the New Siberian Islands to the longitude of Spitzbergen.] But there are more degrees and many directions. The unknown geography of the North Polar world still invites attack, and still holds out the promise of a crowning reward."

To put it in a single sentence, I may say that as long as the larger half of the North Polar basin is still undiscovered, the scientific geographer cannot and will not be satisfied. And to show that I was not alone of this opinion, I will quote one remark made on that occasion by the President of the Royal Geographical Society:—"As Mr. Montefiore Brice has said, there is much more to be done."

(2.) In the light of my quotations in the paper from Mr. Jackson's letters, your statement—"We turn eagerly to see what were the achievements wrought in Franz Josef Land beside which those of Nansen pale"—is a gross inversion of the plain words Jackson wrote on Nansen's work:—"absolutely unequalled in the annals of discovery."

(3.) Referring to the discovery of Cape Mary Harmsworth and my (not Jackson's) identification of this with Gilies Land, you write:—"Interested in this great discovery, we turn to the map only to recognise in the 'newly-found cape' our old friend Cape Lofley." This is a statement, I submit, absolutely unwarranted by the facts. The map made by Jackson and Armitage clearly accounts for the cape which Mr. Benjamin Leigh Smith saw in 1880 beyond C. Ludlow, and called C. Lofley; and it also accounts for the land which Mr. Smith saw in 1881 and laid down as connecting them. Jackson and his colleagues travelled close under the coast the whole way; they ascended, for the first time, nearly all the capes and elevations on the route, and made observations and took bearings from them. And then, on rounding C. Lofley—which was unmistakeable—they opened up, for the first time, a third cape, and this cape is C. Mary Harmsworth. You say that Jackson has removed Mr. Smith's C. Lofley to "an insignificant point," and that C. Mary Harmsworth is Mr. Smith's C. Lofley. My reply to this is that there is no insignificant point on that line of coast which could possibly be mistaken for a cape, and that C. Lofley and the intermediate stages were all and successively identified before reaching C. Mary Harmsworth.

(4.) Then you give what you call "another instance of the Jackson-Harmsworthian method of treating the names of their predecessors"; and state that Jackson has re-named Payer's Markham Sound as the British Channel—adding that Mr. B. L. Smith "accepted the name and mapped the south-western end of the sound." My reply is that Mr. Smith acted rightly in accepting the name, because we now know that he never entered Markham Sound or mapped its south-western end. Jackson, however, has discovered in the British Channel a wide sound—another and more important "Austria Sound"—to the west of Markham Sound, and to the west, even, of Payer's Zichy Land. A knowledge of Payer's map might have saved Jackson's nameless critic from this error. For in Payer's map Markham Sound washes the north of McClintock Island, and this is precisely what Markham Sound still does in Jackson's map. Payer's south-west prolongation of the Sound was guess-work: Jackson's actual survey cuts that prolongation short with a chain of islands. And so little is the British Channel identical with Markham Sound that it is nowhere near McClintock Island, but far away to the west of the Sound, and even west of Payer's Zichy Land.

(5.) Again, you write: "Mr. Jackson's detailed map of the southern coast of Alexandra Land will doubtless be of great value when this part of the world is divided into building lots." If this is the opinion of Natural Science on the importance of accurate and detailed mapping, I may pass on, merely quoting the opinion expressed by Sir Clements Markham (whose name, by the way, Natural Science misspells) on the same point:—"Such a survey will be a most useful and valuable contribution to geography—its value, indeed, can hardly be exaggerated."

(6.) In citing one of my quotations from Mr. Jackson's letter, you print it thus: "At his [Nansen's] winter hut, he believed himself near C. Lofley, and that the land to the westward, which we had discovered (!), was Spitzbergen." What, may I ask, is the object of this interpolated (!)? The land Nansen saw to the westward was the land marked 21, Island 10, and Island 11 on Jackson's map. Your note of exclamation would seem to express doubt or affect surprise at Jackson having discovered this land. It is, however, perfectly certain that he was the first to discover (in 1895) that and all the adjacent land, including the island on which Nansen subsequently wintered. This is beyond all doubt, and your insertion of the note of exclamation is merely offensive.

(7.) You appear unable to understand the generous feelings expressed by Jackson for Nansen, and his request that Nansen should name the island and locality where he wintered you describe as his "gracious permission to re-name an already named island"—Karl Alexander Land. Here, again, you are wrong, and, as you appear to base your opinion on the map in the *Geographical Journal*, December, 1896, I may inform you that I requested the Society's cartographer to insert Payer's map in lighter colours, in its proper place to the east of Jackson's field of work, and without attempting to correct, at the points of contact, the obvious discrepancies. Inference from the areas in contact is consequently useless, unless guided by knowledge; and Karl Alexander Land is *not* identical with the island on which Nansen wintered. Jackson knew this and Nansen knew this, as the former proved by asking Nansen to name it, and the latter proved by calling it after my absent friend, the leader of the one Arctic expedition still at work in the field.

Finally, I must record my protest against these ignoble imputations, these gross inaccuracies, this arm-chair view of Polar work, and this "stabbing in the dark" of a man who is now wintering for the third time in Franz Josef Land, in the cause of geographical science.

Arthur Montefiore Brice.

157 Strand, W.C.,
February 12, 1897.

P.S.—In studying the new map of Franz Josef Land (*Geographical Journal*, December, 1896) it should be remembered that the discoveries of Payer and Smith occasionally overlap with those of Jackson. The latter is only responsible for areas enclosed in heavy lines and names printed in heavy type. A. M. B.

[With Mr. Brice's epithets we deal elsewhere. The geographical questions raised by him are: (3) the relations of C. Mary Harmsworth and C. Lofley; (4) the use of the name Markham Sound; (6) the discoverer of land seen by Nansen to the westward of his winter hut; (7) the name of the island on which Nansen wintered. The late date at which we receive this letter prevents us reproducing any maps, reference to which would easily show geographers the correctness of our previous remarks. These we will attempt to elucidate for Mr. Brice's benefit, as follows:—

(3.) C. Mary Harmsworth is identical with C. Lofley. Leigh Smith saw the coast from a position whence he must have seen the cape called C. Mary Harmsworth by Jackson, and whence (as any geographer but an armchair one will understand) he could not have distinguished the point so obvious to Jackson's party working along the coast. Anyone who will plot Leigh Smith's map on to Jackson's so that C. Ludlow corresponds, will find that C. Lofley of Smith coincides with C. Mary Harmsworth of Jackson.

(4,) As for Markham Sound, we wrote with Payer's map lying before us. This shows that he gave the name to a sound running along the S.E. coast of Zichy Land, which he saw and did *not* guess at. Those who have not Payer's map may refer to the map distributed in illustration of Mr. Brice's own paper at the Geographical Society on November 11, 1895, in which map Markham Sound is represented as separating Zichy Ld. from the islands there grouped under the name Hooker Id. That is the exact position now usurped by Jackson's "British Channel." Further, Payer's Markham Sound runs N.E. and S.W., but Jackson's runs N.W. and S.E.

(6.) We are most willing to admit Jackson's claim to be the discoverer of "the land marked 21, Island 10, and Island 11." But these three small patches are not quite the same as "the land to the westward" of Nansen's winter hut, for this obviously included Zichy Land.

(7.) Nansen wintered on Payer's Karl Alexander Land. Comparison of the maps by Payer, Jackson, and Nansen shows that Hooker Id. and the three islands to the north, were regarded by Payer as the western end of his McClintock Id. North of this is the island with the high ground or mountain which Payer named after Richthofen. Then, crossing Payer's Todesco Fjord, we come to another island, to the N.W. point of which Jackson applied the name of McClintock. It is the next land to the north on which Nansen wintered, and this is the S.W. end of Payer's Karl Alexander Land. This, however, appears from Nansen's map to be three islands, of which the northernmost is a westerly continuation of Payer's Andrée Id., and should bear that name.

Mr. Brice's laborious explanation of the map illustrating his last paper was quite unnecessary; he has forgotten that it was given quite plainly on the map itself. The "obvious discrepancies" between the maps of Payer and Jackson are mostly due to the fact that parts of Payer's map were laid down from observations made at a distance, and are therefore not so correct as those on his actual line of march. Mr. Brice should beware of laying stress on this, or people will infer, most unjustly of course, that he is trying to minimise the results of the Austrian explorer as well as those of the Norwegian.—Editor Natural Science.]

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NOTES AND COMMENTS.

The Modern Scientific (?) Explorer.

THAT exhilarating zoologist, Professor A. L. Herrera of Mexico, is at it again. He has attacked the name-mongers, he has ridiculed the museums of the present, and exalted those of the future, and now he whirls his lath sword about his head and brings his jester's bauble down on the pates of "Messieurs les Explorateurs." He might have taken as his text a paragraph in to-day's paper: "The members of the British Mission to Abyssinia include a few naturalists of repute, and it is fair to surmise that whatever else the mission will accomplish, it will result in a gain to our natural history collections." Collections! why collections? Are these gentlemen not to add to our knowledge, not to make observations, not to collect facts? Alas! no. The aim of the modern explorer, as gathered from his own writings, is to amass the largest number possible of birds, butterflies, eggs, plants, fishes, etc., etc., in the hope of discovering as many new sub-species as possible to bear the name of himself, his female relations, his friends, and his native servants (*e.g. Chrysococcyx Klaasi* Le Vaillant: "ce Klaas ce pauvre et bon hottentot.") As for natural features of the earth, it is no longer necessary to discover new ones, for it is nowadays so easy to alter names given by previous explorers, to say nothing of natives, and to impose one's own from the poles to the equator.

It is not only the explorers, properly so called, with whom Professor Herrera thus gently trifles. He has a kind word for the humble collectors, "dignes confrères de leurs espèces nouvelles." How sad it must be for them that many former varieties are now common and losing their money-value! How dreadful that there should be any difficulty in obtaining new species to describe! And here is a tale to be told with bated breath: "A half-civilised Australian black-fellow presented an exploring commission with a box of bird-skins, supposed to belong to perfectly new species, *about whose biology nothing whatever was known.* The box was sent to the British Museum, where it stayed five years and was returned, still unopened, by the worthy officials, who had not time, they said, to take up the subject. Five years after the same box was sent to the kindred establishment in Paris, and only seven or eight years had passed when the commission heard that the new species had not been described because the box was in a part of the museum where the assistants only went once in twenty-four hours, and they were not quite sure what had destroyed the skins—a mouse or a rat." Only one statement makes us doubt the truth of this tale: we cannot believe that a box which had once entered the British Museum ever left it again.

A "Dunciad" of Zoology.

Then the describers, the cataloguers, the popular natural history writers. They are all belaboured by our witty professor. Thirty-four zoologists at least have written on the non-marine molluscs of Mexico; and yet Professor Herrera challenges them to produce any information as to the bionomics of the animals. An extraordinary number of people collect birds' eggs; and the publications devoted to them, especially in America, are remarkable in number and in many respects. There are 52,510 eggs in the U.S. National Museum. Perhaps the 600 and more oölogists remember Claude Bernard's remark that no mystery is greater than that of the egg. "En effet, il n'y a pour moi [M. Herrera] aucun mystère plus obscur ni plus passionnant."

Here are some choice examples of modern zoological literature culled by Professor Herrera from well-known sources. A "Catalogue of the Birds of Dominica," contains such remarks as these: "*Dendrocea plumbea;* nothing known of it. *Setophaga ruticilla;* not common. *Coccyzus minor;* trusting and stupid; etc., etc." One eminent explorer thinks it necessary to waste printers' ink thus: "Like all pigeons, this one is fond of water." It would have been more interesting, thinks our satirist, if he had written: "Like all explorers, this pigeon is fond of whisky."

The paper ends with three model reports for explorers to follow: the first imitates the style of the poet-explorer; the second is typical of those who observe nothing; the third represents those who know nothing. We need not reproduce these quotations. Similar instances are but too familiar to all of us. They make us wonder whether this clever naturalist is not wasting time and paper quite as much as those whom he attacks. The people who can write drivel like that which he quotes cannot possibly have the humour or intelligence to appreciate Professor Herrera's sarcasm, if ever it reaches them.

"Wit shoots in vain its momentary fires.'

Menelik's Library.

The above-mentioned mission to Abyssinia might well be recommended to make enquiries into the library of Menelik. We learn from *La Naturaleza*, Madrid, that, at the time of the Mohammedan invasion of Ethiopia in the 16th century, the Abyssinians placed all their Ethiopian manuscripts in Debra-Sina, one of the islands on Lake Zonay, and here they remained carefully guarded by the inhabitants, who looked upon the books as tutelary deities. Not long ago the Negus sent an expedition to conquer these holy islands, and has built in his capital, Addis-Ababa, a library for the reception of the manuscripts thus recovered. In ancient times Ethiopia was a great centre of learning, and some of these manuscripts have doubtless extreme value.

The Shell-ornaments of the Mas d'Azil Scholars.

Our readers will remember those still more ancient manuscripts, painted on stones discovered in the cave of Mas d'Azil by Mr. Piette, an account of whose researches was given by Professor Haddon in Natural Science (vol x, p 33, January, 1897). The shells collected from the various levels in that cave have been studied by Mr. H. Fischer, son of the lamented malacologist, P. Fischer, and in *L'Anthropologie* (vol. vii, pp. 633-652) he describes and figures several, some of them ornamented by man.

With the exception of three species, *Helix nemoralis*, *H. hortensis*, and *Unio littoralis*, which appear to have been introduced as food, the *remanié* and marine species seem all to have been used for purposes of adornment, and have been perforated. The perforation has been effected sometimes rudely as though with a stone hammer, sometimes apparently by punching; at others the process has been facilitated by grinding in the first instance or filing a notch with a chipped flint, whilst fossil shells have been treated equally with the recent.

Mr. Fischer takes the successive layers and gives lists of their shelly contents; he finds the prevailing species to vary in each bed, and his results are summarised as follows:—The summit of the Pleistocene is characterised by the predominance of *Pecten*. During the succeeding reindeer period *Turritella communis*, *Littorina littoralis*, and *Littorina littorea* were abundant, the two last species indicating exchanges with the oceanic seaboard. Iu the bed with coloured pebbles were considerable numbers of *Trivia europæa* and *Cyclonassa neritea*. Finally *Dentalium tarentinum*, *Unio littoralis*, and species of *Helix* predominated in the layers of shells and of axes. The fossils, which were much appreciated during the earlier periods, seem to have been neglected during the later.

Some of the species found are now confined to the Atlantic seaboard, and unless examples were, as Mr. Fischer speculates, still living in those days in the Mediterranean, near the shores of which they have been found fossil, they must have been brought from the more distant source, and so too he admits may some of the others, which occur on both sides of the Iberian peninsula. Thus a light is thrown on the trade-routes of these learned troglodytes.

The Royal Botanic Gardens.

Doubtless with a view to justify its continued existence the Royal Botanic Society proposes to establish "an institute for the teaching of botany" at its gardens in Regent's Park. In support of this idea Mr. W. Martindale recently read a paper before the society and various botanists well known as teachers or scientific workers in different parts of London. This paper has been widely reported and copies have been circulated. The Botanic Gardens have, in the past, been of much service to the various institutions already engaged in the teaching of botany. Tickets of admission have been liberally granted to students on the recommendation of their teachers, and specimens have been supplied without stint. As an adjunct to existing schools of botany the gardens have served a valuable purpose, and in this direction there is no limit to further development. But we strongly deprecate the establishment of an institute with the functions indicated by Mr. Martindale, for this would only compete with and not assist the older schools. When competition began assistance would probably cease, and the gardens would end by being far less useful than at present. There are quite enough "teaching institutes" to ensure that healthy rivalry which is sufficient to bring ruin to the antiquated or badly equipped. If, as is suggested, the purses of merchants and City companies, or the zealously guarded (where science is concerned) chest of the Treasury, are to be approached, let it be to give the much needed help in cases where good work is already being done, or to assist the development of the gardens in a manner that shall be supplementary, not antagonistic, to existing effort. Botany is one of the most, perhaps the most, important of the natural sciences. Its application is the basis of agriculture, and many other 'cultures' besides, involving the prosperity of our land and its colonies. But if this is to be duly recognised the science must be popularised. We do not mean degraded or enfeebled, but made interesting to that large body of people who are glad to know something of a science but have not the time or opportunity to master its abstruser technicalities. For such popularising, a show of living illustrative specimens is one necessity, a man who knows his subject and can talk about it in a clear and simple manner is the other. The Royal Botanic Society has the former, cannot it find the latter and bring the two together before an audience in its lecture-theatre on a spring or summer Saturday afternoon? If this is not sufficient outlet for its energy let it set up a laboratory fitted with apparatus for experiments in plant-physiology, and offer the loan of it to the various teachers to whom we have referred. These would, in many cases at any rate, be pleased to meet their classes at the Gardens and explain practically what hitherto they have taught only by means of pictures or diagrams. A research-laboratory for advanced students would be an additional advantage. Efforts in the directions we have indicated would tend to ensure the continuance of the Gardens, and thus relieve the Council from anxiety as well as benefit the public generally.

The Registration of Plants.

A Section of the Association of American Agricultural Colleges has appointed a committee to consider the question of establishing an office for the registration of plants in connection with the present Division of Pomology. "The idea," says *Science*, "is to have some one place in the United States where all plants placed upon the market can be officially registered, numbered, and a description, together with specimens of the bloom, seed, foliage, and fruit, placed on record. When it is not practicable to preserve the original, coloured casts are to be prepared, as in the case of citrons, drupaceous and pomaceous fruit, as well as vegetables. In all cases where plants are sent for registration, specimens of flowers, fruit, root, tuber or seed must accompany the application. All vegetables must be accompanied by a given amount of seed (to be determined) to be preserved for purposes of noting the duration of cultural varieties, the influence of climate during any series of years or in any locality. A further purpose of the seed shall be to grow plants for purposes of identifying the sort."

As usual, the practical people are ahead of the scientific, for it is obvious that the objects of the scheme would apply equally to systematic zoology and botany. They are: (1) to discourage the duplication of names, and the re-naming of old sorts for commercial purposes; (2) to form a national herbarium of economic plants, which shall be made up largely of type-specimens; (3) to simplify the matter of nomenclature; (4) to aid the student of varieties as well as of variation in plants under culture. The only clause that does not apply to pure science is the fifth, namely, to secure to the originator of a truly valuable variety some reward for his labour, the same as is now accorded the inventor. How many more years is it to be before we see international offices for the registration of new species of animals and plants?

The Dispersal of Seeds.

We referred a few months ago to the instructive and beautiful series of specimens, models, and drawings of insectivorous plants which had been arranged in the public gallery of the Department of Botany of the British Museum. The room has lately been enriched with another excellent teaching exhibit, illustrating the methods of dispersal of fruits and seeds.

In many cases scattering of the seed, often over a considerable area, is ensured by the structure of, or certain mechanism in the fruit capsule itself. Elasticity of the dried walls is a common agent. Many pods for instance split suddenly, the two halves twist spirally, thereby jerking out the seeds which lay in a row on their inner surface. The capsule of the violet forms on dehiscence a three-rayed star, each ray bearing the small roundish seeds in its concave upper portion, the edges of which gradually close together and shoot out the seeds. Sir John Lubbock stated that they sometimes fall several yards from the parent plant. The *Impatiens* which has taken possession of our gardens in recent years, is an admirable instance of the success of this device. Here the strips of the lateral walls of the fruit corresponding to each carpel, are at maturity in a state of longitudinal tension. Ultimately the weak place at the base gives way, and the walls curl elastically upwards, each strip slinging its seeds out with considerable force. In the closely allied genus *Geranium* the same principle holds, the outer walls of the carpel when ripe springing backwards and upwards from the central column. In *Hura crepitans*, the sand-box or monkeys' dinner-bell, a tropical American ally of our native spurges, the woody fruit explodes with a loud report by the sudden separation of the individual carpels from the centre, and the flat seeds are shot out. Other fruits and seeds are water-borne. The wide distribution of the coco-nut palm through the islands of tropical seas is thus accounted for. The fibrous husk forms the float, and its tough leathery outer layer keeps out the sea-water. When thrown up on the beach the seed germinates, the embryo growing out through the soft round spot at the end of the hard nut-like inner wall of the fruit. The three spots, like the three longitudinal lines in the wall, are relics of the time when there were three chambers to the fruit each with its seed, instead of a single one as at present.

The *Nipa* is another palm which is distributed in the same way. It grows along muddy shores and brackish estuaries from Ceylon to New Guinea. The fruits are crowded in a large round head as big as a cannon ball; when ripe they separate and fall into the mud or water, and having a thick light fibrous coat will float and may be carried a long way by currents. In his Himalayan Journal, Sir Joseph Hooker refers to the large numbers turned over by the paddles of his steamer at the mouth of the Ganges. The seeds of a widely distributed tropical bean, *Entada scandens* are often carried by the Gulf stream, and thrown up on the Western shores of our islands and of Norway, where they will germinate. Ages ago, they were floating round the estuary of the Thames, and along our southern coast, where they are found to-day as fossils along with other estuarine organisms which go to show that their habitat in those far-away times closely resembled that to which they are now restricted.

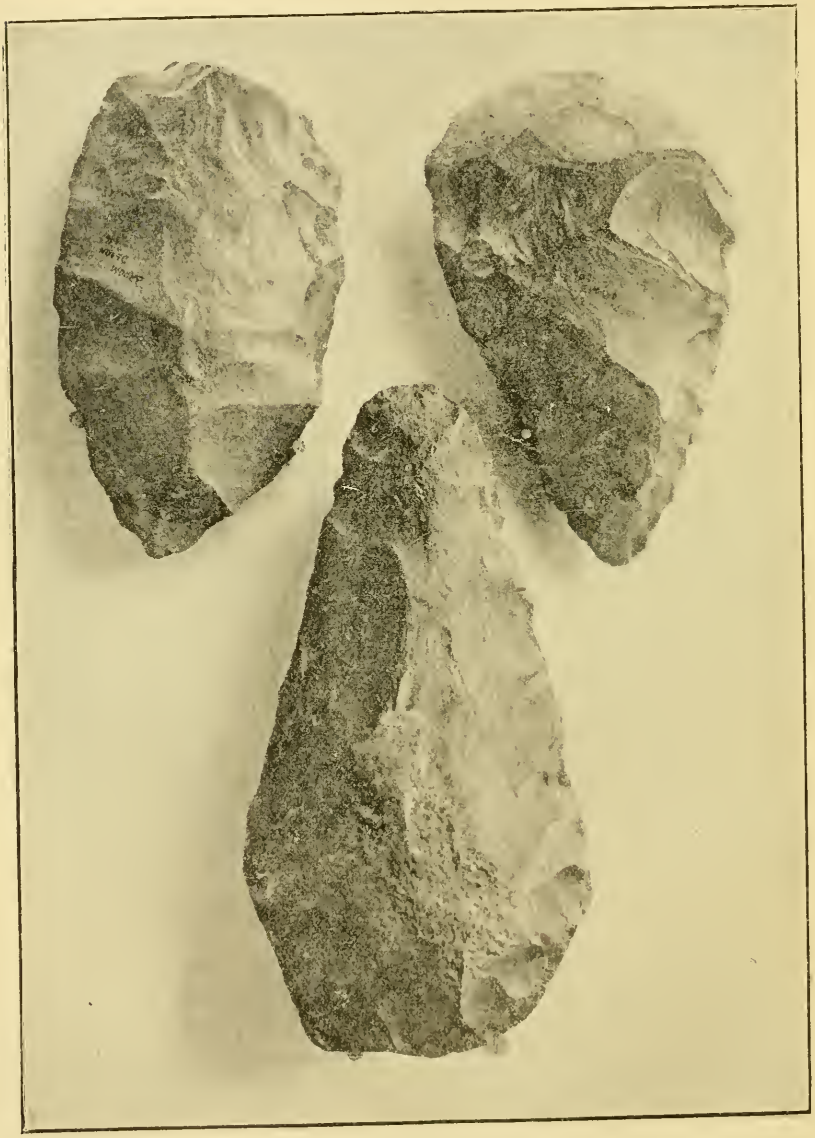
The most effective agent in distribution is the wind. Fruits and seeds carried along by help of wings or feathery appendages are to be seen everywhere in summer and autumn. It is a significant fact that one of the characteristics of the largest and most widely spread family of seed-plants, the Compositae, is a light hairy fruit-appendage known as the pappus, and familiar to everyone in the dandelion 'clocks.' In rare cases the whole plant packs up and travels to a new locality. The Rose of Jericho (*Anastatica Hierochuntina*) is a small annual growing in desert places from Syria to Algeria. When in flower the branches are spreading, but as the seeds ripen the leaves wither and the branches dry up and curl inwards, forming a ball in the inside of which are the seed-pods. In this condition it is soon loosened from the sandy soil and carried or rolled along by the wind, and often blown into the sea. On being wetted, the branches unbend and the pods begin to open, so that if thrown up again on the shore its seeds will germinate in a new locality.

Animals also play an important part, either by carrying in their coats or feathers, or by swallowing and ultimately rejecting, the seed uninjured. For transport by the former means grappling hooks or barbed or sometimes sticky hairs are of service, to the latter we owe our brightly-coloured succulent fruits and seeds. By offering a tempting meal the plant succeeds in propagating its kind in a distant spot. Of course the seed must be protected from injury during its passage through the animal's body, and this explains its hard or tough covering in such cases. The beauty of this part of the exhibit is striking; fruits of the Yew, Guelder Rose, Rowan, Elder-berry and other familiar examples being reproduced in a modelling composition by Miss Emett with life-like brilliance of colour as well as accuracy of form.

Students can learn more by a few minutes' study of these cases than by hours of text-book reading.

The Horniman Museum.

Suburban theatres are springing up all round London, and are prospering. Why not suburban Museums? That there is an opening for them if intelligently and attractively managed is shown by Mr. F. J. Horniman's Museum at Forest Hill, S.E., which during 1896 was attended by no less than 62,119 visitors. The sixth annual *Report*, by the curator, Richard Quick, evidences a commendable activity in many directions. It has the advantage of being illustrated by some excellent photo-process engravings of objects contained in the Museum, also of a group of Burmese visitors to the Museum, showing Mr. Horniman sitting between two pretty ladies. Another plate represents three chert palæolithic implements selected from eight in the Museum collection, which were found last March in a gravel pit near Chard Junction, in the valley of the Axe. This engraving has been kindly lent to us by Mr. Horniman. Similar implements from this neighbourhood have been known at least since 1877,



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when some from near Broom were exhibited by Sir John Evans to the British Association. The present specimens are, however, very fine ones. Fig. 1 is $8\frac{1}{2}$ inches long and $4\frac{1}{2}$ inches wide at the butt end; Fig. 2 measures $6\frac{1}{2}$ by 4 inches, and Fig. 3, $6\frac{1}{2}$ by $3\frac{3}{8}$ inches.

The Museum appears to receive a rather miscellaneous lot of objects, but every attempt is made by Messrs. Quick and Slade to reduce them to scientific order, and make them of real use to their particular public, as well as to others in other places.

A Species-monger on New Zealand Midges.

Owing to their small size and generally inconspicuous coloration, the Diptera, or two-winged flies, usually escape the notice of the scientific explorer, unless they belong to species that display an unpleasant fondness for his blood. The Diptera of New Zealand have been more neglected than most, and Mr. P. Marshall finds that "the total number now described does not amount to more than a hundred and twenty-five species, of which only twenty belong to the Nemocera,"—the division including the midges, gnats, and daddy-long-legs. A paper by Mr. Marshall now before us (*Trans. New Zealand Institute*, 1895; pp. 216–311, pls. v.–xiv.) is a vigorous attempt to supply the deficiency. The present instalment, which is merely a beginning, deals with the Cecidomyidæ (gall-midges), Mycetophilidæ (fungus-midges) and Simulidæ (sand-flies), and includes descriptions of 62 species, 57 of which, belonging to the two former families, are introduced as new. Alas! not only are 23 of these new species described from single specimens, but many of these are females. "I have only one specimen of this insect" is a statement of irritating frequency, accompanied in more than one case by the information that the type is "rather imperfect." Nor is this all. For the fungus-midges the author finds himself constrained to make ten new genera; and of the names chosen for these, seven are already in use, in one instance in the very order which he has undertaken to study, and with a representative in New Zealand.

Those who know anything of Diptera will understand the enormity of describing a fungus-midge from a single female specimen; those who do not may be told that these little flies are but a few millimetres in length (the author, for some inscrutable reason, gives his measurements in decimals of an inch), that they are among the most delicate and fragile of their order, and that their sexual differences are often considerable. How many of these precious types will be available for comparison fifty years hence? But the author has evidently no idea of the importance and sanctity which is nowadays attached to a type-specimen. Of one insect, for which he makes a new species and a new genus (with a pre-occupied name), he writes: "Mr. Hudson has kindly lent me a specimen for drawing up this description."

Only two years before the date of his paper did Mr. Marshall begin to collect Diptera. He had meant to send them to England to have them named, but Captain Hutton advised him to work them out himself. We cannot congratulate either entomology or the author upon the result. We have before now found it necessary to protest against the reckless coining of specific names, but never was there a case more flagrant than this. It is not even as though we were dealing with a portion of the results of a brief collecting expedition sent out from Europe. Mr. Marshall is a resident in New Zealand, and has therefore every facility for amassing an adequate amount of material before commencing author. But, two years exhaust his patience, and he proceeds to make a new species for almost every single specimen obtained. We note that he intends to publish from time to time papers on the other families of Diptera, and these he hopes "to supplement every year by species that have been discovered during the preceding year; so that, ultimately, these papers may perhaps attain to the completeness of monographs on the different families of Diptera." Monographs are not written thus. Specialists should pray that Mr. Marshall may take warning ere it is too late.

Insect Enemies of Domestic Animals.

The Entomological Division of the U.S. Department of Agriculture issues, as *Bulletin* No. 5 of its new series, a monograph of the insects which injure domestic animals. This work, containing over 300 pages, 4 plates, and 170 figures, is by Prof. Herbert Osborn. It is well up to the high level of excellence which the Washington department has taught us to expect in its publications. All the North American insects and mites which feed on the tissues or blood of man and his domestic animals are described. As many of the species are found also on this side of the Atlantic the work will be valuable to naturalists and farmers in our own countries. Though the monograph is primarily intended to instruct the practical man, and to point out methods of prevention and cure, it is well worth the attention of the biologist, who will find in it some excellent illustrations of interesting life-histories.

The Scientific Publications of the American Government.

We have often had the pleasure of alluding to similar valuable publications issued by the U.S. Department of Agriculture. We regret that this pleasure will not be ours in future, for a law has been passed forbidding the general distribution of the serial and scientific publications of the Department. Copies not required for official use will be sold by the Superintendent of Documents, Union Building, Washington, D.C. He is not allowed to sell more than one copy of any document to the same person and he will not receive cheques or postage stamps. This means that it will cost an Englishman one shilling to obtain a 5-cent pamphlet.

On the other hand, Congress has recently passed a resolution providing for the gratuitous distribution of the topographic maps and geological atlases of the U.S. Geological Survey, to the number of 500, among foreign governments and various learned bodies and institutions.

It seems, if we may venture to say so, that the whole system of distribution of these American Government publications requires reform. It is absurd on the face of it that they should be, like wisdom, "not to be purchased for gold"; while their free distribution entails enormous waste, hundreds of copies going to persons that have no need for them. Still more absurd is it that a book should lie perdu in the printing office for two or three years after it has been printed, because of some absurd official restriction. Absurd also that there should be so many editions of the same work, simply to provide the printers with what we believe they term "fat." We are therefore glad to learn from *Science* that there is a bill now before Congress calculated to effect some improvement in these matters. It will have the best wishes of all friends of American scientific men, whose earnest labour is so often dissipated by the wheels within wheels of a creaking official machine.

The U.S. Department of Agriculture.

We have recently called attention to the desire of American scientific men for greater organisation in the scientific work of the Government. An important step in this direction would undoubtedly be the appointment of a permanent director of scientific work for the Department of Agriculture. A bill for this purpose was introduced by the last Senate, and we learn from *Science* that the estimates for the next financial year recommend an appropriation of $6,000 per annum for this office. The Secretary of Agriculture under the new administration is the Hon. J. Wilson, of Iowa, who is Director of the Iowa Agricultural Station and Professor of Agriculture in the Iowa Agricultural College. It is therefore probable that his practical experience will make him a strong supporter of the above proposal.

Apropos of this Department Secretary Morton has recently called attention to the inadequacy of the salaries paid to the officials, in consequence of which the Department is constantly losing some of its ablest and best workers. "Thirty-two leading scientific experts have left the Department during the last few years to take positions in other institutions, at a rate of remuneration averaging fully 50 per cent. more than they received from the Government of the United States." Britons will find a ready parallel to this state of things in their own Natural History Museum, one of whose chief functions is to train untried youths for the service of more far-sighted institutions.

FUNAFUTI.

IN the first number of NATURAL SCIENCE an article was published discussing the proposed attempt to settle the " coral island " controversy by a deep boring into a reef. The proposal has since been carried out, and a small batch of papers gives us the preliminary results. In regard to the question which it was the main object of the expedition to solve, the result is disappointing, because it is inconclusive. The mechanical difficulties which, as pointed out in NATURAL SCIENCE, foiled the attempted borings made by the " Tuscarora," have again proved insurmountable. All students of the coral island question have long known that a coral reef is not a solid block of coral, but consists of nodular masses of coral separated by intervening patches of sand and coral debris, and sometimes even of mud. When the reefs are raised above sea level these intervening patches of loose material are soon cemented by infiltration of carbonate of lime into a fairly hard rock; but so long as the reef is below sea-level, and is saturated with water, the materials remain loose and incoherent. The Funafuti boring tool on both occasions soon reached beds of soft slime, which effectually prevented progress. These beds may be either part of a volcanic basis of the atoll, or may be only exceptionally large masses of the intra-reef sediments or beds of volcanic mud which accumulated during a period when reef-building was temporarily supended, perhaps by a too rapid sinking. So the coral island controversy stands where it stood before.

The expedition, however, has by no means been a general failure. The detailed survey of the ocean bottom around Funafuti, made by H.M.S. " Penguin," is alone of great value. Mr. Gardner has made zoological collections which have not yet been worked out, while Mr. Charles Hedley of the Australian Museum has gathered the materials for a very interesting account of the geology, botany, and ethnology of the island. This has been published as an eighty-six page paper, issued as the third of the *Memoirs of the Australian Museum*. The first batch of the zoological reports is included, but those dealing with the Invertebrata, of which a large collection was made, are not yet issued.

Mr. Hedley's account of the vegetation and ethnology of Funafuti is full of points of interest. Thus in respect to the old controversy as to the distribution of the coco-nut palm, the author opposes the view that the range of this tree is increased by ocean currents.

The natives have all abandoned paganism and are fast forgetting their ancient customs. Nevertheless Mr. Hedley was able to learn a good deal from native tales, which, however, appear to be already suffering from European influence. The Hydrographer to the Admiralty has recently published a note in *Nature*, maintaining that the soundings suggest that Funafuti and the other Ellice Atolls originated by growth on volcanic banks. Mr. Hedley on the contrary thinks that evidence on the whole supports Darwin's theory, though the grounds on which he bases this conclusion are not very substantial.

ARTESIAN WATER.

WE have more than once drawn attention to the valuable work in searching for artesian water that is being carried out by the Geological Survey of Queensland. This has lately given rise to two papers (*Proc. Roy. Soc. Queensland*, vol. xii.), one by A. G. Maitland, "On the geological structure of extra-Australian Artesian Basins," which deals chiefly with those in North America. It appears from this that nowhere in that region, except perhaps at Denver, are the water-bearing rocks disposed in the shape of the textbook diagram. On the contrary, the water-carrying beds are so arranged that there is only one side of a synclinal trough, so that the strata present abundant facilities for the escape of the water, which can actually be seen to leak from them in the form of rivers.

Similar questions are dealt with in the second paper, that by R. L. Jack, "On the submarine leakage of Artesian Water." His conclusions are given as follows:—"The question at issue is no longer whether the water which, when tapped by bores, issues in artesian wells, occurs in porous strata lying underneath impermeable strata. The bores themselves have settled that, and the outcrops of the strata have been to some extent mapped, and found at altitudes sufficient to give a 'head' capable of forcing the water to the surface in the lowlands of the West. These outcrops, moreover, have been detected in the act of absorbing, year by year, more than water enough to supply the wells and springs. The question is, whether the strata form a sealed basin, or crop out beneath the ocean in such a manner as to give rise to a circulation of the underground water. A mass of evidence has been accumulated to prove that, as the strata are periodically filled up with water, they must first have lost a certain amount by leakage. In the case of Australia the only possible escape is beneath the ocean, and although we cannot observe this leakage with the bodily eye, we may believe in it, as we believe in many things we cannot see. A powerful confirmation of this view is supplied by the fact that some important artesian basins elsewhere leak out on land, while the rest have the physical conditions which must inevitably lead to submarine leakage."

DIFFERENTIATION IN ROCK-MAGMAS.

IN the January number of the *American Journal of Science*, Dr. G. F. Becker, under the title "Some Queries on Rock Differentiation," makes a weighty attack upon certain hypotheses which have been advanced by petrologists to account for the apparent derivation of different rock-types from a common source. He approaches the subject from the physical side, and considers in turn the two distinct suggestions of diffusion in a magma owing to differences of temperature, etc., and the separation by cooling of parts of a magma which are miscible only above a certain critical temperature.

As regards the first point, Dr. Becker gives reason for believing that no appreciable effects could be produced by diffusion during any time which can reasonably be postulated. This would be true even in aqueous solutions of salts; in natural rock-magmas viscosity would further check diffusion, while convection-currents and disturbance by mechanical means would speedily undo its work. He considers that viscosity and other causes would also prevent the separation of immiscible fluids into distinct bodies occupying different spaces. As no considerable superheating will be possible in subterranean magmas in contact with solid rocks, the range of temperature must further be very limited.

Without attempting to meet the author's arguments, we may remark that the two hypothetical processes which he examines do not, as he seems to assume, exhaust the possibilities. For instance, he passes over the effects which may be expected to accompany crystallization in a slowly cooling body of rock-magma. It can scarcely be doubted that such crystallization is accompanied by a very considerable evolution of heat. Hence we should expect that, when crystallization has begun in the coolest portion of the magma, the tendency to degradation of energy will determine a movement of the constituent about to crystallize to that place, so as to maintain saturation there.

Nor must we lose sight of the possibility that mechanical forces operating during the progress of crystallization may be an important factor in differentiation. Dr. Becker ascribes the diversity of igneous rocks to original heterogeneity in the composition of the globe, and instances the fact that the eruptive rocks west of the Rocky Mountains exhibit certain peculiarities as a group. The remarkable difference, however, between the igneous rocks of the Atlantic and Pacific slopes of America seems to point not to any primitive difference between those regions of the earth's crust, but rather to the operation of great mountain-building forces, *viz.*, those which produced the mountain-axis which so sharply divides the two regions. How such forces may act as promoting differentiation is doubtless a complicated problem, but one way is very clearly indicated by such researches as those of Mr. Barrow on the igneous gneisses and pegmatites of the south-eastern Highlands of Scotland.

Modern Crystal-gazing.

Crystallography is scarcely a popular science; but that it can be made both interesting and intelligible, even to a Royal Institution audience, was admirably shown by Professor Miers' three afternoon lectures entitled "Some Secrets of Crystals." The first lecture opened with a contrast between the superstitions current in early times respecting the origin and medicinal virtues of crystals—superstitions which indeed still partly survive amongst the quasi-scientific,—and the exact knowledge of crystalline substances which has been gradually and laboriously acquired during the last hundred years. The methods by which crystals grow, their shape and symmetry, and the occurrence of twinning were clearly expounded with the aid of numerous appropriate specimens and of a very beautiful collection of lantern slides. The second lecture was devoted to an able exposition of multitudinous experimental methods which have been applied by crystallographers to the determination of crystalline symmetry; an opportunity was thus afforded for exhibiting a number of pieces of exact crystallographic measuring apparatus which are unfortunately too seldom seen and even less frequently used in this country. The last lecture dealt mainly with the structure of crystals. This subject, which has only been brought to its present satisfactory state during the last few years by the labours of Fedorov and Barlow, is of so complex a nature as to excite no little wonder at the lecturer's temerity in attempting to present even its barest outlines to a lay audience; the discourse, however, left little to be desired in point of lucidity and may possibly be of permanent value in destroying some part of the reputation for profundity hitherto enjoyed by the theory of crystal structure. A brief description of a few of the more important applications of crystallography concluded the course.

The most vivid impression left by these lectures is that the crystallography of to-day with all its far-reaching ramifications constitutes a science founded upon a secure mathematical basis, and so securely founded as to rank in this respect beside the better-known exact physical sciences. This being the case, it is difficult to understand why a subject of such wide application—which bears so minutely upon the most remote problems of physics and chemistry and which could be so advantageously used as a teaching tool in connection with the geometry of space, spherical trigonometry and many branches of physics and chemistry—does not more frequently find a place in the courses of instruction given at our higher schools and colleges.

North American Oligochaetes.

Mr. F. Smith has lately been studying the terrestrial and aquatic Oligochaeta of North America with some success. We have before us (*Bull. Illinois State Lab.*, vol. iv.) the second of his articles on the subject, which includes descriptions of a new Lumbriculid from Illinois and of a new species of *Microscolex* from Florida. The first-named worm is referred to a new genus *Mesoporodrilus*, which is of some interest. The most remarkable fact about it is the unilateral development of the male efferent apparatus, which opens in the middle ventral line. It seems to be much like that of *Eclipidrilus* and *Sutroa*, in that the atrium is surrounded by the sperm-sac. The spermathecae are two, lying in successive segments, and each opening also on to the middle line. The number of new forms from the North American continent described by Mr. Smith and by Gustav Eisen tends to prove the distinctness of the Nearctic region so far as concerns these animals, though this was doubted (previously, however, to much that has been made known lately by these two observers) by Beddard in his Monograph of the Oligochaeta.

The Literary and Philosophical Society of Newcastle-on-Tyne.

In 1793 this society began its existence, and quite recently Dr. Robert Spence Watson has published its "History" from 1793 to 1896. The foundation of this venerable Society was due to the Rev. William Turner, an Unitarian minister, who was one of a circle of friends who met together for conversation and debate. Mr. Turner was asked to draw up a scheme for a permanent Institution and read a paper in the winter of 1792, called "Speculations on a Literary Society." This created no small interest, and a meeting was held on January 24, 1793, to further consider the subject. A committee was formed and at a second meeting on February 7, the proposal took definite shape, and the "Literary and Philosophical Society of Newcastle-on-Tyne" was founded. In the early days of the Society Mr. Turner was the most prominent member, lecturing for 30 years on all branches of science. He delivered the address on the occasion of the opening of the present buildings in 1822, and finally resigned his official connection with the Society in 1833. It is a noteworthy fact that many of the discussions and papers dealt with important local economics, such as:—the embankment of Jarrow Slake; the improvement of the river Tyne; new quays; deepening of the tide-way; chemical and manufacturing subjects; and agriculture. No less than 3,000 lectures have been arranged by the Society since its inauguration, and the value of the work done by it from an educational point of view is incalculable.

In 1793 a library was established and despite a disastrous fire in 1893, has now grown to be of considerable value. A museum was founded about the same time, and has grown into the collection now housed at Barras Bridge. Among the long list of bodies that have owed their origin to this parent society, we may refer to "The Natural History Society of the Counties of Northumberland, Durham, and Newcastle-on-Tyne," "The North of England Institute of Mining Engineers," and "The Farmer's Club for Newcastle." It also in 1806 practically founded the Royal Jubilee School, which provided free teaching for the poor of Newcastle. Dr. Watson has done well to record the history of a Society which has done so much to advance the education and interests of so important a centre.

I.

On the Discovery of Chipped Flint-flakes in the Pliocene of Burma.

IN the *Records* of the Geological Survey of India for 1894 (vol. xxvii., pp. 101–103), I published an account of the discovery of some curiously shaped flint-flakes which were found together with vertebrate remains of the Sewalik type, and in particularly close association with a molar of *Hippotherium antelopinum*. A review of my paper, with figures of the principal flake (here reproduced in Fig. 1), was contributed by Professor T. Rupert Jones to NATURAL SCIENCE (vol. v., pp. 345–349, November, 1894). I did not express any definite opinion as to the origin of these remarkably shaped flakes, although I thought it rather difficult to imagine any natural process by which flakes of such a shape could be produced. I was well aware that the heating of flints by the sun and their rapidly cooling down during the night might result in the production of splintery fragments, as had been actually observed by various travellers in deserts, and described by O. Fraas in "Aus dem Orient" (pp. 38, 39). The dry climate of Central Burma, particularly the country around Yenangyoung, with its rapid changes of temperature, ranging from about 52° to 100° in December or the beginning of January, would be well adapted for the disintegration of exposed flints, and if I had found the specimens lying loose on the surface of the rock, I would certainly have attributed their shape to the influence of temperature. As it was, the original specimens were not found lying loose on the ground, but were imbedded in the matrix of a conglomerate, which in a subsequent paper[1] was referred to as the zone of *Hippotherium antelopinum* and *Acerotherium perimense*. In other words the flakes were *in situ* when found.

As I have personally no large experience with regard to stone implements, I left it to authorities in that matter to decide whether the specimens I discovered were of an artificial nature or not. I claim, however, enough experience to be able to say whether a particular fossil was or was not *in situ* when found. So far, nobody seems to have doubted the story of the find as given in my first paper.

[1] *Records Geol. Surv. India.*, vol. xxviii., p. 84, 1895.

To me it seemed perfectly clear when I expressed myself as follows: "One of the most conspicuous beds, palæontologically as well as petrographically, is a ferruginous conglomerate, upwards of 10 feet in thickness. This bed may be distinguished a long distance off as a dull-red band, running, in a continuous line, across ravines and hills. Besides numerous other vertebrate remains, such as *Rhinoceros perimense*, etc., one of the commonest species is *Hippotherium antelopinum* Caut and Falc. of which numerous isolated teeth can be found. *While stooping to pick up the fine lower molar, which is figured in the accompanying plate, my attention was drawn to some curiously shaped flints partly imbedded in the ferruginous conglomerate.*" (Italics not in original.)

Originally I believed this bed to be Miocene. Subsequent examinations, however, have proved that Dr. Blanford's doubts as to the age were well founded, and that it must be considered as Pliocene. It would lead too far if I were to give here the proofs for this view, and for details I must refer the readerto my Memoir on the petroleum-

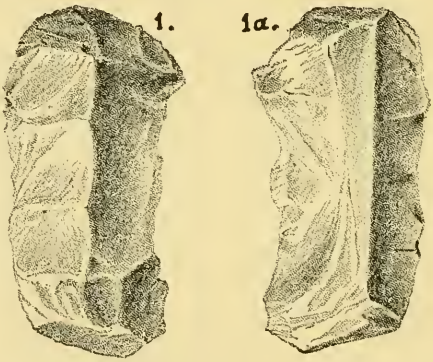

FIG. 1.—FLINT-FLAKE FOUND *in situ* IN LOWER PLIOCENE, BURMA.

field of Yenangyoung, which has unfortunately been greatly delayed in publication, but which I trust will soon be before the public.

In March, 1893, while I was still engaged in the examination of the country around Yenangyoung, Mr. R. Oldham visited me at that place, and desired, amongst other subjects of interest to him, to see the locality where the flakes were found. I pointed out the exact locality, where we spent at the outside a quarter of an hour, without however finding any other flakes; nor did we find any fossil bones or teeth, I might say as a matter of course, as I had repeatedly so well searched the place myself that not a single specimen could have been left unnoticed. We then proceeded further south; I showed Mr. Oldham the locality where countless specimens of *Batissa* (*Cyrena*) *petrolei* and *Batissa* (*Cyrena*) *crawfurdi* occurred in pockets at the base of the ferruginous conglomerate. At another locality we found fragments of fossil bones. Eventually we came at the extreme end of the Yenangyoung dome, near a hill called Minlin (on the maps it is usually called Minlintoung, "toung" being Burmese for "hill") to a place where the ferruginous conglomerate rises to the surface, in other words forms part of the beds that crop out on the plateau. For details I must again refer the reader to my Memoir on the Yenangyoung petroleum-field, as it is impossible to explain the rather complicated stratigraphy without a map and numerous sections, and in the meantime the above statement must be taken on trust. At this locality I had previously found some similar, but not so well shaped flints, which I had, however, disregarded for the following reasons:

1. The specimens were lying loose, and I was not so absolutely sure that they were *in situ* when discovered, as I was with those first mentioned.

2. Being on the plateau, I admitted the possibility of an ancient settlement at this locality.

As both these exceptions might be raised, I thought it more prudent to disregard for the moment the second locality until I had learnt the views expressed by authorities in such matters about the nature of the flakes described in my first paper, as to the position of which I was perfectly sure. If these specimens were not regarded as artificial, it was useless to mention their occurrence at a second locality, but if they were thought to be artificial, there was plenty of opportunity to mention and describe this second occurrence, particularly as I found at a third locality a remarkably polished femur of which I shall have presently to say more. Mention of this femur was withheld, as I wanted to ponder the matter before expressing an opinion.

To my great surprise, Mr. Oldham has in NATURAL SCIENCE for September, 1895, (vol. vii. pp. 201 and 202) given an account of the occurrence of the flakes, so incorrectly stating the facts that a reply and restatement of them appears necessary. I have unfortunately partly been detained by more pressing work from replying at an earlier date; partly I wished to wait for the publication of my Memoir on the Yenangyoung oilfield, because I am now, while on furlough, obliged to write from memory. If therefore some of the figures given below do not quite correspond with those in my Memoir I must ask the reader to excuse these discrepancies in minor matters; but the chief facts will be found to be correct.

I think it best to give first a short description of the country around Yenangyoung, as it is of some importance with regard to the objections raised by Mr. Oldham.

The town of Yenangyoung, famous from the oldest ages of Burmese history for its petroleum wells, is situated on the left bank of the Irrawaddi in Lat. 20° 21′ N. and Long. 94° 56′ E. The country on this side of the river is for miles and miles a barren and almost desert low plateau about 100 to 150 feet above the level of the river, rising gently inland, *i.e.*, towards the East. Seen from a distance

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the country appears a continuous uninterrupted plateau, studded with a low thorny jungle, but on closer examination it is found to be intersected by countless tortuous ravines and gullies which offer considerable difficulty to anyone traversing the country. Owing to the soft nature of the Tertiary rocks the ravines are generally rather narrow, and the slopes steep, but to the geologist they afford in their barrenness delightful opportunities for the study of the strata. For the greater part of the year the ravines are perfectly dry, and if water is found at some depth it is always brackish and extremely unwholesome. The ridges left between the ravines vary of course in extent, owing chiefly to the nature of the rocks which crop out on the surface, but whether narrow or broad, not a drop of water is to be found on any one of them unless it be collected in artificial tanks. There is hardly any humus, and it may be doubted whether any crop worth speaking of could be grown. Everywhere lumps, sometimes even large logs, of fossilised wood may be found scattered all over the ground. Where a conglomerate crops out, it may be followed often for a long distance by a more or less broad band of white quartz pebbles. Patches of diluvial river-gravel resting unconformably on the Tertiary rocks may be seen here and there, topping the ridges. It is important to note that it is often extremely difficult to decide, when only a thin coating of scattered pebbles covers the ground, whether it represents a disintegrated bed of Tertiary age or the last remains of the diluvial gravel that has escaped being washed away. As the chief constituent of both, white quartzite, is the same, and as it is difficult to say whether the fragments of fossilised wood, scattered among the pebbles, come directly from the underlying Tertiary beds, or were already contained in the diluvial gravel, no definite distinguishing feature has hitherto been found. It is true that in the diluvial gravel the fragments of fossilised wood are generally well rolled, but it is not easy to distinguish them when lying scattered on the ground.

From the above remarks it will be seen why I have not mentioned the second locality near Minlintoung. The conglomerate comes here up to the surface; its outcrop is marked by a band of white pebbles, amongst which I found some fine fossil teeth and the flints. Although I myself did not doubt for a moment that both had been derived from the disintegration of the solid rock beneath, I left it open to doubt, at least with regard to the flint-flakes (the teeth belonged unquestionably to Pliocene animals) whether they might not have been transported from some other locality to the place where I found them, and which I pointed out to Mr. Oldham. I confess, however, that not for a moment did I think that Mr. Oldham would so entirely misunderstand this fact as to say, "the implements are not confined to the outcrop of the fossiliferous ferruginous bed, but are scattered over the surface of the plateau above." It is a mistake to say that the implements are scattered over the surface of the plateau;

when found on the plateau, as near Minlintoung, they are strictly confined to the outcrop of the ferruginous conglomerate.

From the foregoing it will be clear why I did not mention the second locality in my first paper, although personally fully convinced that the flakes were derived from the conglomerate underneath, just as much as the fossil bones.

The view that an ancient settlement may have existed at this spot can be discarded at once. Even if we admit that there was room enough for it, what did the people live upon, and what is still more important, where did they get their water from ? There is no spring on the rocks for miles around ; in the ravines the water, when present at all, is always undrinkable ; the water must therefore have been carried from the river for several miles, and I greatly doubt whether the ancient makers of the implements preferred to carry their drinking water over several miles of rugged country, when they could have it much more easily by settling on the river bank.

The strata which are found around Yenangyoung are limited to the Tertiary and Diluvial formation, the latter resting unconformably on the tilted beds of the former.

The Tertiary beds are represented by the younger Tertiary only, the Nummulitic Formation being entirely absent, or at least not coming to the surface. The oldest beds belong to the Miocene, but although they have been proved by deep borings to be of considerable thickness they crop out only over a small area. The beds belong in small part to the Prome stage, and in larger part to the Yenangyoung stage, terminating in the *Batissa (Cyrena)* bed, as explained in vol. xxviii. of the *Records* of the Geological Survey of India (pp. 64, *et seq.*, 1895).

Of the youngest Tertiary, the Pliocene or Irrawaddi Division, not less than 4620 feet is exposed, although probably this does not represent nearly the whole thickness. It consists of a series of soft yellow sandstones, alternating with beds of clay, conglomerates, and bands of very hard siliceous concretions of nodular shape. Single beds do not continue for any distance ; they frequently die out, and are at the same horizontal level replaced by others of a different nature. Numerous instances will be found in my Memoir on the Yenangyoung petroleum-field. It is therefore hopeless to attempt any general sub-division from a lithological point of view, and any, chiefly based on lithological differences, must be considered as merely local.

The conglomerates are frequently only pockets or patches within the sandy beds, but sometimes they may be several miles in length, and as they usually contain numerous remains of Vertebrata, they are of the greatest importance for the study of the Irrawaddi division. For this reason I devoted special attention to their examination wherever I found such a bed, and mostly my researches have been rewarded, although the fossils seem to occur in rather an erratic manner. I remember an instance near the bank of the river, where a bed of conglomerate was fully exposed, measuring about 30 feet in length, 15 feet in height, and four feet in thickness. In this I found a splendidly preserved incisor of *Hippopotamus irravadicus*, but not a single other specimen. It must further be mentioned that these conglomerates vary greatly in hardness; sometimes they are hard and siliceous, ringing under the hammer when struck; sometimes they are loose masses of concretions of hydroxide of iron, formerly used by the natives for smelting; sometimes they are soft and earthy; in fact their physical appearance varies greatly. Some of the smaller beds are exactly like a shingle beach on the sea-shore: more or less rolled clay pebbles, small pieces of wood, and fragments of bone, hardened by a ferruginous solution. Small beds of this kind may extend only for a few feet, but they generally contain an interesting fossil. It was in one of these small layers that I found the polished bone of which I shall presently speak.

At Yenangyoung the Yenangyoung stage begins with a very conspicuous band of ferruginous conglomerate, which from its persistency forms a very good horizon so far as this part of the country is concerned. Its thickness as well as its hardness varies greatly, but the former never exceeds 20 feet. It is followed by soft yellowish sandstone, which at some places immediately above the main bed contains some smaller conglomerate beds. Fossils are, as already stated, frequent but erratic.

The strata form an anticline, the axis of which runs N.N.W.–S.S.E., dipping at about 30° to 32° towards S.W. and N.E. At the same time the axis drops from a point, which is marked by the oil-field of Twingon (northern part of the Yenangyoung oilfield), in a northern as well as in a southern direction, so that it is quite obvious that the strata form here a kind of dome, the highest point of which would be about 6,000–7,000 feet above sea-level, if there had not been any denudation. It will be seen from this that the line of outcrop of any given bed must form a closed circle, which, in fact, is represented by a rather irregular ellipse, stretched in the direction of the strike. This remarkable structure was first revealed by the careful mapping of the run of the ferruginous conglomerate at the base of the Irrawaddi division. The tilted-up strata were eventually planed down to their present level, forming a low plateau, gently rising from west to east, which has now been cut into by numerous watercourses.

From the above description of the stratification it is quite clear that a given bed, which dips East and West respectively on each side of the dome, and which does not come up to the surface of the general plateau, may form part of the surface at the northern and southern end of the dome. It is at such a point, where the ferruginous conglomerate, (*Zone of Hippotherium antelopinum and Acerotherium perimense*) comes to the surface, although it nowhere else forms part of the strata actually cropping out at pleateau level, that the chipped flints were found; and it is from this that Mr. Oldham imagines them to be scattered over the plateau.

I mentioned above that the conglomerates generally contained vertebrate remains, and in collecting such I examined their outcrop very carefully. The extended bed at the base of the Irrawaddi Division afforded special opportunities, owing to the length of its outcrop, and while thus searching for fossils I discovered the flint-flakes.

It will, however, be useful to describe the locality where the find was made, as exactly as is possible without reference to the map. On the eastern side of the Yenangyoung dome the ferruginous conglomerate runs for a considerable distance along the eastern slope of a gully which has a north-and-south direction, and which, by a low ridge, is divided into two sections, one draining northwards and the other southwards. As is usual in this part of the country, the ravines very quickly attain a considerable depth, the slopes becoming very steep, so that they are often impracticable. While following the

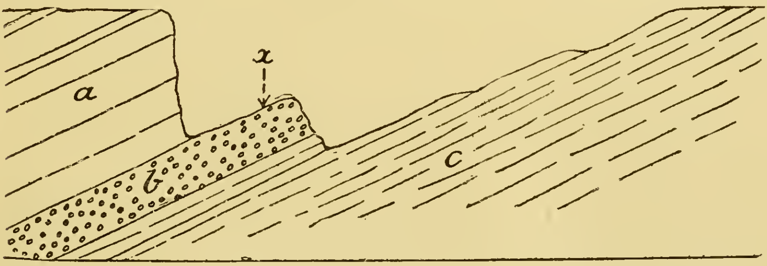


Fig. 2.—Diagrammatic Section across the ravine in which, at x, the Molar of Hippotherium antelopinum and the chipped flints were found.

(*a*). Pliocene. Zone of *Mastodon latidens* and *Hippopotamus irravadicus*.

(*b*). Pliocene, Zone of *Hippotherium antelopinum* and *Acerotherium perimense*. (Ferruginous conglomerate).

(*c*). Miocene. Yenangyoung stage.

course of the ferruginous conglomerate, I was obliged, owing to the steepness of the slope, to walk along the edge of the plateau above, and while doing so I noticed, about 40 to 50 feet below, a ledge of considerable extent formed by the ferruginous conglomerate. The situation was therefore thus: the strata dipping east at about 30° were cut into in the direction of the strike by a deep ravine with a steep, sometimes nearly vertical, slope on the eastern side, along which the ferruginous conglomerate cropped out. At one particular locality all the soft, sandy beds had been removed from the edge of the plateau down to the surface of the ferruginous conglomerate, and being softer than the latter the erosion had worked quicker above than below the ferruginous conglomerate. (see Fig. 2).

The exposure of this conglomerate measured, so far as I recollect, 60 to 80 feet, in a north-and-south direction, and 25 to 30 feet in the direction of the slope, *i.e.*, measured from the edge to the spot where the conglomerate disappeared under the newer beds at the foot of the precipice. The thickness was 12 to 15 feet, and apparently only a very small part of the top of the conglomerate had been removed. The conglomerate was neither very hard, nor very soft; so far as I remember, quartzite-pebbles were absent or very scarce, so that it consisted chiefly of a mass of irregularly shaped nodules of earthy hydroxide of iron, loosely cemented together.

As the exposure seemed a good one to search for fossils, I climbed down, and the first thing I found was the molar of *Hippotherium antelopinum*, still embedded in such a way that only the grinding surface and part of the side were visible. I distinctly remember that I had some difficulty in digging it out with a knife, because I was afraid of using the hammer for fear of damaging it. While thus engaged I noticed some quaint looking flint-flakes, close to the molar, and I also distinctly remember that I wondered at them because of the absence of other quartz pebbles. Quite close to the molar was the largest of the flakes, (Fig. 1, 1a) *imbedded in such a way that about two-thirds of its length was still in the conglomerate, one of the ends sticking out.* On looking further about I found some more, all imbedded in the conglomerate, but I cannot of course now remember the way in which each single specimen was imbedded. Such is the history of the find.

Mr. Oldham, however, describes the locality in quite a different way. "The site," he says, "is on a spur running out into one of the valleys which have been cut back into the plateau; the crest of this spur falls somewhat rapidly, and then rises slightly to the outcrop of the ferruginous conglomerate, whose exposure on the crest of the spur is, to the best of my recollection, about 50 ft. long by 8 to 10 wide. No vestige of soil or sand is here, all having been removed by rain and wind, but there is a thin coating of ferruginous gravel overlaying the solid rock, and it was on this surface, as pointed out to me by Dr. Noetling, that the flakes were found." Now, this description conveys quite a wrong idea of the locality; not that I would blame Mr. Oldham, for his visit of a quarter of an hour can hardly have impressed the situation distinctly on his mind. The statement of "the thin coating of ferruginous gravel overlaying [*sic*] the solid rock" is, however, absolutely wrong. The exposure is formed by the top of the ferruginous conglomerate itself, and here there is no thin coating of ferruginous gravel.

As I have already mentioned the second locality where similar flakes were found, it remains only to add a few words about the polished bone. I found this specimen just at the spot where the low remnant of a ridge still divides the two ravines, about a quarter of a mile to the north of the place where the flints were found, and about 15 to 20 feet, perhaps a little more, above the zone of *Hippotherium antelopinum*, in a small streak of conglomerate, partly imbedded in the overlying sandstone. Fig. 3 will give an idea of the situation.

The lenticular conglomerate looked like a heap of shingle, and when taking out the bone, I noticed at once that the articular ends were polished in a remarkable way. It looked exactly as if it had been rubbed with force at both ends on a hard stone. If I remember rightly, at one end the condyle was rubbed off for about one-third of its width, and there were several facets. Otherwise the bone was well preserved. It was lying, as shown in Fig. 3, with the ground surfaces resting on the underlying sandstone; the dotted lines in the figure represent the parts rubbed off. The sketch is purely diagrammatic; but the bone will eventually be properly described and figured. Only two processes which could have produced such facets, can be imagined: glacial action, or human agency. This, however, can hardly be accepted as proof of glacial action in Burma during Pliocene times, and there remains only the one alternative. While forced to present the evidence in this incomplete fashion, I would not lay too much stress on it; but I am led to mention it, since in the *Neues Jahrbuch für Mineralogie* for 1896 (vol. i., p. 224) Professor Dames has described

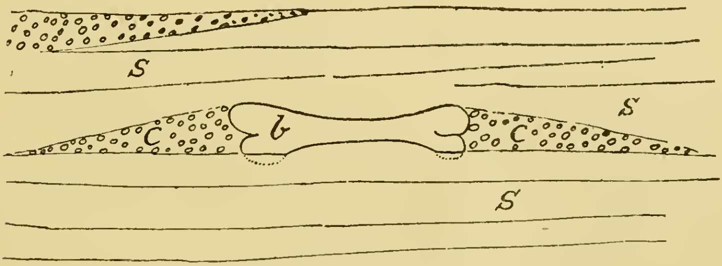


FIG. 3.—SECTION SHOWING WORN BONE, *in situ*.
s, sandstone; *c*, conglomerate; *b*, bone.

the shoulder-blade of a diluvial horse, exhibiting similar loss of substance at the upper end, and has unhesitatingly attributed this to human agency.

To sum up: in the neighbourhood of Yenangyoung curiously shaped flint-flakes, the shape of which it is difficult to explain by any other than human agency, were found at two localities in beds of Lower Pliocene age. It is absolutely certain that they were *in situ* when found at one locality and more than probable that they were so at the second. If their shape is attributed to human agency, additional evidence for this theory is afforded by a remarkably polished femur or humerus, found in the same beds.

FRITZ NOETLING.

II.

Human Evolution.

III.—Mr. Wells Replies.

MR. COSTE makes some illuminating criticisms of my sketch of a theory of human progress[1] and, I must admit, forces a certain modification of phrase upon me. But it does not appear to me that his objections justify his description of them as "fatal." My argument was that, save for culture, for the developing artificial factor, "man is still, mentally, morally, and physically, what he was during the later Palæolithic Period." For the effective development of a subsidiary argument I have already restated that conclusion in an aggravated form.[2] And I see no reason to abandon it.

The main issue between Mr. Coste and myself arises out of a very simple (and probably very frequent) method of misunderstanding. When one speaks of a species, as I speak of "man" in the proposition I am defending, one may have in mind one of two things, (*a*) the normal or average type or (*b*) all the individuals of the species collectively. To be frank I did not clearly see my proposition in the light of the second sense, until I read Mr. Coste's article. My position is that the mean human being has not perceptibly progressed, and may just as possibly have retrograded. But obviously (if only on account of the undeniable numerical increase of the race) the range of variation must be more spacious about that mean than in the later Palæolithic period.

Now Mr. Coste's opening considerations, unless I misunderstand him, amount simply to this, that I have understated the variability of man by assuming that it is only equal to that of the rabbit. He uses the rather vague word "plasticity," but that I take it is his meaning. And he alleges that man is a domesticated animal, an animal removed from his natural conditions, and therefore exceptionally liable to vary. The whole of the question of enhanced variability under domestication would admit I believe of a profitable analysis, but that would be out of place here. My point would be that practically nothing is known of the things that die young in a wild species. But as I said

[1] "The Artificial Factor in Man." *Fortnightly Review*: Oct., 1896.

[2] "Morals and Civilization." *Fortnightly Review*: Feb., 1897.

in my original article, " natural selection is selection by death "—and save for variations affecting fecundity, I fail to see how this greater amplitude of variation (if it obtain) can affect the average product in the case of civilised man. And " another potent cause of variation," he reminds me, " is cross breeding." But the very readiness in racial interbreeding which increases man's tendency to vary, interferes with that process of segregation which (under conditions of general security) is so necessary to the establishment of variations. Neither of these considerations really affects my essential argument of the average man's present stability as far as natural selection goes, and, since Mr. Coste advances no other matter, I do not see that I need deal further with his culminating statement of man's " wonderful plasticity," except by putting a flat denial to his flat assertion. To my mind a great rapidity of multitudinous reproduction must necessarily be the first factor in the estimate of specific plasticity—the ability that is of a species to weather an unexpected cape in the shore-line of circumstance. And we must always remember in discussions of this nature that our discrimination of differences between our fellow men must necessarily be vastly greater than between animals, and that, in clothing and other material, man has an incalculable means of accentuating idiosyncrasy.

Setting aside this issue of relative variability Mr. Coste proceeds to a general attack upon my arguments, an attack based upon the confusion between the intension and extension of the word " man " to which I have already alluded. He begins by stating that my conclusion " that each of us *at birth* is scarcely distinguishable, mentally and morally, from Palæolithic Man, necessarily implies . . . " and he presents a dilemma. But I said nothing of "each of us," and with that his main case against me effectually collapses. I am quite prepared to admit that there are children born in each human generation to-day, potentially viler, more angelic, more stupid, more intellectual, more bestial, than there were born in an average generation of the later Stone Age. I think it will be apparent that Mr. Coste does not hit me, because he does not clearly see where I am. But that he does not do so is, I think, as much my fault as his, inasmuch as I did not make it clearly evident that I was dealing with the specific type.

I may perhaps take this opportunity of restating my view, which is that, *allowing for changes in the numerical ratio of races*, the *average* man is still mentally, morally, and physically what he was during the later Palæolithic Period. I would accentuate " later," because the stage I intend presents man already with speech, a rudimentary family organisation, agricultural beginnings, a wide distribution and therewith the main lines of racial differentiation. From that stage his history has been a history of aggregating communities with a steady fall of the death-rate. Over larger and larger and finally confluent areas natural selection has, I hold, practically ceased her elaboration of him, until now the great mass of mankind is left to the new and semi-conscious process of elaborating mental environment.

My article was in no sense an "alarmist" one, but the implications of my view are very far-reaching, as I have tried to indicate in my second *Fortnightly* article. The tendency of a belief in natural selection as the main factor of human progress, is, in the moral field, towards the glorification of a sort of rampant egotism—of blackguardism in fact,—as the New Gospel. You get that in the Gospel of Nietzsche. But from the standpoint of my article the obvious gospel for the future is the gospel of discipline and education. As a writer of books for the general reader, and a mere interpreter and amateur of biology, my interest in these theories lies chiefly in their application. I feel no doubt whatever that the adequate discussion of this fundamental question is (if I may use a battered but expressive phrase) one of the crying needs of the age. After Darwin it has become inevitable that moral conceptions should be systematically restated in terms of our new conception of the material destiny of man.

H. G. Wells.

Heatherlea, Worcester Park, Surrey.

It is a pleasure to conduct a controversy with so candid an opponent as Mr. Wells; but I am warned that my further share in such a controversy must be confined to a note. After reading his first article I thought that I understood Mr. Wells: his elucidation of his argument has however shaken my confidence. Putting aside, for want of space, all his other arguments, I infer that our main difference lies in this—that Mr. Wells admits the peaks of inborn human greatness to be higher now than before, but contends that the depths are also lower, whilst the median line is stationary. I gravely doubt the justness of such pendulum analogy; and can safely leave Mr. Wells to be tackled by a vastly more dangerous foe than myself—Prof. Weismann to wit. My own position has been, and is (see "Towards Utopia"), that the loftiest peaks do *not* arise out of plains but only out of mountain ranges. Mr. Wells will easily appreciate my analogy, which is diametrically opposed to his pendulum analogy. The onlookers must decide between us.

I believe that to a large extent we approached the question from different standpoints. I rely especially on the arguments from heredity and genius; which Mr. Wells seems inclined to ignore. Also I perceive that he has prudently ignored what I consider to be several of the strongest arguments in my criticism. Our readers will readily perceive which.

F. H. Perry Coste.

[Part IV., Mr. Archdall Reid's re-statement of his position, will, it is hoped, appear in our next.—Ed. Nat. Sci.]

III.

The Migration of British Birds.[1]

THE object of this article is to call attention to the results disclosed by the first systematic attempt, on a sufficient scale, to investigate the phenomena attending the migratory movements of the birds observed on our coasts. It must be distinctly born in mind that in Mr. Eagle Clarke's Digest no attempt has been made "to deal with the problems connected with the causes of the phenomena, the evolution of the migratory instinct, or other purely theoretical aspects of the general subject," but the aim has been to establish actual facts, from which alone the deductions are drawn. In this respect the Report is rightly said to differ from almost everything which has hitherto been published on the subject.

In the year 1879 that indefatigable naturalist, Mr. Harvie-Brown, printed in the *Proceedings* of the Natural History Society of Glasgow an "Ornithological Journal of the winter of 1878–79, with collected notes regarding its effects upon animal life, including remarks on the Migration of Birds in the Autumn of 1878 and the Spring of 1879." Again, in the following year he contributed to the same society a second report, bringing his observations down to the 30th of September, 1880. These two valuable reports referred to Scotland only, but in May, 1880, the scheme was enlarged, and their author, in conjunction with Mr. Cordeaux, published in the *Zoologist* for that month a "Report on the Migration of Birds in the Autumn of 1879.' In this report Mr. Harvie-Brown contributed the section for the East and West Coasts of Scotland, and Mr. Cordeaux that for the East of England; they were also assisted on this occasion by a large number of keepers of lighthouses and light-vessels on both coasts. This valuable additional help was due to the liberality of the Elder Brethren of the Trinity House and the Commissioners of Northern

[1] Report of the Committee, consisting of Professor A. Newton (Chairman) Mr. John Cordeaux (Secretary), Mr. J. A. Harvie-Brown, Mr. W. Eagle Clarke, Mr. R. M. Barrington, and the Rev. E. Ponsonby Knubley, appointed to make a Digest of the Observations on the Migrations of Birds at Lighthouses and Light-vessels, 1880-1887. Digest prepared by Mr. W. Eagle Clarke and presented to the British Association at Liverpool, 1896. *Rep. Brit. Assoc.* for 1896, pp. 451-477. Copies of the Digest may be obtained at the offices of the British Association, Burlington House, W., post-free, at 7d. each.

Lights, who permitted their officers to fill up returns sent to them for that purpose. The last-mentioned report was brought by Professor A. Newton before the meeting of the British Association held at Swansea in 1880, and a committee was then appointed to carry on the work under the auspices of the British Association. This committee worked for eight years, 1880 to 1887, both inclusive; and at the meeting of the Association held last year in Liverpool it presented a report consisting of a "Digest" of the "Observations on the Migrations of Birds at Lighthouses and Lightvessels" for the above period. This "Digest," ably prepared by Mr. William Eagle Clarke of the Edinburgh Museum of Science and Art, is the outcome of seven years' study of the mass of data supplied to the committee, and has necessitated the tabulation of at least 100,000 records, culled from several thousands of forms filled in by the light-keepers, in each of which forms the number of species mentioned was great and the dates of observation wide-ranging; some estimate may thus be formed of Mr. Clarke's labour, but it is harder fully to appreciate his skill and judgment. Even now Mr. Clarke does not claim to have exhausted the material, but believes that further research, to which he is still actively directing his attention, will yield results of a useful, and probably of an important, nature.

Before discussing the results, it may be well to explain how the material was obtained that is embodied in the Annual Reports issued by the Committee. The Report for 1887 (the last issued) consists of 175 pages, accompanied by an excellent outline map of Great Britain and Ireland, including the Orkney and Shetland Islands, showing the stations from which returns were made. The stations are: 4 in Iceland and the Faröes; 27 on the East Coast of Scotland, and 35 on the West Coast; 36 on the East Coast of England, and 44 on the West Coast of England and Wales; 4 in the Isle of Man, and 40 on the Coasts of Ireland. Information was also received from the following outlying stations: Heligoland, Zealand, Malmö (Sweden), the Casquets Lighthouse, and the Harnois Lighthouse (Guernsey). There are 195 stations in all. It will, however, be observed with regret that the South Coast of England from the South Foreland to the Start is not represented. From a large number of these stations returns were regularly received, while much information was also afforded by observers on the coast and culled from natural history journals. The observations from each group of stations were arranged under the month, or even day, by the recorder for the district, giving locality, species seen, direction of the wind, and other particulars of interest. In many cases where the identification of the species was doubtful, the wings of the birds killed at the lighthouses and vessels were sent for determination. Each recorder added such particulars as he deemed to be of value. It was this vast mass of original material furnished to the committee during eight years that Mr. Clarke undertook to examine *de novo*. It was this remarkable girdle of observing stations, forming a nearly complete circuit of the British Isles, with outliers in the most important directions, that constituted the essential feature of this exceptionally valuable series of observations, and the recorders again and again acknowledge the cheerful and intelligent co-operation of the light-keepers, without whose aid the plan could not have been carried out. Thus a record has been obtained such as has never before been attempted in this or any other country, the special character of which, to use the words of the "Digest," "can only be fully appreciated when it is realised that, in order to study the phenomena of bird-migration in the British Islands, it is necessary that the data upon which any deductions may be satisfactorily or safely founded should be based upon observations taken synchronously at stations encircling the entire coast." This cardinal and all-important condition has been now for the first time fulfilled.

The migratory movements appear to be exceedingly complex and subject to many influences, but it was found (1) that our shores form an accustomed highway and resting-place eminently favourable in situation and directly in the course of the legions of migratory birds which annually make a double journey between their northern summer and their southern winter quarters; these we may term "Birds of Passage." (2) "Our islands have a vast bird population of their own, and the majority of these birds belong to purely migratory species. Some of them are 'Summer Visitors' from the southern regions, or 'Winter Visitors' from continental Europe, Iceland, &c." (3) There are many birds sedentary in our islands, but migratory within these limits; these may be termed "Partial Migrants." (4) Mainly owing to our variable climate there are frequent "Migrations within the British area itself, and intermigration with the islands off our western coasts, especially with Ireland. These occur during the winter months, and hence they will be alluded to as 'Winter Movements.'" Often, owing to a combination of meteorological conditions, more than one of these movements may be observed in progress simultaneously.

Two main routes are found to be taken by the birds during migration: those species which spend the summer in Northern Europe pass along our east coast, taking their departure at some point or points between the north coast of Norfolk and the Shetland Islands, proceeding in a north-easterly direction, and returning by the reverse route in the autumn; those birds which visit us from Western Europe arrive in spring on our south-easterly coast, landing between the Wash and the Kentish shore, and return in autumn the same way; this latter is known as the East and West route. There are no essentially northern birds recorded in these latter flights, the species being suggestive of Central Europe, or of countries lying even still more to the eastward. The birds landing on the southern section of England spread either northward, up the east coast, or pass to the west along our southern shores; and it is pointed out that occasionally these immigrants, while passing northward along the eastern seaboard, actually cross the movement of the coasting emigrants proceeding southward. It is also found that the west coast does not receive any immigrants direct from the continent.

The spring movements on the west coast of England and the east coast of Ireland are trifling in comparison with those on the east, and it seems probable that those birds which pass to the Faröes and Iceland pursue the former route. The return autumn journey is by no means so simple on the west coast as on the east; for Ireland, the Isle of Man, the Hebrides, and the extremely irregular coast line exercise their varied influences. Mr. Clarke thus describes the course taken:—"The general route followed by these departing birds has its north-western source in the Outer Hebrides, and after leaving Barra Head it joins an important stream from the Inner Hebrides at Skerryvore. The course then followed is *viâ* Duheartach, Islay, the Wigtonshire coast, the Isle of Man, Anglesey, and the South Bishop (off Pembrokeshire). Finally the south-western coast of England is reached (possibly in part by an overland route across Devonshire and Cornwall) between the Scilly Islands and Start Point." In its course south it receives several tributary streams at various points, and "at the Bristol Channel others from Western England and Wales, and often from the south-eastern coast of Ireland." The coasts of Ireland are not in themselves a main highway, and "the majority of the birds on the shores of the sister isle are probably the migratory members of her own avifauna."

Astonishment has often been aroused by the wonderful migratory flights to be witnessed at the proper season on the little island of Heligoland, so graphically described by the late Mr. Gätke, and it could not fail to be of interest to ornithologists, especially those residing on the east coast, to learn whether and to what extent these migratory hosts affected the arrivals on their own shore. With no little disappointment, therefore, we learn that "a study of the phenomena of migrations at the stations on the east and west sides of the North Sea compels the investigator to come to the conclusion that Heligoland and Britain draw their migratory hosts from different sources," and that although "it is not impossible or improbable that birds may *occasionally* cross the German Ocean by an east to west flight in the latitude of Heligoland, our data lead us to believe that such cases are the *rare exception* and not the rule." There is no more astonishing feature in the whole range of this wonderful subject than the arrival on our east coast of birds whose natural habitat is Eastern Asia, and which must have crossed the whole of Europe, finishing with the passage of the North Sea. Some of these, too, are delicate warblers of the smallest size, and arrive here after the British representatives of their kind have left us for the south. The past autumn has been remarkable for these strange occurrences.

Of equal interest with the discussion of the routes taken is the section devoted to "Seasonal" migration.

The difference between the behaviour of the birds on their autumn and spring passages is very marked. At the close of the summer the great business of the year—procreation—is over, food is still abundant, and the birds pass leisurely along, their numbers swelled by the broods of young ones; but in spring the conditions are quite different, they hurry on to their breeding-grounds, and the opportunities for observation are far more limited.

Commencing with the Autumn Immigration, it is remarked that, notwithstanding the month of July being the height of the Arctic summer, certain birds, such as the whimbrel and knot—and a few others less frequently—are recorded as arriving in that month. In explanation of this fact, Mr. Clarke remarks that the majority of these early arrivals are probably non-breeding birds which have not, perhaps, passed far beyond the limits of Britain on their spring journey northward; and in support of this conclusion he points out that these migratory pioneers, so far as reported, are all adults. In August the return movement of twenty-six species of birds, whose summer haunts lie entirely beyond the British area, is recorded; and, in addition to these, the northern representatives of several species which are summer visitors to Britain also put in an appearance. In September a marked increase takes place in the number of immigrants, both as to species and individuals; over forty species which do not summer in Britain, including those already recorded for August, are enumerated. In October the return-current reaches its maximum, and it is in this month chiefly that the great "rushes" of migrants arrive on our shores; these rushes, as will be explained, are entirely due to meteorological conditions. Forty-seven species of regular birds of passage are recorded in this month, besides many other birds which breed in both Northern Europe and Britain. It is noticed, however, that signs of slackening are apparent in some respects, for certain species have already ceased to occur, and a few others appear less numerously. The migratory movements in November are on a much reduced scale. A few northern summer birds which do not breed in Britain, such as the redwing and field-fare, still arrive early in the month, often in considerable numbers, also a few northern species, which we are accustomed to regard as hard-weather birds, become more numerous; but Mr. Clarke found that "after the middle of the month the immigration of such birds as spend the summer in the north entirely ceases, with the exception of certain marine species (ducks, gulls, grebes, swans), whose late movements to the south are dependent upon severe weather conditions. This is entirely contrary to the views hitherto propounded regarding the limits of these movements, but it is, nevertheless, a fact well sustained by the enquiry."

Besides those derived from the north, there are certain Autumn immigrants that reach our shores by a westerly movement along the east and west route. These birds cross the narrow passage of the North Sea, and land on the south-eastern shores of England, performing the journey by daylight. The chief arrivals are in September and October, ordinarily ceasing in the middle of November; but in exceptionally severe weather they are again renewed, the birds at that time chiefly passing westward to the warmer portions of the kingdom. The latter movements, however, are exceptional, and due to meteorological causes.

The Autumn Emigration begins in July, and is probably led off at the end of that month by the adult cuckoos and swifts. Towards the close of the month also there are occasional records of movements of small birds, such as wheatears, redbreasts, warblers, pipits, swallows, etc.; but Mr. Clarke adds a caution that many of this latter class are "outcasts," whose parents are engaged with second broods, and they may, in their wanderings, reach the coast where their appearance would be duly recognised. During August, however, the retreat sets in in earnest, and a large number of migratory birds take their departure; with them are probably some species which are mere birds of passage on their way south from more northerly breeding-stations. September witnesses the "height and close" of the emigration of the bulk of the smaller British summer visitors; most of these are absent from the records of October, which month witnesses the departure of the laggards. The partial migrants are, however, much on the move in October, their numbers doubtless being largely recruited by arrivals of the same species from the north; but the first half of November brings the movement to a close, and the ordinary southward retreat, both of birds of passage and of British emigrants, ceases.

The first indications of the Spring Immigration sometimes occur as early as February, but these are almost entirely confined to partial migrants within the British area, such as pied wagtails and lapwings; especially mild weather may, however, bring us an occasional wheatear, or even a solitary swallow. March witnesses the return of many species which had quitted our shores in consequence of the severity of winter, and generally a few of our regular summer visitors, such as the whinchat, wheatear, willow wren, chiffchaff, and even an early sand martin or cuckoo; but we must wait till April for the first pronounced influx of the summer migrants. In this month the arrival of thirty-seven species is recorded in the chronicles; the birds of passage, which only visit us on their way to their northern breeding-stations, also begin to put in an appearance at this time. Early in May the spring immigration reaches its full force, and before the close of the month comes practically to an end. The number of passing migrants is also greatly increased, and by the middle of June the bulk of the Polar breeding birds have passed, although some few which go very far north, such as the knot and the grey plover, may linger a little longer.

Some interesting facts are to be gleaned from the records of these spring arrivals. Thus, it is found that the earliest arrivals appear in the south-western, that is, in the warmest section of the British area; also that the earliest arrivals amongst the summer visitors are those individuals of the species with more extended range which stay to breed in Britain; this is exemplified by the snipe, some of which are already breeding in this country before the more northerly breeding birds of the same species have all passed on; the same may be said of the great crested grebe. Other observations are quoted in proof that the migrants bound for the far north are the last of their kind to appear in the British area.

During all this time the corresponding spring Emigration is going on, and those birds which have wintered with us are leaving for their summer haunts. In February the geese begin their northern movements, and larks and rooks are proceeding to the Continent by the east and west route. In March the northerly current becomes more pronounced, shore-larks, geese, ducks and divers taking their departure, and the Continental outpour gains in intensity. In April no less than thirty-four species which have wintered with us are recorded as departing for their northern summer haunts, and the final departures by the east and west route take place. May sees the maximum amount of emigration to the northern breeding grounds, both of species which have wintered with us and of birds of passage. Fifty-three species of regular emigrants are recorded for this month, but it may be the middle of June or even later before the last of the Polar breeding birds have taken their departure. It is noteworthy that this great stream of migrants passing to the north-east, as well as the exodus by the east and west route, each takes place from its allotted section of the eastern coast, "a few only of the species, such as the redwing, wheatear, white wagtail, barnacle goose, swans, whimbrels, etc., passing up our western coasts, possibly *en route* for Iceland."

There remains one other important section of the "Digest" to notice, that is the one devoted to Meteorology, and for the purpose of establishing "the actual relationships between migratorial and meteorological phenomena." The material at the disposal of the committee is said to be both extensive and reliable, the "Daily Weather Reports," based upon observations made at fifty-four widely-distributed stations, and issued by the Meteorological Office, having been used. As Mr. Clarke explains at length, migration is controlled, if at all, by meteorological conditions obtaining in the region from which the birds are starting, and obviously not by those in the region to which they are going. Thus, British immigrants are affected by Continental conditions, and British emigrants by our home conditions. "As a result of an extensive series of comparisons instituted between the two sets of phenomena, it has been ascertained that they are most intimately associated, and that a knowledge of the meteorological conditions prevailing during the movements in most instances contributes in no small degree to a correct interpretation of their precise nature and the seat of their origin." Unless, therefore, the conditions at the point of departure are favourable to emigration, or are such as to render the journey imperative, it is either deferred or undertaken most reluctantly.

These weather conditions are of an exceedingly complex nature, and Mr. Clarke is at some trouble to explain their origin and effects upon migration : under ordinary conditions the migratory movements proceed as usual; in somewhat unsettled weather they may be in some degree quickened, but are still normal ; exceptionally stormy weather arrests all migration, and the barred-up stream is detained for a favourable opportunity; its eventual release results in one of those "rushes," with which dwellers on the east coast are so familiar, but which never fails to excite their astonishment.

Temperature is also an important factor in these movements, and indeed a decided fall of the thermometer is a frequent accompaniment of the anti-cyclonic conditions with atmospheric calms, so highly favourable for sustained flight. Of course these conditions apply equally to both spring and autumn movements. It does not follow, however, that the favourable weather prevailing when the journey is commenced will be sustained during the whole of its course, and possibly the migrants may encounter very unsuitable weather before reaching their destination; under these circumstances they arrive in a state of great exhaustion, and many individuals probably perish *en route*.

Fog, which often prevails during the autumn and winter periods of flight, appears greatly to inconvenience the migrating birds, and it has been noticed that even non-migratory birds, such as the sparrow, have occasionally lost their bearings through it.

The simultaneous autumnal influx and efflux already mentioned has been found to occur in periods when anti-cyclonic conditions prevail over an area sufficiently wide to cover both the points of departure and arrival, the result being "a great simultaneous inpouring of birds on our east coast, and a general outpouring from *all* British coasts of migrants of many species." The comparatively simple east to west route and the more complex northerly route are both governed by the same meteorological influences, and take place chiefly during calm and cold anti-cyclonic periods, being interrupted by rough weather and renewed with increased energy on the return of suitable atmospheric conditions.

Another result of the study of the returns is the more just appreciation of the influence of the winds on migration. Mr. Clarke, after a careful study of the evidence, arrives at the conclusion "(1) that the *direction* of the wind has no influence whatever *as an incentive to migration*; but that (2) its *force* is certainly an important factor, inasmuch as it may make migration an impossibility, arrest to a greater or less degree its progress, or even blow birds out of their course." The fact that easterly and south-easterly winds prevail during the great autumnal migratory movements is shown to be the direct result of the type of weather favourable to such movements, and there appears to be no reason, other things being equal, why W., N.W., and S.W. winds, if not too strong, should not be in every way suitable for migratory purposes, except that these winds indicate cyclonic disturbances in North-Western Europe, which are fatal to migration between that region and the British Isles.

It has been pointed out that occasionally the migrating birds pass into gales *en route*, often with disastrous consequences.[1] Should such disturbances continue even the most hardy birds suffer severely. It has again and again happened that the tide-line has been thickly strewn with guillemots, razor-bills, and puffins; little auks have been known to stray even into the streets of inland towns, flocks of skuas have been met with far from the coast, and even the ocean-loving fulmar has been driven shoreward in numbers. To this cause we owe not only the unusual spectacle of storm-petrels flying inland, but also the record of some of the rarest members of this remarkable family, which probably spend their whole existence as ocean wanderers, with the single exception of a brief visit to the shore at the period of reproduction. If these dire effects are produced on birds possessed of such marvellous powers of flight and protracted endurance, what must be the fate of the delicate summer-migrants when buffetted by the stormy winds in mid-sea!

The above are some of the deductions derived from a careful study of the voluminous data amassed by the British Association Committee on Migration, and it is most satisfactory to learn that Mr. Clarke does not consider he has by any means exhausted the material at his command, and is even now engaged in working out the details for each species. Ornithologists must thoroughly endorse the hope of the Committee that he will receive some encouragement to continue the first really scientific attempt to work out the phenomena attending the migration of birds.

THOMAS SOUTHWELL.

Norwich.

[1] In the *Norwich Mercury* for November 11, 1786, it is stated that the master of a Newcastle collier, being within a few leagues of Easton Ness, on the coast of Suffolk, sailed through water that appeared unusually black, and on inspection it was found to be occasioned by a vast number of drowned birds, and those birds woodcocks.

IV.

The Horn Expedition to Central Australia.[1]

THE expedition of the results of which these volumes furnish an account, was inaugurated and its expense defrayed by Mr. W. A. Horn, who in order to give it a semi-national character invited the Premiers of the principal Australasian Colonies to nominate scientific representatives. In this way Professors Baldwin Spencer and Ralph Tate, Dr. E. C. Stirling, and Mr. J. Alexander Watt were selected, whilst Mr. C. A. Winnecke was chosen as surveyor and meteorologist. These constituted the scientific staff, and in addition there were camel-drivers, collectors, prospectors, &c., the party numbering sixteen in all. The object of the Expedition was "the scientific examination of the country from Oodnadatta to the M'Donnell Range; the collection of specimens illustrative of the fauna, flora, and geological structure and mineralogical resources of that region, and the illustration by photography of any remarkable natural features of the country traversed; the securing of photographs of the aborigines in their primitive state, the collection of information as to their manners, customs, and language, and the reproduction of their mural paintings." How far these aims were achieved the following paragraphs endeavour to show. We may arrange them under the headings of the several reports.

Narrative.

Professor Baldwin Spencer, in the course of about 130 pages, gives an account of the journey, dividing it into five sections, and treating of three different types of country, namely, the desert or eremian—a term frequently, but erroneously, applied to the centre of Australia as a whole—and the steppes, which are subdivided into lower steppes and higher steppes. The lower steppes extend over the area occupied by the great Cretaceous formation, and are characterised by gibber plains, loamy flats, and terraced hills capped with desert sandstone. "At Lake Eyre the land is 39 feet below sea level, gradually rising to a height of one thousand feet at its northern limit." The higher

[1] Report on the Horn Scientific Expedition to Central Australia. Part I. Introduction, Narrative, Summary of Results, Supplement to Zoological Report. Pp. xii., 220, pls. xi. and map. Part II. Zoology. Pp. iv., 431, pls. xxix. Part III. Geology and Botany. Pp. 204, pls. ix. Part IV. Anthropology. Pp. 200, pls. xix. Edited by Professor Baldwin Spencer, of Melbourne University. London: Dulau and Co.; Melbourne: Melville, Mullen & Slade. 1896. Price 21s., 30s., 10s. 6d., and 21s. respectively.

steppes consist of ridges of Ordovician and Precambrian rocks, with an average elevation of 2,000 feet. The climate has rainy seasons alternating with periods of extreme drought; the former are always short, but the latter may be very long. The gibber plains (from a native Queensland word, meaning "a stone") are a striking feature of the lower steppes; they are covered with a layer of purple-brown stones, varying from an inch to perhaps a foot in diameter, smoothed by the constant friction of wind-borne sand grains; they merge into loamy flats, where the Cretaceous rocks are not capped with sandstone.

Professor Spencer is of opinion that the development of thorns, so conspicuous in many of the desert plants, is an adaptation to climatic influences and has very little, if anything, to do with protection against animals. A similar view was, we believe, propounded some years ago on independent evidence by Professor Patrick Geddes. A very striking picture is drawn of the contrast between the appearance of the country in the dry and wet seasons, especially in regard to animal life. In the former everything is parched and silent; dead shells of molluscs, carapaces of *Estheria*, and footmarks of frogs are the only traces of life. In the latter, the trees have put out a fresh growth of leaves, many kinds of birds have suddenly appeared in flocks; the croaking of frogs is incessant, whilst crustaceans and water-beetles swim about in the pools. Numerous observations were recorded on the colours of animals, the net result of which is thus summarised by Prof. Spencer:—"(1) That in the dry season when food is scarce and the sum total of activities is at the lowest point, the various animals such as frogs and lizards are dull coloured, but that this dull coloration has not of necessity (as in the case of *Amphibolurus barbatus*) any definite relation to the environment, though it is often in general accord with it. (2) That in the rainy season when food is plentiful and the sum total of the activities is at the highest point, various animals are highly coloured, but that this often brilliant coloration has nothing to do either with choice of partners (reaching its climax after pairing has taken place) or with protective coloration—sometimes even it renders the animal more conspicuous."

Regarding the relative advantages of rodents, for example, and marsupials in the struggle for existence, the following observation is of interest. The former take care of themselves at an age when the latter take refuge in the maternal pouch. This not only handicaps the marsupial parent in flight, but has the result that if she be captured her family perishes too, whereas if such a mischance happen to the rodent only one life is sacrificed.

With observations and discussions such as these, Professor Spencer varies the descriptions of the journeys from one water-hole to another, or over the sandhills in the waterless waste to visit Ayers Rock and Mount Olga,—two remarkable masses standing up from the level plain, of which he gives us vivid and striking pictures with both pen and camera. Certainly this narrative, brief though it is, will take rank among the classics of this form of literature.

Zoology.

The volume which contains the zoological results of the Expedition contains 431 pages and is illustrated by 29 plates. It is the work of several authors. Professor Spencer, besides editing the whole, treats of the Mammalia (excluding Muridæ), Amphibia, Crustacea, and contributes Appendices on the stridulating organ of *Phlogius crassipes* and on an earthworm (*Acanthodrilus eremicus*). Mr. North describes the Birds; Messrs. Lucas and Frost, the Reptiles; Mr. Zietz, the Fishes; Prof. Ralph Tate and Mr. Hedley, the Mollusca; Mr. Lower, the Lepidoptera; the Rev. T. Blackburn, the Coleoptera (except the Carabidæ for which Mr. T. G. Sloane is responsible); Mr. H. R. Hogg, the Araneidæ; Mr. J. G. O. Tepper, the Orthoptera; Mr. W. W. Froggatt, the Honey-ants; and Mr. E. R. Waite, the Muridæ.

Nineteen species of Mammalia are known from Central Australia, and of these five are peculiar to it. The following are new to science:—*Phascologale macdonnellensis*, *Sminthopsis larapinta*, *S. psammophilus*, and *Dasyuroides byrnei*. *Notoryctes typhlops* is probably the sole representative of the only family confined to this region. Professor Spencer has now had over forty specimens in his possession, and he and Professor Stirling are preparing an account of the teeth, fur, and reproductive organs. It is interesting to note that a rudimentary pouch may be present in the male and that the reproductive organs, which have been examined by transverse sections, show the animal to be a modified marsupial and in no sense a link between this group and the monotremes. As regards the jumping forms (*Hapalotis* and *Antechinomys*) it is remarked that "it is really somewhat difficult to see what is exactly the advantage gained by such small forms of animals in this saltatory mode of progression. . . . Both are far too small to spring over the grass tussocks, and perhaps the most noticeable point in connection with this matter is that the true mouse, which lives in great numbers in the same part and has not taken on this saltatory method, thrives just as well. . . . Possibly the real advantage in the saltatory method of progression amongst these small forms lies in the greater difficulty of pouncing down upon an animal travelling by leaps and bounds rather than in any advantage gained in the way of speed." It is curious that two animals so differently organised as *Hapalotis* and *Antechinomys* have the same habit and live side by side, the former very common, the latter extremely scarce. Mr. Edgar R. Waite discusses the Muridæ in a separate chapter.

The Birds were collected with great care by Mr. Keartland. Seventy-eight species are included in the collection, five of which are described as new, whilst the Princess of Wales Parrakeet (*Polytelis alexandræ*) is made the type of a new genus, *Spathopterus*, on account of the spatulate extremity of the third primary of the wing in the male.

The Reptiles include forty species of lizards (nine new) and twelve of snakes (one new, the type of a new genus *Hornea*). The lizards belong to the families Geckonidæ, Pygopodidæ, Agamidæ, Varanidæ and Scincidæ. A great amount of variation has been shown to exist in many forms, and not a few so-called species are reduced by the authors to varietal rank. The following account by Professor Spencer of the fate of a specimen of *Tiliqua occipitalis* is interesting:—"Having my hands full of specimens, I asked a black fellow to look after it and not to let it escape, when to my surprise he simply put it down on the hot sand. It was perfectly alive when put down, . . . and when placed on the ground it began to travel at some rate, but after going five yards its movements became slower and before ten yards had been traversed they ceased and the animal was quite dead—simply apparently baked to death by contact with the hot sand."

The Amphibia, Professor Spencer tells us, are remarkable, firstly for the paucity of species, and secondly for the great number of individuals found at certain seasons. The characteristic forms belong to five species, four of which may be described as burrowing frogs. *Hyla rubella* is probably a direct descendant of some form which inhabited the region during more favourable climatic conditions; the other species have probably immigrated from the east or south east. *Chiroleptes platycephalus* is remarkable for the power of storing up water in its body, on account of which it is sought for and dug out by the blacks in seasons of drought. *Hyla gilleni* is described as new.

Of Fishes Mr. Zietz describes eight species, of which six are new, but there are no general notes or discussions of moment.

The number of Mollusca known to inhabit this region has been increased from three to twenty-five, of which only four extend beyond the area. "The facies of the fauna approximates more to that of subtropical and temperate West Australia than of any other part of the continent, and is in strong contrast with the highly differentiated fauna of tropical and sub-tropical Queensland, situated to the east of the Cordilleras, to which it is geographically equally near. The limited number of genera represented, together with the facts of their geographic distribution, would seem to indicate a primitive population, which has been maintained in an isolated condition by climatic and geologic changes." Mr. Hedley contributes an anatomical appendix, in which he advocates the view that *Xanthomelon* should be advanced from subgeneric to generic rank, and removes *Microphyura* from the Rhytididæ to the Endodontidæ, remarking that it would seem to be of high antiquity and of antarctic origin and is probably one of the most primitive of Australian snails.

The Crustacea are dealt with by Prof. Spencer and Mr. T. S. Hall. As regards their habits they may be divided into two groups:—"(1) Those which can burrow and so tide over a certain length of dry season (*Astacopsis*, *Telphusa*). (2) Those which require that their eggs shall be dried up prior to development (*Apus*, *Estheria*, *Limnadopsis*, *Limnetis*, *Eulimnadia*)." The rate of growth of *Apus* and *Estheria* seems to be extraordinary: specimens of the former measuring 2½ to 3 inches were found, and there was reason to believe that these had developed from the eggs in a few days. Such capabilities are of obvious utility in a region subject to frequent and prolonged droughts.

Of Insects lists are given of several orders, but there is evidently much more work to be done in this department. Of special interest are the Australian honey-ants, of which Mr. Froggatt gives a particular account. These belong to the genus *Campanotus*, of which more than thirty species are known from Australia, yet only three of these have developed the honey-storing habit. The peculiarity consists in this, that certain individuals are set apart as receptacles and fed with honey by their fellows till their abdomen is hugely distended, and in subsequent periods of scarcity made to disgorge their precious burden.

Finally, we must mention the description of a stridulating organ in *Phlogius* by Prof. Spencer, similar to those which have already been discussed by Mr. Pocock (Natural Science, vol. vi., p. 44).

Geology.

The Physical Geography, Geology, and Palæontology are dealt with by Prof. Ralph Tate, Mr. Watt, and Mr. Smeeth. As already mentioned, it would appear from their observations that the high ground of the McDonnell and other ranges is formed by Precambrian and Ordovician rocks, while the foot-hills consist of Cretaceous strata. The great plains are of post-Cretaceous age, and covered by a series of deposits regarded as intermediate between the Cretaceous and Eocene, and named Paleocene. Most of the valleys result from the erosion of Ordovician limestones by the rivers. The two lakes met with are rapidly drying up, and the largest, Lake Eyre, with an area of 5,000 square miles, is calculated to be 39 feet below the sea-level.

The determination of the great series of highly altered rocks as Precambrian is based upon their general resemblance to undoubted strata of this antiquity in other parts of Central and South Australia. Regional metamorphism is extensive, and large areas are occupied by eruptive rocks, of which several types (*e.g.*, diorites and dolerites) are described in a petrographical section of the Report. Foliation planes are abundantly obvious, and have been previously mistaken for planes of sedimentation; but there is a remarkable absence of contortions.

The Ordovician rocks comprise ripple-marked and false-bedded sandstone, quartzites, and limestones. No evidence of contemporaneous volcanic action was obtained by the expedition; but many typical fossils were found in the limestones. A post-Ordovician conglomerate was also noticed in some localities.

The Desert Sandstone, of "Paleocene Age," is a remarkable deposit. It is never more than fifty feet thick, and is interesting as being the chief source of the Noble Opal so often obtained from Australia. This mineral occurs disseminated through the rock in minute fragments, though sometimes coating joints or fractures and replacing wood and shells.

Botany.

The Report on the Botany is the work of Prof. Ralph Tate. So far as systematic botany is concerned, he has been able to add eight new species to science, to record sixteen species new to South Australia, and 112 new to the Larapintine region. But the most interesting portion of his Report deals with the geographical distribution of the plants observed, with the influence of the physiography of the district on the present flora, and with the origin of the flora of this region. The author has already contributed a valuable paper on this subject, so far as it affects the whole continent, to the Australasian Association for the Advancement of Science, and he had hoped that in the McDonnell Range the explorers might find some of the pristine flora of early Tertiary times, which had died out of the lower-lying regions. In this, however, he was disappointed. Nevertheless, he comes to some valuable conclusions largely confirmatory of his previous views. These are briefly stated as follows. The Larapintine tableland was isolated, except perhaps in a northerly direction, during the deposition of the Upper Cretaceous beds. The marine submergence was replaced by a lacustrine area, which continued during the early Tertiary and into Pliocene times. During this period a cosmopolitan flora prevailed. In Post-Pliocene times a high state of desiccation was reached, which has continued till to-day. The cosmopolitan flora has largely disappeared and its place is occupied by an oriental immigration, more especially over the previously submerged areas.

Thus the flora of the Larapintine region is a mixture of endemic and immigrant (oriental) forms, and a most interesting statistical table shows that a large percentage of the oriental forms have burr-like or adhesive fruits rendering their immigration with that of animals coming over the land-areas possible.

It is unfortunate that Prof. Tate does not discuss more fully the point already touched upon by Professor Spencer in the Narrative of the Expedition relating to the spiny character of the plants making up the scrub and desert flora. That a dry and arid climate requires some modification of the plants will not be denied; but to argue that the spines have nothing to do with protection against animals, firstly, because there are practically no animals in that region, and secondly, because the camels the expedition introduced fed on the thorny acacia as well as on the juicy *Claytonia*, will not settle the question. The very absence of indigenous animals might be taken as proof that they had been driven away by the protective adaptations of the plants, in some cases by the spines and thorns, in others, such as the succulent plants, by the distasteful or poisonous cell-sap. Nor is the fact that the plant is not protected against an enemy, which it has no need to fear, in the least surprising. Indeed it is what might be expected.

Anthropology.

This section of the Reports is the work of Dr. E. C. Stirling and Mr. F. J. Gillen.

For a primitive race, the aborigines of Australia are fairly numerous, the total population being estimated at 100,000; and when we bear in mind the fact that they cover an area of about 3,000,000 square miles, they may be considered to form a remarkably homogeneous group of people. In some anatomical features, such as the capacity of the cranium, the square orbits, the pronounced brow ridges, the shape of the vertebral column, and the form of the tibia, they are undoubtedly more ape-like than any other large race of human beings. On the other hand, in their stature, in the character of their hair and in other respects, they differ in a most remarkable degree from those primitive little groups of negroid people which are, or were quite recently, found in Tasmania, the Andaman islands, the highlands of the Philippines, the centre of Africa and elsewhere.

Now, our knowledge of the Australian race, so far as it is accurate and trustworthy, has hitherto been founded on the facts collected by travellers among the tribes of the coast or of the boundaries of the European settlements; and it has been a matter of some difficulty even to anthropologists to eliminate satisfactorily from the purely aboriginal features, those due to contact, in recent times, with the Malays, the Papuans and the European settlers. A thorough investigation of a tribe of Central Australians has been, therefore, a profound desideratum; and to the members of the Horn Expedition the thanks of the scientific world are due for the care and skill they have displayed in filling up the vacant places in our information.

If, however, the anthropologist expects to find in the volume now before us, anything very startling or novel, he will be doomed to disappointment. No living *Pithecanthropus erectus*, no little negroid race, practically no new or important weapons, customs or myths, have been found. The value of this excellent contribution to our knowledge of the Australians lies in the fact that it will strengthen and confirm our views, which have hitherto been based on insufficient data, concerning their position in the great family circle of human beings; and by all who wish to promote the true interests of science this will be as much appreciated as would have been a series of startling novelties. The evidence that is now before us points to the conclusion that the Australians have sprung from a primitive stock, less primitive perhaps than that which gave rise to the Tasmanians, Andamanese, and other curly headed negroid races, but more primitive than that which gave rise to other races of mankind. It seems that having taken some steps in advance they finally began to degenerate socially.

As regards their primitive anatomical features, the three skulls and one skeleton described in this volume, do not afford us a good basis for further generalisation ; but it may be noted that in the two skulls fully described, the cranial capacity is well under 1,300 cc. and there is marked dolichocephalism. At the same time there is not the pronounced prognathism that we find in some savage races, both skulls being in this respect close to the boundary line between prognathism and mesognathism. This valuable little scrap of anatomical knowledge indicates, then, that the Central Australian strikingly approximates to what we have from previous knowledge considered to be the average of Australian characters.

Turning now to other anthropological features, we find that the Central Australian's weapons are very largely composed of wood. Wooden spears and shields, throwing-sticks, and boomerangs are his characteristic weapons. Metals were unknown to him until the time came when European travellers left meat tins behind them. He can chip stones and even porcelain telegraph insulators and fashion them into rough knives and spear heads ; but, as in the case of the Tasmanian and Palaeolithic man, the idea of polishing stones or grinding them to a smooth even blade has never occurred to him.

The Australian is, however, a step in advance of his late Tasmanian neighbour in the matter of weapons. He can not only firmly fix his rough stone blades to sticks by means of resin and sinews ; but he has invented two characteristic weapons,—the throwing-stick and the boomerang. Professor Stirling's statement that the Central Australian does not expect or intend the boomerang to return to him when he throws it, will be of interest to those who have tried in vain to demonstrate the action of this weapon with specimens taken at random from a museum.

To many anthropologists the most interesting portions of this volume will be those which deal with the social customs, beliefs and mental traits of the tribes visited by the expedition ; for in them we find evidences of a past in the history of the Australian black about which there must still remain much doubt and uncertainty. It is a matter of regret that one who is so competent to form opinions as Prof. Stirling in matters anthropological, should be so modest in expressing them ; but, after attempting to read between the lines, we cannot help feeling that Prof. Stirling must share the view that socially the Australian has considerably degenerated.

Mr. Gillen states in his account of the Arunta tribe that there is no belief in a devil, and this is almost the only reference in the volume to beliefs in spiritual beings. Several customs, however, which exist among many tribes of Australians besides those of the Central regions, indicate that formerly the belief in devils and spirits must have been prevalent. The cutting of the hair, the various mutilations of the body, and more particularly the practice of knocking out the incisor teeth are, in all probability, customs associated with former superstitions which are now lost and forgotten. In his masterly essay entitled "Over de Mutilatie der Tanden etc," the late Dr. Wilken showed that among the Malay races the breaking and filing of the teeth originated as an exorcism of an evil spirit, although in many races at the present day the original meaning of the custom has been forgotten. Again, the widely distributed hair-myth shows itself in the girdle of human hair worn by the Australian warrior who believes that it imparts strength, courage, and accuracy of aim to the wearer. It is difficult to believe that this myth could have arisen independently in a race of savages who did not believe even in one devil. The white paint with which the Australian savages and other dark-skinned races of mankind daub themselves when they mourn the loss of a relative was in all probability, as Mr. Frazer and others have suggested, merely a disguise used to confound the returning ghost of the departed.

The peculiar marriage customs of the Australians also point to degeneration. The marriage of the Australian is what Maclennan called from an Arabic word a "Deega" marriage; that is to say, the man takes his wife to his own camp. Moreover, polygamy occurs, and occasionally at any rate a man captures by force a wife, belonging of course to a proper sub-phratry, from a neighbouring tribe; but associated with these patriarchal customs we have the fact that the child of the marriage belongs to the phratry of the mother and the man may never on any pretence speak to or look upon his wife's mother. The marriage customs then show at least a change from a more primitive matriarchy to a mild form of patriarchy.

The rude art, and the songs at the corroborrees also show that important changes in their history have taken place. The very valuable series of drawings copied from inscriptions found at Ayers rock and elsewhere by members of the expedition, are but conventional signs of which, in many cases, the meaning could not even be guessed. What, it might be asked, was the character of the original drawings from which these signs have been derived? The modern Australian cannot draw like his neighbour the Papuan savage, but, in all probability, his ancestors could, and the art has been to a great extent lost. As regards the songs too, surely they must have had a meaning in bygone times. Is it not at least probable that they were formerly of the nature of incantations or exorcisms, and that their significance has been lost with the ghostly beings to whom they were addressed, and that they remain now merely as formalities?

It might perhaps be urged that, as changes similar to those referred to have also occurred in the history of more civilised races, they ought not to be spoken of as signs of degeneration. Standing by themselves they are not; but it seems to be a peculiarity of the Australian race that no indigenous lines of progress have been made, and the resultant of the changes has been a loss of the principal features of social organisation. And where, it may be asked, are there signs of recent progress? Even in counting the Australian has advanced no further than the number four.

It might be asked why the Australians should remain as the only large race of human beings in the world, occupying a continental island of great area, which shows but little signs of progress since palæolithic times? It seems possible that in bygone days when the Australian continent was, as the expedition proves, a well-watered and fertile land, the human race which inhabited it was progressing on lines comparable to those of the Negroes, American Indians, or even Papuans; but the gradual desiccation of the land in Quaternary times led to splitting up of large social communities and consequently to degeneracy as regards social organisation.

It is only the possibility of living in large communities that has allowed the development of arts and war and social organisation towards a high standard; and races that are obliged, from the nature of their land, to live in small groups to obtain the necessary food for mere existence, must have stood still or gradually degenerated.

Before bringing this notice to a conclusion mention must be made of the very important series of gesture signs that are given. They form a most interesting and valuable chapter in the science of gesture language and will be extremely useful to many students of anthropology.

SUMMARY.

SIXTY pages of Part I. are devoted to an abstract of the results of the Expedition and a statement of some conclusions to which they lead. The Summary we have already attempted to summarise, but this is almost impossible in the case of the chapter entitled "General Conclusions" so concise is it already. It consists of a masterly and suggestive discussion of the origin and relations of the present flora and fauna of Central Australia from a geological as well as a biological point of view. The conclusion at which the author arrives, is that the three sub-regions (Euronotian, Autochthonian and Eremian), into which Prof. Tate divided Australia on botanical grounds, must be modified and replaced by another division into three as follows:—

(1). TORRESIAN, including Papua and north and north-eastern Australia as far south as the Clarence River.

(2). BASSIAN, including the eastern and south-eastern coastal strip, lying between the coast-line and the Dividing Range south of the Clarence River, and also Tasmania.

(3). EYREAN, including the whole of the interior, southern and western part of the continent.

In conclusion, it only remains for us to congratulate Prof. Spencer on the manner in which he has seen these beautifully printed and illustrated volumes through the press. Mr. Horn may justly feel proud of them, for they are the crown and fruition of a work to which he must have devoted not merely treasure, but much anxious thought and consideration.

V.

Are the Arthropoda a Natural Group?

A NATURAL group from the stand-point of genealogical zoology is a group the branches of which all start from a common stem or ancestral stock which, while giving rise to them, grouped and connected at their origin with one another, gives rise to no other line of descent.

The Arthropoda appear to me to form such a natural group. Their ancestral stock, the Pro-Arthropoda, joins with the ancestral stocks of the Chætopoda and Rotifera, forming the large natural group of the Appendiculata. I urged this connection and proposed the term Appendiculata in 1878 in the preface to the English edition of Gegenbaur's Elements of Comparative Anatomy. At that time German zoologists placed the Chætopoda in the group Vermes and cut them off from all association with the Arthropoda. Huxley on the other hand had adopted Macleay's term Annulosa for a group including Chætopoda and Arthropoda but excluding Rotifera.

By associating the Chætopoda with the Arthropoda the parapodia of the former were identified with the appendages of the latter, and it became necessary to interpret the head of the arthropod in relation to the prostomium and peristomium of the chætopod.

I suggested in 1872 that "the ophthalmic segment alone in Arthropoda represents the prostomium [of Chætopoda], the antennary and antennular segments being aboriginally metastomial and only prostomial by later adaptational shifting of the oral aperture."

Moseley's discovery soon after this of the parapodial nature of the jaws of *Peripatus*, and the later discovery of the paired nephridia in every segment of its body, strengthened the hypothesis of a genetic connection between chætopods and arthropods.

In 1881 my study of *Limulus*, *Scorpio*, and *Apus* led me to the view that the prostomial antennæ of the chætopod are retained by *Peripatus*, the Myriopoda, and Hexapoda, but have been entirely lost by Crustacea and Arachnida, whence I proposed to call the former group Ceratophora and the latter Acerata.

The latest results of embryological investigation have shown that this view is probably incorrect, and according to Professors Korschelt and Heider a large and important application of my theory of the passage of metastomial parapodia into association with the prostomium (and therefore a præoral position) is what with all reserve and caution they hold to afford the best explanation of the facts at present known. It appears that the antennæ of *Peripatus* and the antennæ of myriopods and hexapods as well as the two pairs present in Crustacea are all of them post-oral parapodia shifted in relation to the mouth. No adult arthropod retains the prostomial antennæ of the chætopod, but Korschelt and Heider suggest that the strange pair of small processes seen in embryo *Peripatus* in front of the head are these prostomial antennæ in a transient condition, whilst they are also represented (as I have myself long since taught) by the frontal tentacles of certain crustacean Nauplii (Cirripedes). The prostomial eyes of the chætopod appear (?) to be retained by *Peripatus*, but the eyes of all other Arthropoda are a new structure, and it is not impossible that they may after all represent (*cf.* Herbst's experiments on regeneration of crustacean eye as an antenniform palp) a pair of fore-shifted parapodial appendages.

The conclusion to which embryological study (as to segments of body-cavity, so-called cerebral ganglion, and position of early rudiments of antennary appendages of arthropods) tends, is that in the various branches of arthropod descent from the ancestral Pro-Arthropoda there have been forward movements or shiftings of post-oral segments and their parapodia (possibly to the number of three in some cases and of less in other cases), so that the mandible of *Peripatus* does not belong to the same ancestral segment as does the mandible of the insect and crustacean, whilst the cephalic appendages of the Arachnida require a separate interpretation. (*cf.* Korschelt and Heider, "Vergleich. Entwickelungsgesch." p. 906). The development of my original doctrine of prostomiad shifting or attraction of post-oral segments and their appendages to the prostomium has indeed now assumed very serious proportions. It will require renewed researches both upon the developmental history of the chætopod prostomium and that of the head in various Arthropoda, before precise and definite conclusions in the matter can be formulated.

In the meantime I am none the less unable to agree with those who doubt that the Arthropoda form a natural group in the sense indicated above.

There are two remarkable and independent characters possessed by all Arthropoda, including *Peripatus*, which we must suppose either to have been independently acquired by the different lines of descent now united under the name Arthropoda—or to have been inherited by all Arthropoda from common ancestors—the Pro-arthropoda. These two characters are (1) the modification of one or more pairs of post-oral parapodia to serve as opposable nipper-like jaws (hence the term GNATHOPODA which I formerly suggested as more appropriate than ARTHROPODA for the designation of the group); (2) the peculiar condition of the great dorsal heart, which may be termed 'ostiate.' The Arthropod heart is a contractile tube, lying in a pericardial blood-sinus formed by the fusion of the veins whose walls have disappeared, leaving their openings into the heart itself as naked 'ostia.' (The theoretical explanation of the derivation of the arthropod heart from a median dorsal vessel fed by paired lateral veins was given in my article on the "Cœlom and Vascular System of Mollusca and Arthropoda," *Quart. Journ. Micr. Sci.*, vol. xxxiv., 1893).

The parapodial jaws and the ostiate heart cannot be supposed to have been *both* developed independently in each group of arthropods, or in any two of them. That is a supposition which seems to me, in our present state of knowledge as to "homoplastic structures," inadmissible. Hence I conclude that they were inherited from a common ancestral stock, in which they arose (since we find them nowhere outside the Arthropoda), and the derivation of all the Arthropoda from this one stock which had already acquired two great distinctive characters of all known Arthropoda, renders it impossible —without some new and unrecognised use of language—to speak of the Arthropoda as constituting more than one natural group.

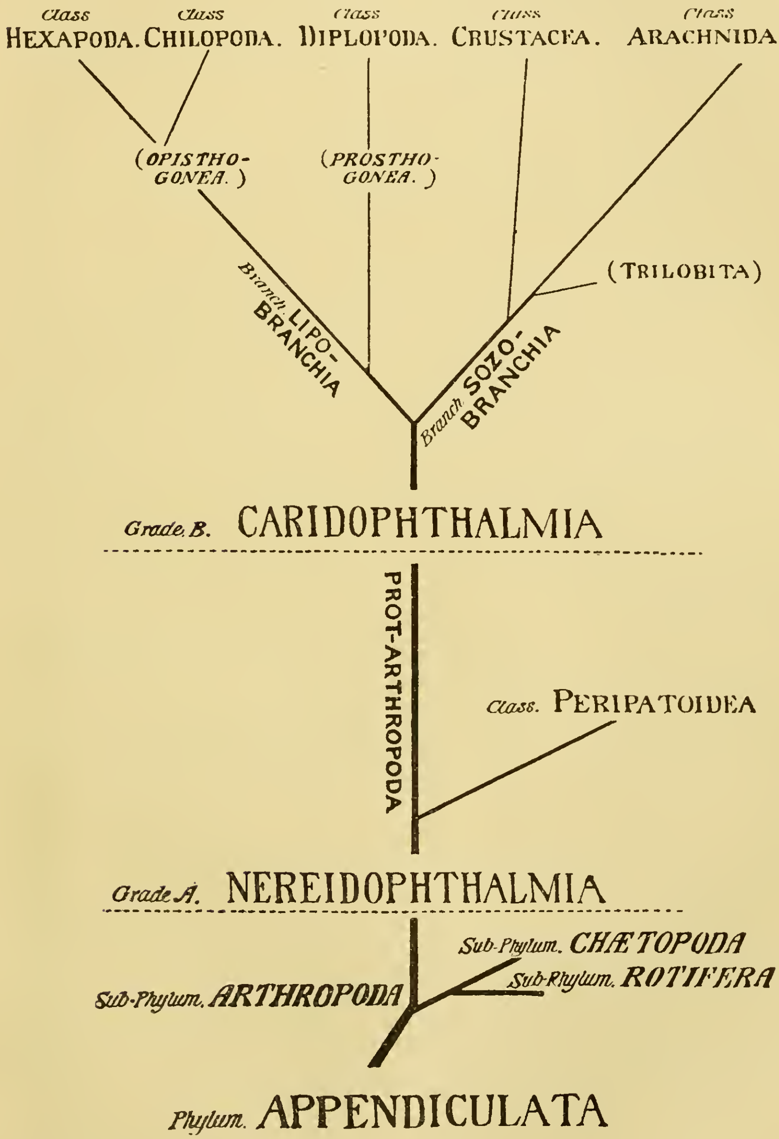
On all hands it is admitted that this group comprises (as most large groups do) several diverging lines of descent.

The conception which I have formed, from the facts now ascertained, as to the relationships of those groups is indicated by the subjoined diagram. I do not introduce the pycnogonids or other degenerate groups into consideration.

I regard the trilobites as a lower ancestral grade of the Arachnida, possessing the parapodial antennæ which the higher grade have lost, and I propose to distinguish these grades as *Arachnida eucerata* and *Arachnida lipocerata*. The trilobites exhibit the same wide range in the number of body-segments which is observed in the lower Crustacea, whereas the most fully-developed Arachnida (Eurypterines, *Limulus*, and *Scorpio*) present eighteen segments arranged in three sets of six. The immediate ancestors of the trilobites and those of the Crustacea were probably more closely allied than were either to the ancestry of hexapods, chilopods, and diplopods, but it must be remembered that the common ancestor of trilobites and *Apus* could not be reckoned a crustacean, since it probably had only one and not two pairs of præoral tactile appendages,

In any scheme of Arthropod classification we have to admit the occurrence of remarkable instances of homoplastic agreement—that is to say, we cannot conceive of the derivation of the classes in such a way as to assign to certain important and remarkably elaborated organs a single point of origin. The facetted eyes of Insecta and Crustacea are examples—also the tracheæ of *Peripatus*, Diplopoda, Chilopoda, Hexapoda, and certain Arachnida—and again the enteric cœcal tubes of Amphipoda and of scorpions, which so much resemble but differ in origin from the proctodœal malpighian tubes of Hexapoda.

There are, no doubt, besides the two great characters which I have cited as holding the Arthropoda together, others which have universal validity and importance, if we except the Peripatoidea. These characters are not in any way difficult to suppose as later developments of conditions already realised in the lower grade repre-



sented by *Peripatus*. Such are the plating and jointing of the body and limbs with dense chitin. *Peripatus* has a soft extensile body, but dense chitin is already deposited on its jaws and claws. It, however, does not actually possess "arthropodous" limbs. Striated muscle and absence of ciliated epithelium go with the increased deposit of cuticular chitin. Due weight was given by some, at least, of the recent writers in this journal to the reduction of the cœlom and the swelling out of the blood spaces as an Arthropod character—a character to which the 'ostiate' condition of the heart is clearly related, and one which I believe I was the first to point out, together with the related derivation of the tubular gonads from nephridia and perigonadial cœlomic remnants (*cf.* my note on Mr. Gulland's paper, "Evidence in favour of the view that the coxal gland of *Limulus* and of other Arachnida is a modified nephridium," *Quart. Journ. Micr. Sci.*, vol. xxv., 1885, p. 515). It is, however, to be noted, that a similar (but not identical) reduction of cœlom and enlargement of blood-spaces occurs in Mollusca—and that if we take it alone, without associating with it the resulting ostiate character of the heart, this character is not distinctive of Arthropoda. It is interesting to recall in this connection what was the most advanced teaching twenty years ago, viz., that of Carl Gegenbaur. He states ("Elements of Comparative Anatomy," English edition, 1878, p. 278) that the cœlom is found in all the Arthropoda and "forms a portion of the blood-vascular system, so that the peri-enteric fluid found in many Vermes as a fluid different from the blood, is represented in the Arthropoda by the blood itself." And similarly in the chapter on the cœlom of mollusca we find (p. 367), "As a rule, the vascular system is freely connected with the cœlom, which therefore forms a portion of the hæmal system."

I think that it must be admitted that an important change has been made in our views of the morphology both of Arthropoda and Mollusca by the result arrived at some years ago through my investigation of the cœlomic and vascular systems of Mollusca and Arthropoda (confirmed and established in the latter case by Sedgwick's researches on *Peripatus* development), though that change is not so widely recognised abroad as in England. Even that most distinguished anatomist, Kowalewsky, in a recent valuable memoir, still calls the ventral blood-sinus of a hexapod insect its cœlom. Many morphologists, both in England and Germany, now recognise that both cœlom and blood-system exist in Mollusca and Arthropoda as distinct systems of spaces shut off from one another, the cœlom being reduced to perigonadial, epi-nephridial, and (in Mollusca) pericardial remnants, whilst the blood-vessels have swollen and united to form an extensive series of blood-sinuses, to which I have given the name "hæmocœl."

Oxford. E. Ray Lankester.

SOME NEW BOOKS.

Nansen's Book.

Farthest North, by Fridtjof Nansen, being the record of a voyage of exploration of the ship *Fram* 1893–1896 and of a fifteen months' sleigh journey by Dr. Nansen and Lieut. Johansen, with an appendix by Otto Sverdrup captain of the *Fram*. Two vols., 8vo. Pp. xvi., 510 and xvi., 672, with 16 coloured plates, 4 folding maps, etched portrait frontispiece, and numerous full-page and text illustrations. Westminster: Archibald Constable & Co., 1897. Price 42s.

Not the telegrams, nor the lectures, nor even the *Daily Chronicle* articles have spoilt our appetite for this vividly written and admirably illustrated account of one of the most remarkable voyages of exploration that the world has yet seen. It is not the results, or the adventures, or the literary style that make the most forcible appeal to all of us; but it is the fulfilment step by step of a plan boldly yet deeply laid, and worked out with a combination of intellectual sagacity and the utmost physical bravery. The sagas of Eric the Red and Leif the Lucky speak of the childhood of the world, but this new Fridtjof's Saga is a tale of its manhood—mind and body striving in full accord towards one definite goal. And so its method as well as its object have interest for men of science most of all: the object, no sensational dash for the pole, but "to investigate the great unknown region that surrounds it"; the method, based from beginning to end, in every item, on observation and experiment. "It is," writes Nansen, when preparing for his sledge expedition, "on the felicitous combination of trifles that ultimate success depends," and the striking success of the whole was due to the most rigorous adherence to this maxim.

The book consists of the following parts. A recapitulation of the scientific basis of the plan, largely reprinted from Nansen's address to the Geographical Society of Christiania; a short account of the preparations, including a description of the "Fram"; the voyage along the north coast of the old world, in which is intercalated the interesting narrative of Alexander Trontheim's journey with the dogs through the Urals and the tundras to meet the ship at Khabarova; the entry into the ice and the freezing in and drifting of the "Fram" to longitude 110° E, in November, 1894 (end of vol. i.); the preparations for the sledge-expedition; the expedition by Nansen and Johansen; their winter (1895–96) in Franz Josef Land, and journey S.W., till the meeting with Jackson; return in the "Windward"; Captain Sverdrup's account of the voyage of the "Fram" after the departure of Nansen and Johansen, occupying 112 pages; conclusion, giving a few main results of the expedition, by Nansen; a good Index. A large part of the book consists of excerpts from Nansen's diaries, so that we follow his varying feelings from day to day, and despite monotonous surroundings neither variety nor vivacity are wanting to the narrative. We are plunged from rhapsodies on the Northern Lights into the excitement of a bear-hunt, or recalled from meditations on eternity and the vanity of our mortal aims to the consideration of a toothsome bill of fare. One can skip the rhapsodies, but they have their place as throwing light on the psychology of the Arctic explorer ; we note that they recur less often after the comfort of the "Fram" has been left. However, we do not propose to review the book from the journalist's standpoint, and shall merely direct attention to a few details of scientific interest.

Meteorological observations, under the direction of Lieut. Scott-Hansen, "were taken every four hours day and night ; indeed, for a considerable part of the time, every two hours." "Hansen had also at stated periods to take observations to determine the magnetic constant." The salinity and temperature of the water at different depths were also observed ; the amount of electricity in the air ascertained ; marine currents were investigated : all this fell to the lot of Nansen. The aurora was regularly observed, first by Nansen, then by Dr. Blessing. Sounding and dredging were constant, and gave occupation to all hands.

Temperature-observations showed that under the cold surface was more saline and warmer water with a temperature sometimes as high as 1° C. Ascribing this to the diving of the Gulf Stream, Nansen speaks of the fact as "surprising" and as modifying theories hitherto entertained. As a matter of fact it does nothing more than confirm the observations of Scoresby, Markham, and Maury, and the views of Lyell, Croll, and other people not unknown.

In connection with the surface layer of fresh water, some curious statements are made on pp. 174, 177 of vol. i. Off Taimur Id. "we could hardly get on at all for the dead-water, and we swept the whole sea along with us. . . . Where a surface layer of fresh water rests upon the salt water of the sea . . . this fresh water is carried along with the ship, gliding on the heavier sea beneath. While we had drinking water on the surface the water we got from the bottom cock of the engine-room was too salt to be used for the boiler. Dead-water manifests itself in the form of larger or smaller ripples or waves stretching across the wake, the one behind the other, arising sometimes as far forward as almost amidships." This appears to have taken away all the momentum of the vessel. But at last "some thin ice scraped the dead water off us. The change was noticeable at once. As the *Fram* cut into the ice crust she gave a sort of spring forward, and, after this, went on at her ordinary speed." To our landlubber mind this sounds like some old *Remora*-superstition, and sadly demands explanation.

Basing his whole plan on the drift of the ice, it is natural that Nansen should have devoted much attention to this phenomenon. The successful issue of the voyage is itself a proof of the reality of the drift, but there were also material evidences in the shape of logs of Siberian larch, observed by Nansen on his sledge expedition, and by Sverdrup on the "Fram" (vol. ii., pp. 153, 580). Further, river-ice with clay and gravel was seen in lat. 85° N and about 90° E. This drift of the ice is due to the prevailing winds, but there is also "a slow current in the water under the ice, travelling in the same direction." Thus not only Nansen's previous conclusions, but also the views of A. E. Nordenskiöld as to the currents in the Kara Sea, are confirmed, while the greater thickness of the ice north of the Atlantic over that north of Siberia receives a simple explanation.

There are some valuable observations on the formation of polar ice (vol. i., p. 404). Ice formed in autumn continues to increase up till next midsummer, but more slowly with increase of thickness.

When the snow melts in summer, the fresh water is cooled down as it comes in contact with the cold salt water, and at the junction of the two a layer of ice is formed, and gradually becomes attached to the bottom of the old ice. The complication in the structure of the ice is further increasing by the pressure producing overthrusts, and so increasing the thickness of the floes enormously. Naturally the older floes are the thicker; and the older are those that have drifted further to the west.

North of Siberia a regular alternation of ice-pressure coinciding with the tides was noticed; but further west this cause did not seem to operate, and pressure here was due to the winds. The pressure-ridges have a regular direction at right angles to the lines of force. These observations chiefly confirm those made by the "Jeannette." Hummocks very rarely exceed 23 feet in height (above sea-level?); those said to attain 50 feet are either bits of icebergs or creatures of the imagination.

It is needless here to say more as to the great depths in the polar sea found by this expedition. It does not all appear to have been so deep, but the statements are not very precise. The only bottom samples mentioned were obtained in about 82° 50′ N., 115° E (exact position nowhere stated) and consisted of "a layer of grey clay 4 to 4¼ inches thick, and below that brown clay or mud." This was at 800 fms. Two pails of mud dredged up a few days later contained "chiefly starfish, waving starfish, medusae (*astrophyton*), sea-slugs, coral insects (*alcyonariae*), worms, sponges, shell-fish, and crustaceans." This very peculiar list conveys little to one's mind, and it does not convey the idea of great depth. The bottom-temperature here was probably, as before, +0.18° C.; but neither depth nor temperature are given. The necton about here, at a depth of 27 fms., contained many "small crustaceans (*kopepodae*, *ostrakodae*, *amphipodae*, etc.) and a little Arctic worm (*spadella*) that swims about in the sea." The only other similar statement is that shortly before reaching Franz Josef Land, Nansen caught in the net "a pteropod (*Clio borealis*) and a few crustacea."

The ice itself yielded many organisms of great interest apparently, though we are told little about them:—Diatoms, algae, and floating in the pools "a number of small viscid lumps, some white, some of a yellowish-red colour" (perhaps some kind of Proteomyxa or Labyrinthulia). All these seem to float at the junction of the fresh-water layer with the salt water. The dark colour of these organisms, absorbing the sun's rays, helps to melt the ice and so form the pools in which they live. In the pools were also Infusoria, Flagellata, and Bacteria.

Much attention has already been directed to the quantities of Ross's gull that were observed; it appears to breed in the N.E. parts of Franz Josef Land. It was first observed in August, at about 84° N., 80° E. About this time and place several birds were seen, of which the names are given as follows: "*Larus eburneus*, *L. glaucus*, *L. argentatus?* [?], *Rissa tridactyla*, *Procellaria glacialis*, *Uria grylle*, a skua, and a snow bunting." *Procellaria glacialis* was also seen the first of any birds on the return sledge journey, May 25, 1895, about 82° 50′ N., 70° E. On June 18 of the same year Brünnich's guillemot appeared, and two days later, with a west wind, numbers of little auks. The most northerly mammal observed was a fox, or rather his tracks, on April 26, at about 85° N. Next came narwhals on May 19, in about 82° N. *Phoca barbata* was not seen till another month and another degree of latitude had been passed.

On August 15, on Houen Id., in the N.E. part of Franz Josef land, *Papaver nudicaule*, *Saxifraga nivalis*, and a *Stellaria* were found in bloom.

Scattered through the book are some observations of geological interest. There are signs of former glaciation on the coast of Siberia, even where the land lies very low. The mountains about C. Chelyuskin are flat-topped with precipitous sides; the rock appears to be sandstone or basalt. In various parts of Franz Josef Land, as on Spitzbergen, are numerous raised beach-lines, often with shells, cetacean bones, and other evidences of the comparatively recent elevation of the land. The most important point, however, is the determination of the age of the sedimentary rocks of Franz Josef Land, and also of the interbedded basalt. Certain plant-remains collected by Johansen, though the locality was originally noted by Mr. Jackson and Dr. Koetlitz, were submitted to Professor Nathorst, who considers without doubt that the remains are of Upper Jurassic age, and that like the contemporary flora of Spitzbergen they do not indicate a particularly genial climate, although enormously more so than that of to-day. A new species, *Gingko polaris*, is figured.

But we must leave this important discovery in order to consider some geographical questions. And here we cannot affect to pass without censure the unfortunate accident of the stopping of the chronometers of both Johansen and Nansen. It is an explorer's first duty to himself and to the world in general to be sure of his positions, but this is impossible if, like any ordinary mortal, he forgets to wind up his watch. On Tuesday, April 2nd, 1895, "when we finally came to a standstill we had been on the move so long that it was too late to wind up our watches. Johansen's had stopped altogether; mine was ticking, and happily still going when I wound it up, so I hope that it is all right." This might have been a warning, and yet on Friday, April 12th, for precisely the same reason, "we had the misfortune to let our watches run down." Of course it was possible to set the watches again approximately, and the necessary corrections could be, and were, made when civilisation was again reached. But the consequence of all this has been a good deal of unnecessary trouble to the explorers and to others. As, however, certain statements have been made tending to suggest that Nansen was 12 or 13 degrees out in his longitude, it is important to note that his error was only 6½ degrees. Moreover, he distinctly states that a part of this error was intentional; he assumed a more westerly longitude than he believed himself to be in, so as to obviate all chance of over-shooting Franz Josef Land. It is clear from his subsequent narrative that it was not the slight error in longitude that threw him out, but the non-existence of the Dove Glacier; when he got to the region explored by Leigh Smith, he recognised his position plainly.

It is perhaps due partly to the error in longitude, partly to the fact that he entered Franz Josef Land from its least explored N.E. end, that led Nansen to over much criticism of Payer, and that led him further to imitate Mr. Jackson, who, we are told by his friend and mouthpiece, "has been forced to ignore or condemn the statements of Payer." Perhaps, too, gratitude for his hospitality induced Nansen to accept Mr. Jackson's criticisms too readily. We do not know how else to account for some strange items on his map of Franz Josef Land. South of Hohenlohe Id., Payer marks a group of five small islands, to the largest of which he gives the name Coburg Id.; Nansen calls this Torup Id., restricting the name Coburg to an insignificant rock. Between Coburg and Rainer Islands, in 81° 30′ N., Payer has placed Andrée Id. Nansen has shown Rainer Id. to consist of two islands; to the smaller westerly portion he gives the name of Leigh Smith, to which no exception can be taken, but he alters Andrée Id. to Karl Alexander Land. Payer was never actually on the land which he named Karl Alexander, so that its position on his chart is only approximate; it appears as a large mass of land between 56° and 58° E. longitude, and 81° 20′ and 81° 30′ N. This position is occupied on Nansen's map by an irregularly shaped island called Frederick Jackson Id.; this then should be called Karl Alexander Land, and south, not north, of it should be Back's Inlet. Between Back's Inlet and Todesco Fjord comes the island that bears Capes McClintock and Fisher. South of Todesco Fjord, which now proves to be a sound, lies the land with Richthofen Peak, C. Fiume, and C. Triest. Lastly, south of an easterly extension from the great Markham Sound, comes the group of islands which Payer, standing on their eastern edge and looking across their flat tops, lumped together as "Mac-Clintock Island?" Thus, and thus only it seems to us can one attain harmony between the pioneer work of Payer and the more detailed work of Leigh Smith, Jackson, and Nansen. Why should not harmony be attained? Why "ignore" Payer?

Next, as to the name Zichy Land. Payer's map shows us, on the east Wilczek Land, on the west Zichy Land, and between the two an archipelago. Standing on the high ground (over 2500 feet) on the east shore of McClintock Island, he looked westward over a number of islands, of which the most important are now distinguished as Brady, Hooker and Fridtjof Nansen, and naturally was unable to see the narrow sounds between them. But west of all these he saw a long and broad sound, running N.E. and S.W., and beyond it the distinct coast-line of a land mass. The sound he called Markham Sound; the land he called Zichy Land. Leigh Smith, who followed, recognised this, and in his second map *(Proc. R. Geog. Soc.* 1883, facing p. 248), the name Zichy Land is assigned to the land due north of Northbrook, Bruce, Mabel, and Bell Islands. The land further west of this was called by him Alexandra Land. It is the land that bears, among other things, the Gratton Glacier, to which Payer and Leigh Smith applied the name Zichy Land. But on Nansen's map that name is removed from that land and stretched over the chain of islands to which Payer most certainly did not apply it. Similarly, in accordance with Mr. Jackson's views, the name Markham Sound is restricted to the small easterly portion of the original sound.

Now we do not stand alone in holding these opinions; but even supposing that we are ultimately shown to be wrong, surely it would have been better that Messrs. Jackson, Nansen, and Co., should have waited until their survey was completed before peppering the country with new names, so that they might have instituted a more exact comparison of their own maps with those of Payer and Leigh Smith, taking into their councils those eminent explorers, who are happily still with us. This would have been better than "to ignore or condemn" them unheard.

Just a word as to the translation. On the whole it is admirable, so good indeed that some credit might have been given by the publishers where credit was due. Some thanks, we have heard, is owing to our learned Ibsen-translator, Mr. William Archer. None the less there are a few slips; it is strange to read (ii, 605) of "10 prismer, or 330 grammes, of gun-cotton," as though "prism" was the Norwegian for 33 grammes, and not a common English word. Then we find certain stratified sandstones alluded to as "schist" (i, 189), doubtless a mistranslation of "skiffer." The word "steinen," the stone, is translated as "Rocks." The Norwegian "strand-linie," should be "beach-line." "Snails" (II. 308) should be "gastropods." As for the zoology, the extracts we have given show that the proofs were read by no scientific Englishman, probably not even by the author. There are a few discrepancies in the spelling of proper names between Nansen and Mr. Montefiore Brice; the Johansen, Koetlitz, and Blomqvist of the former are the Johannesen, Kettlitz, and Blomvist of the latter.

The illustrations deserve more praise than we have space for. It is so nice to feel that they are all from photographs and must be so truthful. And yet—well, who was the photographer Nansen and Johansen went to when they were alone on their sledging journey?

Welwitsch's African Plants.

Catalogue of the African Plants collected by Dr. Friedrich Welwitsch in 1853–61. Dicotyledons, Part I. By W. P. Hiern. British Museum (Natural History). Printed by order of the Trustees. Pp. 336. London, 1896. Price 7s. 6d.

Friedrich Welwitsch was born in 1806. His father, the owner of a large farm in Carinthia, sent him to study law at Vienna University. His own tastes, however, were so strongly botanical that in consequence of his abandoning law for flowers his father withdrew his allowance. He then supported himself at the University for a time by writing dramatic criticisms, until, as the biography prefixed to this catalogue tells us, an "act of youthful indiscretion on his part, in the course of enjoying too freely the gaieties of Vienna, rendered it expedient for him to leave Austria for a time." After some excellent work in Portugal, he left, in 1853, on his great Angolan journey, probably the most fruitful African expedition ever made by a botanist. He explored and thoroughly searched most of Angola and Benguella during eight years, suffering great hardships and frequently prostrated by fever, dysentery, and other African maladies. On one occasion he was besieged for two months by some 15,000 rebellious natives. Finally, he sailed for Lisbon with his great collection, contained in 42 huge packages occupying about 319 cubic feet. This consisted of some 5,000 species of plants and 3,000 specimens of insects and animals. The number of new forms, including the unique *Welwitschia*, was extraordinary. After this it is not pleasant to read of his poverty, ill-health, and death in London, where he stayed in order to work over his collections with but small support from the Portuguese Government. After his death in 1872, that government attempted, by a suit in Chancery, to obtain the whole of his collections; but Messrs. W. Carruthers and F. Justen, who defended at their own risk, were successful in retaining the best set (after the study set) for the British Museum.

We are now presented with the first instalment of a complete catalogue of that collection so happily preserved, and it begins appropriately with a portrait of Welwitsch, and a complete bibliography, not merely of the author's own papers, but of others dealing more or less directly with his collections. This volume, which has been carefully prepared, well printed, and handsomely bound, covers the Thalamifloræ and the Leguminosæ as far as the order Rhizophoraceæ. Would it not have been better to have begun at the other end of the flora? More valuable and novel results would certainly have been obtained from the Monocotyledons and Incompletæ; for the orders now dealt with have been entirely covered by the first two volumes of "The Flora of Tropical Africa." This being so, we are surprised to find two genera and 60 species here described for the first time, rather than disappointed at the smallness of the number. Each order is prefaced by an account of its distribution in Angola, with mention of its more interesting species. The details concerning the latter, though based almost entirely on Welwitsch's notes, derive great value from the many years spent by that botanist in Angola and from the earnest study he made of the country.

The new genera, *Epinetrum* in Menispermaceæ and *Zanha* in Burseraceæ, are described entirely from male flowers, the fruits and female flowers being unknown. A sign of the times, and a feature that will be welcomed by many, is the use of the English language instead of the Latin for the description of all novelties. It is true that such unusual phrases as "strict branches," "depresso-hemispherical discs," and "quasi-sheathing scales," are neither elegant, nor English, nor more intelligible; but in the notes an occasional sentence is more attractive. Thus, we read of plants, "decked in all parts with a silvery lustre," and fruits "which glow with the brightest scarlet red."

Much valuable information as to dyes, timber, medicines, and economic plants generally is to be found in this volume—though not too easily, for there is no index to native or economic names. This addition, which might be made in a future volume, ought to render the work of service to practical men.

Great pains have been taken with the synonymy. Apparently the names of over 160 genera, many of them familiar to us in the "Genera Plantarum" and in "The Flora of Tropical Africa," have been replaced by previously given but unfamiliar and often less euphonious names. Thus, instead of the well-known *Cola*, *Smithia*, and *Swietenia*, one finds *Edwardia*, *Damapana*, and *Entlandrophragma*. Mr Hiern's extreme conscientiousness in these matters is shown by his use of *Annona* for *Anona*, *Cajan* for *Cajanus*, *Sesban* for *Sesbania*, *Canavali* for *Canavalia*. For work of this kind we cannot profess gratitude. All this historical nomenclature changing might safely be left to those incapable of true botanical work. We are not surprised to learn that a protest against it has been raised by many German botanists. Finality, as experience of Dr. Otto Kuntze shows us, can hardly be hoped for; and yet that is more likely to be reached by someone prepared to write a new "Genera Plantarum," dealing with all the plants of the world, and not merely with those of what is at best but a small portion of Africa.

An Unnatural Union.

A Manual and Dictionary of the Flowering Plants and Ferns. By J. C. Willis, M.A. 8vo. Vol. i., pp. xiv., 224; vol. ii., pp. xiii., 429. Cambridge University Press. 1897. Price 10s.

This is the latest member of the biological series of the Cambridge Natural Science Manuals, and forms a useful addition to that series. The two volumes are practically two distinct books, and the recognition of this by issuing them as such would be beneficial to the botanical public, and also, we think, to the publishers. Vol. I., which was originally "intended only to be a kind of index to Part II.," ultimately developed into "what is practically a text-book of morphology, classification, natural history, and geographical distribution." The chapter on morphology is unsatisfactory, and so far as the vegetative organs are concerned leaves the reader with very vague and incomplete ideas; the treatment of the flower, however, is good. Chapters II. and III., dealing with variation, classification, forms of vegetation, geographical distribution, etc., are useful, while the last chapter, "Economic Botany," is scarcely more than a list of plants, concerning which further information can be obtained by reference to Part II. As a text-book Part I. cannot be recommended, though the student will find in those portions to which we refer favourably a great deal that is useful and suggestive, and not to be found in the ordinary text-book. The introductory chapter contains under the head "Botanic Gardens, &c.," a brief outline of the contents of the gardens at Kew and Cambridge; and the student's attention is directed to some of the more note-worthy plants in the various houses and beds.

Vol. II. is quite a different book, and one of the most useful compilations ever offered to those interested in the science. It is a dictionary of "the classes, cohorts, orders and chief genera of phanerogams and ferns, alphabetically arranged under their Latin names." The accounts given of the larger divisions, including the more important natural orders, are full and concise, and the same may be said of those genera which are of common occurrence in cultivation, or of economic value, or of special interest to the morphologist or biologist. The author has drawn largely for his information upon Engler and Prantl's "Pflanzen-familien"; but numerous other works have been consulted, and the copious references to these will assist the enquirer to a more exhaustive knowledge. Considerable attention has been given to details of bionomic interest, such as pollination, the relations existing between the plants in question and insects, or devices for seed-distribution. The second volume ends with a "glossarial index of English names, technical terms, etc." The correlation of the English with the Latin name enables one to look up information about a plant of which one knows only the popular name. But it is a pity to burden the glossary with a reprint of the index of Part I.—another argument in favour of our suggestion of completely divorcing the two volumes.

By the use of small print, thin paper, and thin covers, both volumes are reduced to a small and handy size.

Physiology for Medicals.

Kirkes' Handbook of Physiology. Fourteenth Edition. By W. D. Halliburton, M.D., F.R.S., Professor of Physiology, King's College, London. 8vo. Pp. xvii., 851. London: John Murray, 1896. Price 14s.

It is the necessary fate of a student's handbook which is successful enough to last for some fifty years to fall into the hands of a succession of editors. Written originally by Dr. Kirkes for medical students (largely, we believe, from Sir James Paget's lectures), and probably an excellent book for its time, it has required so much change and alteration with the advance of knowledge, that it may safely be averred that its original author would no longer recognise it. For many years the successive editions have been in medical or surgical hands, and though this has probably not detracted from its value to the medical student, it is certain that its value as a standard text-book of physiology will be increased now that it has fallen into the hands of a writer who is essentially a physiologist, and one so well known as Dr. Halliburton.

We have nothing but praise for the manner in which the author has discharged his task. We say the author, and not the editor, for the book is to all intents and purposes a new one. Nevertheless, the essential character of the book has been admirably preserved, and in particular the special point which renders it of such utility to the medical student—the combination of histology with physiology. It is, indeed, written avowedly for the use of medical students, and it may safely be said that they could not easily find a work which so precisely fits their special needs, though at the same time it is a valuable general introduction to the subject for those who have other ends in view. There is, naturally, nothing here presented which is in itself novel: the book is well brought up to date, and that is sufficient. Criticism resolves itself into an examination of the facts selected for presentation to the student, and the mode and order of their presentment. Dr. Halliburton's experience as a teacher has enabled him to choose his facts judiciously and to present them clearly and concisely, and if one part of the book is better than another it is natural that it should be that which treats of physiological chemistry, the subject in which his name is best known. In the matter of arrangement the only doubt that arises in our minds is concerning the wisdom of presenting the student so early in the book with the difficult subjects of the physiology of the nervous system and special senses. The illustrations are profuse and excellent, and the size and compass of the work are such as to render it in every way convenient.

Scraps from Serials.

Apropos of our Note and Comment on the alternation of generations in plants, based upon Dr. Scott's address to the Botanical Section of the British Association last September, it is interesting to read in *Nature* for March 4, a paper by Sir Edward Fry on the alternations of generations in plant-life, and it is also startling to learn from a footnote that the MS. of this paper "reached the hands of the Editor of *Nature* on 31 August last year."

Mr. F. W. Sardeson has a paper on the Galena and Maquoketa series, running in the *American Geologist*. The second part, in the February number, is devoted to an exhaustive study of the well-known Ordovician and Silurian brachiopod *Orthis testudinaria*, the American forms of which are here split up into 9 species. It is hoped that this minute differentiation will throw light upon the stratification of the beds under discussion.

We are glad to see in the March number of *Knowledge* Mr. F. Enock's interesting account of the life-history of the common tiger-beetle, which he delivered before the Zoological Section at the Liverpool meeting of the British Association, a fact that is not stated by *Knowledge*. The same number contains an interesting article by Dr. H. R. Mill on the Victorian Era in Geography, illustrated by maps of Africa in 1837 and 1897.

The *American Journal of Science* for March contains the conclusion of Dr. Beecher's Classification of the Trilobites, with an index to the genera. R. S. Tarr writes on the Arctic sea-ice as a geological agent. He shows that sea-made ice protects the coast from marine erosion, quite as much as, if not more than, it acts itself as an eroding agent. The action of glacier ice is similarly two-fold. An enormous amount of detritus is carried southwards by the arctic marine and glacier ice. J. S. Diller describes and figures Crater Lake, Oregon; while Messrs. F. D. Adams, A. E. Barlow and R. W. Ells describe the Grenville and Hastings series in the Laurentian rocks of Canada.

Our animated contemporary, *La Revue Scientifique*, is instituting a reform in French orthography. We always imagined that the orthography of the French language had been settled once for all by the Academie Française. The *Revue*, however, has certainly laid its finger upon some weak points. It proposes in future to write "ph" as "f," and to change the "x" of the plural into "s," except in those cases where the singular noun itself ends in "x," but it does not feel that it has yet sufficient support for the changing of "y" into "i" in those cases where it has the equivalent sound.

Humanity for February contains a useful list of rare birds killed in Great Britain last year, compiled by J. Collinson.

From the Geological Institute of the University of Upsala, we have received vol ii. part 2, of its *Bulletin*. This contains Carl Wiman's valuable paper "Ueber die Graptoliten," the chief results of which were kindly summarised by him for the readers of NATURAL SCIENCE, September and October, 1896. An equally important paper in this volume is that by J. G. Andersson on the Cambrian and Silurian phosphoritic stones of Sweden. In this the distribution, relations, and origin of all phosphoritic nodules and beds is fully described and discussed on the evidence of personal observation in the field and laboratory. The illustrations are specially commendable.

A criticism of Mr. Andersson's paper, and a consideration of the occurrence of phosphorites generally, by Herman Hedström, has just appeared in the *Förhandlingar* of the Geological Society in Stockholm (vol. xviii., pp. 560-620).

The "Psychological Index" just issued by the *Psychological Review* is not only published in the *Année Psychologique*, where it will have the co-operation of N. Vaschide in the preparation of French titles, but has also entered into co-operation with the *Zeitschrift für Psychologie u. Physiologie der Sinnesorgane*, exchanging the titles of papers with that publication, and thus making both more complete.

The *Antananarivo Annual* is rapidly assuming importance as a scientific journal. No. 20, for Christmas, 1896, being pp. 385 to 512 of vol. v., contains numerous interesting articles, some of them, it is true, reprinted from other sources, some translated into English. The original ones are: "From Fort Dauphin to Fianarantsoa," by E. F. Knight; "Malagasy Plant-Names"; "Notes on some Madagascar Spiders and their protective Resemblances," and "The Mammals of Madagascar," by Rev. J. Sibree; some notes on Madagascar geology, by Rev. R. Baron; "The 'Mohara' or War-Charm of Imerina," and "By-gone Ornamentation and Dress among the Hova Malagasy," by Rev. W. J. Edmonds; "Ohabolana, or Wit and Wisdom of the Hova of Madagascar," by Rev. J. A. Houlder; "The Zanakantitra Tribe," by H. E. Clark; "A Native Malagasy Lyric," by Rev. W. F. Cousins; "A Taifasy Village," by Rev. C. Collins; "Lanihay, in North-East Madagascar," by H. Hanning. The London agents for the *Annual* are Messrs. John Haddon & Co., Salisbury Square.

NEW SERIALS.

WE have received the first number, issued on the 1st of March, of the *Revue Diplomatique et Coloniale, receuil bi-mensuel de politique extérieure*, published at the office of the *Revue Rose* and the *Revue Bleue*, 19, rue des St. Pères, Paris. The yearly subscription for the postal union is 12 francs 50. We presume that this review, though dealing chiefly with political questions, has been sent to us on account of its containing an article by Prof. Marcel Dubois, entitled "Science et Propagande." The propaganda, however, appear to be those of French colonisation rather than of science. The only other article that at all comes within our purview is that of the navigability of the Mekong, by J. B. d'Attanoux.

The *Open Court* (Chicago) has changed into a monthly magazine in octavo form. The annual subscription, including postage, is one dollar. The February number contains an article by Prof. A. S. Packard on Lamarck and Neo-Lamarckism, which discusses the actúal writings, too often overlooked, of Lamarck himself as well as Prof. Cope's book, "The Factors of Evolution," recently reviewed in our own pages. We wish this interesting and outspoken journal all success in its new shape.

To our French and German Colleagues!

Professor D. T. MacDougal points out in the *Botanical Gazette* for December, that a German botanist has recently duplicated the contribution of an American botanist "in a manner which admits of no extenuation." "His ignorance," says Mr. MacDougal, "may be directly due to the fact that 'the file of the *Botanical Gazette* is not to be found in the Marburg Institute,' but it is a logical outcome of the assumption that the boundaries of botanical science are identical with those of Germany. Our brethren across the water would do well to rid themselves of this erroneous idea, once more nearly true than at present. Their repeated disregard of outside literature will certainly do much to lessen the prestige of the German Institute." We have seen similar complaints elsewhere, indeed, we have ourselves experienced this ignoring habit of our German colleagues. We will not suggest that they might do well to read the pages of Natural Science, but we have certainly been surprised to hear that so indispensable a publication as the *Zoological Record* is hardly known in Germany. The French err in a rather different direction, since they do not seem to care whether their colleagues in other countries read their books or not. It is only thus that we can account for the fact that so few French publishers take the trouble to send us their books for review. A work like "L'Hérédité" of Professor Yves Delage would surely find readers in this country, if only their attention could be drawn to it.

Further Literature Received.

The New Charter, J. C. Kenworthy &c.: Human. League. The Migration of Birds, F. B. Whitlock: Porter. Untersuchungen über den Bau der Cyanophyceen und Bakterien, A. Fischer: Fischer, Jena. Experimental Morphology, C. B Davenport: Macmillan, N.Y. Aims and Practice of Teaching, F. Spencer, &c.: Clay. History of Mankind, F. Rätzel pt. 17: Macmillan. Handbook to Order Lepidoptera, vol. iv, W. F Kirby: Allen. Monogr. Bombycine Moths N. Amer., Pt 1., A. S. Packard: Smithsonian Inst.

Records Aust. Mus,, vol. iii., no. i. La Piscifacture Marine, M. Baudouin: Inst. Bibl. Sci. Fortieth Ann. Rep., Sheffield Mus. Catalogue, Dulau. Lieut. Peary's Expedition, G. H. Barton *Science*. Evidence Former Extension of Glacial Action. G. H Barton: *Amer. Geologist*. Ann Rep. Field Columbian Mus. Probable Causes of Decadence of Brit. Rhopalocera, W. Harcourt-Bath, *Entomologist*. Biennial Rep. Biol. Exper. Station; Entomology Illinois River, C. A. Hart New Parasitic Hymenoptera, W. H. Ashmead; Plankton Studies, C. A. Kofoid; New Spp. Rotifera and Protozoa, A. Hempel; N. Amer. Ostracoda. R. W Sharpe: *Bull. Illinois State Lab.* Bull. Nat. Hist. Soc. New Brunswick, no. xiv. The Chemistry of Some Common Plants, P. Q. Keegan: *Naturalist*. Notes on the Palmer as a Reefing District, R. L. Jack: *Bull. Geol. Surv. Queensland.*

Nature, Feb. 18, 25, March 4, 11, 18; Nature Notes, March; Naturalist, March; Westminster Review, March; Knowledge, March; Photogram, March; Oxford Univ Jr. Sci. Club Journ., 47, 48, 49 Irish Naturalist, March; Scott. Geogr. Magazine, March; Victorian Naturalist, Jan.; Revue Scientifique, Feb. 27, March 6 13, 20; Feuille des jeunes Naturalistes, March; L'Anthropologie Jan.–Feb.; Naturen, Jan.–Feb; Naturæ Novitates, Feb; Literary Digest, Feb. 13, 20 27, March 6; Amer. Journ. Science, March; Science, Feb. 12, 19, 26 March 5; Amer. Geol., March; Botanical Gazette, Feb.; Scientific American, Feb. 13, 20, 27, March 6; The Open Court. Feb.; Chivers' New Book List, March; La Naturaleza (Mexico) 10 & 11; La Naturaleza (Madrid), viii., Nos. 1-8 Amer. Nat., March Review of Reviews, March; Lancet, March 20; Timehri, Dec.

OBITUARY.

CHARLES E. BENDIRE.

Born Hesse-Darmstadt, April 27, 1836.
Died Jacksonville, Florida, February 4, 1897.

MAJOR BENDIRE, the second volume of whose "Life Histories of North American Birds" we have just received, came to America in 1852, and enlisted two years afterwards in the U. S. army. He utilised his service at a number of distant posts in the West, not only for fighting and treating with the Indians to good purpose, nor only in exploring, surveying, and roadmaking, but also in making collections and observations in natural history, many of which were transmitted to Professor Baird at Washington. To Allen, Baird, and Brewer, he wrote important letters, and subsequently appeared as an independent author in his paper on the birds of south-eastern Oregon. His collection of birds' eggs was famous, and he was honorary curator of the department of öology in the U. S. National Museum. He has, however, written on mammals and fishes, as well as on birds. It is greatly to be regretted that his splendid work on North American birds, has been left in an incomplete state.

CONSTANTIN, BARON ETTINGSHAUSEN.

Born 1826. Died February 1, 1897.

IN Professor Ettingshausen of Gratz, palæontology has lost one of the older school of enthusiastic workers who helped to found the science of palæobotany. Beginning in 1850, Ettingshausen continued almost to the last the publication of important papers on fossil plants. His best known work is that in which he deals with the venation-characters as a means of distinguishing between various fossil leaves. By the application of the process of nature-printing (*Naturselbstdruck*), which seems to have been first used to any great extent in the imperial printing office of Vienna, Ettingshausen published a very large number of beautifully executed plates of leaf-venation. His two handsome volumes on the *Blattskelette der Dykotyledonen* (1861), and *Die Farnkräuter der Jetzwelt* (1865), are extremely useful aids to the student of fossil leaves. The larger work prepared in conjunction with Pokormy, and entitled *Physiotypia plantarum Austriacarum*, illustrates in a striking manner the possibilities of nature printing. Among the works dealing with descriptive palæobotany those on the Tertiary plants of Austria, England, Australia, and other countries are among the best known. In addition to numerous contributions to Tertiary botany, Ettingshausen has written important memoirs on

Palæozoic and Mesozoic floras, and in most of his papers he gives evidence of a well-marked ability of dealing with his subject on broad scientific lines. Like many of the older school of palæobotanists, he did not concern himself with the internal structure of fossil plants, but the type of work in which he excelled was admirably dealt with in his hands. It is generally admitted that Ettingshausen overrated the taxonomic value of venation-characters; and in many cases the far-reaching conclusions as to the geographical distribution of plants to which he was led cannot be accepted as based on accurate data. Baron Ettingshausen was a genial, quiet, and unassuming enthusiast, and his name will always occupy an honourable position among the earlier scientific workers in the field of fossil plants. He held for many years the post of Professor of Botany and Palæontology in the University of Gratz.

SINCE our copy of *Science Gossip* for December, 1896, did not reach us till after the middle of January, 1897, we have hitherto been unable to allude to an appreciative note that it contains on the late WILLIAM WILSON, of Hillocks, Alford, in Aberdeenshire, who died on October 18, at the age of 91. A farmer by profession, "he followed closely all attempts of scientific men to show the relationship of the various sciences to husbandry. As a botanist he discovered *Linneæ* (sic) *borealis* growing on an open moor; Alpine sow-thistle at a lower sea-level than previously known to exist. He was the first to notice what has proved to be a remarkable extension in area of the beech fern (*Phegopteris*). He traced many remarkable changes in the distribution of animals as affected by man's agency, such as the departure of some, and appearance and change of habits of others. Of birds, the snipe has practically disappeared from his district, while the curlew and starling have established themselves during his time."

THOMAS CHRISTIAN THOMASSEN, who undertook Nansen's duties as curator of the zoological collections in Bergen Museum, while the latter was in Greenland, died at Bergen, on January 13, 1897, at the age of 57. His most important scientific works were on the earthquakes of Norway.

We regret to record the deaths of:—at Winchester, on Nov. 8 last, the botanist F. I. WARNER, aged 54; DR. C. CONTEJEAN, of the Pasteur Institute, and Paris Museum of Natural History, at Belgrade on February 24, accidentally self-poisoned; E. SAUBINET, President of the Lyons Linnean Society, and an ardent insect-collector; C. F. WIEPKEN, for 60 years Director of the Oldenburg Museum; A. ROGERHOFER, formerly curator of the Imperial Natural History Museum in Vienna; Dr. N. ZDEKAUER, of St. Petersburg, distinguished for his work to advance hygiene and the knowledge of epidemics; on November 26, in Somaliland, the African explorer A. CECCHI, aged 47, murdered by the natives; on December 13, in Alexandria, the meteorologist A. PIRONA, aged 65; on January 1, in Brazil, DR. PAUL TAUBERT, botanist; JOSEPH D. WEEKS, editor of the *American Manufacturer* and a contributor to the reports of the U.S. Geol. Survey, on December 26, at Pittsburg, Pa.; in Newark, N.Y., the botanist, W. HAYES WARD; HENRY L. WHITING, Assistant U.S. Coast and Geodetic Survey and Chairman of the Massachusetts Topographical Survey Commission, on February 4, at West Tisbury, Martha's Vineyard, aged 76.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

The following appointments are announced:—Professor W. J. Sollas, Trinity College, Dublin, to be Professor of Geology at Oxford; Herbert Smith, New College, Oxford, to be Assistant in the Department of Minerals, British Museum; Isaac H. Burkill, of Cambridge, to be Assistant at Kew Gardens; Miss Umpherston, to be Lecturer in Physiology at St. Andrew's University; Dr. S. Mollier, of Munich, to be Professor of Anatomy at Göttingen; Dr. J. Brandl, of Berlin, to be Professor of Pharmacology in the Munich Veterinary College; Dr. J. Kriechbaumer, to be Curator of the State Zoological Collection, Munich; Dr. E. Beckmann, of Erlangen, to be Professor of Pharmacy at Leipzig; Dr Carl Fütterer, to be Professor of Mineralogy and Geology at Karlsruhe Technical High School; Dr. C. Nussbaum, to be Professor of Hygiene in the Technical High School at Hanover; Dr Kirchner, Instructor in Hygiene and Bacteriology in Hanover Technical College, to receive the title of Professor; Dr. K. Kaiser, to be Professor of Physiology at Heidelberg; Dr. F. Lafar, of Stuttgart, to be Professor of Fermentation-physiology and Bacteriology in Vienna Technical High School; A. Engler, of Stans, to be Professor of Forestry at the Polytechnic School, Zurich; Dr. Felix, to be Professor of Anatomy at Zurich; Dr. O. Kruch, of Rome, to the Chair of Botany in the Agricultural Institute in Perugia; E. Chiovenda, to be Curator, and Dr. B. Longo, to be Assistant, at Rome University Botanical Institute; Dr. A. Terracciano, of Naples, to be Assistant at the Botanical Institute, Palermo University; P. Baccarini, to be full Professor of Botany in Catania University; Prof. R. Fusari, of Bologna, to the Chair of Human Anatomy at Modena; Prof. G Valenti, of Perugia, to the Chair of Human Anatomy at Catania; Dr. Umberto Rossi, of Florence, to be Professor of Anatomy at Perugia; Dr. Kolderup, to be Curator of the minerals and rocks in Bergen Museum; Prof. A. F. von Waldheim, of Warsaw, to be Director of the St. Petersburg Botanical Garden; Dr. L. F. Barker, to be Assistant Professor of Anatomy in the Johns Hopkins University; Prof. Alex. C. Abbott, to be Chief of the Bacteriological Division of the Philádelphia Bureau of Health; R. Rathbun, to be Assistant-Secretary of the Smithsonian Institution, with duties connected with the bureaus of the Institution other than the National Museum; Dr. E. Klebs, to be Professor of Pathology in the Rush Medical College, Chicago.

The Marquis of Bute, who is now Lord Rector of the University of St. Andrews, intends to erect there a building containing laboratories, lecture-rooms, and museums for the departments of anatomy, physiology, materia medica, and botany, the School to be known henceforward as the "Bute School of Medicine."

The late Professor W. H. Pancoast has, says *Science*, left his anatomical and surgical collections and $600 per annum to the Medico-chirurgical College of Philadelphia.

Sir Archibald Geikie will deliver six lectures on the principles of geology at the John Hopkins University on Wednesday, April 21, and the five following week-days. He will also give one public lecture in Baltimore. Some geological excursions have been arranged for his auditors.

La Naturaleza (Madrid) for February 18, contains an account of the Institute of Experimental Hygiene of Monte Video, which was inaugurated on March 16, 1896. The director is Dr. José Sanarelli. The stable for animals under observation contains 60 stalls. The Institute is well furnished with laboratories and with a library, and is lit by electric light. Its object is to investigate various branches of hygiene, chemistry, micro-biology, and sanitary physics. Dr. Sanarelli has already discovered in Rio Janeiro the microbe that produces the amarilla fever, an epidemic which periodically attacks certain districts of Brazil, and this gives the Institute a good start in public estimation.

THE Museums Association meets this year in Oxford, under the presidency of Professor Ray Lankester, from June 29 to July 2. The local secretary is Dr. W. B. Benham. So much excellent museum work has been done of late years at Oxford that an interesting meeting is expected. Those wishing to attend, or to present papers, should communicate with the Secretaries, H. M. Platnauer, The Museum York, or E. Howarth, The Museum, Sheffield.

WE have received the fortieth annual *Report* of the Committee of the Sheffield Museum from September, 1895, to August, 1896. The Museum has now completed the twenty-first year of its existence, and a short summary of its growth is given. Owing to the congestion now arrived at, it is suggested that subsidiary museums should be built in different parts of the town.

A SECTOR of a section of *Sequoia gigantea*, being one-eleventh of the circle, has been presented to the Bradford Corporation by Mr. John Clayton. It is from the same tree as the section shown in the American Museum, New York, and the Natural History Museum, London.

THE *Report* of the Museums at Bolton, Yorkshire, for 1896 states that an exhibit has been arranged illustrating the anatomy, affinities, and life-histories of types of insects, with descriptive labels, drawings, and micro-photographs, as well as specimens. This serves as an introduction to the more extensive collections, and has already proved of considerable use to visitors. The arrangement of the mineralogical and palæontological collections is now completed, the fossils being arranged in stratigraphical order; the labels for the various Periods drawn up by Mr. H. Bolton, and already alluded to by us, have been adopted by this museum. The wall-space is used for the exhibition of photographs, drawings, and maps. On the whole, it appears that those in charge of the natural history collections are fully inspired with the modern spirit, and they have their reward in a steady increase in the number of visitors. We are glad to see that the Bolton Botanical Society holds its meetings in one of the rooms of the museum. The *Report* also contains the meteorological records of Bolton during the year, drawn up by W. W. Midgley.

MR. H. J. ERNST, an apothecary of Iceland, has presented to the State Museum in Stockholm a valuable collection of Icelandic minerals.

THE collections of Gustav Nachtigal from the west coast of Africa, made during 1884–85, are now exhibited in the Berlin Museum of Ethnology.

The Berlin Museum für Naturkunde has received from the island of Ralum a collection of the flora and fauna made by Dr. Dahl.

Science states that a public museum and library will be founded at Cettigne, Montenegro, and that the museum will contain antiquities found in the principality itself, including those recently obtained at Dukla.

THE new building of the South African Museum at Cape Town has now been opened to the public.

FROM the *Report* of the Albany Museum, S. Africa, by the Director, Dr. Schönland, we learn that the monthly average attendance during 1896 was 1,795. Many specimens had been received, but great difficulty was felt in accommodating the larger Mammalia, owing to insufficiency of room. It is to be hoped that the new buildings will be hurried on by the Cape Government, as it is a poor economy to pinch science. A properly equipped local museum can do enough economic work in one year to save a Government much fruitless expenditure in the matter of insect pests and mineral quests. The chief acquisitions seem to be a large number of types of new plants collected by Dr. Schlechter in the Western Province, Clanwilliam, and Namaqualand.

THE following are the presidents of the British Association for the Toronto meeting:—President, Sir John Evans; mathematics and physics, A. R. Forsyth; chemistry, William Ramsay; geology, G. M. Dawson; zoology, L. C. Miall; economic science and statistics, E. C K. Gonner; anthropology, Sir William Turner; physiology, Michael Foster; botany, H. M. Ward. Evening lectures will be delivered by Professor James Dewar and Mr. John Milne. Special rates have been obtained from the Canadian railway and steamship lines. The sections will all meet in the buildings of the University of Toronto. It is hoped that, besides British, many foreign men of science will attend the meeting.

A COMMISSION of American botanists has been appointed to consider the establishment and the site of the proposed tropical laboratory. The *Botanical Gazette* states that the assured co-operation of British botanists through a commissioner, and through liberal offers of facilities in case the station is established in British possessions, ensures for the laboratory an international character. A report will be presented by the commission to the meetings of the American and British Associations this year.

C. S. SHERRINGTON, Professor of Physiology in University College, Liverpool, is to deliver the Croonian Lecture of the Royal Society on April 1, taking as his subject "The Spinal Cord and Reflex Action."

THE Royal Zoological Society of Ireland has completed a goat-house and rockery, the latter imitating the natural haunts of the goats and ibexes. A large alligator-pond has been formed in the middle of the aquarium-house, while at one end have been erected heated compartments with plate-glass fronts for tropical snakes and lizards, at the other end cages and a tank for diving birds. The *Irish Naturalist* states that it is intended to erect a new house and paddock for the llamas and camels, two of the latter having died last year in the old house. New quarters for the marsupials are also contemplated.

THERE was once in Great Britain an Aeronautical Society, not to be confused with the Balloon Society. This is being resuscitated, with a committee including Colonel Baden-Powell, Colonel Templar, and Mr. H. S. Maxim. Balloons are now becoming important aids in many kinds of scientific research; thus, on February 18, balloons with observers or self-registering instruments were sent up simultaneously from Berlin, Strasburg, Paris, and St. Petersburg. The Society will have a large field of usefulness.

Science says that Robert H. Lamborn has bequeathed $200,000 to the Academy of Natural Sciences, Philadelphia.

PROFESSOR ANGELO MOSSO of Turin has received from the Reale Istituto Lombardo the Fossati prize of 2,000 lire for his essay on the temperature of the brain.

MR. W. HARCOURT-BATH left England on March 12 upon an entomological expedition to the Himalayas.

The Journal of School Geography for February states that the northern part of Greenland, studied in 1892, by Lieut. R. E. Peary, has been named Peary Land at the suggestion of the Geographical Club of Philadelphia, seconded by many other geographers.

Mr. and Mrs. Theodore Bent, the archæological explorers, have returned from a successful expedition to Socotra.

Mr. J. Whitehead has been investigating the highland fauna of the Philippines, where he obtained a huge fruit-pigeon at a height of 6,000 feet.

According to Reuter, Mr. Poulett Weatherley, who has just circumnavigated Lake Bangweolo in Central Africa, states that it is smaller than usually marked on maps and that, separated from it by a strip of sand about 400 yds. wide, is a long lake named Chifunanti. These lakes, as well as L. Malombwe on the Upper Shiré River, he regards as recently submerged tracts of lands. They are quite shallow, and Bangweolo has no molluscan fauna.

Mr. C. W. Andrews, of the Geological Department of the British Museum, has received leave of absence for nine months in order to investigate the natural history of Christmas Id., one of the few spots on this earth that still remains uninfluenced by man.

It is 400 years since John Cabot, the Venetian navigator, who lived in Bristol, sailed from that city and landed on some coast on the other side of the North Atlantic. The event is to be celebrated both in Bristol and in Halifax, Nova Scotia, on June 24.

We are glad to note that the salary of the lecturer on fisheries to the Cornwall County Council has been increased to £350 per annum.

The huge octopus described by Dr. A. E. Verrill in the *American Journal of Science*, as recounted by us in our March number, turns out not to have been a cephalopod after all. Some portions of the integument have been examined by Dr. Verrill, who, writing to *Science*, suggests that it was probably related to the whale, though what part of any cetacean it could have been is still an unsolved puzzle.

A series of lectures on "The Black Death of the Fourteenth Century," was delivered by Mr. W. North, in February, at Willesden Green Public Library. Mr. North traced the origin and progress of the disease, compared it with the present plague in India, and gave some interesting notes on the sanitation of England at the time, and the value of the plague as a sanitary reformer in this country.

C. H. Fernald, entomologist to the Massachusetts Board of Agriculture, recommends that $200,000 be appropriated for five years in succession, and a smaller appropriation for ten years more, for the extermination of the Gipsy Moth.

Mrs. Asa Gray has been mounting the autograph letters from various botanists received by her late husband. Those preserved number over 1100. The *Botanical Gazette* states that the mounting is now completed, and that, with the letters, whenever possible, an engraving or photograph of the writer has been mounted.

Coal, in seams with a thickness of 16 and 8 feet, has been found seven miles from the northern end of Lake Nyassa.

Borings, instituted in consequence of the discovery of coal at Dover, have succeeded in finding coal at Cape Blanc-Nez on the opposite side of the Channel, further west than coal has hitherto been worked in northern France.

CORRESPONDENCE.

Mr. Jackson's Map of Franz Josef Land.

I am obliged to you for the promptness with which you have sent me the March issue of Natural Science, and only regret that you should have thought fit, in the course of editorial comments in two different places in that journal, to repeat and freshly state certain erroneous statements which, in the absence of Mr. Jackson, I am compelled to correct. My reply, however, shall be as brief as possible.

(1) In the first place—to dismiss at once the personal attack on me—I regret that you should not have imitated the invariable practice of editors of reputable journals and accepted, in a purely personal explanation, my emphatic and absolute denial of the motives you imputed to me. I clearly and strongly protested that "nothing was or could be farther from my intention" than to minimise Nansen's magnificent work, and proved this by quoting several extracts from my paper in the *Geographical Journal* (Dec., 1896), which you are good enough to style "effusive laudation." Yet, in spite of this emphatic repudiation, you persist in your attempt to impute to me motives I never so much as dreamed of. Your last sentence in your second editorial comment (Nat. Sci., March, 97, p. 216) runs thus:—"people will infer, most unjustly of course, that he is trying to minimise the results of the Austrian explorer *as well as those of the Norwegian.*" (The italics are mine). This statement, I repeat, is capable of but one interpretation—and that one incompatible with acceptance of my word in a purely personal matter.

[We accepted and we accept Mr Brice's personal explanation absolutely; what we said and still say, is that his way of putting things did actually lead people to read into his remarks motives which they regretted. We repeat, we are glad to find that it was Mr. Brice's style and not his heart that was at fault, and we hope that he will exercise greater care in future.—Ed. Nat. Sci.]

(2) You repeat—and this time with an assurance which is amazing in one who has never been to Franz Josef Land and apparently knows nothing accurate about it—that "C. Mary Harmsworth is identical with C. Lofley." I can only add now that in 1880 Mr. Benjamin Leigh Smith saw the land he called C. Lofley for the first time. He was then *at a distance of forty miles.* ("The farthest point seen, being over forty miles away, was named C. Lofley." *Proc R. G. S.*, March, 1881, p. 135). In 1881 he saw the cape again, and the land connecting it with C. Ludlow. He was then not nearer than *thirty miles* (see map and track of Mr. B. L. Smith's voyage, *Proc. R.G.S.*, April, 1883). No Arctic explorer would place undoubting reliance on observations at this great distance.

Now Messrs. Jackson and Armitage (with Mr. H. Fisher and others) actually rounded all the capes discovered by Mr. B. L. Smith, and in several instances ascended them and observed from them, *for the first time.* They also observed from a point as far off the land as Mr. Smith was. I naturally prefer their map of the capes in question to one laid down at a range of not less than 30 miles. And so, too, would any sane cartographer.

[We have never doubted the accuracy of Mr. Jackson's maps. Our criticism of the names employed represents the view of people who *have* been to Franz-Josef Land; if such a visit be necessary for forming a correct judgment, perhaps Mr. Brice will say when he was in these parts.—Ed. Nat. Sci.]

(3) You return to your assertion that Markham Sound is identical with the British Channel. For after referring to a supposititious position of Markham

Sound, you declare that "this is the exact position usurped by Jackson's British Channel."

My postscript, pointing out that Mr. Jackson was only responsible for names in heavy type in the recently published map (*Geographical Journal*, Dec., 1896), should have saved you from repeating your error.

For *Mr. Jackson is not responsible* for the position of Zichy Land as there indicated in *light type*. He places Zichy Land to the east of the British Channel, but to the west of Markham Sound. (See *Geographical Journal*, Dec., 1895, p. 517)—"The coast of Zichy Land becomes a group of narrow islands, lying roughly north and south between Mr. Jackson's route and Austria Sound." (Also *G. J.*, Dec., 1896, p. 552). And Markham Sound in Payer's map was an arm of Austria Sound.

It is obvious that you have not mastered the elementary fact that the coast of Zichy Land abutting on Markham Sound has become by *actual traversing*—not by looking at it from a distance of at least 40 miles (see Payer's map and track "New Lands within the Arctic Circle" Vol. II.)—a chain of islands to *the east of the British Channel* and between it and Markham Sound. The supposed S.W. prolongation of Markham Sound (see Payer's and Smith's maps) is arrested and occupied by newly discovered islands. Hence the sudden termination of Markham Sound in a S.W. direction. Nansen's and Jackson's maps exhibit this clearly.

[We certainly have not mastered this: for any amount of traversing to transfer a body of land from the west to the east side of a body of water is a geographical feat quite beyond us It also transcended the powers of the mysterious person responsible for Mr. Brice's previous map; whoever he was we congratulate him on his having placed Zichy Land west of the Elmwood meridian, instead of east of it as Brice, Jackson, and Nansen now do.—Ed. Nat. Sci.]

(4.) As to the land marked 21, and the islands marked 10 and 11, you declare that "these small patches are not quite the same as 'the land to the westward' of Nansen's winter hut, for this obviously included Zichy Land."

I reply that they are the same, and that the land to the westward could not include Zichy Land.

How absurd your statement is may be gathered from Nansen's own map ("Farthest North," Vol. II.) and from his book, with which you have evidently not made yourself acquainted. In his map these lands lie to the west and south-west, while *Zichy Land includes the island on which he wintered*. And this is what he says on p. 398, (Vol. II.).

"At two points on the horizon about W.S.W., I fancied that I could see land looming in the air. The appearance recurred again and again, and at last I was quite certain that it really was land; but it must be very far away; at least 69 miles, I thought." And he adds in a note: "It proved afterwards that the distance was about 56 miles." Now you have only to measure this course and distance on the map, and you have the identical lands to which I referred and no other. They are "quite the same," and Zichy Land could not be "obviously included" in this land to the westward, for the very obvious reason that Nansen was, at the time, actually upon one of the islands which both he and Jackson unite in recognising as Zichy Land!

[Had Mr. Brice written "the small islands to the westward" instead of "the land to the westward" we should, as explained in our last number, have accepted his statement.—Ed. Nat. Sci.]

(5.) But, with even more amazing assurance than this, you reiterate that "Nansen wintered on Payer's Karl Alexander Land." I can only reply that Nansen says that he did *not*; that he definitely maps Karl Alexander Land as *another island* to the N.E.; and that consequently you have no justification for your uncalled-for and offensive remark that Jackson's "gracious permission to rename an already named island has conferred immortality upon the little trip of the Fram."

(6.) As to the rhetoric you expend on the innocent statement that Nansen thought, in his winter quarters, that he was on the west coast of Franz Josef Land—a statement you rashly describe as "incompletely proved"—I merely refer you to the following pages of "Farthest North": Vol. ii., pp. 307, 312, 334, 396, 397, 398, 427, 428, 459—on all of which you will find explicit proof that Nansen believed himself to be somewhere on the west coast of Franz Josef Land. Thus, your challenge of this simple remark, and, further, your attempt to wrest it into "discredit being cast on Nansen," pass, with the rest of your unfounded criticism, into the thinnest of thin air.

Finally, should you feel moved, in the light of these statements of positive fact—authority for every one of which I give—to reiterate the gross inaccuracies and sinister imputations you have now twice seen fit to publish, I desire to take this opportunity of at once saying that I have neither time nor taste for correcting anonymous critics, and therefore, sir, I beg to bid you farewell.

157 Strand, W.C. Arthur Montefiore Brice.

[Our Review copy of "Farthest North" had not arrived when the March number went to press. At any rate that book was not published when Mr. Brice's paper was issued, and statements which may or may not be proved by it now, were not proved then, and ought never to have been made on the incomplete authority of anyone's private correspondence, quoting casual conversations. This is all the "rhetoric" we expended, and we say again, accepting to the full the good faith of Messrs. Brice and Jackson, such statements should not have been published by the Royal Geographical Society. Fortunately we are now able to deal directly with Nansen, and this we do in our Review on p.268. Consequently we have great pleasure in bidding a sincere "farewell" to Mr. Montefiore Brice.—Ed. Nat. Sci.]

P S.—I submit herewith the map of Franz Josef Land, in which you will see how my statements relative to Zichy Land, Markham Sound, and "the land to the westward" are in every particular confirmed.

This map also confirms my statements relative to the work of Mr. Jackson and other explorers.

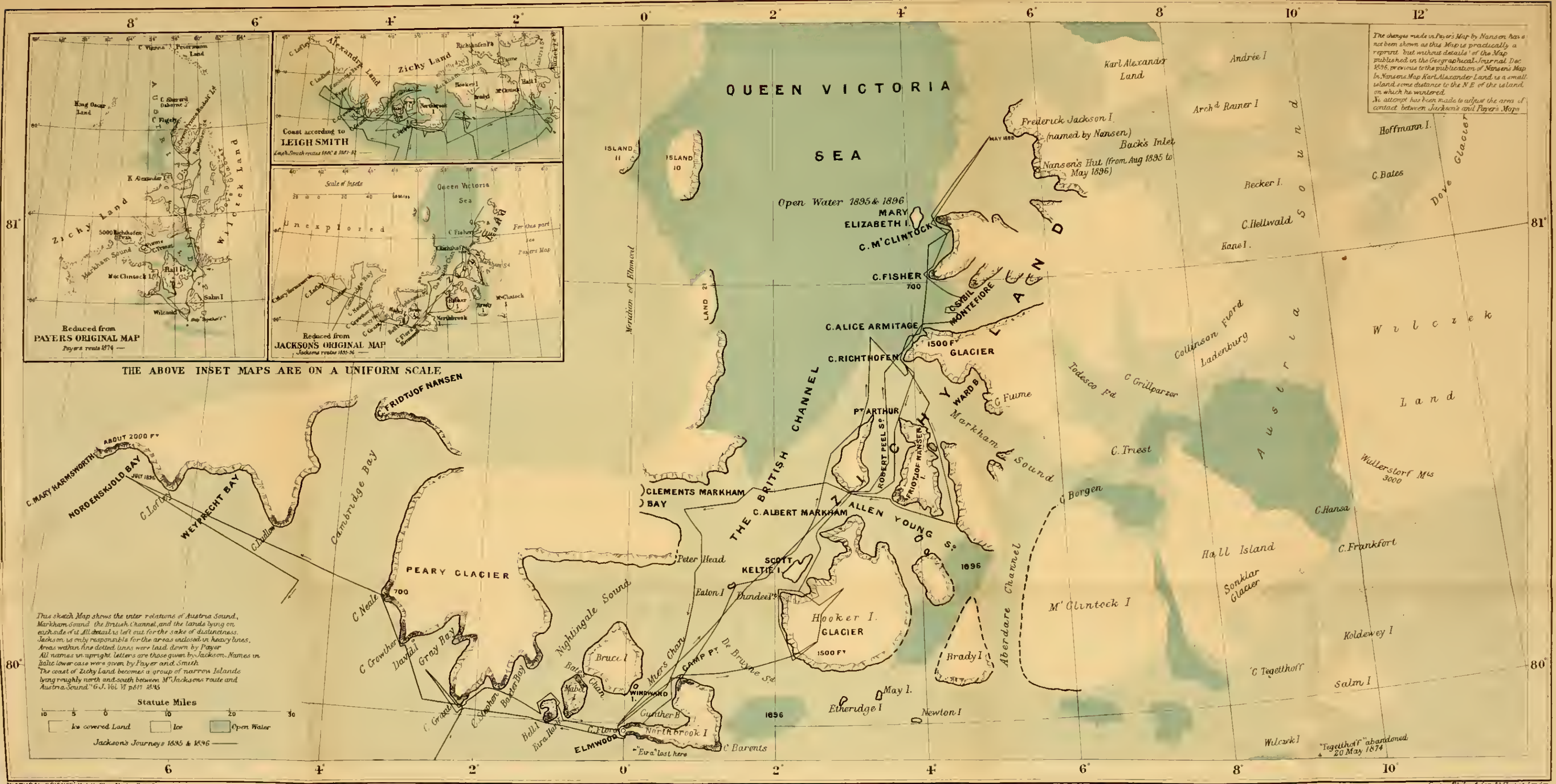
The only point I have not cleared up in this map is your contention that Karl Alexander Land is identical with the land on which Nansen wintered. The reason why I do not is—first, that Nansen has himself abundantly proved in his map and book that he did *not* winter on Karl Alexander Land; and, secondly, because in this composite map—which is practically identical with the one in the *Geographical Journal*, on which you based your criticism—no attempt has been made to reconcile at the points of contact Payer's map with Jackson's, or, indeed, to interfere in any way with Payer's map where it does not touch Jackson's work. A. M. B.

[We are in no way responsible for the names or statements on this map. Had we made any addition to it, it would have been the explanation, prefixed by Mr. H. Fisher to his "Remarks on the Flora of Franz Joseph Archipelago" appended to Mr. Brice's oft-quoted paper: the explanation runs, "In this case I have not considered priority in names of importance." We need only add that we are indebted to Mr. Brice for bearing most of the expense of this map, a courtesy for which we trust he will accept our thanks.—Ed. Nat. Sci.]

We regret that communications from Messrs. Brooks, Cockerell, Collinson, Goodrich, and Keegan should be crowded out of our Correspondence.

Our usual NOTICE will be found on page iii. of wrapper.

SKETCH MAP OF FRANZ JOSEF LAND SHOWING JOURNEYS & DISCOVERIES OF FREDERICK JACKSON F.R.G.S. LEADER OF THE JACKSON-HARMSWORTH POLAR EXPEDITION.



"NATURAL SCIENCE", Vol. X., Plate II April 1897

Elmwood lies in 49° 47' 15" East Longitude

George Philip & Son, 32 Fleet St. London

NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

No. 63. Vol. X. MAY, 1897.

NOTES AND COMMENTS.

The Case of the Chemical Society.

A CONTESTED election for the presidency of one of the great learned societies of London, is an event too rare to be passed by in silence, even by those who, like ourselves, have no connection with the Society in question, and who do not discuss the subjects of its study. In the present instance, moreover, the contest has formed the subject of lively conversation among all classes of scientific men, and has even been alluded to in the daily press.

Now we are not chemists, and we do not propose to appraise either the scientific or the personal merits of the two eminent candidates for the presidential chair of the Chemical Society. But there are certain features of the contest that call for comment upon grounds common to all scientific societies—indeed, to all societies whatsoever.

The essential facts are these: The Council of the Society, as is the custom, nominated a President and Officers. The Council's nominee for President was objected to by some Fellows of the Society, and they nominated for that post another gentleman, who happened to be one of the Council's own nominees for the office of Vice-President. Further, to give their nomination equal publicity to that by the Council, they sent to each Fellow of the Society a circular letter announcing their action and requesting support for their candidate. In this letter no reasons were given, nor was the slightest attack made on the Council's nominee. As counterblast to this, certain other Fellows issued another circular, not merely defending the nomination by the Council, but objecting quite unwarrantably, as it seems to us, to the action of the Council's opponents and introducing a regrettable personal element. This mode of defence was carried beyond all limits of good taste in a letter that a well-known chemist thought fit to send to the *Chemical News*, a letter for which we hope apologies have since been tendered to the gentlemen named in it. These whips, counterwhips, and scorpions brought to the poll some 400 Fellows, and resulted in the election of the Council's nominee. The majority, we understand, was not large; but, as so often occurs with the peculiar system of voting enforced by councils, the scrutineers were unable to declare the exact numbers of the votes.

In spite of the bad feeling stirred up, especially by the supporters of the Council, we rejoice that this fight has taken place. If the councils of other societies are disposed to the autocratic attitude of the Council of the Chemical Society, it is, indeed, about time that a little wholesome public opinion were brought to bear on them. This attitude seems to us illogical, peevish, and ridiculous. Theoretically, the council and officers of each society are elected by the fellows of the society; theoretically, each fellow has a vote, which he can cast for any other fellow, or even for himself; theoretically, though it is the duty of the council to submit to the fellows names for election as officers and council, it is in the power of the fellows to propose other names in their stead, and to reject, by their votes, the nominees of the council. We do not find it stated in the Charter or Bye-laws of any society, that its council alone has the power of election. But practically, each council does elect its successors. The election by the fellows is as much a farce as the election of an archbishop. The *modus operandi* is so arranged that opposition is almost inevitably abortive under ordinary circumstances. The method of voting must indeed be cumbrous when the scrutineers cannot count the votes, even in many hours. And when extraordinary circumstances arise, and publicity affords the opposition some chance of success, then, it appears, the proposers of a rival to the council's anointed, are to be gibbetted in the public press as lovers of disorder, ill-wishers to the commonweal, vile traducers of respectable eminence, and, "unkindest cut of all," as young men.

Experience leads us to sympathise with the difficulties of councils, and with the irritation aroused by opposition. None the less, we think it would be to the advantage of every learned society, in many ways, if its council would remember that, theoretically at least, it was elected by the fellows; if it would cease to regard itself as a superior "Treasury Bench," and the rest of the society as "a factious opposition below the gangway." More openness would promote better feeling. More facilities for discussion of the society's affairs would engender more interest on the part of the fellows. A recognised opportunity for fellows to propose their own candidates for the council and officers, and the full publication of the names of all nominees on one ballotting paper, would withdraw the solemn proceedings at Annual Meetings further from the region of burlesque, and might often extract the council itself from a provoking dilemma. Instances of such dilemmas are best left to present themselves to the mind of the intelligent reader. Finally, it would not be amiss if some members of council, instead of writing abusive letters, devoted their powers of organisation to the invention of a method of voting that should give the nominees of the council and of the fellows an equal chance, and should not result in so many spoiled ballot-papers.

The Army Examinations and Science in Public Schools.

The regulations recently issued by the War Office authorities relating to the examination of candidates for the Army are of such a nature as virtually to render it compulsory that all candidates should offer themselves for examination in some branches of Physical Science. Without expressing unqualified approval of the entire scheme, we welcome this recognition of the educational claims of scientific subjects. It is obvious that not infrequently a knowledge of physics, chemistry, or geology may be of practical value to the Army officer, and thus indirectly to the nation that he serves. The institutions most affected by the new regulations are the Public Schools, for these will be obliged to provide, as indeed some already do, scientific teaching for their army-classes, unless they are prepared to lose all boys who select a military career.

We have lately made inquiries at most of the Public Schools with a view to ascertaining their methods of teaching science, and this leads us to speak not only of their arrangements for army-classes but also of their entire system. At most schools special arrangements are made for army-candidates, and they are accordingly prepared with a view to particular examinations. With regard to the remainder of the boys the provision seems to be utterly inadequate. The Public Schools are dominated to an excessive degree by the classical training; their Head-masters have failed to realise the value of science as an educational instrument, while in many cases the arrangement of the science classes and the number of hours devoted by them to scientific work are such as to render the efforts of the masters nugatory. In short, science is admitted to the Public Schools merely as a sop to satisfy the demands of a public opinion which as yet is too feeble to effect any reforms of value.

We find that at Winchester, Shrewsbury, Charterhouse, Tonbridge, and in part at Eton, Cheltenham and Uppingham, the science classes are arranged according to the aggregate of marks in all subjects, classics, of course, being of greatest weight. Thus, in the worst cases, no matter how incompetent a boy may be in science, if he is a good classic he is hurried up to the senior science division of the school. It is almost inevitable that under such conditions every science class should contain boys of all degrees of ability and its opposite. Thus, the possibility of a rational course of science teaching, beginning with the veriest elements and proceeding to a fairly advanced stage, is absolutely prohibited. The effect produced on any of our classical Head-masters by a term's work with some half-dozen classes composed of similar classical disparities would form an interesting study, and might perchance convince them of the futility of attempting to teach any subject under such conditions.

The great importance of practical work in the laboratory has not yet been grasped. It is true that at many Public Schools practical work can be done, and is taken advantage of by many training for a scientific career; but the proportion of those who do practical work to those who attend science-lectures shows that the real value of the training has not been appreciated at head-quarters. For instance, at Eton no practical work is done "in school"; at Winchester some 90 boys out of 270 avail themselves of their opportunities; at Rugby about 180 out of 400; at Charterhouse some 30 have the opportunity out of 400 odd; at Uppingham but 14 out of 150; while at Harrow, Shrewsbury, Marlborough, Cheltenham, Malvern, Dulwich, and University College School all who learn science also do practical work.

But to turn from details to general principles of education. The idea is prevalent among too many of those who control the Public Schools that the literary education is the sole method of training a growing mind; accordingly by far the greater part of a boy's time in school is occupied with Greek and Latin. Now a literary taste is possessed by but few, and though we are very far from denying the value of such a training to a boy more disposed to science or the mathematics, yet we deem it monstrous that all boys should, by the average curriculum, be compelled to specialise in the Classics through devoting more than half their time to the subjects included under this term and dividing the remainder among Mathematics, Modern Languages and Science. A classical training is, to the average boy, but a series of appeals to the memory; the faculties of observation, deduction based upon experiment, and natural inquisitiveness are absolutely neglected, and their growth discouraged and stunted. Original thought, independence of action, and self-confidence are in no sense educated by the classics. What is the result? We witness at the present day a perfect mania for athleticism, which, starting in the schools, pervades the great mass of the younger male population. Literature as a recreation is at a severe discount. It is our belief that this athletic craze, so deplorable in its excess, is in no small degree a natural revolt of the mind from the trammels of a one-sided education. In athletics at any rate there is scope for a certain training of hand and eye, it is open to any one by his own efforts and perseverance to attain to the highest things, to act upon his own initiative, to devise some refinement hitherto unthought of, and to gain confidence in self by successful grappling with difficulties.

It is not difficult to pick holes in any system, nor would our present remarks be justifiable were we not prepared with something other than mere destructive criticism. We would suggest to the Public Schools that their education be more general and adapted to educating not one only, but all the mental faculties. Let the classics hold premier place by all means, but at least give the other subjects a fair representation, and let them be so put before the pupils as to form a true means of education, and not merely as vehicles of gentlemanly information. We consider that every boy should, for his first two years at a public school, be taught science for at least six hours a week, and that fully half of this time should be assigned to practical work. But most important is it that the science classes should be arranged strictly and solely according to merit in science: a rational course of scientific education is almost impossible on any other basis. At the end of two years it is not unreasonable to suppose that a boy will be desirous of devoting his attention more fully to some one subject, but we should like to see all subjects still included in his time-table up to the end of his school career. Just as it is undesirable that a boy should give up all classics for science, so, too, it is most undesirable that the converse should maintain. It is a common complaint among teachers of science that their efforts are hampered by the necessity of confining the work to the syllabus of an examination. This drawback does not exist for the majority of boys at Public Schools, and therefore in these there is an opportunity, such as is denied to many institutions, of teaching science in an educationally scientific manner.

We urgently appeal to the Royal Society to bestir itself in this matter. The governing bodies of most of our great schools contain a member nominated by the Royal Society. Such nominees presumably realise the educational value of the natural sciences, and to their keeping has been committed a national trust of no small value. We look to them for an active interference with the present state of affairs, in fulfilment of their heavy responsibility.

Young Oxford and Science.

The first volume of the Robert Boyle lectures, delivered before the Oxford University Junior Scientific Club, 1892–96, is now issued. It contains the lectures by Sir Henry Acland, Lord Kelvin, Professor A. Macalister, Professor A. Crum Brown, and Professor W. Ramsay, to several of which we have already drawn attention. To it Sir Henry Acland has contributed a preface which concludes with the following interesting remark: "...Natural Science, pursued in the temper of Robert Boyle, has led, does lead, and will lead the young of Oxford to a love of truth for truth's sake, as was foreseen (it may surprise some to learn) fifty years ago by Dr. Pusey, without whose support and that of his friends the final vote would have been lost for founding the Oxford University Museum."

The Robert Boyle lecture this year is to be delivered by Captain W. de W. Abney.

Supposed Remarkable Sea-anemones.

Every one who is interested in the anatomy of Coelentera must remember the description of figures given by Danielssen of the remarkable actiniarian genera *Fenja* and *Aegir*. It was said that these forms possessed a tubular alimentary canal passing from the mouth to the other end of the body, where it terminated in an anus. This remarkable feature, together with others which it is not necessary to particularise, upset all the current definitions of the Actiniaria and indeed of the Coelentera. It placed an insuperable difficulty in the way of accepting well-known views of the origin of the triploblastic alimentary canal, and in other respects led to modifications in our ideas of invertebrate morphology.

F. E. Schultze and others expressed doubts of the facts described; but the weight of Danielssen's knowledge and experience prevailed, and the genera have found their way into many recently published text-books of zoology.

Dr. A. Appellöf has recently re-examined the original specimens in the Bergen Museum, and the result is that the whole castle of facts and fancies comes crashing to the ground. He proves conclusively that *Fenja* and *Aegir* are but injured, macerated, and introverted specimens of *Halcampoides clavus*, and that the species *H. abyssorum* which came from the same locality is but a synonym of the older species.

Thus ends this ten years' controversy, and a great error is removed from the pages of zoology.

Zoology at Bergen.

Dr. Appellöf's paper is published in the Year-book of the Bergen Museum for 1896, which besides the official reports, alluded to elsewhere, contains many interesting communications.

Mr. O. Nordgaard has proved by experiment, that though the eggs of salmon and sea-trout can be fertilised and hatched in water with a salinity of as much as ·9 per cent., they are all killed by a salinity of 2 per cent.

The same worker concludes his systematic list of Norwegian marine Bryozoa: in this part he enumerates the Cyclostomata, of which he includes twenty-four species belonging to nine genera. None of them are new. Mr. Jas. A. Grieg discusses in detail the affinities of *Funiculina* and *Kophobelemnon* two genera of Pennatulids. He also insists on the close relationship of *Funiculina* and *Leptoptilum*, the latter one of the genera founded on material collected by the "Challenger": Grieg does not, however, at present propose to merge *Leptoptilum*. The most interesting general question considered in the paper, is as to the possibility of some of the northern and southern species being identical.

Mr. Grieg also gives a useful list of the Mollusca (194 species) found in the Sogne Sea, on the West coast of Norway, prefaced by a general survey of the invertebrate and tunicate fauna of the same locality. This is succeeded by a more detailed account of certain nudibranchiate Mollusca, among which a supposed variety of *Tritonia plebeia* receives particular attention. The author nowhere considers the possibility that his "colossal variety" is simply a young specimen of *Tritonia hombergii*, a view which is nevertheless rendered extremely probable by his figures and description.

Professor R. Collett gives a detailed description of the interesting deep-bodied Scomberoid fish *Pterycombus brama*, with beautiful figures both of the external aspect and the skeleton.

In a systematic note on the holothurians of Norway, Mr. Hj. Östergren confirms the view already expressed by Théel, that the supposed spicule-less form named *Holothuria ecalcarea* by Sars has lost all its calcareous structures through the action of acids. It appears to be a *Stichopus* closely allied to, if not identical with, *S. tremulus*. It follows from Östergren's researches that our common British cotton-spinner is the only true *Holothuria* found beyond the two 50th parallels of latitude.

There are also in the *Aarbog* a systematic review of the Collembola of Norway by O. J. Lie-Pettersen, faunistic researches in Osterfjorden by A. Appellöf, and a study of the eyes in *Pecten* and *Lima* by K. E. Schreiner.

THE EYES IN *Pecten* AND *Lima*.

IN *Pecten*, and indeed in many other animals whose eyes are even better known, the relation of the nerves to the retinal cells is still in dispute. According to Patten, a nerve-fibril runs axially through the rod-cells and rod, emerging at the tip of the latter in order to bend back over its outer surface, which is thereby covered with a complicated network of fine nerve-endings. Mr. Schreiner denies the existence of any such axial nerve or canal, but maintains that the appearance is produced merely by the shrinking of the cells so as to form longitudinal ridges, while the axial thread that is to be seen in the rod itself is, according to him, merely the continuation of the protoplasm of the rod-cell. Patten's statement that the outer ends of the rods project slightly through a definite, perforated membrane, which he calls the "vitreous network," though rejected by Rawitz, is confirmed by Mr. Schreiner, who further supports Patten in his view that the rods are not, as maintained by Carrière, embedded in a fatty substance, for such an appearance is easily produced by reagents. External to the rod-cells are the 'ganglion cells,' which, over the central region of the retina, are in three or four layers, but at the periphery form only a single layer. The optic nerve, as is well known, branches, one branch going to the proximal base of the eye, the other running laterally past the retina in order to bend round on to its distal surface. Here the retina, says our author, is covered by a septum, "through which the greater number of the nerve-fibrils pass near the centre"; the fibrils are continuous with the filamentous ends of the rod and ganglion cells.

Mr. Schreiner doubts whether the glittering 'argentea' or 'tapetum,' against which the tips of the rods rest, is, when the adult stage is reached, in any way a cellular structure. The same difficulty arises as to the pigment. Among those who have studied it—Bütschli, Carrière, Hensen, Hickson, Patten, and Rawitz being the foremost—Hickson stands alone in maintaining the pigment-mass to be non-cellular. This is, in fact, the case, according to Schreiner, in *Pecten maximus*, which Hickson examined, and also in *P. islandicus*. In these forms the pigment is a brown-red fluid; but in other cases investigated the pigment-layer is distinctly cellular.

With regard to the interesting problem presented by what is called the inversion of the eye, *i.e.*, the turning of the retinal cells away from the light, the author looks only to the study of its development for a solution.

Mr. Schreiner concludes that the eyes of *Pecten* are true eyes. He will have nothing to say to their being 'heliophags' or energy-absorbers, as suggested by Patten. He rejects, moreover, the interpretation of Rawitz, who has supposed that the numerous eyes all act together like the ocelli of a compound eye.

On the edge of the mantle in *Lima excavata* are some very simple eyes. They are nothing more than open pits, irregularly filled by masses of slime, and are thus of no small importance in the comparative study of visual organs.

Archæology at Bergen.

Papers no. vii., ix., and xv. of the same Year-book refer to recent acquisitions by the Bergen Museum. The first, by G. Gustafson, deals with the pre-Reformation relics acquired in 1895, including several grave-finds of early and late Iron Age, important additions to the already fine series of grave-finds in the museum. The second, by B. E. Benedixen, describes some interesting additions to the Mediæval collection: a painted panel of 14th century date, from the church at Lyster, and a second panel from the church at Eid in the Romsdal, probably of early 14th century date, and differing in style from the other. A fine piece of gold and red textile work is described, with a coloured plate, its *provenance* being Röldal Church in the Hardanger district, also a handsome bronze candelabrum surmounted by a figure of the Virgin (Mutter-gottesleuchter) from Kinservik, probably of German make, c. 1500. The third memoir, also by Mr. Gustafson, treats of an important find of silver coins and ornaments, including over 400 coins of Æthelræd, Knut, Sigtryg and others.

For the sake of completeness we may mention here an elaborate account, by C. F. Kolderup, of the labradorite rocks near Ekersund and Soggendal, the first of a series on these rocks in western Norway.

All the memoirs mentioned in this note are well illustrated by plates and text-figures, and they, like the zoological articles, reflect the greatest credit on the staff of this museum.

Formol for Zoological Specimens.

The following valuable remarks occur in the Report of the Director of the Colombo Museum, Dr. Amyrald Haly, for 1896. They are so likely to escape observation there, that we venture to reprint them *in extenso*.

"I reported last year that the use of formol amounted to a revolution in Museum work; twelve months' further experience has given me no cause to change my opinion. I find one and a half to two per cent. solution quite sufficient for invertebrates in general, and three to three and a half per cent. for vertebrates. It is not, however, its marvellous preservative power that is so striking, as the possibility of keeping objects in it which can be kept in no other way, or only with great difficulty. A striking instance of this is seen in jelly fish, which can be at once placed in a one and a half solution of formol and salt water, and retain all their transparency unimpaired. Earthworms are also perfectly preserved in this medium."

"Formol is an admirable preservative for spiders, and does not seem to injure their colour to any great extent. If sheets of cork painted white are inserted in flat-sided bottles, spiders can be pinned and set in the same way as insects in insect boxes, and as the collection increases they can be classified and arranged in their family and genera with perfect ease, and when space is provided will form a very beautiful exhibit."

"Our frog collection has been deteriorating greatly of late years. The fact is these animals will not keep in spirit in this climate. If the spirit is strong they wither up beyond identification, if weak they get covered with a deep red fungus. I went through the collection carefully, re-identified and re-numbered the specimens, and transferred the whole to three per cent. formol."

For a general account of formol, readers, who have not yet utilised its wonderful properties, may be referred to Mr. James Hornell's article "The Use of Formalin as a Preservative Medium for Marine Animals," Natural Science, vol. vii., pp. 416–420, December, 1895.

The Preservation of Fish and Reptiles.

In the Report of the South African Museum under the head "Fishes" it is stated: "Special attention has not been paid hitherto to increasing the number of specimens for exhibition in this class, owing, not only to the expense of preserving the specimens in spirit, but also to the impossibility of presenting them to the public in anything like their natural condition. Mounting or stuffing fishes is the art in which taxidermy has achieved least, and it is imperative that our South African fishes for exhibition should be modelled and coloured from nature by a skilful artist."

Mr. Haly, however, seems to have solved the problem of preservation for exhibition purposes: he writes thus:—"A specimen of a wrasse prepared by the gum and glycerine process in November, 1884, is now in my office, having been exposed to the light for the last thirteen years, and not a single tint has faded." Dr. Haly's original paper on this method and its alternative of "carbolicised" oil was published in the *Journal* of the Ceylon Branch of the Royal Asiatic Society, vol. xii., pp. 65–73 (1892). The paper concludes with the following recipe, which we quote for the special benefit of our many readers in tropical countries: "Add carbolic acid to cocoanut oil till the oil marks 10 to 20 degrees below proof on an hydrometer. The more powerful the acid, the more powerful the dehydrating effect, and judgment must be used. In this climate it is best, although not absolutely necessary, to remove the entrails. Place the specimen, carefully wrapped in rag, in plenty of this preparation. If wanted to mount for show, drain off the superfluous oil and mount in glycerine."

"Many of our exhibited specimens," continues Dr. Haly in his Report of 1896, "prepared by these methods date from 1885, and none are less than four years old. Casts, however carefully painted, can never give the same effect as the natural tints seen through a highly refractive medium. If, however, the gum and glycerine process is objected to on the score of its expense, or the carbolicised oil process on account of the difficulty of eliminating the oil from the specimen, which is however merely a question of time, mixtures of chloride of zinc in spirit or of formol and carbolic acid are both free from the above objections. I cannot give the exact proportions of the chloride of zinc and spirit, which probably ought to vary with the character of the fish to be preserved, but a three per cent. solution of half carbolic acid and half formol seems to be perfectly satisfactory. I have lately preserved a wrasse, *Julis lunaris* (Linn.), by this means, the colour of which is the most difficult to keep in the whole animal kingdom. The specimens should of course be transferred to glycerine as soon as possible."

"With regard to reptiles, I find the chloride of zinc and spirit mixture one of the best mediums for resisting the desiccating action of glycerine. I employ proof spirit raised to the specific gravity of sea water by the addition of Burnet's solution. A specimen of *Lyriocephalus scutatus* preserved in this way three years ago still retains all the softness of its brown and yellow tints, and the delicate opalescence on the shoulder."

How are Crustacea to be Preserved?

These animals are still a difficulty. Dr. Haly "can find nothing to supplant gum and glycerine for the preservation of their colours; they must not be placed in spirit before being placed in the solution, nor must the solution be reduced by spirit. If they are touched by spirit in any way the glycerine produces in a few hours great white blotches on the carapace, causing a horribly leprous aspect. This necessitates for large specimens the use of a large quantity of the gum and glycerine solution, which has then to be thrown away instead of being used several times as is the case with fish; the expense, therefore, becomes almost prohibitive. The effect of formol and carbolic acid is the same as boiling water, and the chloride of zinc solutions are most destructive. I am in hopes that solutions of gum in formol may solve the question."

We should be glad to receive communications on the above subjects. No doubt the curators of some of our great museums, especially the energetic Assistants in the Zoological Department of the British Museum, have been experimenting on these lines. It would be of interest and value if they would detail the results of their investigations.

The Fixation of Soft-bodied Marine Invertebrates.

On this subject we have already published various notes, and the present observations by Dr. Haly form a welcome addition. "Of late years," he writes, "I have received recipes for the display of marine animals by the Neapolitan methods, but I must confess that I have never seen any sign of success with any of them. I imagine that some practical training by a master of the art is necessary."

"On the contrary, Thulberg's 'Solution of Chloride of Magnesium' promised well from the first, and I have been equally successful with Epsom salts, which was suggested by some English naturalist for sea anemones. No class of sea animals can resist the action of these salts. The secret of their use is very simple: the salt must be dissolved in fresh water until the solution attains the specific gravity of sea water; this solution must then be mixed with from four to six parts of sea water. All forms of marine life display themselves in these mixtures to the utmost perfection. The difficulty is to transfer them to the formol: some require no fixing, and, curious to say, amongst them such delicate and excessively contractile forms as the Tubicolar Annelids and the Gephyræans. On the contrary, the Holothurians and the Nudibranchiates are extremely difficult to preserve in an extended state. Of the ordinary fixing agents, bi-chromate of potash seems to answer better than any. I have also been successful with excessively weak solutions of alcohol and by allowing alcohol to float on the top of the water, and thus mixing with extreme slowness. But I can give no recipe which works with certainty."

"The difficulties of these investigations on the coast of Ceylon are very great. We have no tide, and specimens are only to be found on isolated reefs of limited area, situated at great distances apart; these are only accessible for three or four months out of the year, and at the best of times they afford but little variety. I trust that other naturalists will take this subject up. The method is extremely cheap, simple and effective, and I am sure that with those opportunities which are denied to me a certain and simple process of fixing the more difficult will be easily found."

We echo this wish. And we cannot conclude without congratulating the Colombo Museum on having a Director who not only does such excellent curatorial work, but whose zoological studies on the fauna of Ceylon, numerous evidences of which appear in the Report from which we have quoted, are so varied and so valuable.

Preservation of Vertebrate Tissues.

In connection with Dr. Haly's experiments, we may allude to a method of preserving the tissues of Vertebrata, described not long ago by Dr. Kaiserling in the *Klinische Wochenschrift* (Berlin). The method has, it is stated, been tried with satisfactory results, the natural colour of the blood and the transparency of most of the organs having, so far, been maintained. The organ is first placed in a solution of 750 c. cm. of formalin, 1,000 c. cm. of distilled water, 10 gr. nitrate of potash, 30 gr. acetate of potash, and allowed to remain for some twenty-four hours; it is then immersed for two hours in 80 per cent. alcohol, for two hours in 95 per cent., and is subsequently preserved in equal parts of water and glycerine, with the addition of 30 parts of acetate of potash. Very delicate tissues are best kept in equal quantities of glycerine and water, after the addition of absolute alcohol in the proportion of one part of alcohol to ten of the mixture.

Fever in Plants.

Mr. H. M. Richards, who has previously studied the effect of wounds on plant-respiration, now describes (*Annals of Botany*, xi., 29) a course of experiments on the evolution of heat by wounded plants. He finds that accompanying the increased rate of respiration is an increase in the temperature of the parts affected. A kind of fever supervenes, and as in the case of respiration, the disturbance runs a definite course, and attains its maximum some twenty-four hours after injury. It is interesting to note that the attempt to rally from an injury is accompanied by somewhat the same symptoms, increased rate of respiration and evolution of heat, in plants as in animals. Owing to the nature of the case the reaction is less obvious in the former than in the latter, and a delicate thermo-electric element was required to appreciate the rise in temperature; but compared with the ordinary temperature of plants in relation to the surrounding medium, the rise after injury is "as great, if not greater than in animals." The maximum in all the plants investigated was between two and three times the ordinary excess above the surrounding air. Potatoes proved the most satisfactory objects for experiment, and it was found that in massive tissues (such as potatoes or radishes afford) the effect of injury was local, whereas in the case of leaves (*e.g.*, onion-bulbs) a much greater extent of tissue was sympathetically affected.

Bulb-Growing.

Daffodils and other species of *Narcissus*, hyacinths, tulips, and other bulbous plants are so common and so cheap in the early spring markets that we are prone to wonder where and how they are grown. We remember noticing a statement some years ago that early potato cultivation had been given up in parts of the Scilly Islands in favour of the daffodil. At present, however, we are interested in a suggestion by Mr. F. W. Burbidge, in a lecture to the Birmingham Gardeners' Mutual Improvement Association, reported in the *Gardeners' Chronicle* (March 27). "We are told" he says "by politicians nowadays that thrift and home industries are essential, and while some advocate milk or meat, or poultry and eggs, or jam-making, or the 'Busy Bee' industry, I will advocate bulb-culture." In Holland and elsewhere it is the small growers who supply the wholesale growers and the merchants, and Mr. Burbidge thinks that what can be done in Holland could be done by the small farmer, the cottager, the allotment-holder, and even the railway porter, in our own country. Bulb-culture, he tells us, on suitable sites and soils has proved to be the most profitable of all cultures in neighbouring countries, as well as in Cornwall and the Scilly Islands, and if the English gardener will only have more faith in himself, and in the soil and climate of his native land, the day may come when our great bulb-merchants will not be obliged to go abroad with their big cheque-books once or twice a year. We are glad to note this suggestion in applied botany, and are seriously thinking of trying to work out the hints on cultivation, &c., which Mr. Burbidge supplies, and grow a few bulbs on our own account. At any rate it is a point for the depressed agriculturist to note. Fruit-growing for jam-making depends largely for success on the cheap sugar which we buy from the Continent to the ruin of our West Indian Islands. The present Sugar Commission may perhaps alter this. In such a case it is reassuring to think that we can then fall back on bulbs, if only they can be got cheaply enough to market. Apropos of this last remark we notice in the same number of the *Gardeners' Chronicle* (p. 206) a paragraph entitled "Pleasant for the British Farmer." One steamboat, to be soon followed by two others, has lately been put on the service between Antwerp and London with the result that "any package of poultry, fruit, eggs, vegetables, &c., consigned to a Belgian station before 8 a.m. is delivered the next morning in the London markets at 5 a.m." The carriage rates are not given, but their advertisement would probably not add to the farmers' joy.

The Antiquity of Man in the Eastern United States.

The Delaware caves and the Glacial Gravels of Trenton, New Jersey, have long been regarded by some writers as affording evidence of Glacial Man, although others have not accepted this conclusion. The question has been thoroughly investigated by Mr. Henry C. Mercer, curator of the Museum of American and Pre-historic Archæology of Pennsylvania University. Mr. Mercer had previously sought for the remains of pre-historic man in the hill-caves of Yucatan, Central America (*see* Natural Science, vol. viii., p. 159, 1896), and had further qualified himself for the present researches by study of drift implements in the museums and private collections of Europe, and of the localities at which they have been found in England, Belgium, France, and Spain. The result of his investigations, which may therefore be accepted with considerable confidence, appears, with with one or two other essays, in a well illustrated volume of 178 pages, entitled "The Antiquity of Man in the Delaware Valley and the Eastern United States" (Ginn & Co., Boston, U.S., 1897).

Mr. Mercer failed to find a single specimen in place at Trenton, and became convinced that the argillite "turtlebacks" on which so much stress has been laid, were "rejects" or wasters "the work of modern Indians and intruded" into the glacial gravels. He shows that whole quarries of argillite were worked by Indians, whose village-sites are in the vicinity. The fact that after most careful and prolonged search nothing was discoverable anywhere else in the Delaware Valley to corroborate the alleged antiquity of the chipped blades from Trenton has led him to the conclusion "that the question of glacial man has been narrowed down to evidence produced at one site, and to a question of the correctness of observations of individuals."

The same volume records the story of the exploration of an Indian ossuary on the Choptauk River, Maryland. Professor E. D. Cope describes the physical characters of the bones found there, and Dr. R. H. Harte discusses their diseased conditions. Mr. Mercer's investigations of an aboriginal shell-heap on York River, Maine, yielded evidences of cannibalistic practices. He also describes an interesting rock shelter in Triassic shale, known as the "Indian House," in the Delaware Valley, and recounts his exploration in 1893 of the famous Durham cave, which made known the fact, interesting to palæontologists, that a species of an extinct genus of the peccaries (*Mylohyus pennsylvanicus*, Cope) had far out-lived the epoch usually attributed to the animal in North America. Its remains were undoubtedly "mingled with those of still existing animals, if not with the contemporary refuse of Indian cookery of post-glacial times." They recur elsewhere associated with those of the tapir, mastodon, and fossil sloth, which thus within "a comparatively few centuries" all roamed the forests of the Eastern United States.

The Extinction of British Butterflies,

In a recent short article in the *Entomologist*, Mr. W. Harcourt-Bath discusses the causes which have led to the dying-out of several of our native butterflies. He does not believe that climate has much to do with the extermination of these species, and he considers that even the rapacity of collectors has been less destructive than two other factors: the abnormal number of insectivorous birds owing to their protection and the persecution of their enemies, the hawks and owls, and the wearing-out of the butterflies by insular isolation and consequent in-and-in breeding.

That the senseless destruction of birds of prey by game-preservers may have such an effect on insect-life as Mr. Harcourt-Bath supposes is likely enough, but we fear the consideration will not have any effect on the slaughterers of so-called "vermin." The isolation factor is much more doubtful. Mr. Harcourt-Bath supports it by stating that the extinct or dying species have weak powers of flight, while our dominant butterflies are constantly recruited by immigrants from the Continent. On the other hand it may be urged that many species of Lepidoptera, with an excessively limited range—confined to a few discontinuous strips of our western coasts and presumably among the oldest inhabitants of our islands—show no signs of dying out except by the greed of the insect-hunter or the money-hunter; for unfortunately British Lepidoptera have a most sad pre-eminence among natural objects in their high market value. Their extinction will only be checked by the extinction of the "mere collector" and the dealer who supplies him.

We have said that Mr. Harcourt-Bath's article appeared in a recent number of the *Entomologist*. From the reprint supplied we can derive further information only with difficulty. The covers tell us the names of the editors and publishers, the price and terms of subscription of the magazine, but we look in vain for the date or number of the volume. However, at the bottom of the article, we find that it was written in November, 1896, and we therefore venture to give the reference as *Entomologist*, 1897, pp. 55–8.

The Extermination of the Golden Eagle.

Mr. Joseph Collinson sends to us from the Humanitarian League the following protest:

"The bird butchers are at it again. In the *North Eastern Daily Gazette* of February 17th, it is recorded that a golden eagle has just been shot in East Yorkshire. This is the second killed within three months near the coast, and I have authentic records of two more instances which occurred in Scotland during 1896.

"Seebohm, one of our most able and reliable naturalists, says:—'Before it is too late, Scotchmen, protect your national bird, the eagle of your ancestors, and stay the cruel war waged by grouse-shooter, deer-stalker, sheep-farmer, and skin-collector—war which will, ere long, play its part but too surely and take the eagle from your mountains for ever.'

"What are our County Councils about? The powers given them by the Wild Birds Protection Acts of 1880 and 1896 ought to be enforced more vigorously and universally than they are. A period of protection for the golden eagle is eminently desirable, and it is to be hoped that the councils representing the counties in which the golden eagles have recently been destroyed will make application to the Home Secretary to deal with the matter in a way which will in effect secure protection for the bird all the year round."

The sentiments are admirable; but we are somewhat perplexed to find so competent an ornithologist as Mr. G. W. Murdoch of the *Yorkshire Weekly Post* casting doubt on Mr. Collinson's statements: "I do not believe," says he, "a golden eagle has been killed in Yorkshire for nearly half a century."

Buffaloes.

The *Scientific American* for February 20 contains an interesting account by G. E. Walch of the destruction of the buffalo in North America, and of the attempts that are being made to domesticate and preserve the few individuals that are left. There is a small herd in the Texas Pan Handle, numbering less than 75, a larger one at Ravalli, Montana, numbering nearly 200. The latter belongs to Mr. C. Allard, who has crossed the wild animals with the polled Angus stock, thus producing a breed more fitted to withstand blizzards than are ordinary cattle. There is a small herd of buffalo, numbering 20, on Antelope Island in the midst of the Great Salt Lake, where during 1896 four calves were born.

Further evidence of the age of the plant-bearing beds of South Africa has just been brought before the Geological Society of London by Mr. A. C. Seward, from material sent to him by Mr. David Draper, of Johannesburg. Mr. Seward is led to the conclusion that these beds are of Permo-Carboniferous age. We are inclined to believe, however, that the evidence of the fishes and of the reptiles (as pointed out by Professor Seeley) is more in accordance with a Permian or even Triassic age than with a Carboniferous one. The "Lepidodendroid" plants, stated by Professor Seeley to occur in association with *Glossopteris*, at Aliwal North, have, however, not yet been seen or examined by any botanical expert in this country.

I.

Human Evolution.

IV.—Mr. Archdall Reid States His Position.

AT the outset I desire to thank Mr. Buckman for the obviously friendly tone of his article in your March number. As to the first part of his indictment I must plead guilty: the title of my book does not cover its whole contents. But the first part which deals with "Organic Evolution" was originally intended to be merely preliminary to the second part, which deals especially with man's present evolution. Without it the book would have been unintelligible to the general reader, and full of mere dogma to the biologist. For example, the theories of Retrogression and Variability, which were essential to my argument, and which appear to have met very general acceptance, have not to my knowledge been propounded elsewhere.

But as to the second count of Mr. Buckman's indictment I must demur. I think I may fairly claim that my book does indicate in all essentials the whole of man's present evolution. Mr. Buckman is, however, in excellent company. For example, Mr. Alfred R. Wallace wrote in *Nature* (April 16th, 1896), "The latter portion of the book, which gives the title to the work, though original, is somewhat disappointing, as it is entirely limited to evolution against disease." Professor Ray Lankester wrote in the *Fortnightly Review* (September, 1896), "I am by no means satisfied that the present and future evolution of man is being determined exclusively or even mainly in the simple way, and by the obvious factors which he has placed before us." Other critics have expressed themselves in similar terms, the implication always being that man's present evolution is exceedingly complex, and that the evolution against disease and narcotics forms but a small part of it. I think however that the contrary can be proved.

The whole matter, in my opinion, turns on the question so long and so hotly debated, as to whether acquired traits are, or are not, transmissible. If they are not transmissible, and evolution, therefore, results solely from the selection of favourable congenital variations, then the problem is simple, for, by taking note of the main lines of elimination, we can easily ascertain all the directions of evolution. Now men, at any rate civilised men, perish almost solely of disease or of the effects of various narcotics such as alcohol and opium. Man's present evolution is therefore almost solely against disease and narcotics. Practically speaking, all other causes of elimination too slightly effect the survival rate to be causes of evolution: and moreover, these other causes do not, under modern conditions, generally eliminate particular types of individuals; for example, individuals who perish of starvation, are not necessarily those who are least able to endure prolonged abstinence from food, but generally those who, owing to other causes (*e.g.* the effects of disease), are unable to procure food; again, those who perish of drowning are not in particular those who are least fitted to acquire the art of swimming, but quite chance individuals. Little or no evolution, therefore, can result from these minor causes of elimination, and therefore the present evolution of man is in a direction altogether different from his past evolution during which were evolved his physical and mental parts.

It is possible that had these considerations been present to the minds of my critics, some of their strictures would not have been written. For example, Mr. Buckman says: "It is strange that a work entitled 'The Present Evolution of Man' should take no account of the reproduction question. For instance, there is at the present day a sudden decrease in the birth-rate. It undoubtedly corresponds with the rapidly-spreading knowledge of chemical means for checking fecundation. The less fertile race will inevitably succumb to the more fertile, and this decrease of the birth-rate heralds its disappearance. It is remarkable that a book dealing with the evolution of man says nothing of a factor more important than any it treats."

But supposing the English, for example, used artificial checks to fecundation and the Irish did not, what, let me ask Mr. Buckman, does he conceive would be the direction of the resulting evolution? What would be the physical or mental change? Surely it is clear that, since no particular type of individual would be selected, no evolution could result. Even if the English became altogether extinct from this cause and were replaced by the Irish, it is clear that the surviving race would not undergo evolution, since among them there would have been no elimination. In fact, if we premise that congenital or inborn variations are alone transmissible, we must conclude that the evolution of even a very highly complex animal such as man can seldom at any one time have proceeded on very complex lines. His different structures must have been evolved, under the changing stress of Natural Selection, during different but overlapping epochs of a long-extending past. What perceptible evolution, for instance, have such highly important organs as his hands and feet, his eyes and ears, his heart and lungs, etc., undergone for thousands of years? When any of these attained such perfection as to place man in harmony with that portion of his environment with which it was concerned, its evolution ceased, and the direction of man's evolution changed.

If, however, we premise that acquired modifications are transmissible, then, it must be admitted, that the evolution of man and every other complex animal is proceeding on enormously complex lines—on lines so complex that it would be hopeless for any man to attempt to delineate them in their entirety. But are they transmissible? Mr. Buckman thinks they are. He writes: "The germ is a unicellular organism, and therefore it should be modifiable in accordance with its environment. Such environment would be different in the body of a sedentary clerk, and of a hard-working agricultural labourer; and on this hypothesis the offspring in these cases would be different."

He misses the point at issue. It is not denied that changes in the germ's environment (*i.e.*, in the body of the parent) may result in modifications in the organism into which the germ subsequently proliferates; but it is strenuously denied that acquired modifications in the parent tend specially so to modify the germ as to cause the organism into which it subsequently proliferates to reproduce congenitally the modifications which the parent acquired. To take a case mentioned by Mr. Buckman: "Visits to the Inventions Exhibition by the mother during the period of gestation resulted in a child which, unlike the others, has shown, since infancy, a remarkable mechanical proclivity." So that Mr. Buckman contends that the slight cerebral change in the mother so affected the child, situated on the other side of the placenta and at the end of the long umbilical cord, that a much greater cerebral change resulted in it. The seeing of inventions by the mother rendered the child inventive. Surely this hypothesis, savouring as it does of the miraculous, is unnecessary when we remember how greatly twins or the individual members of a litter of puppies (which cannot inherit different acquired traits from their parents since the circumstances attending their genesis are precisely alike) may differ mentally and physically.

Several of my critics have declared that they are weary of the endless controversy as to the transmissibility or non-transmissibility of acquired traits. Not words but deeds are required, say they. Not arguments, but careful physiological experiments. They are difficult to please. It is impossible to imagine how physiological experiments can be devised more profound and convincing than that enormous series, conducted by the micro-organisms of disease, to which I have called their attention. Thus, for example, for uncounted generations, almost every individual in Europe, who has reached an age to propagate his species, has acquired, through illness and recovery, immunity to measles. Yet this acquired immunity, this profound constitutional change, that not merely affects this or that organ, but the whole body, has not in the slightest degree been transmitted. The European child is as liable to infection as were the children of his remote ancestors, or as are the children of Polynesians, who have only lately been afflicted with the disease. He differs, however, from the Polynesians, and presumably also from his remote ancestors, in that after infection he is more resistant to the disease, *i.e.*, in that he more easily attains immunity, and does not so readily perish. Mr. Buckman thinks that this evolution of resisting power "looks remarkably like inheritance of acquired characters." But, if acquired powers of resisting are transmissible, acquired weaknesses should also be transmissible. And, therefore, in the case of such diseases as tuberculosis and malaria (against which immunity cannot be acquired, and of which one attack predisposes to subsequent attacks), races, long afflicted by them, should be much less resistant than races that have had little or no experience of them. The contrary however is true, for the negro is more resistant to malaria, and much less resistant to tuberculosis than the Englishman. In this case, therefore, acquired immunity can have played no part.

Again, indulgence in certain narcotics increases the craving for them; and some of these narcotics (*e.g.*, alcohol and opium) are the cause of a vast number of deaths. If, then, acquired traits are transmissible, a race that has long used a powerful narcotic should crave more for excessive indulgence in it than a race that has had but a short acquaintance with it; on the other hand if acquired traits are not transmissible, then the elimination of individuals with a great craving would render the former race less liable to excessive indulgence than the latter. Now the evidence is overwhelming that races that have longest used a powerful narcotic are invariably those that are least inclined to excessive indulgence in it. Here then is another great series of physiological experiments proving, just as disease proves with regard to acquired physical traits, that acquired mental traits are not transmissible.

We must conclude, therefore, that since the great causes of elimination are not very numerous, man's present evolution cannot be very complex; and, therefore, since I have dealt with all the great causes of elimination, that I am justified in my claim that my work really does cover the whole ground.

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II.

The Lemurs as Ancestors of the Apes.

THE question whether the lemurs are the ancestors of the apes needs to be discussed again, as the result of the recent investigations of Hubrecht (1) and Leche (2) on the Tarsier (*Tarsius*) of the Indo-Malayan Archipelago. This little squirrel-like animal, as is well-known, has long been placed in zoological works in a rather isolated position among the lemurs, on account of its anomalous characters. Hubrecht now finds, from the study of the placental connection between the embryo and the parent, that this strange genus can no longer be regarded as a lemur, but must be placed among the apes in the higher division of the Primates. Leche, on the other hand, from a study of its teeth, considers that it is a lemur beyond all doubt.

I propose, therefore, as briefly as possible, to discuss the characters of the skeleton of *Tarsius*, to determine whether or not this existing genus can be considered as a synthetic type connecting the lemurs with the apes, and whether it truly shows that the two sub-orders of the Primates, as commonly understood, are derived from one and the same ancestral stock. The animal is, indeed, one of those generalised forms, preserving in its skeleton many characters found in its primitive ancestors of the Eocene period, as shown by an examination of the skull and teeth.

The skull of *Tarsius* very closely resembles that of *Anaptomorphus* from the North American Eocene, and of *Microchoerus* from the French Lower Tertiary Phosphorites. All these animals have a rather short and broad head, with much enlarged orbits, and huge auditory bullæ. In the skull of the two extinct genera, however, the rim of the orbit is incomplete below, while in *Tarsius* it is nearly closed here by a well-marked lamina of bone extending from the alisphenoid to the malar. The latter feature has not been observed in any other lemuroid, and more closely associates *Tarsius* with the apes.

The teeth of this animal are of a very primitive type, which is common in the lowermost Eocene strata. Like nearly all the Mammalia of the North American Puerco Formation (lowermost Eocene), it exhibits only three tubercles on the crown of its back teeth in the upper jaw, while its lower molars bear a raised triangle of three cusps in front with a low heel behind ("tuberculo-sectorial" type of Cope). The front teeth are peculiarly modified, and in respect to these *Tarsius* resembles the Insectivora much more than any existing lemur. Whereas the upper canines of the true lemurs are large, those of *Tarsius* are comparatively small, while its upper median incisors are much enlarged and upright in position. Leche considers that this arrangement is primitive, since he finds that in the milk-dentition of the Eocene lemuroid *Adapis* the canines are much smaller than their permanent successors and more closely resemble the anterior premolars.

The latter conclusion, however, is not supported by the palæontological evidence when we examine it closely. According to our present knowledge of the fossil Primates, two well-marked series of supposed lemuroids were already differentiated as early as the beginning of the Eocene period (Puerco stage). In one series, that of the Mixodectidæ, the lower median incisors were larger than the adjoining teeth, and there were only three premolars. In the second series, that of the Chriacidæ, the canines were larger than the incisors and normal in form, while there were four premolars.

Now, *Tarsius* is similar to the Eocene Mixodectidæ in exhibiting one pair of anterior lower teeth enlarged, though in this case it happens to be the second pair instead of the first. One of the essential characters of the animal may thus be traced back to its ancestors at the beginning of the Tertiary period. But notwithstanding the specialisation of the front teeth in *Tarsius*, the molars are of a generalised type from which those both of the lemurs and the apes might have been derived; and it is interesting to add that the teeth of the American Eocene *Anaptomorphus*, so far as known, are very much of the same character. It must be remembered, however, that the number of premolars in *Anaptomorphus* is not yet satisfactorily known, and this is an important point still to be settled; though it seems probable there were three of these teeth as in *Tarsius*.

The best known of the extinct lemurs are the genera *Adapis* and *Microchoerus* from the upper Eocene of Europe; and here we meet with two widely differentiated types, the second closely related to the American Mixodectidæ. *Adapis* is evidently very similar to the modern lemurs, only differing from them in the normal form of its canines and incisors. The actual pattern of its molar teeth differs much less from that of the recent lemurs, for example, than does that of *Mycetes* from the American monkeys generally. The peculiar proclivous position of the incisors and canines of modern lemurs is a character probably of late acquisition, as it appears probable that *Megaladapis* of the late Tertiary of Madagåscar had the canines and incisors of a normal form. The shape of the jaw symphysis in *Megaladapis* leads me to this conclusion. The ancestral form, then, of all the lemurs was probably provided with large upright canines and normal incisors. In some of its cranial characters, *Adapis* more closely resembles the apes than the lemurs. I refer to the nearer approach of the lachrymal foramen to the orbit than in the typical lemurs; and then again the jaw symphysis is strongly anchylosed and the angle rounded as in the apes. These characters, with the large canines of *Adapis magnus*, give the skull of this species quite an ape-like appearance. In general, the genus *Adapis* might be considered as ancestral to some of the recent lemurs, as the complex character of the last premolar of *Adapis* is found in the recent genera, *Hapalemur* and *Galago*.

Turning to the characters of the skeleton of *Tarsius*, we find them nearly all essentially those of the lemurs. As in the latter, the fourth digit of the pes is longer than the others, and the index and middle digits of the hind feet are furnished with claws. Again, the calcaneal and navicular bones of the tarsus are elongated as in *Galago* and *Cheirogaleus*. It is interesting to note, and this goes hand in hand with the other primitive characters of *Tarsius*, that there is one more claw on the hind foot of *Tarsius* than in other lemurs; and I believe that this point is an important one in its bearing on the phylogeny of the Lemuroidea.

The view has been advanced that the lemurs were descended from condylarthrous ancestors. Now, by Professor Cope's latest definition (3) of the Condylarthra, they were all hoofed quadrupeds, and to my mind it would be impossible to derive the claws and nails of the lemurs from any hoof-like type. I am convinced from a study of the terminal phalanges of the lemurs, that these have been developed from claws and not hoofs. Nails arise by the distal expansion of claws, and this is proven in the case of the lemurs by tracing this development in the individual digits of living lemurs.

There are a number of forms included in the Condylarthra, such as *Periptychus*, which are very doubtful, and excluding these uncertain condylarths, we are confined for our comparison to the type genus *Phenacodus*. If we compare the structure of *Phenacodus* with that of *Adapis*, the best known of the fossil lemurs, we find little in common between them. *Phenacodus* is a typical ungulate closely related to the perissodactyle division of that order. On the other hand *Adapis* in its skeletal structure is in general different from *Phenacodus*, and this applies as well to the details of its cranial anatomy as to the structure of the limbs. If we take into consideration the difference in structure between the lemurs and condylarths, I am sure that the idea of deriving the former from the latter is untenable. The serial arrangement of the carpus and tarsus in the condylarths and lemurs, and the same number of teeth in the primitive extinct lemurs as in the condylarths, can be better explained, it appears to me, as characters common to the ancestors of the Mammalia in general, than evidence that the lemurs and condylarths are closely related phylogenetically.

Having attempted to show that the Condylarthra cannot be considered as ancestral to the Lemuroidea, it remains now to discover to which group of the Mammalia they are related. I have endeavored to prove that it is probably not correct to derive the lemurs from any of the forms bearing unguliform terminal phalanges, and that the Unguiculata is the group from which the lemurs have arisen. The Insectivora are by general consent acknowledged to be the most primitive group of the Mammalia, and it is in some generalised member of this order now extinct, that we must look for the ancestor of the Primates.

Now the placentation of *Tarsius*, as shown by Hubrecht, closely resembles that of the Insectivora, and both in the latter order and in the true Primates the placental area is very limited in extent, and thus differs widely from that of the lemurs, where the placenta is of the diffuse type. In man the chorion extends in an early stage of development around the whole embryo and is entirely villous. This represents to a certain extent the diffuse stage of the placenta of the lemurs. At any rate, on the criterion of the placenta, I see no objection to the view that the lemurs are related to the apes. The diffuse form of placenta of the lemurs has probably arisen from a non-deciduate restricted type, which would be also the ancestral form of placenta of the Anthropoidea. This is the view of the evolution of the placenta in the Primates as stated by Balfour (4).

In conclusion, the decidedly mixed character of *Tarsius*, relating it on one hand to the apes and on the other by so many characters to the lemurs, leads to the conclusion, that this genus approaches structurally the common ancestral type from which apes and lemurs have descended. If we do not grant this, then we are forced to the conclusion that the type of placenta found in *Tarsius*, the straight colon, and the closure behind and below of the orbit are all cases of parallelism, and that *Tarsius* is not genetically related to the apes. I am aware of the fact that it is quite the fashion of late to push the theory of parallelism to the extreme, with the result that we find few types that fit exactly in our phylogenetic series. Too much of this sort of thing would rather bear against the theory of evolution, as we would have innumerable parallel series, but few tending to convergence. I believe the characters of *Tarsius* that are like those of the Anthropoidea to be essential, and not adaptive. They prove that *Tarsius* is a true connecting link, genetically related to both lemurs and apes.

On palæontological grounds I cannot accept Leche's dictum as to the significance of the milk dentition in regard to the evolution of the lemurs, and it appears most probable that the ancestral lemur was a small insectivore-like animal, somewhat similar to *Tupaia*, an arboreal type, which had canines of the normal upright form, and larger than the incisors. In many of its characters this hypothetical ancestral type would more closely resemble *Tarsius* than any other living primate. I fully realise the fact that *Tarsius* is quite specialised in some respects; but as a whole it is decidedly primitive.

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III.

The Museums of Public Schools.[1]

III.—RUGBY.

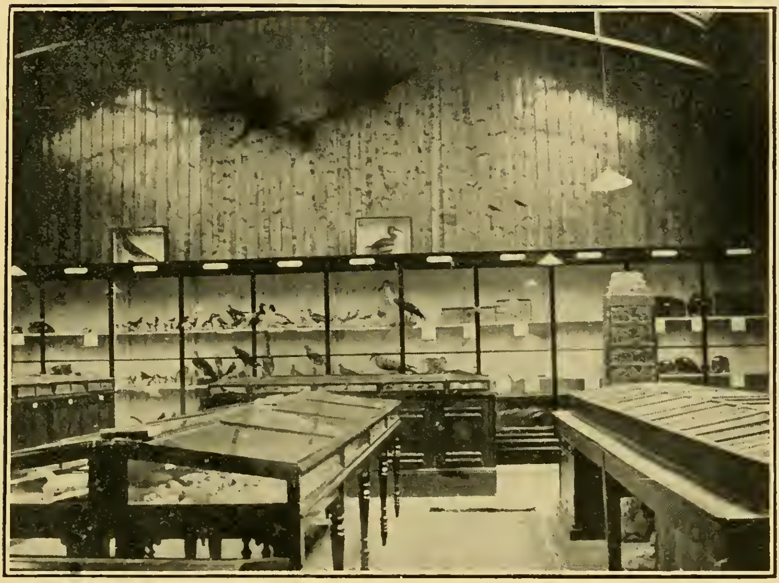
FOR the first beginning of a collection of natural history specimens in connection with Rugby School, we must go back 50 to 60 years. It appears that Dr. Arnold, the great Rugby headmaster, requested the boys of the school to bring back with them specimens of the common stones and fossils of their respective neighbourhoods. The boys answered this appeal with great alacrity, and a large heap of stones was the result. No attempt seems to have been made to label the specimens in any way, until Mr. J. M. (now Archdeacon) Wilson, on joining the staff of masters in 1859, took them in hand and examined them. A great many had been thrown away before that date, but there remained a very large number of specimens, most of them perfectly worthless, but a few score of some interest, and a few really valuable.

Mr. Wilson with the help of successive generations of pupils collected vigorously in the various brick-fields and lime-stone pits in the neighbourhood, and got together a good representative collection of the fossils of the local Lias.

A little before 1859 one of the masters, Mr. C. T. Arnold, presented to the school a small collection of fossils, British and foreign, embracing types of the fossils found in the successive geological formations; to these were added a few local fossils, presented, it is believed, by the late Dr. Highton, then a master in the school.

These collections were, in 1859, all placed in a single case in the Arnold Library, the first School Library, built in memory of Dr. Arnold. To this collection Mr. Wilson added very largely, by collecting in the neighbourhood of Rugby and by gifts of series of specimens obtained elsewhere, including some from the Silurian, Carboniferous Limestone, Chalk, Greensand, Gault, and Eocene, as well as a collection of serpentines and granites from Cornwall. The collection soon outgrew its original case and filled the bookshelves of the adjoining section of the library; the table-case being then, and ever since, devoted to the strictly local collection.

[1] Previous museums dealt with in this series were: Charterhouse, vol. iii., p. 40, July, 1893; and Eton College, vol. vi., p. 201, March, 1894.



LOOKING TOWARDS THE SOUTH-EAST.

LOOKING SOUTHWARDS.

RUGBY SCHOOL NATURAL HISTORY MUSEUM.

In 1867 the Rugby School Natural History Society was formed under the presidency of Mr. F. E. Kitchener (late headmaster of the School at Newcastle-under-Lyne) who took charge of the botany, and began a botanical collection for the school. At the same time Mr. A. Sidgwick took up the entomology, and a collection of British Lepidoptera was commenced. Members of the school added to these, while collections of birds' eggs and fresh-water shells were formed by boys interested in them. These collections in the seventies were housed in a small room in the then new block of buildings abutting on Lawrence Sheriffe Street, while the geological collection, still under charge of Mr. Wilson, remained in the Arnold Library. These collections all grew apace by gifts from the boys and by gifts of specimens of all sorts, found in every quarter of the globe, from former members of the school.

During the headmastership of Dr. Jex Blake, the present Dean of Wells, a new Reading Room with Library accommodation was built, in memory of Dr. Temple (formerly headmaster, now Archbishop of Canterbury); a large number of books were removed to the Temple Reading Room from the Arnold Library, in which were left only a large collection of editions of the Classics and Divines not likely to be much consulted by the general mass of the school. The space thus set free was handed over to the Natural History Society, to which it proved somewhat of a white elephant, as no funds were provided for dealing with it, and the condition was made that the existing book-cases and furniture of the room should not be disturbed. However, boys and masters worked with a will, and the collections were transferred from their old narrow quarters and disposed on the shelves and floor-space assigned to them. This room, however, was never well adapted to the purposes of a museum, the book-cases being mostly ill-lighted and altogether too lofty to show to any advantage the objects placed upon them.

In 1892 increased accommodation for school purposes became a paramount necessity and Dr. Percival (then headmaster, now Bishop of Hereford) determined to use a portion of the Arnold Library for teaching the Sixth Form. At the same time he planned a block of temporary buildings, embracing accommodation for teaching physical science and music, together with a Natural History Museum. This block, occupying a portion of the headmaster's kitchen garden on the Hillmorton Road, was completed in the summer of 1894, and the collections belonging to the Natural History Society were immediately moved into it. This room, of which we give two views from photographs taken by members of the school (Plate III) has a floor-space of almost an exact square of 45 feet, and is entirely lighted from above. It is also ventilated by an open ridge extending through the whole length of the buildings almost exactly north and south. The heating is by hot water pipes running in gullies under the floor, the whole floor and wall space thus being made available for cases and book-shelves.

The southern end of the room is occupied entirely by a wall-case devoted to the zoology of the Vertebrata, designed and arranged by Mr. Collinge, of Mason College, Birmingham, and surmounted by a pair of antlers of the Irish elk found in County Tipperary in 1845 and presented to the school by the late Rev. J. P. Rhoades. These antlers were at that time the largest and finest which had been found in Ireland, but larger ones have since been found.

Along the east wall are book-shelves filled with books devoted to natural history. This library has been gradually and carefully selected, and is now fairly representative of the more popular side of the various branches of the subject. The rest of the wall-space is occupied by cases and cabinets. One case contains an admirably stuffed specimen of a badger, shot some years ago in the neighbourhood of Rugby, and set up with a model of its natural run. Another case is intended for a crystallographic collection in course of arrangement by a member of the school.

The north wall is similarly occupied by the entomological and other cabinets. On this wall is a case containing numerous specimens of exotic butterflies. The centre of the wall-space is occupied by the fossil remains of one of the large extinct New Zealand birds. On the same side stands a mummy, whose history is unknown to the present writer, and on whose genuineness many aspersions had been cast, until a photograph taken through the wrappings by means of the X Rays revealed the bones *in situ*.

The entrance door is in the north-west corner and the western wall is occupied by the cabinet containing a herbarium and two cases containing native Samoan clothing, presented by the Rev. S. H. Whitmee, a former missionary. There is also one of the "Magic Mirrors" of Japan, presented by Professor Sylvanus Thompson.

The floor-space is occupied by cases. One extending three-quarters of the length of the room contains the collection of local fossils whose history has been sketched above. Parallel to this, in the centre of the room, are two cases devoted to a purely educational geological collection, partly arranged by Mr. Collinge; while on the opposite side are a series of cupboards quite unarranged and containing the rest of the geological specimens. On the top of these cupboards is a table-case containing a type-series of Invertebrata, also arranged by Mr. Collinge. Crossing these on the south side is another series of cupboards, brought, like the others, from the Arnold Library, with a table-case on the top containing polished agates and other stones. Corresponding to this on the north side is another table-case of mahogany containing a series of minerals, most of which have been selected from the old dusty collections, arranged, and skilfully labelled by a present member of the school. These now form an excellent representative series of all the more common, and some rare, minerals, arranged on the basis of their chemical composition. Near this, in the corner of the room opposite the door, is a model of the neighbourhood of Rugby extending four miles in all directions, on a horizontal scale of 4 inches to the mile and a vertical scale of 1 in. to 120 ft. This was originally projected by Mr. Wilson with a view to exhibiting some of the geological features of the district. The ground was surveyed by him and some of his pupils, and a plaster cast made, which for several years remained in the school-room occupied by him. When the natural history collections were removed to the Arnold Library space was found for the model, which, by the help of Mr. G. M. Seabroke (himself an old pupil of Mr. Wilson) and others, was worked over inch by inch and compared with the ordnance survey maps by means of proportional compasses, until it is now believed to represent with fair accuracy all the surface features of the district it embraces. It has also been painted, roads, streams, railways, canals being marked upon it and the sites of the villages shown.

Among various portraits on the walls is one of the late M. H. Bloxam, F.S.A., himself an old Rugbeian and all his life a devoted friend of the Rugby School Natural History Society. During his lifetime he presented to the school many treasures from his own valuable collection of books, pictures, and antiquities, bequeathing by his will many more. Although they find a more fitting home in the Art Museum, any notice of the Natural History Museum without mention of his name seems sadly incomplete to one who has watched the growth of the Society during the last twenty years.

Such are the chief features of the collections exhibited in the new Museum. Of course much remains to be done, but those who knew the collections as they were planted down in this Museum less than 30 months ago will admit that excellent progress has been made, and all who are interested in the development of a taste for natural history will join the present writer in the earnest hope that the succeeding generations of Rugby boys will not fall behind their predecessors in devoting part of their spare time to improving the various collections in this or that direction.

The president of the Rugby School Natural History Society, the Rev. R. Waterfield, has kindly permitted the use of the blocks for the two illustrations, which appeared originally in the Society's report for 1895. The writer is indebted also to Archdeacon Wilson for supplying him with information concerning the early history of the collections.

L. Cumming.

IV.

The Suprarenal Bodies of Fishes.

DURING the past ten or twelve years a voluminous literature has appeared dealing with the structure and physiology of the suprarenal bodies of mammals; but it is only within the last few years that the attention of zoologists and physiologists has been directed to these structures in the lower vertebrates. The results of these investigations having proved of more than usual interest, a brief summary of the same may perhaps be useful.

The suprarenal bodies of mammals are situated one on each side of the body in close proximity to the kidney. Each consists of a 'medulla' or central portion and a 'cortex' or external portion, and is supposed to be a double internal secreting gland whose presence and functional activity is essential to life; for so far as is known at present from the researches of Abelous and Langlois, Tizzoni, Brown-Séquard, Cybulski, Schäfer and others, all animals die when these bodies are extirpated. In this connection, it is curious to note that suprarenal bodies have not been described in any but vertebrate animals.

I do not purpose entering in any detail into the question of the physiology of these bodies, a brief and useful summary having recently been given by Vincent (23).

Commencing with the Cyclostomata, the earliest reference to the subject is that by Rathke in 1827 (16) and again in 1828 (17). In 1834 Johannes Müller described a clustered gland in *Myxine*, which, however, seems to have been the pronephric portion of the kidney. In a recent paper (written in conjunction with Mr. Vincent) (4) I have reviewed in some detail the literature relating to these bodies in the cyclostomes, and have shown that as yet there is no satisfactory evidence of any suprarenal bodies in this group. Since the publication of the above paper, Pettit (15) has stated that he has found glandular structures which he thinks may be regarded as suprarenal bodies, but further evidence is yet wanting.

The earliest account respecting these bodies in Fishes is that given in 1819 by Retzius (18), who described them in certain species of dog-fish and skate. Stannius in 1839 (20) discovered similar bodies in teleostean fishes, and in 1846 (21) he gave a general account of them in elasmobranchs, teleosts, and the sturgeon. In the same year Ecker (7) verified Stannius' observations and gave a description of the minute anatomy of the bodies. Hyrtl 1851 (10) and Leydig (11) contributed further papers to the subject, the former dealing with teleosts, and the latter with *Chimæra.*

In 1852 Frey (8) published a general *resumé* of what was known of these bodies, in Todd's Cyclopædia. Leydig in 1852 (12) pointed out their segmental arrangement; and a further contribution was made by Stannius. In 1875 Semper (19), in a very valuable paper, emphasised the importance of the segmental arrangement. F. M. Balfour in 1878 (1) gave an account of the morphology and development of the suprarenal bodies in elasmobranchs in his well known "Monograph." It is to this writer that we owe the term 'interrenal.' A description of the suprarenals of *Amiurus* was published by M'Kenzie in 1884 (14).

Coming now to more recent work and better methods of investigation, we find a series of papers by Fusari (9), Chevrel (2 and 3), Diamare (5–6), Pettit (15), and Vincent (22–25). The following short account of the suprarenal bodies is based upon the observations of the above five writers; and I am especially indebted to Mr. Vincent for his kindness in allowing me to read through a paged proof of his larger paper (26) as well as for copies of his other writings on the subject.

In the elasmobranchs the term 'suprarenal' has been applied to two totally distinct sets of organs, viz.:—(i.) the *interrenal bodies* of Balfour, which are somewhat rod-shaped bodies, sometimes paired, in which case they are situated on the internal dorsal backs of each kidney, and sometimes unpaired, lying in the median line on the posterior region of the kidney; and (ii.) the segmental series of bodies arranged in pairs on each side of the dorsal aorta, and in close relationship to the sympathetic nervous system, the *suprarenal bodies.*

The Interrenal body.—Among elasmobranchs this body is unpaired in the sharks, but paired in the rays. In the former group it is largest posteriorly, tapering anteriorly in the form of an ochre-yellow strip; sometimes there are *accessory interrenals* in the form of minute dots of similar substance. In the latter group these bodies are never quite symmetrical. Semper was of opinion that a connection existed between the interrenal and suprarenal bodies, but later investigations have shown that such a view was erroneous.

The Segmental Suprarenal bodies.—Chevrel (2) has figured and described the relations of these bodies with the sympathetic nervous system. In the elasmobranchs they are arranged in pairs in a more or less definite manner, from the Cuvierian duct to the posterior end of the kidney. The first pair are always the largest; posteriorly they become smaller and fragmentary. In the sturgeon they are similar in many respects to those found in elasmobranchs, only they are most plentiful in the anterior portion of the kidney. The sturgeon has only the cortex present, *i.e.*, the suprarenals here are equal to the interrenals of elasmobranchs and the known suprarenals (bodies of

Stannius) in teleosts. In the Teleostei the cortex only is present. The bodies are much fewer in number, usually two rounded or oval-shaped bodies of a pale pink colour, situated on the dorsal or ventral surface, usually in the posterior region, of the kidney. Sometimes they are imbedded in the substance of the kidney. Diamare (6), Pettit (15) and Vincent (25) have all described the suprarenal bodies of teleosts as usually being two in number. Writing of these organs in the Gadidæ, Mr. Vincent says (25, p. 58), "there is a considerable variation in number and size of the suprarenals, although their shape is nearly always rounded. As for number, two must be regarded as the rule, but there is even more variation in this respect than in the Pleuronectidæ. Thus, in a specimen of *Gadus morrhua* I have found as many as five; in *G. merlangus* I have found only one. I have occasionally found one also in *Molva vulgaris*. In *Merluccius vulgaris* I have found in one case as many as five; in this case the suprarenal of the left side was represented by four small bodies instead of one larger one. Their position is usually one on each side, but, as in other families, the right and left are rarely on the same level, one being usually anterior or posterior to the other. They also vary as to their relation to the middle line, some being more lateral, others more central. The suprarenals in this family are almost always visible on the ventral surface of the kidney, though they may (either one or both) be lateralised, or even exceptionally may be partially on the spinal surface. They are usually more or less imbedded in the kidney substance."

Similar statements to the above are to be found in the writings of other workers. I have merely quoted Mr. Vincent as being the latest.

In a dissection of *Gadus morrhua* now before me, I find in addition to the bodies mentioned above, numerous small bodies, varying greatly in size scattered throughout the substance of the kidney. I have isolated thirteen from the posterior region. Whether or not similar bodies are present in the substance of the kidney of other Teleosts I cannot now say; but the point is one deserving special attention from workers on this subject, as from their naked eye appearance these structures seem to be small suprarenal bodies, and might be termed *accessory suprarenals*.

In the Dipnoi, Pettit (15) has described the suprarenals, but he does not say whether the cortex and medulla are present. Vincent (22), from *à priori* considerations, had previously pointed out that suprarenals were "almost certainly present."

Thus, from the researches of Fusari, Diamare, Pettit, and Vincent we may draw the following conclusions:—

1. That suprarenal bodies are present in all fishes.
2. That the *interrenal bodies* of elasmobranchs correspond to the suprarenal bodies of teleosts, and to the cortex of the suprarenal bodies of higher vertebrates.

3. The *segmental suprarenal bodies* of elasmobranchs appear to be wanting in other orders of fishes (with the possible exception of Dipnoi), and correspond to the medulla of the suprarenal bodies in higher vertebrates.

From recent experiments made by Vincent (23–24) it would appear that these really correspond, physiologically as well as structurally. In teleosts the known suprarenal bodies appear to consist entirely of cortical substance, since they have no physiological action upon blood pressure, such as we know in connection with medulla ; but that they are active secreting glands has been shown by Pettit (15). The same condition appears to obtain in ganoids.

In all higher vertebrates both portions are present, but combined in one organ, and it seems very probable that each part fulfils its own function, that of the medulla being to pour into the blood some material or materials which maintain the normal tone of the muscles throughout the body, while that of the cortex is yet unknown. Vincent (23, p. 6) has suggested that the function of the cortex is to manufacture and pour into the blood some substance which possesses the power of getting rid of pigments. He points out that in Addison's disease, "when this part of the gland is damaged the necessary secretion does not take place, and bronzing of the skin is the result."

One might form two conjectures concerning the function of the cortex :—(i) that it is in some way an accessory to the medulla, *e.g.*, that it prepares the material for final elaboration in the medulla ; or (ii) that it is a totally distinct and independent gland, and its association with the medulla is merely accidental. From a consideration of the suprarenal bodies in the lower vertebrates, this latter view is the one I personally take ; and I understand from Mr. Vincent that he also is inclined to agree with it.

The following table from my paper in the *Anatomischer Anzeiger* briefly summarises the distribution of the cortex and medulla in the Cyclostomata and Pisces as known at present.

	Suprarenals.	
	Cortex.	Medulla.
Cyclostomata	Possibly degenerate.	
Elasmobranchii ..	Interrenal	Paired suprarenals
Holocephala	do.	do.
Ganoidei	Suprarenals in kidney ..	Apparently absent
Teleostei	Suprarenals on kidney ..	do.
Dipnoi	No suprarenals as yet described. Perirenal large-celled adenoid tissue ? (Vincent, 22). Certain bodies described by Pettit (15),	

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V.

Numerical Variation of Parts in Ranunculus repens.

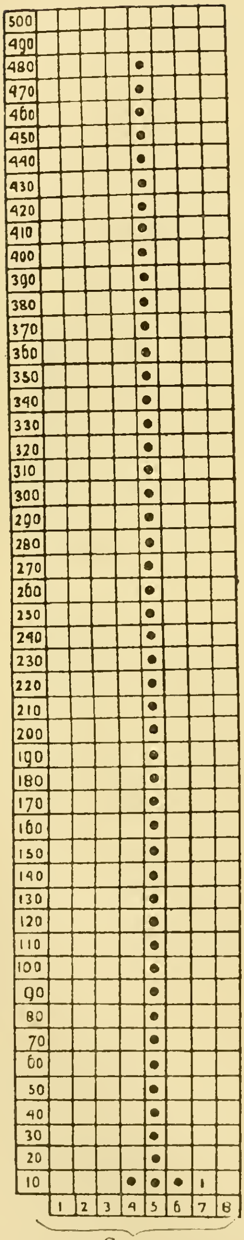
THE following paper was submitted to us by a friend, not for publication, but as an example of the kind of work that can be done by one having only an elementary knowledge, but with his energies turned into profitable channels. Considering the importance now attached to the study of variation, none will deny that this account of actual facts is a useful, though a small, contribution. The writer is one who has but little time at his own disposal, and the most limited facilities for the study of either natural history or science. We have therefore obtained permission to publish this first essay of his as an encouragement to others in a similar position. It would not, perhaps, be altogether waste of time if series of *Ranunculus repens* from other parts of the country were examined in the same way, and the results compared with those of Mr. Pledge. At all events, both in zoology and botany, there are abundant opportunities for those who are not satisfied to remain "mere collectors."—ED. NAT. SCI.

The 500 specimens examined were kindly supplied by Miss K. M. Hall, curator of Whitechapel Public Museum, since my occupation prevented me from collecting them myself. They were all obtained from one locality. It may be interesting to note that the sum-total of all the parts counted, viz., the sepals, petals, stamens, and carpels of 500 specimens, amounted to 50,642, averaging over 100 pieces to each specimen tabulated.

The method employed in the investigation was as follows:—Four separate sheets of square-ruled paper were used, one sheet for sepals, one for petals, and one each for stamens and carpels. Each specimen done was numbered, the number being the same on all four sheets, thus rendering it easy to ascertain the particulars of the variation in each whcrl of any given specimen.

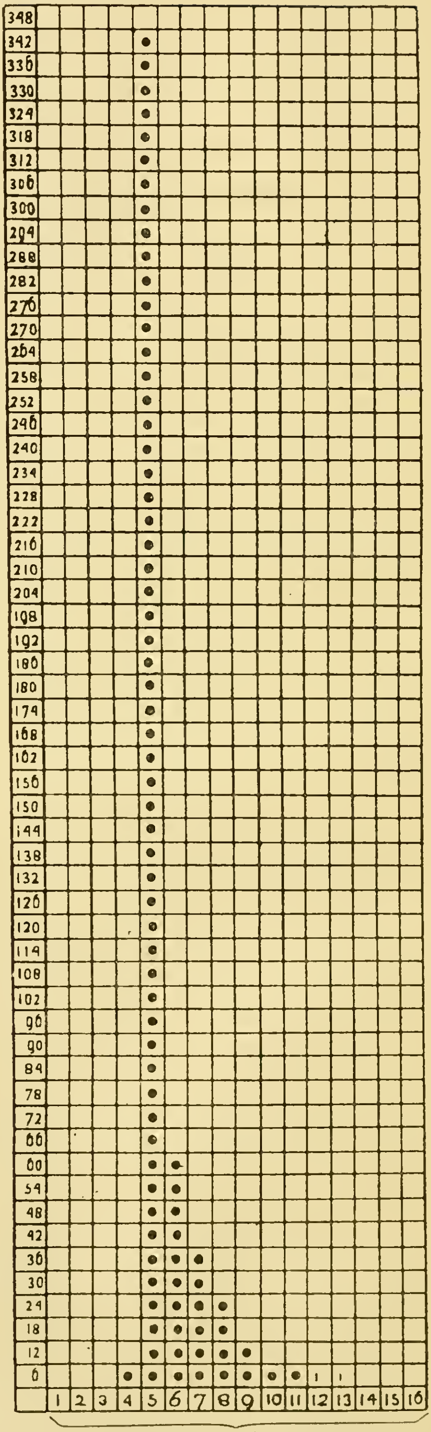
The floral formula, calculated from the data obtained, is:—Sepals, 5·004; Petals, 5·672; Stamens, 54·534; Carpels, 36·074. Taking it as 5, 5, 54, and 36 respectively, not one specimen in the whole 500 conforms to it. If taken as 5, 6, 55, and 36, however, there is one specimen (No. 433) in agreement. So that practically the formula thus obtained is purely a mathematical conception and has no real existence.

Sepals.—Fig. 1 shows the variation in the Calyx, each dot representing 10 individuals. The actual numbers are:—



SEPALS.

FIG. 1.—Variation in Sepals of 500 specimens.



NUMBER OF PETALS.

FIG. 2.—Variation in Petals of 500 specimens.

Specimens with 4 sepals	.. ..	9
,, ,, 5 ,,	.. ..	481
,, ,, 6 ,,	.. ..	9
,, ,, 7 ,,	.. ..	1

In the nine cases with four sepals there was a re-adjustment of symmetry, the sepals being arranged at right angles to each other.

Appended is an analysis and summary of the 19 cases of variation, from which one sees that in the cases with four sepals the means of petals and stamens are above the average, and that of the carpels somewhat below. In the instances of six sepals all three means are considerably above the average; and, finally, the means of all the four variations are also all above the average. It appears, therefore, that each whorl varies independently.

ANALYSIS OF 19 VARIATIONS IN THE CALYX.

	With 4 sepals.			With 6 sepals.		
	10 petals.	64 stamens.	45 carpels.	6 petals.	61 stamens.	49 carpels.
	7 ,,	62 ,,	43 ,,	8 ,,	63 ,,	46 ,,
	6 ,,	49 ,,	27 ,,	5 ,,	71 ,,	43 ,,
	6 ,,	50 ,,	39 ,,	6 ,,	68 ,,	38 ,,
	5 ,,	62 ,,	29 ,,	5 ,,	63 ,,	39 ,,
	5 ,,	45 ,,	42 ,,	6 ,,	55 ,,	34 ,,
	6 ,,	36 ,,	25 ,,	11 ,,	48 ,,	42 ,,
	5 ,,	39 ,,	25 ,,	10 ,,	43 ,,	37 ,,
	5 ,,	58 ,,	25 ,,	8 ,,	41 ,,	27 ,,
	55	465	300	65	513	355
Mean	6·1	51·6	33·3	7·2	57·0	39·4

With 7 sepals:—5 petals, 64 stamens, 45 carpels.

Petals.—(Fig. 2). In this diagram each dot represents six specimens, the actual numbers being:—

Specimens with 4 petals	.. ..	7
,, ,, 5 ,,	.. ..	345
,, ,, 6 ,,	.. ..	60
,, ,, 7 ,,	.. ..	36
,, ,, 8 ,,	.. ..	24
,, ,, 9 ,,	.. ..	13
,, ,, 10 ,,	.. ..	6
,, ,, 11 ,,	.. ..	7
,, ,, 12 ,,	.. ..	1
,, ,, 13 ,,	.. ..	1

In the case of a batch of 34 specimens (Nos. 337–370 inclusive) analysed on p. 328, the mean number of petals is as high as 7·8, the mean of their sepals being 5·0, of stamens 44·3, and carpels 29·3. In this case the petal whorl (corolla) appears to have increased at the expense of the stamen and carpel whorls. Such exchange is apparently not general, at least to any marked extent, but the number of members of any given whorl varies, as previously stated, independently of the others.

Stamens.—(Fig. 3). The chief point to note in this diagram is the fair regularity of the curve, the recorded maximum, 55, closely agreeing with the calculated mean, 54·534.

Carpels.—(Fig. 4). Among other points of interest in this diagram, the first to be observed is the extreme irregularity of the

Fig. 3.—Variations in Stamens of 500 specimens of *Ranunculus repens*.

Each dot represents
one individual.

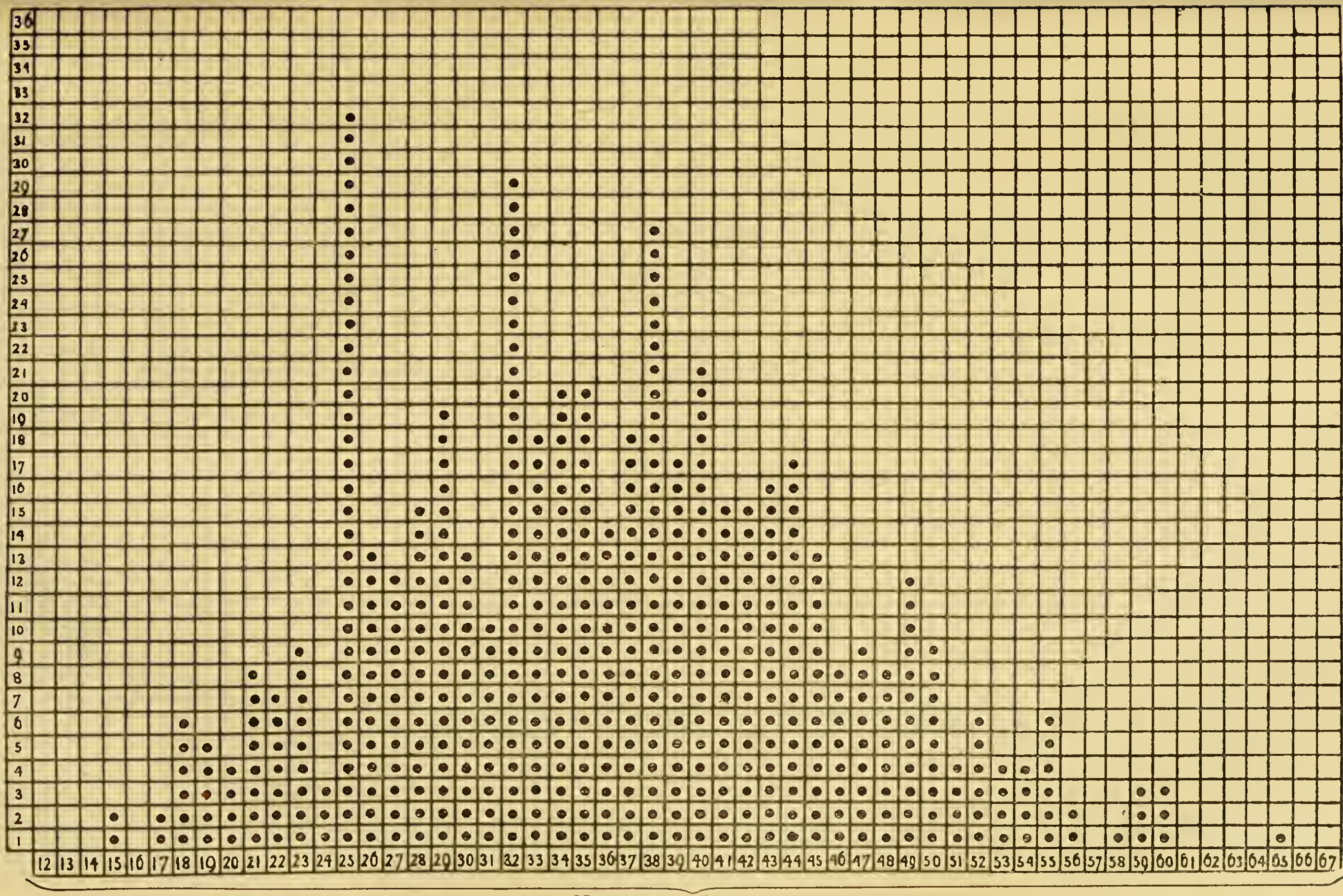


FIG. 4.—Variations in Carpels of 500 specimens of *Ranunculus repens*.

curve, in strong contrast to the last. The maximum is at 25 and the calculated mean 36·074. There is, however, a secondary maximum at 32, and a tertiary one at 38. If from the diagram of the stamens (Fig. 3) there are deducted those dots whose numbers have, in Fig. 4, made the curve imperfect, that is to say, the last or highest 21 of the 25 carpels column, the last 2 of the 29 column, and the last 10 of the 32 column, then the stamen curve remains practically the same. The deductions are fairly distributed on each side of the maximum and not localized at all, showing that here also the whorls vary independently.

Analysis of Certain Specimens.

Number.	Sepals.	Petals.	Stamens.	Carpels.
337	5	11	42	45
338	5	11	44	39
339	5	13	42	38
340	5	10	50	42
341	5	8	38	27
342	5	6	45	21
343	5	9	41	40
344	5	5	49	31
345	5	5	55	31
346	5	6	41	20
347	5	10	34	29
348	5	9	42	26
349	5	11	43	43
350	5	11	39	37
351	5	5	61	20
352	6	10	43	37
353	5	5	54	26
354	5	10	43	32
355	5	5	51	26
356	5	5	42	22
357	5	5	50	23
258	5	5	49	15
359	5	8	41	26
360	5	8	47	36
361	5	5	49	29
362	5	5	48	20
363	5	5	43	25
364	5	12	38	32
365	5	11	44	41
366	6	8	41	27
367	5	8	21	26
368	5	8	41	26
369	5	7	53	25
370	5	6	45	45
Mean	5·0	7·8	44·3	29·3

There being so few members of the calyx and corolla, I would suggest that, if there were more, there would be greater variation; that is, given a greater range of variation, the chances would be against such a high maximum occurring then as now.

As regards the irregularities in the curves of the stamens and carpels, is it probable, that, if a much greater number of specimens (say 10,000) were examined, these curves would become more regular; or should these figures be taken as fairly representative?

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SOME NEW BOOKS.

Moths and their Classification.

The Lepidoptera of the British Islands. By Charles G. Barrett. Vol. iii. Heterocera, Bombyces, Noctuæ. 8vo. Pp. 396. London: L. Reeve & Co., 1896. Price 12s. (Large paper edition with coloured plates. Price, in parts, £3).

A Handbook to the Order Lepidoptera. By W. F. Kirby. Vol. iii. Pp. xxvii., 308, 26 coloured plates and many figures in text. Vol. iv. Pp. 246, 31 plates. 8vo. Allen's Naturalists' Library. London: W. H. Allen & Co., 1897. Price 6s. each.

Monograph of the Bombycine Moths of America North of Mexico. Part ii. Family I.—Notodontidæ. By Alfred S. Packard. 4to. Pp. 292, 49 pls., 9 maps, and 88 figs. in text. Philadelphia: National Academy of Sciences, vol. vii., first memoir, 1895. Received March 13, 1897.

Two years elapsed between the notices in Natural Science of the first and second volumes of Mr. Barrett's work. It is satisfactory to be able to review the third volume seventeen months only after the second. The author has now given us the remaining families of the "Bombyces"—the "Bombycidæ," Endromidæ, Saturniidæ, Drepanulidæ, and Notodontidæ; and the first instalment of the "Noctuæ"—including the Cymatophoridæ and four genera of the trifid Noctuidæ.

In commenting on Mr. Barrett's second volume, we expressed regret that he had adhered to that old classification of British moths which has been so largely amended by modern workers. In the present volume it will be noticed that he adheres to Stainton's plan of classing the Cymatophoridæ with the Noctuidæ on account of "the structure and usefulness of the tongue, their love of sweet substances as food and their crepuscular flight." These, however, are adaptive characters which cannot be allowed much weight as indications of true affinity. In the more recent classification of Staudinger the Cymatophoridæ are placed among the "Bombyces"; but this group must now be considered quite untenable as a natural assemblage of families. It might possibly be better to abandon all divisions of the Lepidoptera between the order and the family, and we should then see the Cymatophoridæ in their natural position between the Notodontidæ and Noctuidæ. The genus *Asteroscopus* or *Petasia*, which is now usually regarded as noctuid, is retained by Mr. Barrett among the notodonts.

Mr. Barrett keeps the name "Bombycidæ" for the first family dealt with in this volume, although he rightly uses *Lasiocampa* for the genus of eggar moths, which many British entomologists erroneously call "*Bombyx*." The latter name belongs properly to the common silkworm; the true Bombycidæ are unrepresented in the British fauna, and the eggar family should be known as Lasiocampidæ.

Mr. Barrett's description of our commonest *Lasiocampa—L. quercus*—is of exceptional interest. He does not consider that the small, dark, northern race *L. callunæ* has any claim to be considered a distinct species, pointing out, among other facts, that in Wales the males of *L. callunæ* pair with females referable to typical *L. quercus*. There are, however, instances of nearly allied forms, which, though they interbreed where their ranges overlap, are considered good species, *e.g.*, the carrion and hooded crows. Mr. Barrett gives a lively account of the "assembling" of male moths of *L. quercus* around a captive female, "some flying steadily to the spot and fluttering or running all over the place in which the female is hidden . . . or sweeping backwards and forwards in wide beats; while others go quite frantic, dash themselves on the ground and spin on their heads." There is also a vivid description of a flock of males trying to cross the ridge of Hindhead in the teeth of a strong gale. Mr. Barrett watched the moths work their way up a sheltered hollow to the summit, only to be "instantly carried back over the valley, and so tumbled ignominously down, to renew the strife with the same ill-success." The student of bionomics will also find interesting matter among the notodonts in Mr. Barrett's account of the protective resemblance and mimicry of the "Puss" and "Lobster" moths—*Curtula vinula* and *Stauropus fagi*—and their caterpillars, insects rendered so familiar to naturalists by the writings of Professor Poulton and other students of animal colour and form.

There are only four genera of noctuid moths dealt with in the present volume—*Diphthera*, *Acronycta*, *Arsiloncke*, and *Agrotis*. We are glad to see that Mr. Barrett is going to favour us with a new classification of the British noctuids; the arrangement of genera with which our entomologists have so long been content places closely-allied species in different "families." Mr. Barrett writes wisely on the difficulty of finding reliable structural characters for the differentiation of these insects. They are a large and dominant family, probably still in process of comparatively rapid evolution, and hence the difficulty which the naturalist experiences in dividing them up into generic groups. In dealing with the *Acronyctæ*, Mr. Barrett has largely availed himself of Dr. Chapman's well-known studies of the early stages. It is a pity that so few references are given to the papers whence these and other particulars are taken, for one must hope that some of Mr. Barrett's readers will wish to consult the original memoirs. The next volume of the work will bring out the author's views on the noctuids more fully. We notice that he eschews names for varieties and aberrations in dealing with the very variable species of the large and important genus *Agrotis*, but he gives a succinct account of the principal types of variation in such species. *A. aquilina*, by most recent students considered a form of *A. tritici*, is retained by Mr. Barrett as a "good" species. In dealing with the varieties of *A. tritici*, the author points out how the different forms tend to "mimic" various species of *Agrotis* and other noctuid genera. This fact is very suggestive as throwing light on the problem of mimicry generally. In nocturnal insects like the agrotids, no advantage to the individual can be supposed. But the fact that one species may vary so as to resemble half a dozen others suggests a starting point from which—in day-flying insects—one or more mimetic forms might be preserved and the rest eliminated by the action of natural selection.

As in the previous volumes, the specific descriptions of the moths and their preparatory stages are very good. We notice with regret, however, that synoptical tables such as Mr. Barrett gave in his early volumes are altogether wanting in the present one. The descriptions of families and genera, too, are very meagre, and, in most cases, quite insufficient for purposes of determination. This may not be a matter of so much importance in the edition with the coloured plates, if the student's only object is to name his insects, but it renders the small edition well-nigh useless to beginners for that purpose. In most of the reviews (in NATURAL SCIENCE and elsewhere) of previous volumes of Messrs. Reeve's series of works on British insects, complaint was made that not even an advertisement in the small edition informed the reader that there was also an issue with coloured figures. At the end of the present volume is a general catalogue of Messrs. Reeve's publications in which will be found an announcement that volumes i. and ii. of the present work were issued in both forms. It may be inferred from this, on the principle of analogy, that a similar course has been adopted with regard to volume iii.

Mr. Kirby's volumes form part of a manual on the moths and butterflies, in which the author's aim is to make the study of British species introductory to that of the Lepidoptera of the world. It was intended to include all the butterflies in the first two volumes, but pressure of space in them has led to the third, now before us, commencing with an account of the Hesperiidæ or "skippers"—generally considered the lowest butterfly-family. As Mr. Kirby points out, the old, primary division of the Lepidoptera into butterflies and moths cannot be maintained; and although he places the Hesperiidæ under the heading "Rhopolocera," he states his opinion that if any line between "butterflies" and "moths" can be drawn, that family should be classed rather with the moths than with the butterflies.

In the classification of the Hesperiidæ, Mr. Kirby follows Capt. Watson's revision. The more typical genera are described, with one or two species of each, all the British species being figured; there are also short accounts of the eggs, caterpillars and pupæ, and the ranges of the insects are in most cases indicated. In discussing the general distribution of the family, Mr. Kirby points out that "skippers" are relatively much more abundant in North America than in Europe, though numerous in the tropics of both hemispheres, and that these insects are unrepresented in Greenland where nymphalid, lycaenid, and pierid butterflies occur.

The greater part of the volume is devoted to certain families of moths. In this and the succeeding volume it is intended to complete the "Sphinges" and "Bombyces." Mr. Kirby's arrangement of the families closely follows that which he adopted in the first volume of his "Catalogue of Lepidoptera Heterocera." In the present volume twenty-six families are dealt with; the principal being the Castniidæ, Uraniidæ (Cydimonidæ), Agaristidæ, Chalcosiidæ, Zygænidæ, Arctiidæ, Lithosiidæ, Hypsidæ, Lymantriidæ (Liparidæ), Psychidæ, Limacodidæ, and Notodontidæ. Several new families are suggested by Mr. Kirby for isolated or aberrant forms. The family Euschemonidæ is proposed for the single Australian species *Euschemon Rafflesiæ*, a curious insect which has been classed by different authors with the butterflies and with the moths. Another moth which receives the distinction of a family to itself is the West African *Pseudopontia paradoxa*, one of the most puzzling of insects, classed by most German writers with the pierid butterflies, but showing by its wing-neuration some affinity to the chalcosiid moths.

The great majority of the coloured figures are good, yet one is tempted to ask why are moths so often drawn without legs? A fair number of species are figured in the present work for the first time, preference being given to unfigured insects when new plates were thought desirable. On this account alone the book will be valuable for reference. Most of the British species are mentioned and figured and the more important foreign genera are noticed. In some cases particulars of the early stages of the exotic forms make a welcome addition. A plate is devoted to metamorphosis of the large South American psychid moth, *Oiketicus Kirbyi*; but there is unfortunately only a very meagre account of the British species of this most interesting family. Throughout the volume, an altogether disproportionate amount of space has been devoted to the synonymy and references of the species which are mentioned; in many cases these take up as much space as the account of the insect itself, and sometimes more room is allotted to a list of names and books than to the description of the moth, its haunts, and its habits. These are consequently passed over very summarily and the important question of variation is often not mentioned at all. It would be ungracious to complain of omissions in a book covering so wide a field, were it not that nearly a whole page is at times devoted to matter proper to a catalogue.

Mr. Kirby is so high an authority on questions of nomenclature that it is impossible not to give respectful consideration to the alterations in familiar names which he thinks necessary. But we surely might be allowed to retain a well-known name like *Urania* which is pre-occupied only in botany. And the suggested transference of the specific name *lubricipeda* from the buff to the white ermine moth would cause well-nigh intolerable inconvenience. Linné called both species *lubricipeda*, distinguishing the buff insect, which has borne the name unquestioned for more than a century, by the addition of the letter *β*. To make the law of priority so "stern and unbending" that a single letter causes such confusion in nomenclature as this, will ensure the continual disregard of that law by the majority of working naturalists.

In the fourth volume of Mr. Kirby's book the systematic portion is comparatively short, comprising only the Sphingidæ, Bombycidæ, Saturniidæ, Drepanulidæ, Lasiocampidæ, Zeuzeridæ, Hepialidæ, and a few small families. The rest of the volume is occupied with valuable essays on the classification and bibliography of the Lepidoptera. We are told in the preface that these were to have been issued in the fifth (last) volume of the series, but they are included in the fourth in order to avoid dividing the systematic account of the noctuid moths into two instalments.

Some of the most attractive of all moths, such as the hawk-moths and the silk-producing insects, are here dealt with, and it is satisfactory to notice that more space is given to description and less to references than in the preceding volume. Still, it is disappointing, after nearly half a page devoted to the literature of *Lasiocampa quercus v. callunæ*, to find no word as to how that insect differs from the type. All the British species are described and figured, together with typical exotic forms; the caterpillars—in these families often very striking in colour and form—receive a fair amount of attention, but it is surprising to find no figure of the common silkworm. And, in describing the larvæ of the hawk-moths, some reference might, with advantage, have been made to the supposed biological import of the two forms of caterpillar (brown and green) occurring in so many of the species, of the series of side-stripes on the caterpillars of the sphingids, and of the large 'eyes' on those of the choerocampines.

The essay on the literature of the Lepidoptera, which concludes the volume, is a useful and valuable piece of work. Mr. Kirby specially insists that it is far from being exhaustive; it would be ungracious therefore to point out omissions, but it may be stated that the essay deals almost exclusively with the systematic and faunistic aspect of the study of moths. After mentioning the more important authors dealing with Lepidoptera in general, from Linné onwards, Mr. Kirby furnishes references to the works which treat of the fauna of special countries, arranged under the recognised zoological regions.

In the introductory essay, Mr. Kirby gives a summary of the more important schemes of classification which have been proposed for moths, beginning with the tenth edition of the "Systema Naturæ." From the works of Fabricius, Schiffermüller, and Hübner, the reader is led on to the systems of Stephens, Herrich-Schäffer, Boisduval, Guenée, and Walker. For this last writer Mr. Kirby has a good word to say:—"his work, like that of other authors, differed much at different times, and is by no means deserving of the sweeping and wholesale condemnation it has received at the hands of many whose own work is far from faultless." Coming then to modern writers, Mr. Kirby deals briefly with the classifications of Snellen, Hampson, Meyrick, and Comstock, founded mainly on the structure of the imago, with Dyar's scheme based on the form of the larva, and Chapman's on that of the pupa. The researches of these writers, which have been often referred to in the pages of NATURAL SCIENCE, have in many respects transformed the older views as to the affinities of the families of moths.

The last classification mentioned by Mr. Kirby is that suggested in Professor Packard's elaborate memoir, a copy of which we have just received. Though the title suggests only a monograph of North American notodonts, there are a series of introductory essays, of which the most important deals with the classification of the Lepidoptera as a whole. Comstock divided the order into two primary divisions, the Jugatæ and Frenatæ, the former of which includes two families, the Micropterygidæ and Hepialidæ, characterised by the presence of a jugum or fold of membrane on the inner margin of the forewing. This structure, which occurs in the Trichoptera or caddis flies, is unknown in all other moths, and Comstock therefore regarded the two families named as forming a primary archaic division of the Lepidoptera. A similar view is held by Hampson, Meyrick, and Grote, the neuration of the hindwings in these families agreeing with that of the forewings, while in all other moths the number of nervures in the hindwings has become reduced. Packard, however, considers, with Chapman and Tutt, that the mouth-organs of the imago and the structure of the pupa are of greater importance than the form and neuration of the wings. He agrees with Chapman's division of the old genus *Micropteryx* into two families — the Eriocephalidæ and Micropterygidæ; and pointing out that the tiny moths included in the former of these have mandibles not reduced to a vestigial state, and maxillæ in which the typical parts of those jaws as developed in the lower insect-orders can be recognised (an observation first due to Walker) he ranks them as the primary archaic division of the moths (Lepidoptera laciniata) and sets over against them all the other insects of the order (Lepidoptera haustellata). Then he divides this latter section also into two very unequal groups. The Micropterygidæ (*sens. str.*), whose pupæ have limbs entirely free, like those of a caddis-fly pupa, and large functional mandibles, form the group Paleolepidoptera, while all the other families are united in the group

Neolepidoptera, which again is divided, mainly on the lines suggested by Chapman, into a lower series of families with incomplete, and a higher series with obtect, pupæ.

A specially valuable section in Packard's work is his description with figures of a number of larvæ and pupæ of tineoid moths, which he considers worthy of division into ten or fifteen families. He gives in conclusion a tentative genealogical tree (copied in Mr. Kirby's work). A comparison of this with the genealogy given in the first volume of Hampson's "Moths of India" is interesting and suggestive. Though there are numerous divergencies in points of detail, both writers agree in placing the Saturniidæ, Noctuidæ, Agaristidæ, Geometriidæ, Sphingidæ, and Notodontidæ higher than most of the old group Bombyces, and both agree in the near affinity of several "bombycine" families—as the Cossidæ, Sesiidæ, and Zygaenidæ—with the so-called "microlepidoptera." Packard includes the butterflies in his tree, and derives them through the Castniidæ from the Hypsidæ. This view cannot be maintained in face of Chapman's recent work on the pupæ of butterflies, which shows the origin of the group from low lepidopterous forms independently of the higher moths.

The sections of Professor Packard's memoir which deal specially with the North American notodonts are worthy of the highest praise, and should act as a stimulus to British workers to investigate their own insect fauna in an equally thorough style. The species are described at length, in most cases in all their stages, and are excellently illustrated with coloured plates and structural figures. The only unsatisfactory feature in the illustrations is to be found in many of the photographs of moths from actual specimens; the results of this process are often indistinct and the figures would be nearly useless for purposes of identification. Specially valuable are the detailed accounts of life-histories; and, in a very suggestive introductory essay on the incongruence between the larval and adult characters, the author points out how in a certain species of a genus, the caterpillar may become vastly modified in its later stages, while the imago agrees closely with that of an insect whose larva retains primitive characters throughout its growth. From some of the modifications, believed to be primitively adaptational, which occur regularly at a certain stage in the life of a caterpillar, the author argues for the inheritance of acquired characters.

In his essay on the distribution of the notodonts Professor Packard objects to the term "Nearctic" of Sclater as well as to "Sonoran" of Merriam. He divides North America up to and including Merriam's Transition zone into four provinces: the Appalachian (N.E. and Central States—S. Canada) Austroriparian (S.E. and Gulf States) Campestrian (West Central and Pacific States) and Mexican. There are eleven genera found in the eastern provinces which are absent from the Campestrian, while no genus is peculiar to the latter province. Nine genera are common to Eurasia and North America, eight are peculiar to North America, while only five are common to North and South America. It is of interest to note that while most of the genera common to the Old and New Worlds are characteristically northern in America, none of the eight peculiar North American genera range northwards beyond Merriam's Transition zone. The division of the insects into a Holarctic and a Sonoran fauna would thus appear to be justified.

Geo. H. Carpenter.

A Study in Variation.

On Mechanical Selection and other Problems. By Karl Jordan. Pp., 110., iv. plates. (From *Novitates Zoologicæ*, vol. iii. Tring: 1896.)

This memoir is the record of a careful and valuable piece of research which throws light on some factors of organic evolution. Dr. Jordan has compared the variation of the genital armature in a number of species and sub-species of butterflies (*Papilio*, auctt.), with the variation of the wing-patterns, and has been able to formulate several suggestive generalisations from the facts established. Incidentally he discusses many current problems.

In his introductory remarks, the author ventures to answer a question more easy to ask than to solve. "What is meant by a species?" He criticises the definitions of the term which have been given by Eimer, Romanes, and Wallace, and concludes that "the chief criterion of specific distinctness of a given form of animal or plant is the impossibility of fusion with other forms." There comes a point, according to Dr. Jordan, in the development of races, when, a certain degree of divergency having been attained, "the form exhibiting this divergency can never become one with any other form." All distinctions short of this point are treated as varietal or sub-specific, and these lesser divisions are held by the author to differ from the true species, not only in degree but in kind. Even granting that there is a point of divergence past which fusion becomes impossible, doubt as to whether certain forms have or have not gone beyond it must always remain until set at rest by experiment; so that there will be room for differences of opinion among those who accept Dr. Jordan's conception of the species. But not every naturalist will agree with his premise that the course of evolution must be represented by a tree with constantly dichotomous branching. "If we should have to concede the possibility," he writes, "that the lines of ancestors of any two forms of plants and animals, say of the lion and the giraffe, were such that they first became widely divergent, then identical, then again widely divergent, not only the figurative tree, but also the kind of evolution it is meant to illustrate, would be pure nonsense." The lion and giraffe are rather extreme examples, but the opinion that lines of descent might converge or anastomose as well as diverge would not be summarily set down as "pure nonsense" by all who have thought on the subject.[1]

Dr. Jordan adopts the trinomial form in naming the sub-species with which he deals, and in cases where the name first given to an individual of the species is evidently applicable to a particular sub-species he holds that that name must be repeated to distinguish that subspecies, *e.g.*, *Papilio priamus priamus*. Sometimes indeed the specific name has been applied to a particular form of one sex in a polymorphic sub-species; then we get *Papilio polytes polytes* ♀.-f. *polytes*. "The meaning of this name is exclusive and hence precise, and that is the highest praise we can give to a name." It will be re-assuring to those not enthusiastic over such results of advanced nomenclature, to know that not many butterflies have to be labelled after that fashion.

The parts whose variation is particularly described are the *harpes*, or series of ridges, teeth, and hooks to be found on the inner surfaces of the paired valves or claspers, which are lateral wings of the ninth abdominal segment in male butterflies. Detailed descriptions and

[1] Cf. E. W. McBride, Nat. Sci., vol. x., p. 31. Jan., 1897.

figures of the varying forms assumed by these harpes in different individuals of the same species and sub-species are given, and enough material has been examined to lead to the conclusion that there is no correspondence between the variation of these organs and the variation of the wing-patterns on which the sub-species are founded. The relative length of the prehensile section of the harpe differs to the extent of 73 per cent. in *Papilio machaon*. The *uncus*—or dorsal hook belonging to the tenth segment—of the British forms of this butterfly is closely like that of the Japanese sub-species and different from the forms found in continental European and Asiatic insects. Similar differences are noticed among the races of eastern species which Dr. Jordan has examined, and a division of a species according to the genital armature would, in most cases, give quite a different set of sub-species from that which has been established on the patterns of the wings.

Though the amount of variation in the harpes is generally considerable, there are for each species limits which are not overstepped, and the author therefore considers these organs of the greatest use for the determination of true specific differences. It is certain, however, that many of the various forms within certain species figured by Dr. Jordan would be considered of specific value by systematists unacquainted with the amount of variation which may occur. An investigation into the limits of divergence seems therefore necessary before the use of these organs as specific characters can be relied upon. But the comparative constancy of the genital armature within each species has an evident bearing on the development of the race. Dr. Jordan has made some observations on the corresponding organs of female Papilios and finds a set of variable ridges and furrows on an intersegmental membrane, in each case adapted to be held by the harpe of the male clasper. He infers that, by a process of mechanical selection, the variation of the genital armature in both sexes must be kept within limits, because individuals with abnormal organs would be incapable of copulation. And it follows that any considerable modification of the armature, generally affecting both sexes of a race, would lead to practical isolation and accelerate the formation of a new species. The author quotes a passage from the "Origin of Species" in which Darwin dwelt on the difficulty of accounting for the infertility of "natural varieties, as soon as they have been permanently modified in a sufficient degree to take rank as a species." The facts dealt with in the present memoir show at least one way in which such segregation as Romanes' theory of physiological selection was proposed to explain, might be produced; though "the cause of infertility would not be physiological diversity but mechanical impossibility of fertilisation." The importance of the principle here invoked has been long recognised by various naturalists, and on p. 48 of the current volume of NATURAL SCIENCE, Mr. O. H. Latter quotes an observation of Dr. Marchal, bearing on its importance in wasps.

Dr. Jordan is careful to point out that mechanical selection can only work on variations produced by other factors. No direct evidence as to these other factors is furnished by his researches, but some incidental remarks are worth notice. There is an Indo-Malayan butterfly, *Euplœa rhadamanthus*, mimicked by a *Papilio*, *P. caunus*, and both these species have distinct forms in Sumatra, Java, and Borneo, separable only by minute differences in the extent of the white markings on the wings. As the author points out, it is hard to believe that such minute variation in the mimicking species can be the result of natural selection; even one who considers natural selection the true explanation of mimicry generally would admit that. The similar variation in the same locality of two species of different families, but with the same wing pattern, tells in favour of the direct action of the local surroundings on the markings. And the same view is upheld by the facts as to the geographical distribution of some of the sub-species considered. A variable species may give rise to two similar or closely allied sub-species in widely-separate localities; the Ceylonese form of a butterfly may be closer to the Indo-Malayan than to the continental Indian form. To invoke land-connections to explain the ranges of sub-species is less reasonable than to suppose—as we know by experiment may be the fact—that similar conditions may induce similar variations in distant localities. But if any sub-species may in course of time develop into a true species, it follows that the same species might arise independently in two or more separate localities.

Dr. Jordan is content to class himself with the classifiers; in more than one passage he refers to the work of "us systematists." This memoir shows clearly how much biological importance the careful study of species may have, and should encourage his fellow-systematists to fresh and more painstaking effort by reminding them of the many unsolved problems that still lie behind the dry labour of the species-maker. GEO. H. CARPENTER.

BRITISH HOMOPTERA.

THE HEMIPTERA HOMOPTERA OF THE BRITISH ISLANDS. By James Edwards, F.E.S. 8vo. Pp. 271, and ii. plates. London: L. Reeve & Co., 1896. Price 12s.

THIS forms a welcome addition to the series of Entomological volumes in course of publication by Messrs. Reeve. Mr. Edwards is the recognised authority on the Homoptera in this country, so that all that comes from his pen on this subject is of value. The plates of details are clear, and will be of great use. There is said to be a large-paper edition with twenty-eight coloured plates, sold at forty-three shillings, but of these plates we cannot speak, as we have only the small edition before us.

Seriously to criticise a specialist's writings on a very difficult group of insects like the present would be beyond our powers, so we will only notice a few points which appear to us not quite up to the standard of the rest of the book. In the introductory remarks on anatomy, the elytra and wings are treated too much as if they had nothing in common, in fact, a beginner might be led to imagine that a Homopteron had only two wings. This is especially unfortunate in a group where both pairs of wings are sometimes of almost similar texture, as in *Cicadetta*, and where the term elytra applied to the front wings almost contradicts the name of the sub-order—"Homoptera." We also think that, when considering the wings in their outstretched position, it is misleading to call the apical margin (*i.e.*, that furthest from the body) "the hind margin."

In the body of the work we prefer the plan adopted under *Liburnia pellucida*, where the description of the macropterous form precedes that of the brachypterous, and we think that the tables of species would have been clearer had the specific names been thrown into the margin.

We may also, perhaps, be allowed to wonder at the numerous species still retained, whose claim to a place in our list rests on a single specimen, often without locality, and to hope that our author may soon be able to clear some of these up, or, if not, to clear them out. Unknown as many of them are on the Continent, there is great ground for suspicion that they are not valid.

The descriptive part of the work appears excellent, and the hints on collecting will be most useful. The scanty records of localities are deplored as much by the author as they can be by any one. We only hope the book will prove a stimulus to collectors and students generally.

Deep-Sea and Pelagic Fishes.

Oceanic Ichthyology: a Treatise on the Deep-sea and Pelagic Fishes of the World, based chiefly upon the Collections made by the Steamers *Blake*, *Albatross*, and *Fish Hawk* in the Northwestern Atlantic. By George Brown Goode and Tarleton H. Bean. 4to. Pp. xxxv., 553, with Atlas containing 417 figures. Washington: Smithsonian Institution (United States National Museum Special Bulletin), dated 1895, received direct through the Smithsonian Exchange, January, 1897.

The United States National Museum has done a great service to ichthyology in publishing these two handsome volumes (text and atlas) on the fishes of the deep sea and the extra-littoral parts of the ocean. The work is an attempt, by two of the most distinguished American ichthyologists, to summarise all that is known concerning these fishes, incorporating the results of their own researches with those of their predecessors, and making the whole a useful handbook not merely for specialists in the subject but also for biologists more immediately interested in other fields. It has, moreover, a melancholy interest as being the last great undertaking of the senior author, whose loss we have recently had to mourn (Nat. Sci., vol. ix., p. 339, Nov., 1896).

All oceanic fishes are included in this Treatise, partly because it is not yet possible to distinguish strictly between the two classes, and partly because the pelagic forms have often been mentioned in the discussions by previous writers on the deep-sea fishes. The strangest, however, are those from great depths, and it is in these that the chief interest of the work centres. The authors estimate that about 600 species have already been obtained from a depth of 1,000 feet or more. They also conclude that a very good general idea of the character of this fauna has already been acquired; for no startling novelties have appeared for several years, and the chief result of recent dredgings is the re-discovery of known types in new localities.

At the same time, there are many new genera and species to be found in most untried spots; and scarcely a beginning has been made in capturing the larger, swifter, and more cunning forms. The gigantic oar-fish (*Regalecus*), for example, a serpent-shaped, rapidly swimming creature, sometimes over twenty feet in length, has never yet been captured; although within the last century and a half dead specimens have been picked up on the shores of Norway, the Faroe Islands, Scotland, Ireland, England, France, the Mediterranean, Bermuda, the Cape of Good Hope, Hindustan, and New Zealand. Its world-wide distribution and the number of waifs give evidence that it is abundant in mid-ocean, yet the exploring ships in all the years of their combined searchings have found no vestiges of it, old or young. Where *Regalecus* lives, there may be others. Indeed, Drs. Goode and Bean think "it cannot be doubted that somewhere in the sea, at an unknown distance below the surface, there are living certain fish-like animals, unknown to science and of great size, which come occasionally to the surface and give a foundation to such stories as those of the sea serpent."

It is also to be remembered that about half of the deep-sea species are known only by single specimens, which, of course, cannot be sacrificed for dissection. Others are so delicate that their dilapidated remains, when brought to the surface, need great skill in interpretation.

The introduction, referring to matters such as we have just noted, extends only over five pages. The work itself begins as it ends with descriptive technicalities, lists of localities, and references to literature, without any attempt at generalisation or any anatomical notes even on the commonest forms. It is prefaced by a systematic index and a list of the new genera and species described by the authors; it concludes with a valuable alphabetical index of the whole.

The Chimæroids are regarded as a division of the Elasmobranchii, termed a sub-class on p. 5, an order on p. 30. No new facts of importance are recorded. The order Malacopterygii (of a class or sub-class not stated) follows, occupying no less than 94 pages, with family-divisions of a more restricted nature than those usually adopted. The arrangement of the Scopeloids is interesting, based upon the disposition of the luminous organs, a character already determined to be of systematic importance by Lütken. The authors, however, obscure their results by the unearthing of long-forgotten names and the (as it appears to us) useless adoption or invention of many new terms. The strange Sternoptychid fish *Chauliodus* is shown for the first time to possess an air-bladder. The order Lyopomi, established by Gill for the Halosauridæ, follows that of the Malacopterygii, and is itself succeeded by the great order of Apodes (eels), comprising some of the strangest forms of deep-sea fishes. A new species of *Pisodontophis* from the American coast is recorded as a boring parasitic fish, the dried and shrivelled remains of a closely similar animal being sometimes taken from the interior of salted halibut and codfish. The validity of the genus *Cyema* is doubted, and the authors think it may perhaps have been founded on a young *Nemichthys*, with its jaws and tail mutilated and partly repaired. The Heteromi follow with nothing new, and then are arranged the Berycoids, Scomberoids, Percoids, Scorpænoids, etc., under the order Teleocephali.

The work as a whole is so admirable and of such great utility, that it may seem ungracious to conclude our brief notice with severe criticism; more especially as the authors themselves beg indulgence for their shortcomings, on account of the difficulties and multifarious distractions amid which it was prepared. A treatise of this kind, however, if worth doing at all, is worth doing well; and a National Museum undertaking it ought at least to provide its officers with every facility to carry out their instructions in the best possible manner. As matters stand, it is obvious that the Government Printing Office deserves a large share of the blame. The preface and introduction are dated before midsummer 1895, and the title-page is marked with the same year; yet the publication was not issued until the end of 1896, and it did not reach Europe until early this year. If the dilatoriness of the Office is such after everything is completed, what must it be while the authors are reading the proofs? Their papers during the last few years, indeed, have often borne witness to their dissatisfaction; and it ought to be remarked that many of the fishes termed new genera and species in this work have previously been defined in the *Proceedings* of the same Museum. It is a serious omission in the present treatise, not to have pointed out this fact either in the Introduction or the Appendix. Again it is to be noted that the literary part of the work bears signs of having been entrusted to a third author not under proper control of the two responsible authors. To say nothing of the numerous "family diagnoses" based on ridiculously trivial characters, and the imposing "keys" filled with anatomical statements not verified in half the genera with which they deal—this collaboration has resulted in endless confusion and inconsistency. We are not now concerned with details, so will only cite one illustration of our meaning to caution the unwary reader. Take the case of the Order Malacopterygii, as it is termed. At the top of page 34, it is said to be characterized by "a mesocoracoid arch"; a few lines further down we are told that the large majority of the families comprised in this Order are destitute of the arch in question ("mesocoracoid wanting or atrophied"). As a matter of fact, the vast majority of the fishes placed here have never been examined in reference to the point. In the same table—which, like nearly all these higher "definitions," is marked "(*Gill*)"—the "Pterothrissidæ" are stated to have the "dorsal fin similar to and opposite anal," whereas on p. 50 Günther is correctly quoted in reference to this point thus:—"dorsal fin much elongate, many rayed; anal fin short." To make matters worse, Günther's figure of *Alepocephalus niger* is reproduced to serve for "*Pterothrissus gissu*," although the very same figure is copied under its correct name on a previous plate. Nor have the authors rectified this confusion in the copious Appendix. Finally, there is a total lack of decision as to what are "family" characters and what are "generic" characters, the same feature being continually mentioned in the higher definition and then repeated in the definitions subordinate to it. As to the lists of dredgings, they are printed in the most confusing manner possible, there being no attempt at tabulation for convenient reference. Even the list of deep-sea and pelagic fishes is carelessly compiled; and one whole genus of pelagic fishes *par excellence*, that of the flying-fishes *(Exocœtus)*, is entirely omitted.

We have nothing to add concerning nomenclature and the changing of familiar names; those are questions of personal idiosyncrasy in most cases, and no code of Rules can control them. We would only repeat the expression of our gratitude to the authors for the great and useful task they have accomplished, and conclude with the hope that the U.S. National Museum will soon succeed in removing some of the disabilities under which its larger publications labour.

A Book for Billingsgate.

Marketable Marine Fishes of the British Isles. By J. T. Cunningham. Pp. xvi., 375, illustrated. London: Macmillan, 1896. Price 7s. 6d.

This book, published under the auspices of the Marine Biological Association, may be succinctly described as a popular account of most of the scientific work done upon the life-histories of our food-fishes up to two or three years ago, supplemented by short sketches of the leading anatomical features of the common species of food-fishes, gleaned from Day and other reliable authors. As the blue-books and journals in which the former has appeared, and the voluminous works of the latter, are, to some extent, inaccessible to the amateur, this volume should prove a useful guide to the general subject, at least so far as it goes.

The book is divided into two parts, the first of which deals with the subject from general points of view, and the second consists of special remarks upon each common species.

In chapter I. the author undertakes to give a short *resumé* of the "History of Modern Investigations of the Subject," and here we do not think he is seen at his best: first, because the account is brought barely down to 1894, and considering the rapid progress of modern science, it is a grave defect for a book to be already over two years behind the times on the day of publication; secondly, because not only are there one or two misstatements of facts, but the whole *resumé* is calculated to give a layman a wrong impression with regard to the comparative magnitude and importance of the different observers' work therein referred to. To particularise, on p. 5 we are told that "Professor Allman . . . . . was the first to establish the fact that the spawn of the herring . . . . adheres to solid objects in the water." This was in 1862; yet more than half-a-century before, *i.e.* in 1803, Professor Walker gave an accurate account of the spawning of the herring, which had been also well known to fishermen years and years before. Later, in 1860, Dr. Boeck, acting under the Norwegian Government, produced an exhaustive work upon the spawning of this species,. and also contested the suggestion that shoals of herrings performed vast annual migrations from the north. Mr. Cunningham is not the first scientific writer to ignore the early literature of the herring and its reproductive habits. Again, Professor McIntosh was appointed to "make observations" under the Royal Commission in 1883 and not in 1884 (p. 14), and from his Report it appears that the instructions of the Commission were carried out with an accuracy seldom excelled. One would hardly conclude this from the author's remarks upon it. Further on we note,—"Professor Huxley was unable to take any part in drawing up the Report, and therefore did not share in the responsibility for its conclusions and recommendations." (p. 15). In point of fact, the Report was subjected to his inspection, and he especially referred to the scientific part as a most valuable contribution.

On p. 8 is the following remarkable sentence:—"The investigation of problems relating to the fisheries is not of a kind to attract voluntary private effort. It does not promise great individual rewards in the shape of either fame or fortune, nor are the researches of that abstract philosophical kind, which, like virtue, are their own reward, and are therefore pursued for their own sake with no ulterior object." This may be the opinion of the author from his own standpoint; but to follow up such a dogmatic statement with references to the work of Professor McIntosh, Mr. Duncan Matthews, and a number of other naturalists, is, at the least, calculated to convey a wrong impression as to the motives underlying their labours.

With reference to the second point, we have no hesitation in saying that the investigations of the St. Andrews School (with the single exception of Mr. Holt, who, trained at St. Andrews, later became more intimately connected with the Marine Biological Association) have been either studiously ignored or merely referred to in a casual way. "Professor McIntosh and his pupils and assistants" (the order is quaint but perhaps euphonious) "describe as usual the results, etc." (p. 30), and three or four references of the same kind serve to indicate the work of the St. Andrews Laboratory, which afforded the greater proportion of the facts narrated in Part II. of the work before us.

Such misrepresentations are already bearing their fruit, as may be seen in the *Spectator* of December 19th, in which appeared an extraordinary article "On the practical study of fish." The anonymous author unblushingly ascribes to Mr. Cunningham the discovery that the eggs of the majority of food-fishes are buoyant or pelagic: a discovery which, he remarks with unconscious humour, Mr. Cunningham was able to announce without the aid of a steam-launch.

Chapter II. gives a popular account of our "Valuable Marine Fishes," copiously illustrated by outline sketches of the species under consideration, which, with the exception of that of the angler, a very poor copy of Day's figure, help to convey a good idea of their general appearance. We were not up till now aware that the stickleback and the pipe-fish were "valuable food-fishes," and we were even under the impression that the lancelet was not usually regarded as a fish. The author is at some pains to explain to his readers that a lobster, an oyster, a star-fish, a jelly-fish, and a whale, are not true fishes: if his explanation is really necessary with our present standard of national education it is a very humbling thought. The account of the generation of fishes and their fecundity is a trifle voluminous, and, in parts, out of place in a popular work of this description.

The next two chapters deal with the eggs and larvæ, growth, migrations and habits. They are interesting reading and contain a useful collection of facts. The author is a little inclined to state *his* views upon disputed points as established facts, a habit which should be avoided by one who tells the "professional fishman" that "naturalists, although generally sick whilst at sea," are "on their guard against jumping at conclusions" (p. 4). "There can be no doubt that, in the sea, conger of both sexes die after spawning" (p. 84). This sentence, if read in one way, is with regard to the females a platitude, an impossibility in the case of the males, whilst if read in the other way, is a conclusion arrived at by a process closely akin to that of "jumping."

At the end of Part I. the author discusses the preservation of immature forms and the artificial propagation of fishes. He shows the inadequacy of the size-limits recommended by the Parliamentary Committee of 1893, and remarks: "As the circumstances of the trawling industry and the habits of the fish do not appear to admit of the preservation of all plaice, soles, turbot and brill......till they have spawned once, we have to face the possibility of a continued diminution in the supply of these fish, in spite of all protection of the young, as a consequence of the excessive capture of the mature individuals." As a remedy for this lugubrious state of affairs, artificial propagation is discussed, and calculations entered into to show that "the total results of the work of the [Dunbar] hatchery is equivalent to leaving in the sea one mature female out of every 2,000 killed at Grimsby, a proceeding which would clearly not make much difference to the total supply of plaice in the North Sea" (p. 141). Although we believe the author's opinions to be erroneous, we can only point out here that the whole deduction depends upon the following:—"We may suppose that the destruction is nine times as great in the sea, which is certainly more than the actual proportion." A consideration of the whole physical environment of pelagic eggs and the enormous fecundity of the fishes spawning in this habitat points to a factor nearer ninety times nine than the author's estimate. His suggestion of keeping tame spawners in order to sow the sea with "vast quantities" of pelagic eggs can hardly be taken seriously.

With Part II., the "History of Particular Species," we must honestly say we are disappointed. Perhaps the "Monograph on the Sole," with its exquisite coloured illustrations, may have led us to expect too much; but with such a theme as our young fishes, one or two skilfully-executed coloured plates might at least have been forthcoming. This is not all; for the figures provided for our perusal are in many cases anatomically inaccurate and poorly executed. It almost appears that the author has a special desire to depict his larval fishes, undergoing all sorts of contortions, illustrating much better the torsional and shrinking abilities of the larval anatomy than the beautiful symmetry of these little beings in their natural surroundings. The many figures borrowed from Holt, Prince, and Raffaele, are better, but scarcely benefit by their new environment.

The text shows evidence of great labour in the accumulation of facts, but there is a certain looseness of writing in evidence here and there. The author refers repeatedly to the anal fin as the 'ventral,' whilst the true ventrals or pelvics he sometimes designates 'pelvics,' at other times 'throat' or 'abdominal' fins. It would have been preferable, as he himself points out, to use the term 'pelvic' throughout, and thus to indicate an homology which might not of necessity be clear to the popular mind. The scientific term 'post-larval' is not referred to, though the post-larval stage is recognised as representing a well marked period of developmental history by other workers, from whose labours the author derives the majority of his facts. The attempt to avoid this term leads to an inextricable confusion between 'transition stage,' 'transformation stage,' 'young of,' 'later larval stage,' 'older stage of......larva,' 'larva at a very late stage,' and so on.

The labour and energy that Mr. Cunningham has for years thrown into this department, and the excellent work he has done, must be freely acknowledged. But, for this very reason, the book before us cannot be placed to his credit. With an impartial statement of facts, a recognition of the work of others on the part of the author, and with a 'ha'porth of tar' from the publishers or the Marine Biological Association, in the shape of the employment of a trained artist and the consequent inclusion of a few coloured plates, a work of real value would have resulted.

A. T. Masterman.

Laboratory Botany.

Das botanische Practicum: Anleitung zum Selbststudium der mikroskopischen Botanik, für Anfänger und Geübtere. Zugleich ein Handbuch der Mikroskopischen Technik. By Dr. Edward Strasburger. Third revised edition, with 221 woodcuts. Pp. 740. Jena: Gustav Fischer, 1897. Price 20 marks.

Since its first appearance in 1884 the Practical Botany ("das botanische Practicum") of Prof. Strasburger, has ranked as beyond question the leading book, in any language, on the study of the structure of plants. It was recognized at once as the work of a master, of one who has probably a greater experience in vegetable histology than any other living man, and who threw all his energies into the preparation of the book, so as to render the fruits of his experience available to others.

The first edition was an eminently readable book—a quality which one scarcely looks for in a laboratory guide. There was an agreeable freshness about it; all the illustrations were original, and everything in the book manifestly represented first-hand work. Apart altogether from its practical uses, the "Practicum" formed an interesting introduction to the histology of plants, for it covered a wide field, and contained much information not otherwise accessible. If the book, in its original form, had a fault, it lay, perhaps, in a certain want of system in its arrangement.

When a second edition was called for, within three years of the first, the author found it necessary to give his book a more severely practical character. The author then said in his Preface: "One thing has become perfectly clear to me; the centre of gravity of the book must lie in microscopic technology, and in guidance to scientific observation. This book could not be allowed to grow into a hand-book of vegetable anatomy, but in microtechnical respects it had to reach such a degree of completeness that everybody could find in it what he wanted. I have therefore somewhat restricted the number of examples, while considerably increasing the technical instructions." Now that, after an interval of about ten years, a third edition has appeared, we find that further changes have been made in the same direction. "This time also" says the author (p. xii) "I have diminished the number of plants investigated, in order to give the necessary extension to that task in which the centre of gravity of the work lies, namely microscopic technology." In consequence of this, the book has no doubt become of even greater value to the practical investigator, but it has certainly lost something of its attractiveness for the general botanical reader, for nothing is so unreadable as long descriptions of methods and apparatus. However, a laboratory guide is nothing if not practical, and we have no right to complain if Prof. Strasburger's work, like some highly specialized organism, has become more and more rigidly adapted to the conditions of its existence. The book has grown, since the last edition, from 685 to 739 pages, but the increase is entirely in the technological Introduction and in the indices; the actual descriptive text has somewhat diminished.

The great feature of the Introduction is the prominence now given to the microtome and methods connected with its use. Since the second edition appeared, the microtome, as the author says (p. xi) has made its way into every institution which keeps up with the progress of the microscopic art. "The knowledge how to use it, must now be acquired by every body who wishes to work with success in the province of histology" (p. 31). English botanists have taken at least their full share in this important change, which in many cases has raised section-cutting from the region of chance into that of certainty. A number of forms of microtome are described and illustrated—among them our old friend the "Cambridge rocker," an imitation of which is now, it appears, "made in Germany." The part of the Introduction relating to the microscope and other optical apparatus has also undergone a thorough revision, in accordance with the present position of microscopy.

The arrangement of the contents of the 32 chapters on "Pensa" which form the main part of the text, has been somewhat modified, especially in the anatomical part, so as to give the whole course a more logical order. The headings of the chapters have been extended, in order that they may afford a complete idea of the contents of each, and of the material required. The large-print paragraphs contain all that is needed by the ordinary student, while a botanist who has worked through the whole, will be qualified for any histological investigation.

The author has evidently spent a vast amount of labour on this edition, for the latest results of research have been everywhere introduced, and usually tested by the personal observations of the writer. For example, the development of the starch-grains is now described in *Pellionia Daveauana*; the enzyme-sacs of Cruciferæ are explained in accordance with Guignard's researches; *Protosiphon* is separated from *Botrydium*, as the result of quite recent work by Klebs, and the sexuality of the ascomycetous fungi is now definitely asserted, chiefly on the basis of Harper's observations.

The number of figures has grown from 193 to 221; the increase, however, is due to new illustrations of apparatus. In the descriptive chapters a certain number of old figures have disappeared, and some fresh ones have taken their place. Some of the omissions, both as regards descriptions and illustrations, will certainly be regretted by a reader who is familiar with the earlier editions. The roots of vascular cryptogams, for example, now come off very badly, with 5½ lines of text, and no figures. (p. 254). In the chapter on algæ the excisions are still more lamentable, for *Batrachospermum* and *Fucus* are now left out altogether; we cannot but regard these omissions as a change for the worse. The best figures illustrating the reproductive processes in the fir have also been sacrificed. On the other hand, we find a few welcome additions to the illustrations, especially among those relating to the anatomy of the Scotch fir and the lime-tree. On the whole, however, we fear that the loss, in the matter of illustrations, outweighs the gain.

The ideas of Van Tieghem (*see* NATURAL SCIENCE, vol. ix., p. 147) have had their due influence on the anatomical part of the book, though the term "stele," is not introduced until the Pteridophyta are reached (p. 247). The homology between the central cylinder of the stem, and that of the root is rightly emphasised, and justice is done to the French anatomist to whom we owe this fertile conception (p. 320). In one point, however, we have noticed that the stelar theory is not consistently applied. On p. 289 the central cylinder in the stem of *Hippuris* is still described as a "cauline vascular bundle," whereas it is manifestly not a single bundle at all, but an entire stele.

Many readers will turn at once to the concluding chapter, in order to learn the author's present views on the questions in dispute as to the nucleus and its division. Their curiosity will be rewarded, for on two of the burning histological questions of the day the author takes up a somewhat unexpected position. As regards the existence of centrosomes in plants he has very decided views, saying, in so many words, that "As the investigations on the botanical side now show, individualised centrosomes are peculiar, in the Vegetable Kingdom, to the Thallophyta and Bryophyta; they are absent from Pteridophyta and Phanerogams" (p. 611). Considering how recently almost all histologists (including the author) accepted Guignard's observations on the lily as affording the classical examples of centrosomes in plants, this strong negative expression of opinion is somewhat surprising. It must be alllowed, however, that the remarkable absence, so far, of confirmatory evidence, goes far to justify it. Prof. J. Bretland Farmer's discoveries of centrosomes in the Hepaticæ now occupy a prominent place in this chapter.

On the question of a reduction-division in the maturation of the sexual cells, Prof. Strasburger, whose openness of mind is proverbial among botanists, completely changes his position. He now asserts positively that in the second nuclear division of the pollen-mother-cells, the chromosomes are divided transversely and not longitudinally, so that here a true reduction-division, in Weismann's sense, takes place. This statement is in direct contradiction to the results of the most recent and careful work on the subject, notably that of Miss E. Sargant, and we cannot but regret the introduction of a dogmatic pronouncement, on so doubtful a point, into a book designed for students.

If we have found certain features of the book open to criticism, it is none the less true that Prof. Strasburger's work remains at the head of all guides to the study of laboratory botany. No pains have been spared to render the "Practicum" more useful than ever to the practical worker. It may have lost something in freshness and in general botanical interest, but in completeness and elaboration of detail, it stands unrivalled as a guide to histological method.

D. H. S.

A New Edition of Brush's Mineralogy.

Manual of Determinative Mineralogy. With an Introduction on Blowpipe Analysis. By George J. Brush. Revised and enlarged by Samuel L. Penfield. Fourteenth Edition. 8vo. pp. ix.,+164+63 to 96 in double-page tables+101 to 108 of Index, with 50 figs. London and New York: Chapman and Hall. 1896. Price 15s.

The appearance of a new revised and enlarged edition of this well-known text-book is an event worth recording. As the result of Professor Penfield's work, the book, although retaining the general plan of previous editions, has been practically re-written, and at the same time almost doubled in size by the addition of new matter and illustrations. The additional matter includes an introductory chapter giving a summary of important chemical principles. These are enunciated very clearly but, we fear, too briefly to be of much service to the student (for whom they are presumably intended) who has had no previous knowledge of chemistry.

The chapters on apparatus and re-agents have been considerably extended, but follow on the whole the lines of the last edition. It is in the chapter on the reactions of the elements that the greatest departure from previous editions will be noted. From barely 14 pages this chapter has now been extended to 94. The elements, in alphabetical order, are treated under the headings, Occurrence and Detection. The enumeration of the principal minerals in which each element occurs is a new and useful feature. Where blowpipe tests are not decisive, full details are given of tests in the wet way: *e.g.*, in the case of the rare earths, the general methods of separation by means of oxalic acid, sodium thiosulphate and potassium sulphate are described. In this connection, however, we note at the bottom of page 65 a sentence which is so worded as possibly to lead the unwary student to the erroneous conclusion that Thorium may be separated from Zirconium by a double precipitation with sodium thiosulphate.

The descriptions of tests with known minerals which are given in smaller type under most of the elements are very suggestive and instructive. Throughout the book particular care has been taken to point out to the student the precautions necessary to insure success in any test; thus he is warned that in the case of fused material a blue colour with cobalt nitrate is not necessarily an indication of the presence of alumina, and that in testing for carbonates concentrated acids must not be used nor ebullition mistaken for effervescence. We are glad to note the subsidiary position assigned by the author to those stumbling-blocks, the cobalt nitrate test for magnesia and the microcosmic bead test for silica. That the book is well up to date may be judged by the fact that at least half a dozen lines are devoted to the consideration of Helium.

As stated in the preface, the book, as it is now presented, is only the beginning of a revision, for the tables for the determination of minerals have been left as in the last edition by Professor Brush. A complete revision of these tables, however, is to be made as soon as possible, and a chapter added on crystallography and the physical properties of minerals.

G. T. P.

A New Science.

Gemmographical Tables for the use of Diamond and Gem Merchants, Jewellers and Students, exhibiting in tabulated form, the distinguishing characteristics of rough and cut stones. By W. J. Lewis Abbott, F.G.S., London: Heywood and Co. Price 1s. 6d.

We are inclined to welcome with approval any effort to introduce scientific methods to the notice of jewellers and merchants of precious stones, to whom they are certainly not too familiar, and Mr. Lewis Abbott's tables aim at this desirable result.

The main features of the various gem-stones are arranged in five tables dealing respectively with their names and colours, their composition and physical properties, their crystalline form, and the colours which they present in the dichroscope. It is to be hoped that these tables will find a use among those for whom they are intended, and will lead them to recognise the practical value of a subject which is taught by the author at the Polytechnic Institute, and apparently with considerable success, though under the unfortunately hybrid name of gemmology.

The reader must be warned that garnet does not crystallise in pentagonal dodecahedra, nor spinel in tetrahedra; neither must he suppose that all the stones included in the formidable list of table 1 are usually recognised as gem-stones.

But these are minor blemishes and we may safely commend these tables to the attention of jewellers, feeling sure that they will derive benefit from some slight insight into scientific ways of testing precious stones.

Histology of the Stomach.

Lehrbuch der Vergleichenden Mikroskopischen Anatomie der Wirbeltiere. I. Der Magen. By Dr. Med. Albert Oppel. 8vo. Pp. viii., 543, with 5 plates and many text-figures. Jena: Fischer, 1896. Price 14 Marks.

It is now forty years since Franz Leydig published his text-book, and since then there has been no serious attempt to bring together in a systematic fashion our accumulated knowledge of comparative microscopic anatomy. The delay was natural, and perhaps not to be regretted. The forty years have seen the whole science of anatomy informed by a new principle, and evolution had its youthful wild oats to sow. Vertebrate anatomy, and certainly the details of vertebrate histology, save so far as they bore directly on physiology, were deserted by many acute minds for the new world of promise opened out by comparative embryology and the investigation of the widely diverse invertebrate groups. It seemed an old-fashioned trifling to discuss the minute structural differences to be found in vertebrate mucous coats, when the ancestry of the whole vertebrate group was to be expected from serial sections through a pelagic worm. Probably the best and the worst of this attractive method of investigation are now known, and anatomists are turning again to the closest comparisons between the minutest details of nearly allied forms. And these 500 pages on the histology of the vertebrate stomach are a sign of the new direction of anatomical activity. A great bulk of the literature cited is not more than ten years old, and it is plain that the book will have its greatest value as a stimulus to still more detailed investigation.

The author begins by a general account of the conformation of the region between the end of the œsophagus and the beginning of the duodenum. He describes the kaleidoscopic varieties of external form into which the region is thrown, and discusses generally the minute structure of the epithelial, muscular and glandular layers of which it it composed. The greater part of the volume is occupied by a systematic account of the various groups. The classification employed is broadly popular, including for instance *Amphioxus* and the cyclostomes among fishes, but it serves well enough as a guide to the contents of the volume. We have spied minutely into the author's use and knowledge of the literature of the subject in various branches, and we have never found him wanting. A large number of figures, reproduced or original, are given; the index, references, and bibliography seem admirable, and we have nothing but the highest praise for the accomplishment of a laborious and useful task.

A Catalogue of Mammals.

Catalogus Mammalium, tam Viventium quam fossilium, a Dr. E. L. Trouessart. Nova editio (primus completa). Fasciculus I.—Primates, Prosimiæ, Chiroptera, Insectivora. Pp. 218. Berlin: R. Friedländer und Sohn, 1897. Price 10 marks.

Few works of so unpretending a character have been of greater service to working mammalogists than the "Catalogue des Mammifères vivants et fossiles" published by Dr. Trouessart in 1879–86, and as it is now both out of date and almost unobtainable, the present revised edition of it will certainly receive a cordial welcome. The original edition was never completed, mainly, we believe, on account of the difficulty of finding a publisher, and it speaks little for French enterprise that the learned author of this list has now had to send it to Berlin to be published.

As may be gathered from the title of this fascicule, the author considers the Lemurs to be worthy of full ordinal rank. While many naturalists would have agreed with him on this point a short time ago, Dr. Major's discovery of *Nesopithecus* (*see* Natural Science, vol. x., p. 83) would perhaps make them pause, as its evident relationship both to monkeys and lemurs would seem to reduce the width of the gap between them.

The new edition is got up by Messrs. Friedländer in a style far superior to the previous one, is better printed, and, considering the thousands of names and references that crowd its pages, there seem to be remarkably few misprints. Indeed, in the 218 closely printed pages of which the part consists, we have only noticed some three or four ordinary misprints, although there are a score or more due to the practice of giving capital letters to specific names derived from persons. This plan, besides its other demerits, seems, owing to its destroying the uniformity of the printing, to offer exceptional facilities for such errors. Thus we notice *Scapulatus*, *Centurio*, *Samoensis*, *Satyrus*, *Gorilla*, *Nictitans*, and *sancti-johannis*, all printed in a manner contrary to the author's general rule in such cases.

The geographical distribution of each species has been carefully recorded; but it would have been a boon to workers, if the typical locality for each synonym had been printed separately opposite the name, instead of being lumped up with a "ditto" under the general range for the whole. For knowledge of the type-locality for every synonym of any species under review is a prime necessity to the worker, and the form of Dr. Trouessart's list is such that this information could have been, and we hope in future fasciculi may be, easily inserted without increase of space or labour.

Being avowedly a compilation, the subject-matter is scarcely open to criticism, and we can only congratulate Dr. Trouessart on the full and admirably selected number of references he has given and on the way in which he has been able to bring them absolutely up to date. We notice, however, that in the part on the Bats, no reference to Blanford's "Mammals of India" is given, while in that on the Primates the work is referred to continually, but as 1891 instead of 1888, the latter date being correctly quoted in the Prosimiæ and Insectivora.

We do not quite understand on what grounds Dr. Trouessart rejects all the names recently given by Mr. Lydekker to genera of bats and returns to the pre-occupied names used by Dobson, and similarly adopts *Brachyurus* for the Ouakari monkeys. If he does not admit that pre-occupation invalidates a name, he ought surely to use the widely-known *Troglodytes* for the Chimpanzee, instead of the technically correct *Anthropopithecus*. He has also often rightly re-named pre-occupied names himself.

These slight blemishes apart, Dr. Trouessart has given us a work which will have to be in the library of every mammalogist, and, when there, will probably be of more use than the great majority of more pretentious works. O.T.

A Guide to Geological Literature.

Catalogue des Bibliographies Geologiques. Par E. de Margerie. Pp. xx., 733. Paris: Gauthier-Villars, 1896.

This most useful work is produced under the auspices of the International Geological Congress, which in 1891 and again in 1894 appointed a Committee for the purpose. Though help has been received from the members of this Committee, every item has been checked and the whole arranged by Mr. de Margerie himself. There are nowadays so many bibliographies or bibliographic lists published for the guidance of the student, that they themselves require a bibliography. A geologist wishing to consult the literature of any subject will therefore begin with the present work, which will tell him, not what books have actually been written about the subject, but what books contain lists of the literature pertaining to the subject; to such books he will next turn, and so be guided ultimately to the literature itself.

The work is divided into a "Partie Générale" and a "Partie Régionale." The former contains histories and bibliographies of general geology, periodical bibligraphies, personal bibliographies (*i.e.*, author-catalogues), subject-bibliographies, and geographical geology including map-lists. The regional part is divided according to countries, and each of these is subdivided in much the same way as the general part, but contains in addition lists of Survey publications and library-catalogues. In both sections the term bibliography has received a wide interpretation; thus, the list includes H. S. Williams: "The scope of paleontology" which is really historical, as well as many items that have little more than a biographical interest. Bibliographies still in MS. are also entered: thus, we find C. D. Sherborn's "Index Animalium." There are three indices: of authors, subjects, and countries.

In a work so embracing, perhaps too much so, one could hardly fail to note many omissions, some of them serious. It seems, however, more profitable to send additions and corrections to Mr. de Margerie, at 132 rue de Grenelle, Paris, and we hope that other users of the work will follow our example. Copies of the work are presented to geologists who attended the congresses at Washington and Zurich, while others may purchase it for (we believe) £1.

Bibliographies.

Under the direction of the late G. Brown Goode, the Smithsonian Institution has produced, as *Bulletin* No. 49 of the U.S. National Museum, a "Bibliography of the published writings of Philip Lutley Sclater" (Washington, 1896, xx. and 135 pages). It is divided into: Introduction; Biographical Sketch; Part I., Chronological Catalogue of Separate Works (26 items); Part II., Chronological Catalogue of Papers (items 27–1239); Part III., List of new genera and families described; Part IV., List of new species described; Part V., List of species figured; Part VI., Index to subjects in Parts I. and II.; Appendix, List of Works, "published subsequent to December, 1894," down to the description of Count von Götzen's journey across Africa, contributed by Mr. Sclater to our last July number (items 1240–1287). It is only fair to say that of these 1287 items nearly half are Exhibitions, or Reports, or Abstracts of matters received by the Secretary of the Zoological Society, and brought by him before the Fellows at their evening meetings in the ordinary course of his duties. The rest form a remarkable contribution to knowledge from a man who has every prospect of a much longer career of activity. The typography and arrangement of the lists is compact and clear; but more emphasis might have been laid on dates. Clearly the items are not in strict chronological order, and we should not be at all surprised to learn that No. 1182, for instance, quoted as *Proc. Zool. Soc. London*, 1893, was really published in 1894. The work of copying and arranging the titles was performed by Mr. G. A. Doubleday.

The Geological Commission of the Colony of the Cape of Good Hope has issued, as its first publication, a "Bibliography of South African Geology," compiled by H. P. Saunders, Secretary to the Commission. It forms an octavo paper-covered pamphlet of 56 pages, and records 592 items. It is proposed to issue supplements with the Annual Reports of the Committee. This publication should prove most valuable to the numerous people now interested in the Geology of South Africa.

Willesden.

One does not usually expect to find in such a publication as the *Willesden Year-Book and Ratepayers' Guide*, 1897, sold at the humble price of 3d., any article of interest to our readers. We are the more pleased to see herein some interesting remarks by Mr. W. North on the River Brent. Among other things, he draws a comparison between the Brent as it was and as it is, greatly to the disadvantage of the present conditions. "The once clean and vigorous life of the stream is gone, and its place is taken by lowly forms that do their duty manfully, if one may use the term, yet vainly, trying to cope with the excess of food supplied to them by Philip's successors." Philip was a farmer, but his land is now given over to stray donkeys and gypsies. It certainly seems conducive neither to health, morals, nor the higher education of the people, that a stream which once served as background for the idyllic scenes of Lytton's "My Novel" should have been turned by the local authorities into nothing more than a stinking sewer. The Year-Book also contains a charming little article by Dr. F. A. Walker on the natural history of what was once "Price's Field," but is now obliterated by suburban villas of the usual type. There are also notes on the geology of the neighbourhood of Willesden, useful from containing particulars of a well sunk to a depth of 464 feet by Messrs. Welford & Sons, at their dairy in Elgin Avenue.

Scraps from Serials.

We have received *Bulletin* No. 6 (n.s.) of the Entomological Division of the U.S. Department of Agriculture, which contains the Proceedings of the Eighth Annual Meeting of the Association of Economic Entomologists, held at Buffalo in August, 1896. The address of the President, Mr. C. H. Fernald, deals with the Evolution of Economic Entomology, giving a brief history of insect ravages from the plagues of Egypt to the present day. There are a number of other papers bearing on the special work of the Association. Of general interest is an account by Mr. F. M. Webster of the areas in Ohio affected by the Chinch Bug in three successive years (1894-6). Although the successive appearance of the insects in different tracts of the state could be in part explained by meteorological conditions, several anomalous facts still await elucidation.

Timehri (vol. x., pt. 2, Dec., 1896) is full of interesting matters, chief among which is Mr. Quelch's "Migratory Birds in British Guiana." Some notes of fly-fishing in the colony are given by "Oxon," and from his account one sees that far more than ordinary patience is required, as the sand-flies and mosquitos are unwearying in their attacks, and before 8 a.m., the fierce horizontal rays of the sun burn back, arms, and hands, so that they become swollen and scarlet, while, reflected from the water, they take the skin off the nose. H. B. Van Ree writes practically on the culture and manufacture of Tobacco, while history has its exponent in the Hon. N. Darnall Davis, who gives an account of the Early English Colonies in Trinidad.

La Naturaleza, vol. ii., nos. 10 and 11, reach us from Mexico, both published in 1896. Herpetology is represented by the description of new species of *Geophis* and *Amblystoma* by Dr. Dugès, who also gives a note on the intestine of *Crocodilus americanus*, and a list of the Reptilia and Batrachia of the Mexican States; and by Dr. M. G. Peracca, who writes on the reproduction of *Iguana tuberculata*. Botany includes pp. 185-263 of Mociño y Sesse's Flora Mexicana, with index A–H; *Casimiroa pubescens* by Dr. Manuel M. Villada; and *Tabebuia donnell-smithii* by J. N. Rose. Juan Ignacio de Armas writes on the Zoology of Colon; Mexican Anthropology is dealt with by Herrera and Cicero; and Dr. José Ramirez' discourse to the eleventh Congress of Americanists, on the autochthony of the primitive races of America, is also printed. Dugès also describes a new moth *Ophideres raphael*.

We have received vol. III, no. I, of *Records of the Australian Museum*, edited by the curator, R. Etheridge, Jr., who contributes to it articles on the circular and spiral incised ornament on Australian aboriginal implements and weapons, including a spear from Angeldool, N.S.W.; on an *Actinoceras* from N.W. Australia, a genus of cephalopod not hitherto recorded from the Carboniferous rocks of that country; on the discovery of bones, chiefly of *Diprotodon*, in granitic detritus at Cunningham's Creek, near Hawarden, N.S.W. The late F. A. A. Skuse announces that *Peripatus leuckharti* has been discovered 72 miles S. of Sydney and near the Jenolan Caves, Blue Mts., N.S.W., and at a height of 2000 feet at Cunningham's Gap, South Queensland. The specimens stated by F. J. Bell and A. Sedgwick to come from Wide Bay, Queensland, were, says Mr. Skuse, really collected by himself in Brisbane. C. Hedley describes a North Papuan land-shell under the name *Thersites septentrionalis*. A. J. North, after having delivered himself of a new sub-species of *Psophodes crepitans*, gives some interesting ornithological notes. We note that Mr. Hedley gives his measurements in the metric system; cannot the editor follow his example, and induce Mr. North to do the same, or at all events not to give us measurements in decimals of an inch?

New Serials.

The *Natural Science Journal* is published monthly by the Atlantic Scientific Bureau, New Bedford, Mass., at 1 dollar per annum. There is a staff of six editors. No. 1, which has been sent to us, consists of 28 pages, including advertisements. There are short articles, platitudes, and poems, none of which are vulgar; and the following conundrum is offered to readers of the *Journal*: "What is an xld specimen?"[1] The *Journal* appears to be the organ of a company of dealers in natural history specimens. Despite the similarity of title, we do not think that it is likely to be confused with Natural Science.

We have received the second number (February 1897) of the *Scottish Medical and Surgical Journal*, an octavo monthly of 96 pages, edited by Dr. William Russell, under the direction of eleven leading Scottish medical men. It is published by W. F. Clay of Edinburgh, at a price of two shillings: annual subscription one guinea. The present part contains several good original articles of medical and surgical interest, with reports of meetings of societies, and abstracts of current literature.

The resuscitated Aeronautical Society, alluded to in our last number, has issued the first number of the *Aeronautical Journal.*

We also learn that Messrs. Marshall, Russell and Co., London, and Messrs. Hughes and Harber, Longton, Staffordshire, are to publish a quarterly, dealing, as its name implies, with *East Asia*. The editor is Mr. H. Faulds, of Fenton, Stoke-on-Trent. Papers dealing with natural history will be included, as well as an index to magazine and newspaper articles touching on Eastern Asia.

Further Literature Received.

Relics of Primeval Life, J. W. Dawson: Hodder & Stoughton. First Principles of Natural Philosophy, A. E. Dolbear: Ginn, Boston, U.S. Manuel de Geographie Botanique, Drude Poirault livr. 14–16: Klincksieck. The Ancient Volcanoes of Great Britain, A. Geikie: Macmillan.

The Danger to American Horticulture from the Introduction of Injurious Insects, T. D. A. Cockerell. The Comparative Physiology of the Suprarenal Capsules, S. Vincent: *Roy. Soc.* (proof). Rep. Proc. Soc. Nat. Hist. Cheltenham College, 1896. A National Department of Science necessary for the Co-ordination of the Scientific Work of the U.S. Government, C. W. Dabney, Jr.: *Science*. Civil Service in the Department of Agriculture, C. W. Dabney, Jr.: *U.S. Dep. Agric. Circ.* The National University, A Growth, not a Creation, C. W. Dabney, Jr.: *Science*. Nansen the Norseman, A. Crane: *Brighton Herald*. Descrip. of two new Murine Opossums. Phenacomys preblei, a new Vole from the Mountains of Colorado. Revision of the Coyotes or Prairie Wolves, etc., C. Hart Merriam: *Proc. Biol. Soc. Washington*. Some Spinning Activities of Protoplasm in Starfish and Echinus Eggs, G. F. Andrews: *Journ. Morphol.*, Abstr.: *Amer. Nat.* Bibliogr. of S. African Geol., Geol. Com. Cape of Good Hope. Mem. Austral. Museum, iii. The Ganodonta and their Relationship to the Edentata, J. L. Wortman: *Bull. Amer. Mus. Nat. Hist.* Guide to the Fossil Invertebr. and Plants, Dept. Geol. and Palæont., Brit. Mus. Nat. Hist. Rep. Colombo Mus., 1896, pt. iv.

Scot. Geogr. Mag., April; Amer. Journ. Sci., April; Victorian Nat., Feb.; Botan. Gazette, March; Chivers' New Book List, April; Feuille des jeunes Nat., April; Halifax Natural., ii., No. 7; Illinois Wes. Mag., March; Irish Nat., April; Westminster Rev., April; Journ. Mar. Biol. Assoc., iv., No. 4; Yorksh. W. Post, April 10; Journ. School. Geogr., March; Knowledge, April; Literary Digest, March 13, 20, 27, April 3; McClure's Mag., April; Naturæ Novit., March; Naturaleza (Madrid), viii., Nos. 9 and 10; Naturalist, April; Nature, March 25, April 1, 8; Nature Notes, April; Naturen, March; Photogram, April; Revue Scient., March 27, April 3, 10; Science, March 12, 19, 26, April 2; Science Gossip, April; Scientific Amer., March 13, 20, 27, April 3; Proc. Biol. Soc. Washington, vol. xi.; Journ. Coll. Sci. Imp. Univ. Japan, vol. ix. (2).

[1] We are given to understand that xld is dealers' jargon for "crystallised."

OBITUARY.

EDWARD DRINKER COPE.

BORN JULY 28, 1840. DIED APRIL 12, 1897.

AMERICA has lost one of its most eminent biologists in the person of Professor Cope, of Philadelphia. For some time the friends most intimately associated with him had noted with concern a rapid failing in his health and sad aberrations in his once great intellect. Born in Philadelphia, where he spent the greater part of his life, Cope received his earliest scientific training in the University of Pennsylvania. He completed his studies in Europe, and obtained the degree of Ph.D. at Heidelberg in 1864. In the same year he was appointed Professor of Natural Science in Haverford College in his native city. He resigned this position three years later, partly from ill-health, and between the years 1871 and 1873 took part in many geological exploring expeditions in Kansas, Wyoming, and Colorado. From 1873 to 1878 he was engaged in field-work with the Wheeler Survey, established by the United States Government. He was also Vertebrate Palæontologist to the Hayden Survey. At this time his private fortune was ample, and he was able to pursue his general biological and palæontological researches quite independently of State appropriations; but during the next decade misfortune overtook his investments, and in 1889 he gladly accepted from the Pennsylvanian University the Professorship of Geology and Mineralogy, which he held until two years ago, when he was transferred to the Professorship of Zoology and Comparative Anatomy. In 1895 he became President of the American Association for the Advancement of Science.

Cope's first paper, on the primary divisions of the Salamandridæ, was published in the *Proceedings of the Philadelphia Academy* so long ago as 1859, and subsequently to that date he made about 400 contributions to various scientific serials, in addition to his numerous important official publications issued by the United States Government. He not only treated of the facts of zoology, derived both from living and extinct groups, arriving at many fundamentally new and important results in classification; but since 1869 he also wrote much on the philosophy of the subject, particularly the problems of evolution. His latest book, "The Primary Factors of Organic Evolution," summarising much of his work, was reviewed in our January number. Cope, however, has left so enduring a mark on the progress of zoology that no brief obituary can do adequate justice to his genius. We propose to publish a more extensive appreciation of his work, by Mr. A. S. Woodward, next month.

THERE are also announced the deaths of:—Dr. KENNGOTT, Professor of Mineralogy in Zurich University, and author of "Handwörterbuch der Mineralogie," at Lugano; L. N. JOHNSON, formerly botanical instructor at Michigan University, on February 27, at Boulder, Colorado, aged 34; on February 23, in Paris, GEORGES VILLE, Professor of Plant-physiology at the Musée d'Histoire Naturelle, aged 73; JAROSLAV KOŠTÁL, assistant in zoology at the Technical High School, Prague; on March 14, aged 79, Dr. ROBERT HOGG, author of the "Fruit Manual," and formerly editor of the *Cottage Gardener*, subsequently the *Journal of Horticulture*.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

THE following appointments are announced :—

Dr. R. von Lendenfeld, to be Professor of Zoology at Prague ; A. W. Hughes, to be Professor of Anatomy at King's College, London, his place as Professor of Anatomy at the University College of S. Wales and Monmouthshire being taken by Dr. A. F. Dixon of Dublin University ; Dr. Ernst Gaupp, of Breslau, to be Professor Extraordinarius of Embryology at Freiburg.

G. F. Scott-Elliot, to be Lecturer in Botany at the Glasgow and West of Scotland Technical College ; Dr. H. Ross, to be Curator of the Botanical Garden at Munich ; Dr. W. I. Palladin, of Charkow, to be Professor of Botany at Warsaw ; Dr. P. Cannarella, to be Assistant in the Botanical Garden at Catania ; Dr. Luigi Buscalioni, of Turin, to be Assistant in the Botanical Institute of Rome University.

Dr. Carl Burckhardt, of Basle, and Dr. Leo Wehrli, of Zurich, to be Geologists at the Museum of La Plata.

Dr. Erich Wernicke, to be Professor Extraordinarius of Hygiene at Marburg University ; Dr. J. Petruschky, to be Director of the Bacteriological Institute, Dantzig ; Dr. Jas. Clark to succeed J. Muir as Professor of Agriculture at the Yorkshire College, Leeds.

WE rejoice to learn that Sir William Flower, who has just reached the age-limit of the Civil Service, has been granted by the Treasury an extension for three years. The work accomplished last year under the immediate supervision of the Director of the British Museum (Natural History) is proof, if proof be needed, that Sir William has not yet grown stale in office. We wish him all health and strength to carry out his numerous plans.

MESSRS. GODMAN and Salvin have offered to the Hope Department of the Oxford University Museum a valuable collection of butterflies, especially rich in species from Central America, a district poorly represented in the Hope collection.

THE Museum of Economic Geology in New York University has received series of specimens illustrating the geology of various anthracite basins, and numerous important mines in the United States. The new museum of the University is now nearly finished.

THE Colombo Museum, Ceylon, from the 1896 Report of which we quote largely in our Notes and Comments, shows a very satisfactory state of advance so far as the scientific part of the work is concerned. Extension of the buildings, however, is greatly needed, and other expenditure seems desirable. Bats, for instance, have taken to entering the building through the open ironwork above the doors, and caused much unpleasantness and even damage.

THE Glasgow Museum has recently secured from the executors of the late David Corse Glen, under very favourable conditions, that geologist's well-known collection of over 8,000 mineral specimens, as well as many rock specimens, fossils, recent shells, and other natural and prehistoric objects. It has also purchased Alfred Brown's fine collection of British shells. Among the donations we notice a specimen of *Spirula lævis* from the South Pacific, but it is not stated whether the soft parts are preserved.

The following are forthcoming lecture arrangements of the Royal Institution:—Dr. Tempest Anderson, four lectures on Volcanoes (the Tyndall lectures), April 27, May 4, 11, and 18, at 3 p.m.; Dr. Ernest H. Starling, three lectures on The Heart and its Work, May 25, June 1 and 8, at 3 p.m.; and Prof. Dewar, three lectures on Liquid Air as an Agent of Research, May 27, June 3 and 10, at 3 p.m. The Friday evening meetings will be resumed on April 30, when a discourse will be given at 9 p.m., by Prof. J. J. Thomson, on Cathode Rays; succeeding discourses will be given by Prof. Harold Dixon, May 14, Lord Kelvin, May 21, Prof. H. Moissan, May 28, Mr. W. H. Preece, June 4, Mr. W. Crookes, June 11, and other gentlemen.

Through the instrumentality of Mr. Frank Finn, three specimens of the Indian pigmy goose (*Nettopus coromandelanus*) have been added to the Zoölogical Society's collection in Regent's Park. Although not by any means a rare species, this bird has never, says the *Daily Chronicle*, been exhibited previously in any zoological collection anywhere, all attempts to bring it alive to Europe during the past quarter of a century having failed ignominiously.

Mr. H. M. Vernon, of Merton College, Oxford, and Rolleston prizeman, has been elected to the Radcliffe Travelling Fellowship of the University of Oxford.

The Report of the Director of the Marine Biological Association states that between August and February last seven naturalists worked at the Plymouth Laboratory, which was also visited by four fishermen making a tour to the various fishing centres of England and Scotland, under the auspices of the Aberdeenshire County Council and the guidance of Mr. R. Turnbull, B.Sc. Trawling in the bays on the Devonshire coast showed that in January the larger plaice had left, probably for the spawning-grounds, whilst the fish from the estuaries and close inshore had come out into the bays. Experiments have been started for determining the surface drift in the western portion of the English Channel by means of soda-water bottles, which are to be put overboard by torpedo-boat destroyers cruising in the neighbourhood. The steam launch "The Busy Bee" has cost £700, towards which amount £537. 14s. has, so far, been subscribed. During the Easter Vacation, Mr. W. Garstang has again conducted the course in marine zoology started by him last year.

Similarly Prof. H. C. Bumpus has been taking seventy students of comparative anatomy for an excursion on Narragansett Bay.

The South-Eastern Union of Scientific Societies will hold its second annual Congress at Tunbridge Wells on May 21, 22. The following papers are notified:—Rev. J. J. Scargill, "What can be done to save our Fauna and Flora from unnecessary destruction?"; S. Atwood and J. W. Tutt, "How can the Technical Education Grant assist local Scientific Societies?"; W. Cole, "Local Museums"; G. S. Boulger, "The Committee on Field Clubs"; H. G. Seeley, "Current Bedding in Clay"; H. E. Turner and W. Whitaker, "Search for Coal in S.E. England"; W. J. Lewis Abbott, "History of the Weald in special reference to the Age of the Plateau Deposit." At a conversazione, given by the Mayor, Dr. Rowe will demonstrate the method of preparing Chalk fossils described by him in Natural Science for November, 1896. Members of Natural History or Scientific Societies in the S.E. District affiliated to the Union are admitted on payment of half-a-crown; delegates of similar societies pay five shillings; others interested are admitted for three shillings and sixpence.

The Literary and Philosophical Society of Sheffield intend to celebrate the semi-centenary of Dr. H. C. Sorby's scientific activity, by having his portrait painted. Subscriptions will be received by A. T. Watson, Assay Offices, Leopold Street, Sheffield.

The programme of the Hull Scientific and Field Naturalists' Club for April, May, and June, shows a goodly list of excursions and papers. Since the club is affiliated to the Yorkshire Naturalists' Union, its members can avail themselves of the circulars issued by the Union descriptive of the natural history of various districts visited. The Recorders of this Club take note of all important finds made by its members, and undertake the identification of doubtful specimens.

THE Belfast Naturalists' Field-Club has again arranged with Professor Grenville Cole for a course of instruction in practical geology. This Club does useful work in organising the scientific activities of its members. Its geological section has been investigating the erratic blocks of the neighbourhood on the lines of the International Boulder Committee of the British Association.

THE Limerick and Clare Field-Club has, we learn from the *Irish Naturalist*, added archæology to its field of work, with the immediate result of raising its list of members from 60 to nearly 200. We trust that the archæologists intend to do serious work, such as is now being done by so many of the Irish Field-Clubs, and that they are not merely on the look-out for picnic excursions.

FROM the *Report* of the Cheltenham College Natural History Society for 1896 we learn that the Old Chapel is about to be used as a Library and Art Museum, so that it will perhaps be possible to devote the present Museum room entirely to natural history, and so to arrange the objects more adequately. The Report contains some faunal and floral lists, giving dates of first flowering, of arrival of birds, and of egg-taking. Such observations are of real value in themselves, and of ever greater value in their educational effect.

Similar work used to be done by the Winchester College Natural History Society, which, however, if not actually extinct, has issued no *Report* for some sixteen years. Still the geologists, under the guidance of Mr. C. Griffith, have occasionally given evidence of fruitful activity. Possibly the new museum, which is to be opened on June 16, will afford opportunities for natural history work that have hitherto been lacking.

ON May 18, Mr. Andrée again leaves Gothenburg for Spitzbergen, in the gunboat "Svensksund," to resume his attempt to cross the polar ocean in a balloon. The balloon has been enlarged, so as to remain dilated for six weeks. Dr. Ekholm, however, considering that it is not strong enough, has retired from the expedition, and his place has been taken by Mr. Knut Fränkel, á civil engineer. It is expected that all will be ready for a start by June 20. A similar attempt is projected by the French aeronauts, Messrs. Godard and Surcouf, who intend to start next spring in a baloon with a capacity of over 35,000 cubic feet.

Science for March 26 announced that a tariff Bill then before Congress imposed an *ad valorem* duty of 45 per cent. on scientific apparatus "imported especially for colleges and other institutions," of 25 per cent. on books imported for public libraries, on books "printed in languages other than English," on books "printed more than twenty years," on books "devoted to original scientific research," and on works of art.

It is also announced that the State of Minnesota intends to appoint a State Phrenologist, who is to examine at least 2,000 heads *per annum*.

We do not know which of these two pieces of legislation is the more ridiculous or astounding; there is no question but that the former is the more dangerous, and we rejoice to learn from a later number of *Science* that some modifications are to be introduced into this extraordinary Bill.

Octopus giganteus, Verrill (*see* NATURAL SCIENCE, March, p. 207) turns out to be nothing more than a mass of blubber, probably from the head of some huge cetacean. The moral of this is that one should not attempt to describe specimens stranded on the coast of Florida, while sitting in one's study in Connecticut.

CORRESPONDENCE.

Time and Change in Foraminifera.

I cannot permit the observations made in the March number of Natural Science (p. 153 *ante*) respecting some criticisms I made in the *Revue critique de Paléontologie* on the recent work of Messrs. Rupert Jones, Burrows, Chapman and others on the Foraminifera of the Crag, to pass without making some reply thereto.

I am sorry not to have expressed myself "with the lucidity of my nation"; what I meant to say was that descriptions of fossils to a large extent fail when they do not recognise separate species as such. The palæontologist should seize upon every convenient opportunity to distinguish species where they characterise horizons or geographical provinces. In the present instance I may remark that when such a careful observer as Mr. Schlumberger definitely states that he can separate Eocene Foraminifera from those now living, and describes and figures the differentiating characters, palæontologists surely must accept his work or say in what way it is at fault. Mr. Schlumberger shows us that under the name of *Biloculina ringens*, which Lamarck proposed for an Eocene species from the Paris Basin, two living species have been confounded, to which he gives the names *Biloculina sarsii* and *B. bradyi.* In like manner the living *Biloculina bulloides*, d'Orbigny, must be distinguished from the fossil species and becomes *B. anomala*, Schl. and *B. lucernula*, Schl.; and many other examples could be quoted. Palæontology teaches us that throughout long periods of time animals usually undergo some change, so that before assigning a species to one belonging to a different stratigraphical horizon, or to another geographical province, it is necessary to study that species very carefully and to give it the benefit of any differentiating characters that may be found to exist. It is better to err in recognising these differences as of value in separating species, than to submerge them and thus invite worse confusion in the distinction of species. I am a little doubtful whether the learned authors of the "Crag Foraminifera" have not contributed to our difficulties in the manner last mentioned.

Gustave F. Dollfus,
Paris, *March* 18, 1897. *For. Corr. Geol. Soc.*

[We are in full accord with the position assumed by Mr. Dollfus in this interesting letter. It must of course be left to the specialists engaged in the discussion to decide as to the specific independence of each form studied. All that we maintained was that difference of locality or horizon was not in itself sufficient ground for separating species. Some palæontologists do not seem to have very clear ideas on this matter.—Ed. Nat. Sci.]

Polychæta in the Cambridge Natural History.

May I point out some mistakes on the part of the writer of the Review, in the March number of Natural Science, of the section on Polychæta contributed by Dr. W. B. Benham to the second volume of the Cambridge Natural History.

The Reviewer draws up a somewhat alarming list of "Certain errors (*inter alia*) in general anatomy." The inclusion of *Haplobranchus* amongst the hermaphrodite Amphicorinidæ is certainly erroneous, as pointed out; but let us examine the other alleged "errors" *seriatim.* "The septa of the body are not so complete as to isolate the compartments . . the parapodia are essentially hollow organs, facts not explicitly stated"—if Dr. Benham's statements are not sufficiently clear on these points, they are not erroneous, but incomplete. "Prostomial tentacles are probably not restricted to the sub-order Nereidiformia"—a fact the reviewer might have learnt even from his cursory reading of the book, since Dr. Benham clearly states (p. 260, figs. 133 and 134) that "in all the Nereidiformia, as well as in Sabelliformia and Chlorhæmidæ, the prostomium bears . . . tentacles," and again (p. 334) "The prostomium carries . . . several green tentacles," etc. If, as the reviewer states, Meyer (not "Mayer") was of opinion that "the tentacles of Spio may be prostomial," it can be answered that Benham has re-investigated the question, and is of opinion that they are peristomial. This may or may not be an advance in our knowledge; it cannot be called an "error." "The modified chætæ of *Capitella* are confined to the male"—as explicitly stated on p. 331. "Modified anterior chætæ occur in other Polychæta"—they are described and figured in several places (p. 315, 330, etc.). "All polycirrids are devoid of a vascular system"—it therefore cannot be an "error" to say (p. 253) that in "*Polycirrus hæmatodes* . . . the vascular system is absent." "In *Capitella* some, not all, of the cœlomic corpuscles are red"—it is nowhere explicitly stated that all the cœlomic corpuscles are red (p. 253), though no doubt it might have been clearer had the author stated that the presence of red corpuscles does not exclude the ordinary leucocytes mentioned as of universal occurrence in the previous paragraph. "The siphon is found in some Eunicidæ"—if this is not mentioned it is an omission, not an "error." If the author does not describe the longitudinal nephridial duct of Lanice—it is again not an "error," but an omission, perhaps intentional. "*Capitella* has one pair of gonoducts"—a peculiar fact among polychætes given on p. 331. "Other genera . . . more than one pair"—as mentioned on p. 305. The Reviewer complains that the development of the nephridia of *Polygordius* has not been described; he is apparently ignorant of the fact that the account we have (from Hatschek) is of very doubtful accuracy, and has not been confirmed. Moreover, as mentioned by Dr. Benham in the book, a detailed account of *Polygordius* and its development is to be found in another recent popular and accessible work, and it may be added in almost every text-book since Balfour's. I must confess that I feel somewhat relieved, when not always confronted with the same zoological "chestnuts."

Having levelled his false accusations at the author's head, the Reviewer has not a word of praise left for the many excellent qualities of the book. Not only does he appear quite to misunderstand the class of readers for which the work is intended, but also to be quite incapable of appreciating the originality and usefulness of the contributions. He is apparently not aware that this is the *first modern attempt* to deal in a broad yet thorough manner with the group Polychæta, and that it is undoubtedly the best general account of them given in any single work in any language. As a student of the anatomy of the polychætes, rather than dwell on a few incomplete statements, I feel bound to express my gratitude for, and wonder at, the immense amount of accurate and useful information the author has managed to condense into the hundred pages allowed him by the editors. Hitherto the difficult group Polychæta has been notoriously neglected in text-books—nowhere else can we find such clear and excellent illustrated accounts of the many puzzling questions in the morphology of these worms—such as those relating to the head, parapodia, chætæ, and gills.

Oxford University Museum. Edwin S. Goodrich.

[The Sabelliformia and Chlorhæmidæ were, the Reviewer finds, duly included with the nereidiformia in his Notes. The names must have been accidentally omitted in copying out for the press, and the omission, to the Reviewer's most sincere regret, passed unnoticed in the proof at a time of great stress of other work. The criticism was directed to the words, p. 261 *ad fin.*, as to the non-existence of prostomial tentacles in "the other suborders." In the general description of chætæ (pp. 266–8), it is said on p. 267, *ad fin.*, that "certain modifications" of them "presented by various worms deserve mention." The worms thus singled out are *Polydera* and *Chætopterus* with exceptionally strong chætæ in one segment, *Capitella*, and *Aphrodite* with its iridescent bristles. As to *Capitella* the words run, "In *Capitella* those of the notopodium of the eighth and ninth segments are specially modified; they are analogous to the copulatory chæta of Oligochæta." The passage p. 331 relates to the species *Capitella capitala*. The words as to the red corpuscles of the Capitelliformia are "the red corpuscles become coloured with hæmoglobin" (p. 253), and "the cœlomic corpuscles are red" (p. 305)—words which are certainly taken to mean *all corpuscles*. As to the gonoducts of the same suborder (not of "other genera" only, as Mr. Goodrich has it), it is said, p. 305, "special genital funnels exist in more or fewer of the anterior segments of the hind body." The passage, p. 331, relates to *Capitella capitata* alone, a smentioned before. The Reviewer has always regarded incomplete statements of fact, however they may arise, as very dangerous, and in nine cases out of ten as erroneous, especially if they appear to be complete or are certain to be taken as complete; and experience proves that they are a most fruitful source of error to others. He is well aware that Hatschek's observations on the larval *Polygordius* have not been confirmed, *nor yet disproved*, a position in which stand many most important and much-used facts. He is sorry to read the last paragraph of the letter. The writer has assuredly forgotten in particular the opening remarks of ample praise in the review. They were applied to the contents of the whole volume, and therefore to Polychæta; and they have at least sufficiently gratified the publishers of the Natural History to be quoted in a recent advertisement of the work.]

Mr. Berthelot on Science and Morality.

There are one or two points brought forward in Mr. Berthelot's recent work "Science et Morale" that ought to be carefully considered, and are especially interesting and important as bearing on the attitude of certain eminent men in this country towards science and culture. The first point relates to the ultimate predominance of science in all human affairs. "Science," says Mr. Berthelot, "dominates all things, it alone is of any definite utility. No man, no institution shall henceforth have an enduring authority if they do not conform themselves to its precepts." Now, it may be remarked that, although in a mundane sense science actually does *dominate* everything, it is scarcely correct to hold, as Mr. Berthelot does, that morality has no other basis than that which science furnishes. The true basis of morality, humanly speaking, is humanity, *i.e.* it is the emotions, not the intellect—it is the native-born appetency of the heart, which, however, being a mere blind force and not a guiding law, will inevitably lead to ruin unless it be authoritatively dominated by the highest intelligence, *i.e.* by science. Hence, moreover, it is just this misconception involved in the attributing everything to science, or rather in not confining it strictly within its own functional limits, that is the true cause of the enmity exhibited towards the ecclesiastical spirit by Berthelot, Huxley, and other eminent men. The ecclesiastical spirit is, like everything else, as these castigators would admit if they could only see it, "dominated by science"; and where this influence fails, it degenerates into a form of selfishness, which certainly merits all the vituperation shed on it by clever platform rhetoricians. Mr. Berthelot, like Huxley, is a good hater of "false pretence and unscientific method," but it is not very creditable to these men that they should seek to fasten these characteristics on the ecclesiastical spirit properly so-called, or on its sound-headed impersonators. In fact, it is exactly this sort of thing that has inflicted on science in our country the most deterrent and retrograde blow that it has perhaps ever received at any epoch. Why cannot our fashionable orators and writers of lay sermons understand this much, viz., that the kind and quality of intelligence and ideas that lead to the correct and proper study of science lead also to correct and proper ideas anent religion and morals.

If the foregoing views be correct, it will be tolerably easy for anyone to understand how it is that so much of Mr. Berthelot's powers and talents have been diverted from the channel of pure chemistry and veritable science to that of mechanics. In point of fact, it would seem, according to his later works at all events, that his real *forte* is that of a mechanical engineer, *i.e.*, the skilful, accurate, and definite application of science to industry. If this be so, then we can further understand most of "the rest of it." For instance, his hostility to the divine origin of moral laws, his failure to see that no amount of what he calls "le mecanisme de la preparation" for admission into the higher schools can seriously and permanently affect the health of an originally sound mind or sound body. It may be averred with considerable assurance that the strain inflicted on youthful candidates, save and unless they are swayed by monetary and selfish considerations, or are compelled unnecessarily to undergo extra physical exertion at the same time, does not, as Mr. Berthelot maintains, produce "an abdication of their individuality, a hindrance of their normal development, or a loss of their curiosity and love of original reflection."

P. Q. KEEGAN.

London, *March* 12, 1897.

ANGLO-SAXON *versus* GRAECO-LATIN.

I HAVE been much pleased with the editorial "Notes and Comments" in which, from time to time, you have rebuked the pedantry of certain writers on science, although I have sometimes thought you fail to make allowance for the hard fate which drives them to endless search for new technical words, even when these seem needless and shocking to the mere literary man.

I know I shall have your sympathy if I tell of my own humiliation. This is the cause of my grief. I had written that the heart of the grasshopper is under the middle line of its back: and a compiler of books has, by his emendations, given me to understand that scientific precision demands that I should have drawn on the resources of dog-Latin and bastard Greek, and said: "The pseud-haemal vessel is pseudo-ventral to the pseudo-dorsi-meson." I have taken the correction for my own good, with chastened gratitude; and I note with pleasure that while you express your well-founded doubt of the existence in nature of anything to give the name to, you nevertheless hold fast to the good old Saxon name "Telegony" (NATURAL SCIENCE, Feb., 1897, p. 80).

Johns Hopkins University, Baltimore, Md., U.S.A. W. K. BROOKS.

W. G. BINNEY AS A MALACOLOGIST.

WITH reference to the Note in our January number, on Messrs. Pilsbry and Vanatta's Revision of North American Slugs, Mr. T. D. A. Cockerell protests against our statement: "Here for the first time the introduction of new species is accompanied by descriptions and figures of the internal parts," &c. He complains that we are ignoring the works of W. G. Binney. Binney's work was good so far as it went—very good for a pioneer; but our sentence continued—"in a manner similar to that employed by such English and Continental malacologists as Simroth, Lessona, *et alii*." Binney's descriptions and figures are, we maintain, *not* similar to those of the writers we mentioned, or to those of Pilsbry and Vanatta. They are in some cases far too short, and place too much reliance on external characters and those of the lingual ribbon.

NOTICE.

TO CONTRIBUTORS.—*Communications for the June Number to be addressed to the* EDITOR *of* NATURAL SCIENCE, *at* 22 ST. ANDREW STREET, HOLBORN CIRCUS, LONDON, E.C.

The July Number (being the first of Volume XI.) will be published by MESSRS. J. M. DENT & CO., 67 ST. JAMES'S STREET, S.W., *to which address the Editorial Offices will be transferred.*

TO THE TRADE.—NATURAL SCIENCE *is published on the 25th of each month; all advertisements should be in the Publishers' hands not later than the 20th.*

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NOTES AND COMMENTS.

Change of Address.

IT is now two years and a half since we attempted to make a bold push for success by assuming more direct control of the publishing of this Review. While we have succeeded, at least as much as we anticipated, in increasing our circulation, and in decreasing our financial loss, we have nevertheless found the combination of publisher's with editor's duties too great a tax upon our time and strength. It is therefore with much pleasure that we draw the attention of our readers to the announcement that in future the publication of this Review will be undertaken by Messrs. J. M. Dent and Co.; for we feel that the co-operation of this well-known firm will enable us to put yet more energy into the editorial management of Natural Science. We shall continue to conduct it on the same lines as have been followed from the beginning, with the same editorial staff. Further, we shall hope to receive, in the future as in the past, that kindly help from men of science all over the world, for which our most heartfelt thanks can but poorly repay them. We trust, however, that they, as well as our readers, will recognise that our efforts, if occasionally mistaken, are sincere; and that our aims are such as to demand the cordial help of all who desire the advancement of science and the independence of scientific thought.

It is hoped that the July number, which begins our eleventh Volume, will be specially attractive, by reason of the eminence of the contributors to it, and the illustrations that are being prepared. Orders for Natural Science, and all communications to the Editor, should henceforth be addressed to the care of Messrs. J. M. Dent and Co., 67 St. James's Street, London, S.W.

The Scientific Method in Education.

It is one of the most hopeful signs of the times that, throughout that half of the community which is engaged in teaching the other, a strong desire is being manifested that the rising generation may be not only instructed but also so trained that all the mental faculties may be cultivated together, and thus enable "men and women to carry on the work of their lives with the greatest possible advantage to themselves and the community." We quote from a little book[1] that has recently come before us, in which are discussed by various experienced teachers the methods of handling those subjects usually included in the curriculum of Secondary Schools.

It is interesting and most encouraging to find that the insistence by the scientific on the paramount importance of practical work has not been without effect on the devotees of the "humanities." Well do we remember in the days of our childhood learning by heart rules and exceptions to rules of gender, syntax, etc.—can we not reel off pages of them even now after years of disuse and application to other things? and, pray, to what purpose? However, *tempora mutantur*, and now we find Mr. Paton, when speaking of Latin, using such language as—"It is a good thing to take first selected Latin sentences, to go through these with the class, giving them sufficient clue to discover the proper meaning, and then making them deduce the rule for themselves. What they have thus found out and formulated for themselves is far more likely to impress itself upon the memory than if they are told, 'This is the rule, here is an example, now apply it to the sentences given.'" The advice is excellent;—we rather wonder how many classical masters and mistresses make any endeavour to work on such lines.

To pass on to those subjects with which we are more nearly concerned. Professor R. W. Stewart, in the article on "Physical Science," while advocating methods with which we agree, introduces the subject of the proper mental attitude of the beginner, and pronounces against the "research attitude," saying "until the scholars have acquired a little knowledge of the subject it is useless to expect them to reason for themselves in the way necessary to follow out even the simplest research." With this we venture to disagree. Pupils cannot begin too young to find out for themselves, and to become aware that by employing their senses they can acquire knowledge and power. An instance will make our point clear: the differences between solids, liquids and gases will probably come early in any elementary course; now if specimens of each are placed before a class and the teacher *points out* the differences, truly the class will learn what he tells them and repeat their lesson more or less correctly. But surely the "research attitude" is possible here, and on every account to be preferred. A teacher who is worth his salt will assuredly by the form of his questions lead the class to notice what differences exist in this particular and that, and finally get the entire class, each pupil for himself, to set down in writing a formulated expression of the facts ascertained, in response to queries, by the united observa-

[1] "Aims and Practice of Teaching." F. S. Spencer, M.A., Cambridge University Press. Price 6s.

tions of the juvenile researchers. True it is useless for the inexpert to observe experiments without knowing what to look for; but we maintain that to *tell* a class what to look for is not much better. A guide is certainly required, but this should be provided by the form in which the question is addressed.

An Appeal to the Teachers.

Professor Stewart strikes a true note when he complains that too often the only scholars who are allowed to take up science are those who are not likely to do well in classics or mathematics; and this affords the opportunity of calling attention to the scant amount of time allotted to science as a subject of general education, and of advising science-teachers to take care that their subject is "not only properly taught under the best possible conditions, but that it is also associated with that amount of literary and mathematical instruction which is most essential to its fullest development."

These, among other ideas, we attempted to impress on the masters of our Public Schools in a recent editorial Note (Natural Science, May 1897, p. 292). But we gladly reiterate this call to arms, for such we fear it must be if the existing preponderance of other subjects and the present one-sided education of our middle and upper classes is ever to be re-adjusted. Some ground, perhaps, has been gained; but there is need for all responsible for the teaching of science to organise their scattered forces, and to sound an advance all along the line. The book before us offers a weapon. If any one would convince an obdurate opponent, let him put before his notice the article by the master-hand of Professor Armstrong, who has contributed a most valuable chapter on the general principles of scientific teaching and the particular way in which chemistry may be dealt with by young students. He has in fact put together a most complete object-lesson couched in language that cannot fail to convince. It was once our privilege to place in the hands of an old man, who was devoted to teaching, but quite ignorant of science, a copy of *Nature* containing Professor Armstrong's well-known elementary syllabus. "How magnificent! had I learned to teach like this, I should have done more good in the world!" was the criticism we received.

The Educational Value of Botany and Physiology.

In the book under review, biological subjects receive their due share of attention. It cannot be denied that botany is an excellent subject for class-teaching. The apparatus required is very simple; the material is cleanly and frequently of considerable beauty; it is possible for every student to have his own specimens and to work them out for and by himself; much of the subject can be learned out of school, and a growing interest added to every walk; the powers of observation, manipulation, drawing, literary expression can all with proper treatment of the subject be trained and brought towards efficiency. One of the greatest 'bug-bears' of botany is the host of technical terms; the value of these can, however, soon be taught by the use of periphrases first, and the subsequent introduction of the abbreviated expression, which can often be coined by members of a class possessing even but a slight knowledge of Greek and Latin. Interest in the subject can be readily roused and increased by slight digressions (if such a term can be used), dealing with the relation of plants to soils, and insects and other animals. Indeed we are of opinion that in many schools no more stimulating scientific subject can be included than this, though we doubt if as much system can be introduced into the training thus afforded as into physics or chemistry.

It is not often that there is found an advocate pleading for a place for human physiology in a school course, and yet, as the Master of Downing College points out, it is a subject which stands quite alone in its unique power of attracting attention and exciting interest. The opponents of physiology are not few, and the objections urged against it are certainly not entirely groundless. Improperly handled it may lead to an undesirable self-consciousness and loss of the blissful ignorant innocence of childhood—this the abuse of the subject. Properly used there are few if any subjects which so readily arouse the interest; there is an undeniable fascination in knowing even a little of how the fearfully and wonderfully made machine performs its work.

Further, frequent opportunities occur of introducing other subjects, such as the action of levers, elementary facts in hydrostatics, the phenomena of refraction of light and formation of images, the laws of sound—instances might be multiplied indefinitely. We have ourselves more than once conducted courses of lectures in physiology to pupils of from 15 to 17 years of age, and can testify to the remarkable keenness invariably exhibited and to many budding medical students who then for the first time realised that the study of physics and chemistry was well worth the candle if it led to an understanding of the human body.

Boys and girls are but human, and require, like many of their elders, some stimulating and enlivening motive for their work: if a boy can see, or thinks he can see what good his lessons are going to be to him, he will work with zeal; but what he cannot at all easily be made to realise is that mental training in the abstract will create in him habits and powers that will stand him in good stead in after life whatever be his calling. It is the *cui bono* attitude that is the greatest difficulty with most teachers. Boys do *not* desire to be educated, it is enough for them if they be instructed in those subjects and the particular parts of subjects which will 'pay.' Physiology has this great point in its favour: unknown to the pupil, a sufficient and absorbing motive is introduced to help him over the more remote and arduous paths leading to a knowledge of that most engrossing subject—himself.

The Illinois Fresh-water Biological Station.

The sound practical work being done in the United States in fresh-water biology has been alluded to more than once in these pages, and now we have still further evidence of the activity displayed in this direction in the shape of the Biennial Report of the Biological Experiment Station of the Illinois University, and a batch of papers giving some of the results obtained since the establishment of the Station in April, 1894. It may be pointed out in passing that a particular interest attaches to the work of this station, since it is situated in a district, at Havana on the Illinois River, having, on a larger scale to be sure, several features in common with our own 'Broads' country, and the experience gained in America should be carefully considered by all those interested in the proposal to found a fresh-water biological station in East Anglia.

Much of the work so far undertaken at Havana has necessarily been of a preliminary nature, being more especially directed to a determination of the local aquatic fauna. The results obtained, however, even if falling short of the ultimate ideals cherished by the energetic director, Professor S. A. Forbes, are by no means to be despised, and constitute in themselves really valuable contributions to science. Mr. C. A. Hart's paper on the Entomology of the Illinois River and adjacent waters, for example (referred to in Natural Science, vol. viii., p. 228, April, 1896) is an exceedingly useful piece of work in a much neglected field, and those whose appetites have been whetted by a perusal of Professor Miall's delightful book, "The Natural History of Aquatic Insects," will find in Mr. Hart's work much additional information, including keys to the genera of aquatic insect larvæ. Other published papers deal with Protozoa and Rotifera, with oligochæte worms, with parasitic Hymenoptera, and with Ostracoda, while many others are in preparation.

Besides these faunistic studies, an excellent beginning has also been made with the extensive problems of the inter-relations of the different organisms to one another and to the environment. A regular system of collecting at a number of carefully selected sub-stations has been maintained without intermission since the opening of the station. It will give some idea perhaps of the thoroughgoing nature of this work when it is stated that of quantitative plankton collections alone about a thousand have already been taken, while the total number of collections, including insects, exceeds 12,000. When this rich accumulation of material shall have been worked out (and a large part of the work has already been accomplished), we may expect to be put in possession of the finest mass of data concerning the distribution and periodicity of fresh-water organisms ever brought together.

"Pump Plankton."

The only article yet issued on the plankton work of the Illinois Biological Station, is an introductory one by Dr. Kofoid, the station superintendent, dealing with methods and apparatus. (*Bull. Ill. State Lab. Nat. Hist.*, Vol. v.) Owing to the shallowness of the water in the district investigated, it was found impossible to carry out the collection of plankton for quantitative determination in the usual way, *i.e.*, by vertical hauls of the plankton net. A method of oblique hauls was consequently devised and used for some time, but it appears to have been rather troublesome in practice, and was abandoned in favour of the pumping method. Although collections of plankton organisms have been previously made by pumping water through a net, to the Illinois Station belongs the credit of having first developed this method for use in quantitative work. The advantages claimed for this method over those relying upon the drawing of a net through the water are, mainly, greater accuracy and wider application; for in the first place the exact volume of water passing through the net from the pump can be exactly ascertained, whereas with a net hauled through the water this amount can only be calculated approximately, and secondly, the collection of plankton by means of a pump and hose can be carried on in very shallow water, and even among masses of vegetation. There seems no doubt that the introduction of the pumping method is an important departure in plankton investigation. We can only see one theoretical objection to its use, namely, that the water sucked up by the pump at any particular level does not wholly come from that level but also in part from higher and lower levels. This is probably, however, a point of little practical importance in the majority of cases. Nevertheless we do not think that the pumping method will altogether supplant the earlier practice, for, in addition to its much greater initial cost, the new method demands the attention of two men, one to work the pump and the other to haul in the hose, and further, it could not be easily applied to very deep water owing to the large amount of hose required.

Another important modification introduced into the plankton work at the Havana Station has been the employment of a centrifugal machine for rapidly precipitating the plankton contained in the collections, instead of allowing it to settle of its own accord for twenty-four hours. This innovation not only saves time, it gives more reliable results than heretofore. It was found by experimenting with the same collection of plankton that the settling method gave results differing from one another by as much as 30 per cent. in extreme cases, whereas the action of the "centrifuge" was quite uniform, the plankton always being subjected to the same amount of compression.

We trust that it will not be long before we see similar investigations being carried on in our own country. *The Lancet*, commenting on our recent articles directed to this end, remarks with truth: "The movement is surely a worthy one, and in the interests of science and even national affairs should be supported." Our reinvigorated contemporary, *The Zoologist*, also says in reference to our articles: "This investigation has already been commenced in America, Bohemia, Germany, and Russia, and it is quite time England joined that scientific concert." We shall be glad to receive the names (not necessarily for publication) of those who are anxious to support the proposal, so that more active measures may be devised. Communications should be addressed to the Editor, at 67 St. James's Street, S.W.

The Classification of Rodents.

We have received from Mr. Oldfield Thomas a list of "The Genera of Rodents: an attempt to bring up to date the Current Arrangement of the Order" (*Proc. Zool. Soc.* for 1896, pp. 1012–1028). Unpretending though this list be, it deserves more than a simple mention. In the introduction the author states that he has prepared his paper to help such persons as museum-curators and writers either of a text-book or of a faunistic work. And again: "the special object of the list is the proper allocation of the genera in their respective subfamilies, and I have purposely been as conservative as possible with regard to the groups of higher rank, following Alston wherever there has not been very special reason for departing from his arrangement."

We therefore take the paper for what it is intended to be, and limit our remarks on the "groups of higher rank" chiefly to the innovations proposed. And here, frankly, we do not like the "Anomaluri" and "Aplodontiæ" as they stand in the list. Mr. Thomas does not think it at present advisable to consider these groups "as of the same rank as the Sciuromorpha and the others;" but in reality they figure in the list as though they had the same rank. Thus, the gap between them and the Sciuromorpha, etc., contrary to what ought to be the aim of classification, is made wider than in the "current" arrangements; whereas Winge, to whose classification the author objects, has endeavoured to bridge over many of these gaps, in a work whose chief demerit is the fact that it is written in Danish.

With all due respect for the authority of Mr. Thomas, we further think, on the same grounds, that the reasons given for the maintenance of the "Sciuromorpha" and the other "morpha," namely, that they are "time-honoured" and "convenient," are not free from objection. From the beginning, when the "morpha" were not yet time-honoured, the "awkward" families have objected to the Procrustean beds in which they were being forced, and it was to be foreseen that, when the mists by which they were surrounded should be somewhat dispelled, these very stumbling-blocks, together with extinct forms, would furnish the clue for a more natural classification.

To turn to the arrangement of the groups. When we see the Anomaluri at the top of the list and the Lagomorpha at the bottom, we should like to have been told whether, in the author's opinion, the former are the highest of the Order and the latter the lowest, or *vice versâ*. And are the Aplodontia to be considered as intermediate between the Sciuromorpha and the Myomorpha, as their place in the list seems to imply?

Where unfamiliar names are introduced, on the ground of priority, care has been taken to add the better-known terms in brackets. In matters of nomenclature we agree that there is *dura lex sed lex*; but there is a limit to everything, and *Coendou*, for example, is beyond that limit, since it is merely a gallicised barbarism.

This "Attempt," however, must we repeat, be taken for what it is meant, and here we think that not only the category of persons mentioned by the author will be thankful to him, but the paper will be still more appreciated by all specialists of the Order. Numbers of genera have not been known hitherto otherwise than by quite insufficient diagnoses, the authors being apparently themselves often uncertain where to place them. In several instances of this kind, amongst the *Muridæ*, the present list is quite a revelation; and so is the assigning to *Typhlomys* its place side by side with *Platacanthomys*.

Only, what Mr. Thomas has given us, is not enough; it must do for the present, but we want something more from him hereafter. We want a Catalogue of Rodentia, a work which can only be done with the help of the material in the British Museum; and the scientific world is entitled to claim it as a right from the authorities of that establishment. Not only is no specialist living so well situated as Mr. Thomas for such an undertaking, but at the same time none is so well qualified for it as himself.

"Guts and Other Charnel-house Stuff."

Dr. Gadow showed us long since that, though despised and spurned by Ruskin, the "more uncomely parts" of our feathered friends are as capable of affording us valuable material for study as is their more pleasing exterior.

Mr. P. Chalmers Mitchell, in a paper in the *Proceedings of the Zoological Society* for January, 1896, attacks the study of intestinal convolutions from a different standpoint, but with results very similar to those of Dr. Gadow.

The method of procedure differs entirely from that of Gadow. The intestinal tract is removed from the body-cavity after section of the oesophagus and rectum. The stomach is then placed to the right, ventral surface uppermost, and the loops of the intestine are carefully displayed. Three principal loops are now distinguishable: (1) the duodenal loop, (2) the circular loop, and (3) the rectal loop: and each of these loops possesses some characteristic feature.

The duodenal loop is usually a simple loop containing the pancreas, but it may be expanded into a branching system of folds, or form spirally twisted coils. Its vein is the anterior mesenteric. Branches from this, bridging the intermediate space, supply part of the posterior region of the gut. In displaying the intestine this bridging vessel has to be cut through, as well as a portion of the mesenteric fold, so as to free the rectal from the duodenal loop, and expose the circular system. These "bridging vessels" are "remarkably constant in the groups in which they occur, and they seem to present a striking instance of a feature which, apparently, could only have arisen from the 'accident' of contiguous position, and is fixed as a normal part of the structure." The relations of the anterior, median and posterior mesenteric, and of the "short-circuitings" or "bridging-vessels" hold an important place in this paper.

The circular loop, in its most typical form, is thrown into a number of corrugated folds or secondary loops, suspended at the circumference of the semi-circular expansion of the mesentery after leaving the duodenum. The median mesenteric vein traverses the centre of this fold and runs out to the remnant of the yolk-sac, situated more or less nearly in the centre of this loop. The size and position of this vestige of the yolk-duct, is one of the features of the circular loop, though second in importance to the nature of its convolutions. It seems that some sort of compensation for the loss of cæca is often made by the elongation of the last secondary loop of this circular system. The numerous figures which illustrate this paper show the wonderfully varied forms which the circular loop may present.

The rectal loop, or last part of the gut, as a rule maintains its primitive straight position, closely attached to the dorsal wall of cœlomic cavity by the posterior part of the primitive straight mesentery. It may, however, take the form of an enormous coil swung at the circumference of a semi-circular expansion of the mesentery. Its vein is the posterior mesenteric.

By his system the author holds that he is able to "display more clearly the relation of the individual cases to each other," and to what he takes to be "the primitive type, and to show the mesentery and the intestinal veins."

Many, on the strength of less work than is here presented, would have foisted upon us a phylogenetic tree, and added another to the legion of systems already extant or defunct. Not so Mr. Mitchell; on the contrary, he tells us that he does not "feel justified in attempting to draw any general conclusions as to the relations of the various divergences from the common type" that he has described. Enough however has been done, he thinks, to show that when more facts have been collected, the method of investigation here adopted may furnish "another clue to that riddle of zoology the classification of birds." In this we heartily concur.

Applying this new method of investigation as far as it will go, the author is led to conclude that, from the point of divergence of type, the Ratitæ deserve their place at the bottom of the avian tree, and that Ciconiiformes, Falconiformes, and Charadriiformes seem to have departed furthest from the common type. The resemblance which Dr. Gadow claimed for the intestinal convolutions between the Falconiformes and Ciconiiformes, Mr. Mitchell seems unable to support.

The superiority of Mr. Mitchell's system of studying the intestinal convolutions lies in its greater simplicity. It now remains for him to follow the example of Dr. Gadow, and to group into a series of types the various methods of stowing away the intestines, a task by no means easy, and only to be attempted after further research.

The Hoatzin.

In his second paper, "a contribution to the anatomy of the Hoatzin, *Opisthocomus cristatus*," which appeared in the *Proceedings of the Zoological Society* for June, 1896, Mr. Mitchell adds a few facts concerning the intestinal convolutions and certain of the muscles of the visceral skeletal apparatus, and of the leg.

The intestinal convolutions have been studied after the fashion just described. As a result it is found that the mid-gut presents a definite divergence from the primitive arrangement. Thereby it is more specialised than in the Galliformes or Struthiones, in which the primitive type is very nearly retained. It seems intermediate between *Pterocles* and the pigeons, but presents many points in common with the cuckoos, thus adding another to the many features common to these latter and *Opisthocomus*.

In spite of the fact that the myology of *Opisthocomus* has been described already by Garrod and Perrin, Mr. Mitchell has succeeded in bringing to light many new points. The most interesting of these deal with the variations of the ambiens muscle, a subject which has already engaged much of the author's attention; indeed, he was the first to point out the existence of the ambiens, in a vestigial condition, in forms in which it had hitherto been supposed to have been lost. This will be best appreciated by those who know the important place held by this muscle as a taxonomic character. *Opisthocomus* furnishes us with an example, so far unique, of a species in which the ambiens has been found in every gradation from complete development to a mere vestige. "Apart from possible systemic value, it is of interest to find variations of so great magnitude in a few specimens of a bird. Professor Weldon has recently shown, after examination of an exceedingly large number of individuals of shore-crabs that very slight deviations may be associated with a larger death-rate. In the case of creatures so difficult to shoot as *Opisthocomus* it may be the case that those actually examined have, from the greater magnitude of their variations, been less able to escape."

The "Index Animalium."

The number of species of recent animals already described has been computed by our Zoological Recorders as 386,000; including fossils we may well put the number at 550,000. The number of names applied to those animals is, however, far greater. Owing to the ignorance or perversity of men, or to the gradual advance of knowledge, some animals are burdened with as many as twenty names, but—unlike " the ways of constructing tribal lays "—only one of them is right. Now, whatever be the principles that guide a systematist in determining the name that should be applied to a known species, it is clear that he must have all the claimant names before him when making his decision; and not only the names that have been applied to the species in question, but names applied to other genera and species, some of which may have received identical names. Consequently, as we have often urged, a complete list of all names that have ever been proposed is an indispensable preliminary to any effective revision of nomenclature. Such a list is the " Index Animalium," now being prepared under the auspices of a Committee of the British Association.

It has already been shown in NATURAL SCIENCE (Jan., 1896) that the average number of names applied to each species mentioned in the British Museum Catalogue of Birds is $5\frac{1}{2}$. But on a more moderate estimate of three names to each known species in the animal kingdom, this Index will have to contain no less than 1,650,000 references. The mere writing of these is almost a life's work; and to this labour must be added that of searching for scarce literature, of verifying dates—a most important part of the work, of interpreting obscure passages often in strange tongues, and of sorting the slips into alphabetical order under genera. To this gigantic task, Mr. C. Davis Sherborn, an enthusiastic and competent bibliographer, has devoted himself. In the course of some four years he has completed 140,000 references, each on a separate slip and in duplicate. These are arranged under genera as the work goes on, so that they are always available for reference. Any zoologist can see them by applying at the librar of the Geological Department of the British Museum (Natural History), and in this way or through correspondence many have already derived important help from Mr. Sherborn's labours.

Descriptions of the methods and progress of the work have recently appeared in the *Proceedings of the Zoological Society* (1896, pp. 610–614) and the *Geological Magazine* (Dec. iv., vol. iii., pp. 557–561, Dec., 1896); but, feeling that zoologists in general are not sufficiently aware of the service being rendered them, the Committee has asked us to draw attention to it. We do so with great pleasure, and we earnestly trust that some practical expression of sympathy may be the outcome. The British Association was able last year to make a grant of £100 towards the work; but this does not go far, and there is no guarantee that a repetition even of this will always be practicable. To make satisfactory progress, at least £200 a year is needed, and the zoologists and palæontologists of the world should surely be able to provide that sum.

The Dutch Scientific and Medical Congress.

The sixth Meeting of the Nederlandsch Natuur en Geneeskundig Congres took place at Delft, on April 22nd, 23rd, and 24th, and afforded two English visitors, who were privileged to be the guests of Prof. Lobry de Bruyn, an opportunity of seeing how well these things are done in Holland, and of carrying away a very pleasant impression of Dutch hospitality. The proceedings opened on Thursday with a reception at the Stads Doelen, and closed on Saturday with a dinner at the same place, from which members and guests adjourned for the further entertainment of tableaux vivants and a social gathering. The "Phœnix," or students' club house, was also generously thrown open for the occasion. The general President was Prof. J. M. Telders, and the five sections were presided over respectively by W. A. van Dorp of Amsterdam, W. Kapteijn of Utrecht, P. P. C. Hoek of Helder, H. Treub of Amsterdam, and K. Martin of Leyden. The sectional meetings were held in the various laboratories and lecture-rooms of the Polytechnic School, in both morning and afternoon. Several of the sections had very attractive programmes; among the most interesting papers in Section II (Biology) may be mentioned one by Prof. Hubrecht on "Primates and Lemurs," and one by M. C. Dekhuijzen, of Leyden, on methods of investigating the micro-organisms in fresh water. The Section for Physics and Chemistry had an able paper by Col. C. F. Geij van Pittius, on various explosives, and one by H. A. van Ijsselstein on an interesting form of telephone. But the paper which formed the last of the series on Saturday was one so brilliant as almost to throw all others into the shade; this was a lecture by Prof. M. W. Beijerinck, on luminous bacteria, illustrated by lantern-slides and a really magnificent series of experiments, which received the applause they deserved. Prof. Beijerinck's work in bacteriology is well-known; he has discovered at least two new species of marine bacteria which cause phosphorescence, and his experiments to show the nature and extent of their luminosity, and the conditions under which they exhibit the phenomenon could hardly be equalled for beauty and interest, and were worthy of a far larger audience than could be accommodated in the lecture-room of the Natuurkundig Laboratorium. In the intervals between meetings, members and visitors were given the opportunity of seeing the beautiful porcelain-manufacture, for which Delft has always been famous, a process of etching on glass for decorative purposes, various exhibits of scientific apparatus and other objects, and last, but not least, many places of historic interest, as, for instance, the staircase where William the Silent met his death. The quaint picturesqueness of the old town, with its waterways, and its delightfully fresh and bracing air will make the Delft Congress a pleasant memory to many of the 1,000 people who gathered to do it honour.

Publication by the U.S. Department of Agriculture.

A note in our April number has caused somewhat of a scare among the numerous European workers who have hitherto esteemed highly the receipt of the valuable publications of the U.S. Department of Agriculture, publications which cover many departments of natural science. From information courteously sent to us by Mr. Charles Dabney, Acting Secretary of the Department, we are glad to learn that matters are not quite so bad as they seemed to be.

The law to which we referred excepts, as we said, copies required "for Official use." Under this saving clause the officers of the Department and of its bureaus are allowed a considerable number of copies of each publication, and the term "official use" has been construed very liberally, so as to include exchanges. "Every person," says Mr. Dabney, "who forwards to the Department of Agriculture any publication of value, or contributes anything to its library, will, so long as the supply holds out, be placed upon a list of exchanges for the bureau to which he contributes. All correspondents who supply information, including scientific men who send their *separata*, may be included among these exchanges at the discretion of the head of the bureau. All others can secure these publications by registering for them and paying the price thereof at the office of the Superintendent of Documents, Union Building, Washington, D.C.

"As a matter of fact, the Department of Agriculture makes, under this construction of the law, a tolerably liberal distribution of its documents. During the fiscal year ending June 30, 1896, it mailed abroad publications aggregating in weight 230,000 ounces, upon which postage amounting to $1150.00 was paid. Five hundred copies of the Experiment Station Record, which is one-tenth of the entire edition, are mailed free to foreign exchanges, while to other individuals it is supplied at $1.00 per annum or ten cents per number."

The only difficulty in the present arrangement is that the Superintendent of Documents will not accept cheques or postage stamps, but must always have money orders. We learn that an effort will be made to remedy this difficulty. Meanwhile the Americans can retort that it is not peculiar to America, since an American who wants a publication of the British Government has to send a money order to the publisher in order to obtain it. A pamphlet which costs an Englishman twopence halfpenny, costs an American in this way 24 cents.

Civilisation in the United States Congress.

We quote the following from the *Daily Chronicle* :—

"The Indians of North America have a terrible reckoning against the white man's civilisation, and it is disheartening to learn from Washington that the bad business still goes on. By a beneficent law passed nine years ago, an Indian woman marrying a white citizen became a citizen herself, and her offspring were entirely separated from the degrading influences of Reserve life. All this has been changed by a measure which has just been hurried through Congress. The children of mixed marriages are to remain Indians, and thus one of the most effectual means of removing the Indian question from the problems of the State has been destroyed. Another retrograde enactment hands over to mining speculators enormous deposits of gilsonite found in the reserves set apart for the Utes. The Board of the Indian Commissioners earnestly protested that if these mineral lands were to be opened it should be done by selling or leasing the deposits to the highest bidders for the benefit of the Indians, but Congress seemingly went its own way."

Theoretical Darwinism.

We have the misfortune to find Mr. G. D. Haviland, M.B., F.L.S., a singularly trying author. We have received from him a pamphlet in two parts ("Some Factors in the Evolution of Adaptations": R. H. Porter, London, 1897), in which he discusses in a purely abstract fashion several very interesting problems. He lays down certain definitions and axioms, and then, in a series of propositions, deduces in a most logical fashion some biological results of great importance. Unfortunately, however, biology, the doctrine of life, is not founded on a series of abstract propositions, but on the infinite variety of living nature. Mr. Haviland, at any rate in this pamphlet, does not condescend to observation or to citation of the observations of others. Logical deduction, however cleverly contrived, is of little interest by itself. As a preliminary to observation, as a means of framing provisional hypotheses that may serve as directions in devising experiments, it is an invaluable scientific weapon. But most people who wish to make investigations are able to frame their own preliminary deductions; and we imagine that, in its present form, Mr. Haviland's ingenious logomachy will be of little service. If he wishes to claim the attention of biologists, he should attempt observation on the lines of some of his own propositions. For instance, the third corollary to one of these propositions is that "neither polygamy nor prostitution will influence the proportion of the sexes." Let Mr. Haviland collect and publish information bearing on this, and we shall all read him.

Colour Variation in Flowers.

So much has been written of late on the question whether variation takes place in a definite direction or indefinitely, and, to the would-be explainer of evolution, the question is in fact so important, that a note by Mr. F. A. Waugh, in the March number of the *Botanical Gazette*, on the definiteness of colour-variation in many flowers, seemed to promise something of interest.

"The centrifugal encroachment of a darker upon a lighter colour in blossoms is," says Mr. Waugh, "one of the commonest lines of definite variation." It furnishes the dark-eyed varieties of *Primula Chinensis*, and governs the variations in markings of marigold, poppies, pelargoniums, and many others. A striking case is afforded by a cultivated species of *Coreopsis* (*C. tinctoria*). This is a Composite flower, the rays of which are yellow with a small but variable maroon base. In the very numerous varieties the maroon constantly encroaches upon the yellow, until, in extreme forms, it quite supersedes the body-colour. The impression given (though for some reason this was not proved by sections) is that "the brownish pigments were spread first over the upper surface and subsequently increased in depth, first showing through in the thin areas." A similar series is presented by the flowers of a *Freesia* (*F. refracta* var. *alba*). In this case it is the appearance and gradual spread of an orange-yellow over the pure white that is claimed as an instance of definite variation. The colour shows itself first at the base of the upper petals, and extends over their inner surface, then appears on the backs of the petals, following the same definite centrifugal lines as in *Coreopsis tinctoria*. Sections made through petals from various flowers in the series showed that the yellow pigment on its first appearance was confined to a single layer of subepidermal cells, whence it extended through the intermediate cells to the under surface.

The facts are interesting enough, but, after all, they seem to have but little bearing on the question of definite variation, except in so far as they suggest that many supposed cases are the natural outcome of physiological causes. It is not difficult to imagine a reason why colour should spread in this centrifugal manner. It is doubtless in some way connected with nutrition, and as the petals are attached to the floral axis by their bases, a change might be expected to start at, and spread from, the point most nearly connected with the main axis from which the member receives its supply of nourishment.

Boulder Clay, a Marine Deposit.

In 1843 Portlock recorded the occurrence of shells in the Boulder Clay of the N. of Ireland, and stated that it was a marine sedimentary deposit. In 1879–80, S. D. Stewart published a list of no less than sixty-nine species of Mollusca from the Boulder-Clay of the N.E. of Ireland, and remarked on the occurrence of fragile forms, as *Leda pygmæa* and *L. minuta*, with united valves, as indicating that these shells had lived and died in the position in which they were found. Joseph Wright and James Neilson have now systematically recorded the Foraminifera found by them in Ireland and around Glasgow in the Boulder Clay, and thus give additional evidence as to the marine sedimentary nature of this deposit. According to these authors (*Trans. Geol. Soc. Glasgow*, X., 263–279), the Foraminifera found therein are exceedingly abundant in places, are mainly boreal forms, and are as perfectly preserved as are those of a recent sounding. Messrs.

Wright and Neilson's results are of considerable interest, and should be compared with Victor Madsen's lists noticed in Natural Science, May, 1895, p. 355.

An extension of this investigation to supposed glacial deposits of North America has been made by Dr. G. M. Dawson, who contributes some results to the last number of the *Journal of Geology*. Mingled with fossils and rock-fragments that are obviously derived, he finds many tests of Foraminifera which must have lived on the spot where they are now found.

An Old Zoological Record and Address Book.

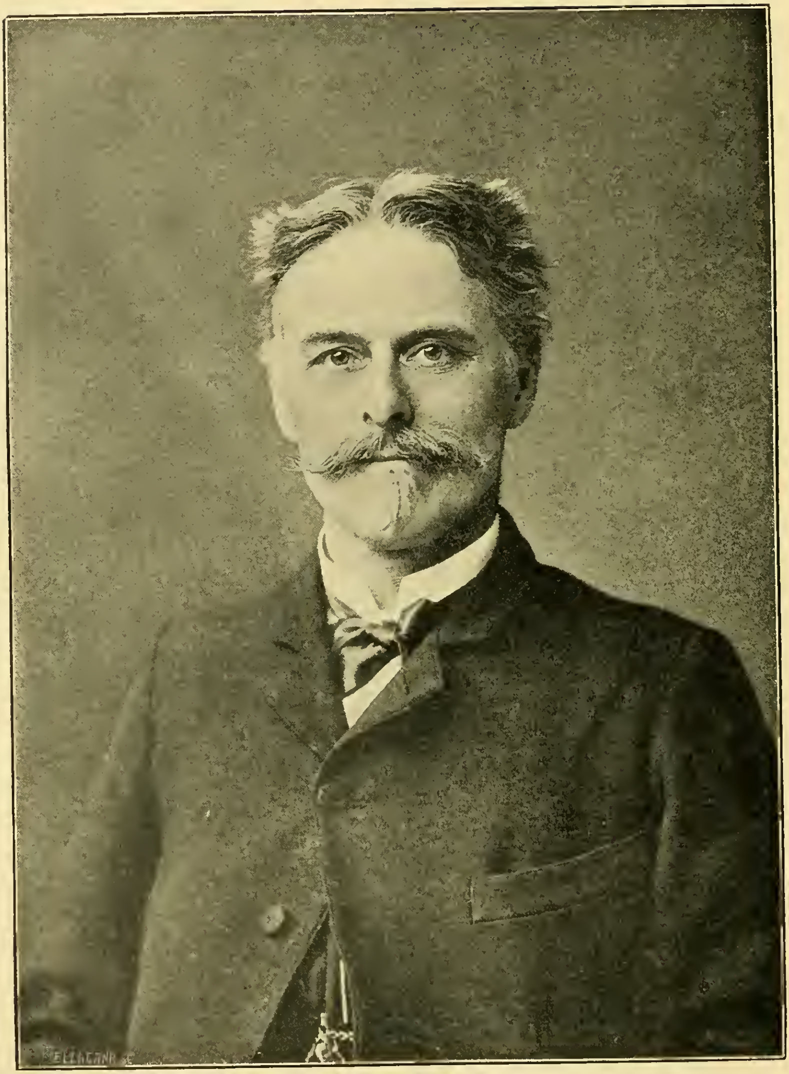
Possibly few are aware that over a century ago there was founded a zoological record, viz., F. A. A. Meyer's *Zoologische Annalen*, which was published at Weimar in 1794, and ran for one year only. Meyer gave an account of zoological works issued up to Easter and up to Michaelmas 1793, an alphabetical list of all living zoologists, an account of all the zoological collections known to him, and a sketch of all the new animals described during the year, in systematic order. It must have been extremely useful to workers at the time, and one is somewhat surprised that it was allowed to drop so soon.

The Falsification of the Record.

The *Irish Naturalist* has done such good work in the study of the geographical distribution of animals and plants in Ireland that it is entitled to protest, as it is now doing, against the indiscriminate attempts that are occasionally made to introduce foreign species into that island. The record of former changes on the earth's surface, preserved to us through the present distribution of species, is most important, and anything that unnecessarily tends to vitiate it should always seem reprehensible to the naturalist, Irish or otherwise.

To the Humanitarian League.

Mr. Southwell's letter on the alleged golden eagle, and the review of "The New Charter" by an esteemed contributor, are not the only protests against the methods of the Humanitarian League that have come to our letter-box. All naturalists must wish well to the objects of this League, and therefore in sincere kindness we suggest to its members that they should not let their feelings carry them into such inaccuracies and exaggerations of statement as they are too often guilty of. There are in the three kingdoms plenty of trustworthy field-naturalists who would be glad to help the League with a little advice gratis.



EDWARD DRINKER COPE.

Born July 28, 1840.
Died April 12, 1897.

I.

Edward Drinker Cope.

BY the death of Professor Cope of Philadelphia, announced last month, biological science has been deprived of one of the most restless and fertile brains ever devoted to its interests. Continually on the alert, devouring every new zoological paper or memoir with almost feverish excitement, though confining his own personal researches exclusively to the Vertebrata, Cope made many of the boldest and, in some respects, the most successful attempts, which have hitherto been devised, to solve the problems of organic evolution. Though, perhaps, often premature, and sometimes mingled with much error, which a more cautious enquirer would have avoided by waiting for additional evidence, his remarkable speculations—some have even dared to regard them as wild guesses—have had an influence on the progress of modern biological research which it is impossible to estimate. There sometimes proved to be only a small kernel of truth in the new conception as it first appeared; the original statement was often gradually modified in a rapid series of provokingly desultory notes, perhaps mostly evoked by the writings of his fellow-workers, whose interest in the subject he had aroused; but there was always a spark of suggestiveness which opened up some new line of enquiry, and there was an almost never-failing acumen in distinguishing the important from the unimportant features of a case. When others published a plain statement of facts, Cope would often interpose, usually on the spur of the moment, to add a sparkle of philosophy to the contribution; and much of his brilliant generalization appeared in the characteristic reviews in the *American Naturalist*. His influence in personal intercourse, too, has probably not been equalled since the days of Louis Agassiz. To know him in his museum-house in Pine Street, Philadelphia—to see him at work, to hear him discuss the piles of new literature he happened to be reading—was a source of inspiration which could never fade from the memory of any young biologist who had the privilege of thus coming in contact with him. When financial misfortune overtook him some years ago, before he was called to a Chair in the University of Pennsylvania, he cheerfully adapted himself to the new and, to him, strange conditions, not relinguishing his scientific occupations in the least. To the very end he was absorbed in the fascinating problems which he began to study forty years ago.

To allude to his own special researches, those, namely, in the structure, classification, and evolution of the vertebrated animals, perhaps the most significant feature to note is the importance Cope ascribed to the characters of the skeleton. In view of the researches of Oken, Cuvier, Owen, Huxley, and others, it cannot be said that the study of the skeleton had been neglected before Cope began his work. It is, nevertheless, true that he was the first to emphasise the importance of certain osteological characters in many groups where their significance had been overlooked; and he was more successful in determining the natural affinities of certain forms, even in defining some great groups, than any of his predecessors, who had been misled by mere external resemblances. So long ago as 1865, for example, he arrived at the broad classification of the frogs, which everyone now admits to be truly natural, by a detailed examination of the skeleton in the various genera. A little later, he similarly laid the foundation of the now generally-accepted classification of the lizards; still later, he proposed the only really natural arrangement of the Chelonia, by reference to the characters of the neck-vertebræ. His arrangement of the bony fishes, also, first sketched roughly in 1871, was a great advance on any scheme previously published, owing to the acumen with which he employed the osteological characters of the group. In this work he was immensely aided by the unique collection of skeletons prepared by Professor Josef Hyrtl of Vienna, which he purchased at an early stage in his scientific career.

During the first decade of his original researches, Cope confined his attention almost entirely to the existing Vertebrata. It was only later, when he began exploration, partly on his own account, partly under the auspices of the United States Government, that he made a special study of extinct types. Between 1859 and 1864 he wrote on recent Batrachia and Reptilia; and in the latter year he published his first ichthyological paper, on a blind siluroid. In 1865, he seems to have described a fossil vertebrate for the first time, the Labyrinthodont *Amphibamus grandiceps* from the Coal Measures. In the same and the following year he paid much attention to the existing Cetacea. Then came the first of his long series of papers on extinct reptiles (on the Dinosaurian genus *Lælaps*, 1866). His work on extinct Vertebrata for the United States Geological Survey was not confined merely to the numerous small papers he published, but was also issued in a more handsome form in two illustrated quarto volumes, the first on "The Vertebrata of the Cretaceous Formations of the West" (1875), the second on "The Vertebrata of the Tertiary Formations of the West" (1884). The latter, though an especially bulky tome, is described as "Book I," and was, unfortunately, never followed by the succeeding "Book," which Cope duly prepared but never had published, owing to political changes which led to his exclusion from the public service.

It is impossible to enter into the details of Cope's life-work on the extinct Vertebrata; it must suffice to note a few of the salient points. Of the lowest ancestral Vertebrata, he had practically no examples to study; but he closely followed the descriptions of others, and it is mainly due to his initiative that we now regard the strange Pteraspidæ, Cephalaspidæ, and Asterolepidæ (Ostracodermi or Ostracophori, as Cope termed them), of the Upper Silurian and Devonian rocks, as the armoured extinct allies of the modern lampreys (Marsipobranchii). The suggestion had been made before; but Cope's "wild guess" that *Bothriolepis* and *Pterichthys* might be compared with the modern armoured tunicate, *Chelyosoma*, excited renewed interest and drew closer attention to the subject. As new facts accumulated he gradually veered round to the now generally-accepted view, that these strange armoured creatures were to be placed a little higher in the scale than he at first supposed. Among the lowest fishes Cope was the first to recognise the primitive Palæozoic order of sharks, termed by him Ichthyotomi. This he claimed for some time to be ancestral both to sharks, and to all other fishes; but as soon as *Cladoselache* was described by Dean from the Upper Devonian he at once gave precedence to the latter. He also seems to have been the first to suggest that the double ventral series of spines between the paired fins of certain Lower Devonian Acanthodian sharks (e.g., *Climatius*) are remnants of the two continuous lateral folds in which the paired fins had their origin. His most important generalization concerning the so-called "Ganoids," which of course he did not accept as a definable group, was his clear enunciation of the stages in the evolution of the fins and their primary importance in classification—a result partly anticipated by Huxley and Traquair. Among bony fishes, he began to instil order into the chaos which he found in published writings on the fish-remains from the Chalk; and he described many fine specimens from Kansas. He also added enormously to our knowledge of the Tertiary bony fishes of North America.

Turning to the extinct Amphibia, he made known a great series of Labyrinthodontia (Stegocephala, as he termed them) from the Coal Measures of Ohio and the Permian of Texas. The anomodont reptiles (which he termed Theromorpha or Theromora) also interested him deeply, and he described a great number of forms from the Permian of Texas and other regions. He followed the lead of Owen, and declared these reptiles to be the ancestors of the Mammalia. Many of the remains described, however, were so fragmentary that there is still much uncertainty as to their true nature. Among higher reptiles, Cope paid much attention to dinosaurs, though most of his specimens were very fragmentary, and he only distinguished in a vague way many features which later discoveries by contemporaries revealed clearly. He shares with Leidy, Huxley, and Phillips, the credit of first recognising the true character of the pelvis in the

Iguanodont Dinosauria. His early descriptions of the Pythonomorpha (as he termed the extinct Mosasaurian reptiles of the Chalk) led to most interesting discussions with Owen, and added greatly to our knowledge of these marine lizards.

In reference to the Mammalia, one of Cope's most remarkable generalizations was his so-called Tritubercular Theory, explaining the origin of the cusps in the molar teeth. He believed palæontology proved that even the most complicated molars could be traced back to simple tricuspid or tritubercular teeth. He also extended Kovalevsky's work on the feet of the Ungulata, and proposed the most natural arrangement of this great Order hitherto suggested. He elucidated the evolution of the camels in great detail, from American fossils; he also defined a great number of genera and species of other extinct ungulates.. His most important contribution to the subject, however, was the detailed description of the nearly complete skeleton of a Lower Eocence quadruped, which he named *Phenacodus*, and made the type of a suborder Condylarthra, believed to be ancestral not only to the true Ungulates but also to most other divisions of placental mammals, including man himself. Finally, he defined the primitive suborder of Carnivora, now universally adopted under his name of Creodonta; and he added much to our knowledge of the whole order, especially of the true cats.

One great feature of this systematic work, everywhere conspicuous, is the attempt to define every term, whether specific, generic, of family or higher rank, in a concise diagnosis. Before Cope's time, this method had rarely been applied to extinct animals; even at the present day it does not prevail so widely as it ought to do. Cope, however, made all his definitions as precise as the variously imperfect materials would allow; and he naturally waxed wroth in his reviews of some contemporary literature which contained new names with nothing but an artist's drawing to justify their introduction into scientific terminology.

As to Cope's philosophy, he began to write on the problems of organic evolution so long ago as 1869, and his researches ran curiously parallel with those of his friend, Alpheus Hyatt, who arrived at the same result from a study of the shells of the extinct cephalopod molluscs. He and Hyatt, indeed, founded the Neo-Lamarckian school which has flourished so vigorously of late in America, and begins to find increasing favour in the Old World. Several of his most suggestive essays were collected in a volume entitled "The Origin of the Fittest," published in 1887; and only last year he summarized his latest views in a smaller treatise, "The Primary Factors of Organic Evolution." A critical and detailed account of the latter work, by Mr. F. A. Bather, was published in Natural Science for January last, so that a long exposition is not required here.

Cope believed that all organisms, impelled by some inherent growth-force, which he termed "bathmism," varied in certain definite directions, and that all modifications ultimately depended on the mechanical conditions of the environment. Palæontology, according to him, proved beyond all doubt that characters thus acquired were inherited. Still further, he promulgated the doctrine, that this development of new characters takes place by an acceleration or retardation in the growth of the parts changed; that, in fact, the adult of an ancestral organism is the exact parallel of an immature stage in its descendant, which only advances or becomes degraded in certain characters during the latest phase of its growth. He was also the first to point out, as the result of these premisses, that the genera of systematists, as commonly understood, are often polyphyletic. According to him, it is the species that are permanent, while genera are but our expression of various grades of organisation through which many species pass. The environment moulds species into genera, and genera into families; and a genus or a family by no means contains forms that are of necessity descended from a common ancestor.

Finally, and not unnaturally, Cope wandered into the domain of mental phenomena, and applied his principles to these. He believed that consciousness preceded the form in which we are accustomed to witness its manifestation, namely organic tissue. His latest definition of life was:—"Energy directed by sensibility, or by a mechanism which has originated under the direction of sensibility."

Cope received the degree of A.M. from the University of Pennsylvania, and Ph.D. from that of Heidelberg; but the honorary distinctions conferred upon him were few compared with those which fall to the lot of most workers of his eminence. He was elected a Corresponding Member of the Zoological Society of London in 1864, a Foreign Correspondent of the Geological Society of London in 1881. He also received the Bigsby Medal of the latter Society in 1879. He was a member of the National Academy of Sciences, and was elected President of the American Association for the Advancement of Science in 1895. His closest connections were always with the University of Pennsylvania and the Philadelphia Academy of Natural Sciences, to which bodies he has left all his valuable scientific collections.

Further interesting facts relative to this remarkable man will be found in the excellent account by Professor H. F. Osborn in *Science* for May 7, while appreciative notices by Dr. Persifor Frazer and Professor J. S. Kingsley are given in the *American Naturalist* for May, accompanied by a very complete series of portraits. Our own plate is from a photograph taken of Professor Cope in 1895, shortly after he had ceased growing a beard.

A. Smith Woodward.

Natural History Museum, London, S.W.

II.

Some Effects of Pelagic Spawning-habit on the Life-histories of Teleostean Fishes.[1]

FOR the last few years several naturalists at home and abroad have freely bestowed their time and labour upon the study of the eggs and young stages of teleostean fishes, and, as a result, there are now only one or two species the ontogeny of which is not known. We thus have to hand a long series of memoirs in which the eggs and larvæ of these fishes are figured and minutely described, at all stages in their career, and it is now made evident that our commonest food-fishes go through a remarkable series of anatomical changes before arriving at the adult condition. Intimately connected with this metamorphosis is the change of environment of the young fish during its career; and although at present we know less about this part of the life-history, an increase of our knowledge promises to furnish even more important results, in every way, than the facts of structural change.

This is not the place to discuss how far the structural features are connected with, or determined by, the environmental factors; but we may note, in passing, that teleostean fishes and their ontogeny offer a very advantageous field for the investigation of this general biological problem. A brief survey of what we know concerning the environmental history of two of our common fishes may illustrate this point. In the case of the plaice, the eggs, pelagic and buoyant, are laid in enormous numbers at a varying distance from shore. Tossed about just below the surface, they are carried by currents here and there, eventually inshore, at least in the case of the majority. The larva, some time after hatching and after absorbing its yolk-sac (when it has reached the post-larval stage), migrates downwards to the bottom and then takes up its station close inshore. As growth proceeds, the young fish slowly moves off seawards till the adult stage is reached. In the case of the herring, on the other hand, the eggs (demersal) are laid comparatively near inshore in masses, adhering to any object at the sea-bottom; and the larvæ, on hatching, tarry awhile at the bottom. In a few days, however, they, with an unerring instinct, migrate upwards into the surface-water and move offshore. Later

[1] This paper, in more extended form, was read before Section D of the British Association, September, 1895.

on, they, like the young plaice, are found close inshore, in the littoral region.

It is seen that the life-histories of these two common fishes agree in this particular, that the eggs and larvæ, before arriving at the adult stage, have to pass through many vicissitudes and changes of environment, which in their general features are not accidental, but are determined in their order of succession for each species. These changes we may term the 'ontogenetic migration.'[1] The first and most marked difference of environment is that of the egg itself, and the two general types are illustrated by the plaice and the herring, *i.e.*, the pelagic and demersal. The pelagic egg floats free (rarely in masses) in the open water; the demersal is usually fixed to foreign bodies on the sea-bottom. All the gadoids and pleuronectids appear to have pelagic eggs, whilst the clupeoids vary greatly in type, and the smaller acanthopterous littoral fishes have demersal eggs.

It would be well to inquire into the meaning of these two types of eggs and the remarkable difference of environmental conditions involved; and a careful review of the facts will convince one that the pelagic spawning-habit is the more primitive, and is probably to be interpreted upon phyletic grounds, whereas the demersal has been secondarily acquired in the course of time. A few of the considerations leading to this conclusion may be here given:—

1. The pelagic-spawning fishes are, almost without exception, more prolific: their fecundity may be expressed by as high a figure as 150,000,000 (in the ling), a number beyond all human comprehension.

2. They have more primitive methods of fertilisation and no sexual dimorphism: the eggs and sperms are simply extruded into the water, and there is no 'pairing.'

3. Their methods of reproduction are primitive: there is an extended period of ripening, and after the extrusion of the sexual elements, the demand of the future generation upon the parent comes to an end; whereas in the demersal-spawning types we have, amongst numerous instances, the nest-building habits of the stickleback and its parental devotion to the young, the jealous watching of the lump-sucker, and the careful selection of sites for oviposition by the gobies, features which culminate in the viviparous habit of the blennies.

4. The ontogeny in the demersal types is more protected, and the larvæ are usually hatched at a much later stage than is the case in the pelagic types.

5. There is no conceivable advantage to be obtained by the secondary adoption of a pelagic spawning-habit, but there is in the secondary assumption of a demersal spawning-habit.

These considerations are all correlated, but they form strong

[1] For method of determining exactly this migration, see A.T.M., *Rep. Fish. Board Scotland*, xiv., p. 294, 1896; and *Ann. Mag. Nat. Hist.*, ser. 6, vol. xvi., pp. 285–288, October, 1895.

reasons for maintaining that the pelagic-spawning fishes retain this habit as a survival of a primitive condition, and that this habit is therefore of phyletic significance.

The enormous fecundity of these fishes has not up till now been sufficiently emphasised nor its significance pointed out. Taken as a measure of the amount of mortality amongst the young stages, it points to the fact that this pelagic habit involves a subjection of the young stages to powerful destructive agencies. Prof. G. O. Sars, in his work upon the development of the cod,[1] recognised this fact, and more recently a few observers have given expression to the same ideas; but it is not uncommon to find writers speaking of the great advantages derived by pelagic eggs in that they are removed from the danger of ravenous littoral enemies and even of the trawl. These dangers are brought vividly home to the reader by graphic accounts of countless shoals of haddock following in the wake of spawning herring and feeding on the newly-shed spawn. Whilst granting the truth of these statements, we must recall the fact that the few thousand eggs of each female herring suffice to ensure the preservation of the species, whilst it requires as many millions to perform the same function in some of the gadoids. Some have attempted to explain these facts by an appeal to the appetites of sundry copepods and other crustaceans, to account for a great loss of pelagic eggs; but there seems to be no doubt that these eggs, by their very environment, are subjected to the vicissitudes of the physical world,[2] the destructive powers of which are not limited by the capacity of a stomach, and which cannot be guarded against by translucency, activity, or defensive armour. In changing their spawning-habit from pelagic to demersal, some of the teleostean fishes have removed their eggs and young stages from the action of every intermittent change of wind, tide, and temperature to, in many cases, the direct supervision of the parent. This change has been so successful that, in spite of many enemies hunting the littoral region, the demersal-spawning fishes can hold their own with far less tax upon the organic energies of the parent; in other words, they do not require nearly so high a fecundity.

We may now enquire into the conditions of pelagic habitat, and their effects upon the life-histories of the food-fishes. For this purpose the eggs and early larval stages may be regarded as passive objects so far as the determination of their own destiny is concerned. Some modification of this statement may result from experiments now in progress: the egg, a living object, may react to certain changes of environment in a way which will have to be taken into account, and

[1] Report to Norwegian Home Department, 1864.

[2] *Cf.* C. G. Petersen. 'It is my conviction, however, that the physical conditions here play an exceedingly important part, and this is particularly conspicuous if we try to imagine how all the larval fish which have come out of the eggs are to reach the shore.' *Report Danish Biol. Stat.* 1893, p. 15.

which shows the relations of floating eggs to be markedly different from those of drifting inanimate objects. Broadly speaking, however, it is only when the yolk-sac has become absorbed and the post-larval stage has been entered upon that the little fish is capable of active movement, and can then, to some extent, perform its own migrations.

Taking then a typical case of a certain species, with a pelagic spawning-habit, the eggs will be laid offshore in the surface-water and will be subjected during their development to a pelagic drift, which, in the great majority, will be shorewards. The direction of this drift will be determined by that of the prevalent winds at the particular season, and its duration will depend upon the length of time it takes for the post-larval stage to be reached and the drift-current to be forsaken. In the accompanying diagram (Fig. 1) the point A represents the spawning-area of the species under consideration, and the resultant pelagic drift is in the direction from A to D, at which latter spot the young forms descend to the bottom. It is obvious that this drift can be resolved into a drift A C parallel to the shore and a drift A B at right angles to it. In different areas the comparative magnitude of these two components may vary to any extent; but by the time the adult stage is reached there must be a return to the point A, by a compensating migration of the young forms, or the area would be annually depleted of the species.

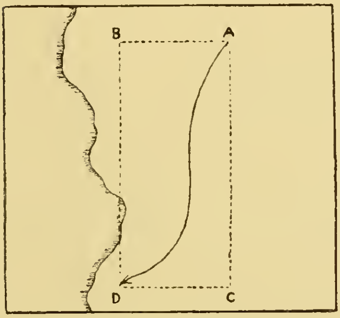
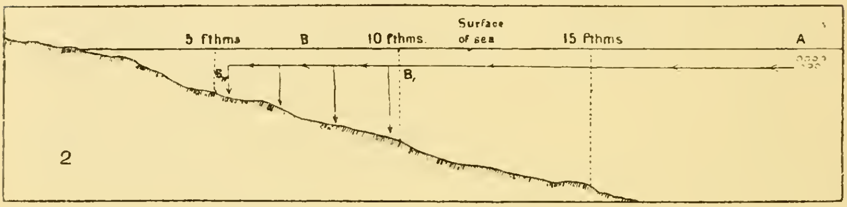


FIG. 1.

In the case of the North Sea, in the east coast of Scotland area, experiments with marked plaice have shown that they slowly migrate northwards parallel to the coast before moving out to the spawning-grounds.[1] Independent observations with drift-bottles tend to show that many of the eggs of the plaice laid off the east coast are drifted southwards in their course.[2] In other words, if in Fig. 1 the top of the diagram be taken as north, A D will represent the course taken by these eggs of the plaice, and the young plaice are found to migrate from D to B along the coast and then outwards to A. There can be little doubt that the two are correlated phenomena. The extent of the drift A C will depend upon the rate of the current, and upon the time during which it acts upon the eggs and larva, but as the drift in this direction can be compensated for, and its lengthening or shortening is apparently not attended by fatal consequences, we may, for the moment, neglect it and consider the drift in the direction A B, assuming in our particular instance that A C=0, or that the eggs are drifted directly inshore.

[1] T. W. Fulton, *Rep. Fish. Board, Scotland*, xi., p. 186, 1893.
[2] T. W. Fulton, *Op. cit.*, xiii., p. 158, 1895.

Let us examine the conditions of pelagic drift along A B by means of two perpendicular sections (Figg. 2 and 3). In Fig 2. are seen the conditions in a shallow-water area, and in Fig. 3, those in an area in which the coast slopes off suddenly into deep-water. The point A is supposed to represent the spawning-area, and the eggs are drifted along A B till at B the young fishes migrate downwards to the bottom, which is, in the ideal case, at a suitable depth for them (in this particular instance, supposed to be from 5 to 10 fathoms). Then it is



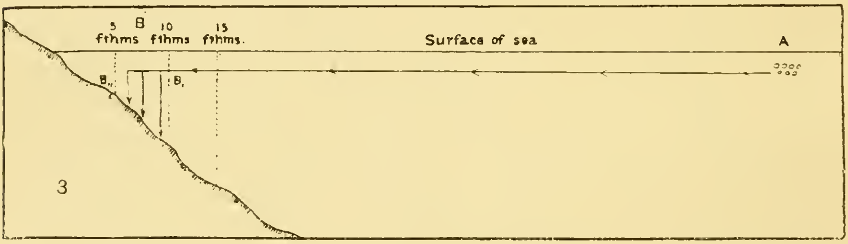


FIG. 2.—PELAGIC DRIFT IN A SHALLOW-WATER AREA.

FIG. 3.—PELAGIC DRIFT IN A DEEP-WATER AREA.

evident that the length of A B must, for the survival of the species, be between the limits A $B_{,}$ and A $B_{,,}$ as all the larvæ, which reach the bottom sooner or later than this, are in an environment to which they are not adapted, and in which they would therefore, in the main, succumb. In Fig. 2, where the conditions of a gradually-shelving shore are shown, the limits $B_{,}$ $B_{,,}$ are wider apart and this may largely explain why slowly-shelving shores and friths are selected by spawning-fish as nurseries for their young in preference to areas with deep water close in shore. The limits of $B_{,}$ $B_{,,}$, and hence of the position of A, are therefore determined, on the one hand, by the bathymetric conditions, and on the other, by the adaptability of the young forms. Suppose the existence of a species in which the young were adapted for living in all the environments which obtain in from 5 to 15 fathoms, then in the area represented by Fig. 2 the species would be able to spawn successfully over a very extended area.

Again, if a species were adapted to a general distribution at various depths, and this adaptability were to be reflected back to its young stages on their assuming a bottom-habitat, then the position of

A (the spawning-area) would only be limited by the necessity for being a certain distance offshore, and beyond this the adults could spawn with impunity in whatever environment they might happen to be. A spawning-migration would then be eliminated, except in the case of individuals living in very close proximity to land. It is probable that the dab is a species largely conforming to this type of spawning.

The first factor in the determination of the spawning-area of a given species is, then, the adaptability or otherwise of the young forms. The second is the nature of the inshore nursery, whether it slopes rapidly or shelves gently.

Let us now consider the other factors determining the length of A B, assuming that B is to some extent fixed, as a mean between $B_{/}$ and $B_{//}$. Any agents which tend to lengthen A B will then necessitate a spawning-area (A) further out to sea and *vice versâ.*

An egg, or larva, floating inshore remains, as already said, in the pelagic-habitat, till the migration downwards is effected; and this takes place at a definite stage in development. The time taken to reach this stage in ontogeny may be termed the 'pelagic-period,' and it is evident that, for maximum results in the way of successful development, this 'pelagic-period' must correspond exactly with the time taken for an egg or any object to drift from A to B. Suppose that the rate of drift be too rapid, then by the time the egg has reached B the pelagic-period will not have come to an end, and the egg or larva will be washed ashore and perish. Some observers (*e.g.* G. O. Sars, *loc. cit.*) have recorded cases in which myriads of cod's eggs have been thus 'beached,' forming a long glistening line at high-tide-mark. On the other hand, if the rate of drift be too slow, the pelagic period will come to an end prematurely, and the young forms will migrate downwards into deeper water than they are suited for, and, at least in large part, pay the penalty by extermination. There must, therefore, be a close agreement between the pelagic period and the time of drift.

The rate of drift in a given area depends upon the average strength of the prevalent winds; and this on its part depends largely upon the season of the year. Further, as the rate of drift is simply $\frac{AB}{\text{time of drift}}$, the position of the spawning-area must be regulated by the season of the year at which spawning takes place, in such a way that the stronger the winds, the further seawards it must be removed.

Again, the duration of the pelagic period has to be taken into account, for as seen above, it must correspond with that of pelagic drift. The two known factors which determine the length of the pelagic period are temperature and bulk. It has been found that the eggs of fishes, unlike those of higher animals (*e.g.* birds) have not a fixed and definite period of 'incubation,' but that the hatching and development of a young fish can be markedly hastened or retarded by alterations in temperature of the surrounding medium.[1] Thus, especially when dealing with the lower temperatures, many days or weeks may be added to the length of the period of incubation by lowering the temperature a few degrees. A determination, by direct experiment, of the period of incubation of a given species at various temperatures, and a series of determinations of the mean surface-temperature in any given district for different periods of the year, would thus make it possible to state the incubatory-period for the species in that district. Thus if the spawning-period be late, development will be rapid, and the pelagic-period will be short. It follows from this that the time of drift must be reduced, and, the drift-current being beyond control, the spawning-area (A) must be further inshore. In the case of bulk, it is found as a general rule that the greater the size of the egg the longer is the period of incubation ; so that in relation to this factor, the spawning-area (A) will be fixed in position according to the specific size of the egg. Each species will have its spawning-area determined by the size of the egg.

Lastly, we have the factor of specific gravity.[2] Pelagic eggs invariably sink in fresh, and usually do so in brackish water. Hence a fresh-water fish with eggs and larvæ suited to a pelagic existence will have to be katadromous in habit, that is, it must have a spawning migration seawards. The eel and, to a less extent, the flounder may be cases in point. Again, there are relationships of the eggs to their medium of which we know very little. The pelagic faunas of Crustacea and Mollusca are known to be susceptible to climatic conditions, and at different times to be found at the surface, in mid-water, or even deeper. Though the *modus operandi* is not so intelligible, there are facts indicating that, under certain conditions of which we know nothing, the masses of pelagic eggs of fishes are to be found now at the surface, at other times in the mid-water and, again, at the bottom. In each case they are perfectly healthy and developing normally. All that can be said with certainty is that under some conditions the specific gravity of the eggs relative to that of the sea-water is altered, resulting in a sinking or rising. In the Baltic, the eggs of the plaice have been found far below the surface, floating underneath the stratum of brackish water.

The various factors of wind, temperature, and specific gravity have thus been briefly touched upon, and their relationship to the important economic question of the determination of spawning-areas has been shewn. Thus, for the most successful results in the survival of the brood, the spot A in a given area must be selected by the particular species and must be determined by these factors, amongst others :—

[1] H. Dannevig. *Rep. Fish. Board Scotland*, xiii., p. 147; 1895.

[2] *Cf.* J. Hjort. "The distribution of the eggs of pelagic fishes therefore follows the same laws which govern all the Plankton, it is dependent on the amount of saltness and the currents." Studies of the Norwegian Fisheries, 1896.

A. The stronger the drift (dependent upon the prevalent wind),
B. The earlier in point of season the spawning takes place,
C. The greater the bulk of the egg,—
the further offshore must the spawn be deposited.

It follows from this that in a given season of the year in any one district, the larger eggs should be found (spawned) further out from the shore, and that in the case of a species with a wide distribution in time there should also be a wide distribution in place. In the limits of this paper we can only add that these deductions are borne out to a remarkable degree by the work which has been already done upon pelagic eggs.[1] It is, however, interesting to note that by far the largest British pelagic egg, that of the halibut, has not yet been found in British (North Sea) waters, possibly because not sought for far enough out to sea.

When we consider the foregoing, there is little room for wonder that many of the pelagic-spawning fish find it necessary to spawn their eggs by millions, though it is probable that a further study of the spawning-districts will supply definite reasons for the specific variation in fecundity. It is evident that, for the successful development of the young fish, a concatenation of favourable circumstances is necessary, which depends in the main upon such essentially fickle phenomena as wind and temperature. Let the wind blow shorewards with abnormal strength and duration, and untold millions of unhatched cod may perish, or let the temperature for a few weeks during the summer months be abnormally low, and the same fate may overtake hosts of embryonic gurnards. Under such conditions it is only by the selection of suitable spawning-sites, a prolongation of the spawning-time (on the principle of not putting all the eggs in one basket), and other devices, that the pelagic-spawning fishes have held their own.

One might be inclined to inquire, Why do not all these fishes spawn at the same most suitable time and have the same-sized eggs? The answer to this question brings out yet another avenue of destruction for pelagic eggs. Fertilisation of these eggs takes place in a promiscuous manner, the sperms being freely shed in the water, and diffusing themselves in a very rapid manner in all directions. Under this system an enormous number of sperms and of eggs must perish; for though it is a rare thing to meet with unfertilised eggs in the surface-water, yet it must be recollected that, if they do not meet with a sperm, they soon sink to the bottom. It is conceivable, in the case of such gadoids as the cod, which congregate together in "vast heaps," that the mid-water in the whole district simply teems with the sexual elements, and this loss from want of fertilisation is minimised; but in the case of the dab, a diffuse spawner, with no known pairing instinct, there must be enormous losses. But from the very cosmopolitan and ubiquitous character of these sperms there arises another danger—namely, that of cross-fertilisation. Three or four closely-

[1] *See* A.T.M. *Rep. Fish. Board Scotland*, xi., *p.* 250; 1893.

allied species may be found spawning close together in one confined area, so that if fertilisation is a pretty certain process, the chances appear to be nearly even that cross-fertilisation would also take place. Experiments have shown that the pelagic eggs of one species may be readily fertilised with the sperms of another, not necessarily closely related, though the resulting hybrids usually perish at an early stage.[1] The diversity in specific spawning-season and of size of egg may be largely accounted for as a partial safeguard against the evils of cross-fertilisation.

The enumeration of these factors of pelagic spawning points out the lines along which future scientific fishery-work should proceed in the investigation of the life-histories of our food-fishes. Only when we know exactly their spawning-areas and the spawning-seasons can we attempt to close these areas with a maximum of beneficial effect and a minimum of trouble and difficulty to the industries involved. Only when we know the exact spots in the inshore waters most suited for the fry, can we successfully crown the labours of artificial hatching, which attempt to eliminate the natural depleting ravages of wind and temperature, not to mention the fatal effects of non- and cross-fertilisation. Those who deprecate the attempts at artificial propagation now being made would do well to look into this subject of the conditions of pelagic existence. In every important district are required countless meteorological observations of wind and temperature, close investigations of the surface-currents, and a study of the distribution of eggs and fry in the surface- and mid-water.

Reasons have already been given for regarding the pelagic-spawning habit as the more primitive, and this is borne out by the further study of the specific ontogenetic migrations. These may be expressed graphically in cycles (Fig. 4), showing the gradual transition between the pelagic type (plaice) with a full ontogenetic migration and the demersal type of littoral fish with no migration at all, such as the stickleback.

In Fig. 4a there is represented the typical cycle of an ideal ancestor of the fishes. Starting at (P) the pelagic water, we may suppose that this form migrated inwards to the pelagic water inshore ($P_{,}$), returning at the breeding-season to its offshore spawning-area, so that the drift inshore of the eggs and larvæ repeated in ontogeny the phyletic history. The same principle held when, from the pelagic inshore water, the littoral region was reached, and later the deeper offshore bottom-areas. In each case, the pelagic eggs formed a starting-point for the ontogenetic migration.

The plaice (Fig. 4b) will be seen to follow this migration almost exactly, its ontogenetic migration being well-marked throughout.

The flounder (Fig. 4c), on the other hand, has complicated matters by itself becoming partially adapted to a fresh-water habitat (F), so that the young forms migrate up the rivers. In this case there

[1] *Cf. Rep. Fish. Board Scotland*, ix., p. 317, 1891; and T. Scott, *op. cit.*, vii., p. 382, 1889.

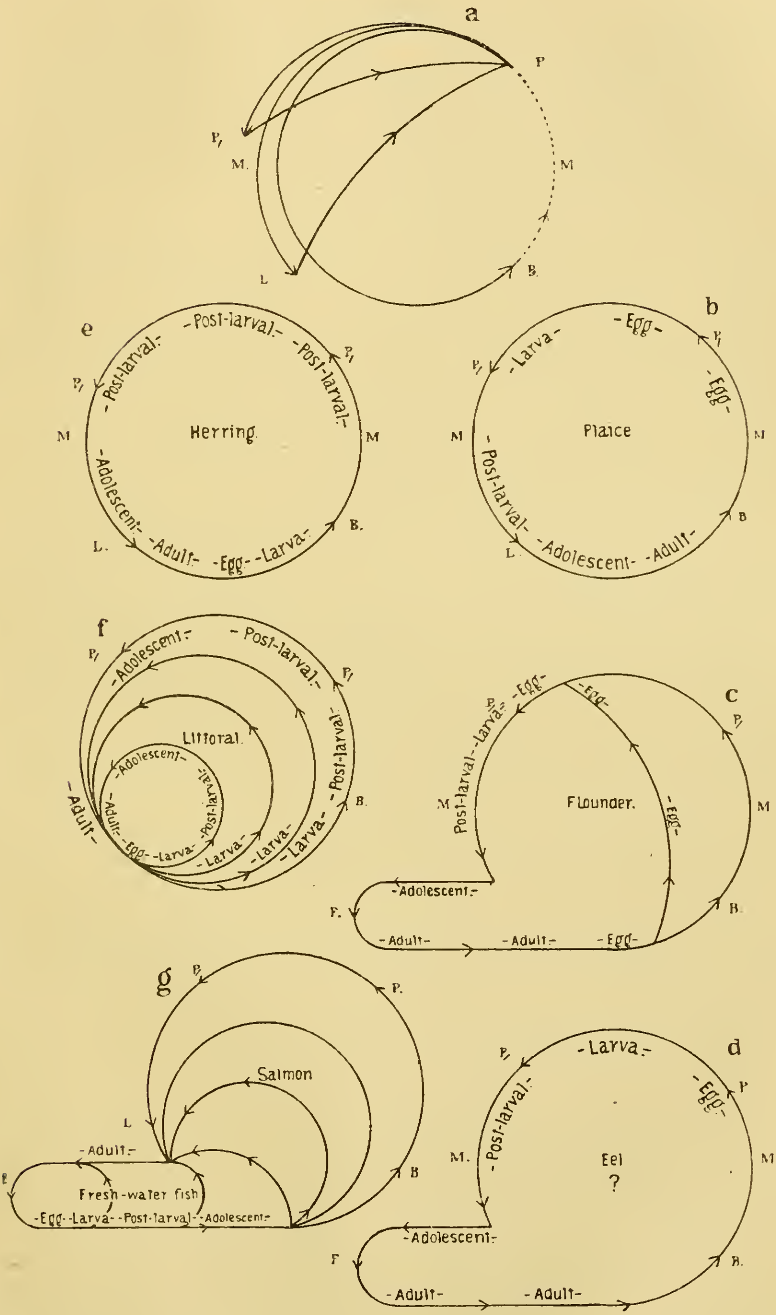


FIG. 4.—DIAGRAMS SHOWING THE ONTOGENY OF MIGRATIONS.

P = pelagic water offshore; P_1 = pelagic water inshore; M = midwater region; B = bottom water region; F. = fresh water region; L. = littoral water region.

is a migration sea-wards, but the eggs are small and laid comparatively close inshore.

The next stage in this process is probably seen in the eel (Fig. 4d.), in which the fresh-water habitat has become more pronounced, and, indeed, the normal one for the adult. It is possible that the egg is pelagic, and, after the curious leptocephaline postlarval stage, the adolescent forms (elvers) migrate up the rivers.

In the demersal series, the herring (Fig. 4e) lays its eggs on the sea-floor in moderately deep water, and the larva remains in the same habitat. The postlarval stage, however, moves upward, and passes its time in the pelagic water, eventually coming in to the littoral region. Here the demersal condition is seen in its earliest stage, and from the other littoral fish can be selected a series (Fig. 4f) in which the ontogenetic migration is gradually given up, till, in such as the viviparous blenny and the stickleback, the young forms immediately assume the habits and habitat of the adult.

Certain of these littoral fish have migrated to the fresh-water—*e.g.*, salmon (Fig. 4g),—and these, having in their young stages no adaptations to pelagic life (as in the case of the eel) to contend with, the eggs are laid far up the rivers, secure from the many dangers of the teeming littoral region, though the young salmon, in obedience to ontogenetic exigencies, come down the rivers and complete a migration in the sea. The extent of this is not quite known, but salmon have been caught in the pelagic water far out at sea, so that it is possible that the cycle is fairly well repeated. In the case of the freshwater fish, this cycle is finally given up.

The katadromous type is essentially that of a pelagic-spawning fish with secondary freshwater leanings, and the anadromous that of a demersal-spawning fish with the same proclivities.

It is instructive to note that the ontogenetic migration is eliminated by a process of shifting environmental necessities to a later stage of development. In the herring the sojourn in pelagic waters devolves on the post-larval form, and in the salmon it is further relegated to the adolescent. A stage further back eliminates the migration from the life-cycle altogether. In one case, the angler (*Lophius piscatorius*), there are good grounds for believing that an inshore pelagic spawning-habit has been acquired. The eggs float about in huge masses, and their structure and that of the larvæ is distinctly of the demersal type. The fate of the larvæ on hatching is unknown.

It must not be supposed that, by these types being placed in gradation, there is necessarily any genetic connexion implied between the adults. In this, as in other groups of animals, the closest allies exhibit remarkable differences in their ontogeny, and the gradual transition in ontogenetic processes may quite well be worked out from the life-histories of a series of widely divergent species.

University of St. Andrews, N.B. Arthur T. Masterman.

III.

Human Evolution.

V.—According to Nietzsche.

THOUGH Mr. Wells thinks it proper "to put a flat denial to Mr. Coste's flat assertion," he himself, in a most careless manner, introduces into the discussion new and false assertions, which require even a more emphatic flat denial.

Mr. Wells would make the readers of Natural Science believe that Nietzsche's Gospel is "blackguardism" (or at least the glorification of blackguardism), whereas it is quite the reverse. There is no philosopher more opposed than Nietzsche to everything that is injurious to the human race, and that has a deteriorating influence, while to the anarchism which advocates unlimited liberty for bad men —the prerequisite to blackguardism,—he is, if possible, more opposed than to anything else. He accordingly favours *master-morality* (aristocratic morality), under which the advancement of society takes place, in opposition to *slave-morality* (democratic morality), under which society tends to deteriorate. In every well-regulated and progressive society there must necessarily be a system of ranks and classes corresponding approximately to the merit of individuals and families. The best men should rule and the inferior should obey, if things are to go on well; and when inferior men will not obey reasonable rules, they should be coerced. It is, however, entirely false, and in accordance with the methods of *slave-morality*, to charge Nietzsche with the glorification of blackguardism because he sees clearly that the appropriate coercion of inferior individuals by their superiors is often necessary in order to secure and maintain the highest excellence of society.

Mr. Wells, like many others who assume the functions of moral teachers, does not seem to have any adequate notion of the nature of true morality. The following remarks of Nietzsche apply alike to Mr. Wells's sociology and to his ideal:—"My objection against all the sociology of England and France is that it only knows *decaying types* of society by experience, and quite innocently takes its own instincts of decay as the standard for sociological valuations. *Deteriorating* life, the decline of all organising power (*i.e.*, separating, gap-making, subordinating, and superordinating power) formulates itself as the *ideal* in the sociology of the present day."

Nietzsche makes a further remark in the same book ("Twilight of the Idols"), which people in this country would do well to ponder. He says that "morality is not yet a problem for the Englishman." It is almost universally taken for granted that measures which make any class of persons happy and comfortable are necessarily good; whereas the facts are almost always overlooked by English moralists that happiness and comfort may in many cases be unmerited, and that measures which result in unmerited advantages to a certain class bring about indirectly undeserved disadvantages elsewhere, and may be prejudicial to society as a whole. Nietzsche saw clearly that the popular notions of morality (which have hitherto also been supported by academic influence) were quite superficial, and exercised an injurious effect. He therefore devoted himself to the great task of effecting a transvaluation, or rather a counter-valuation, of moral values, which involved, in fact, as the title of his unfinished work indicated, a re-appraisement of all the estimates of worth.

It is not enough, however, for the philosopher and the scientist to discover that a system is wrong; their task is not completed until they have explained how the wrong system came into existence and maintained itself. This task Nietzsche has fully accomplished, and has thereby, as it were, put the copestone on the Darwinian edifice, by explaining the evolution of morality—a problem which remained unsolved until Nietzsche's *Genealogy of Morals* appeared. Though he speaks somewhat slightingly of Darwin, his philosophy can, nevertheless, be best understood from the Darwinian point of view—indeed it forms the last and most important chapter of the Darwinian system. The most important distinction among animals is their fitness or unfitness for the conditions of existence, in fact, their superiority or inferiority. It is the same among human beings. As among the animals, however, we see that certain inferior creatures have adopted devices which enable them to maintain themselves (*e.g.*, the serpent by means of its venom, the skunk by means of its stench, the porcupine by means of its quills, other creatures by means of mimicry and suitable coloration), so among human beings the inferior slave-morality has been successfully promulgated under a series of favourable conditions during the past two thousand years, in the interest of the inferior class of human beings, and has partially displaced the older master-morality in those nations which are "in the foremost files of time," and in which, consequently, abnormal social evolution has the best opportunity of developing without producing immediately fatal results. It is somewhat on these lines that Nietzsche explains the origin of the modern notions of morality. It seems to me, therefore, considering the importance of these ideas, that no one is properly qualified to write on morals, or even on human evolution who is unacquainted with Nietzsche's writings.

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IV.

The Genealogy of the Sciences as the Basis of their Bibliography.

HUMANITY, that "Great Man," possesses a knowledge so vast, that in our days even a "God" could scarcely compass it. No one can see and comprehend all that has been made clear and comprehensible. For man cannot become master of all sciences, nor, if there were but one, could he master it. We do not suspect the quantity of work, the wealth of knowledge, represented by our scientific periodicals; and at every moment, while engaged in our researches, we deceive ourselves. No encyclopædia can contain everything. Special dictionaries and treatises may serve as guides to lead us to the original sources. But what devious paths we have to follow before we know what to read to find out about any given subject! And yet we only arrive at it by searching and searching, often without success.

Bibliographies are far from rendering us the services they might. Besides, it is only of late that we have occupied ourselves seriously with scientific bibliography, and what has been done up to the present is merely preparatory. Thus, for example, the "Catalogue of Scientific Papers" of the Royal Society of London can only be considered as a first attempt, in which English, French, and German publications alone have been seriously dealt with, whilst other languages, *e. g.* Russian, have been omitted altogether, or put in here and there. Nevertheless it forms a list of works under authors, very useful in certain cases, but not for those who seek the literature relating to the subject of their study.

Despairing of achieving single-handed a larger undertaking, the English invited every nation to take part in such a work. An International Bibliographical Congress assembled in July 1896, under the auspices of the Royal Society at London. According to the newspapers, the Congress dined well, listened to fine speeches, and then, before separating, certain rules were made, regulating in some way the workers in other countries. It was unanimously resolved that it was desirable to finish the "Catalogue of Scientific Papers," so as to have as complete a list as possible of the scientific literature of the century. It was also resolved that it would be well to classify the material. Classify—yes—but how classify? Here doubtless there were as many opinions as there were men who had considered the question. They discussed . . . . and they postponed the discussion to a later date; for one must further discuss and further reflect, and again reflect, and discuss and re-discuss, for this is a question of principle.

The question of classification of scientific papers is a difficult one; it is a question which each can decide in a purely conventional manner, but which perhaps will never be decided rationally and in a really scientific way.

The decimal system[1] of classification is only a method, a *notation*, not a classification strictly speaking. One can apply such a notation to any classification. But the classification of subjects, and of sciences and their subdivisions, ought to be quite independent of any preconceived system. The decimal classification, already in use for science, is based on a purely conventional system which may satisfy a librarian, but should not satisfy a man of science. Therefore the London Congress rejected both the good (of the notation) and the bad (of the classification), and called for a further pause before classifying.

In order to understand the difficulty better let us take an example:

Knudsen, Martin. De l' influence du plankton sur les quantités d'oxygène et d'acide carbonique dissous dans l'eau de mer. *Comptes Rendus*, cxxiii., p. 1088. 1896.

How shall we classify this? Is it zoology, botany, or physiology? No, it is rather chemistry, or perhaps physico-chemistry? It is certain that the work belongs to science, thus the first cipher of the decimal classification is obtained; but the second cipher is still doubtful. Having plenty of time, we write to Copenhagen to ask the author. He replies "Oceanography." Oceanography! that is not included in the decimal classification. Never mind, let us admit it as a new subject. But oceanography is "Physical Geography," and Physical Geography is Geography, and thus our first cipher for classification becomes inexact. The work falls outside the domain of science, into the same great division as history. I refer here to the classification as emended by the Brussels Institute, and not to the American system in which Physical Geography is comprised under Geology, a subject with which this paper of Knudsen's has nothing in common. But any geographer of the modern school will tell you that geography forms part of the domain of science, that oceanography is a distinct science, a portion of Physical Geography, which is nowadays divided into mathematical geography, morphology of the terrestrial surface, oceanography, climatology, etc., etc. Now, to classify this work in its proper place so as to be able to find it again at need, so that its catalogue-slip may be of use to the original

[1] Mr. Dewey's system was described in Natural Science for July 1896, vol. ix., pp. 43-52

investigator, demands an acquaintance with the most modern tendencies of oceanography; and this can only be expected of an oceanographer. What is true of oceanography is equally true of other sciences. All scientific men recognise this, and this is why the Royal Society Committee preferred to wait and reflect, before setting to work. The London Congress did well in refusing to adopt any system.

The decimal notation then does not solve the question. Even if one admits this notation for the classification of scientific papers, a base of operations is necessary, *i.e.* a classification of the sciences. And one cannot take any classification; a purely conventional one will not suffice, it must be a natural one, *i.e.* simple and corresponding to reality: a philosophical classification.

It is not possible to trace in a few words the history of the classification of the sciences. I will not even describe the classifications of Ampère, A. Comte, Herbert Spencer, Raoul de la Grasserie, and many others,—because that would have nothing in common with the end in view. On the other hand I have no intention of formulating a new classification in all its details. I desire simply to explain the principles of a classification based on the history of the natural development of science, a classification which must be drawn up by competent specialists, and which would supply, without doubt, the needs not only of a bibliography of science, but many other needs as well. The subject is too vast and difficult for me to discuss adequately; I content myself with setting forth various considerations merely to fix our ideas.

The universal division of science is into pure and applied; then, setting aside applied sciences, we are wont to divide the pure sciences into abstract and concrete. For the rest, classifiers merely enumerate the chief "abstract" sciences without defining them. Thus it is left to each of us, with his own peculiar prejudices, to decide what is meant by mathematics, astronomy, physics, and so forth. Helmholtz, in dividing science into "Naturwissenschaften" and "Geisterwissenschaften," was equally conventional. Equally with those who speak of concrete and abstract sciences, he regarded sciences as definite and stable units, believing it possible to define a science. But what do we understand by the Natural Sciences? For Delbœuf, Geometry is a natural science. And I, who had the good fortune to follow his lectures on the philosophy of the sciences, was convinced by his arguments, and so am perfectly prepared to admit that geometry comes within the realm of Natural Science. Nevertheless only few naturalists, I think, will accept such a heresy. We should, then, have to discuss what was meant by natural sciences,—and it would be just the same with any conventional term.

To take an example. Since sciences are divided into concrete and abstract, let us ask if any science, say chemistry, is an abstract science? If yes—what do we understand by chemistry? All the assemblage of chemical sciences? Certainly not! In chemistry some portions are purely abstract, but others depend on observation pure and simple, on experiment, application, and so on. By the side of purely mathematical sections are others of speculative philosophy, hardly to be considered science at all, and yet finding place in that whole called chemistry. And it is really a whole which we cannot disintegrate merely to preserve the abstract part of it for abstract science and so maintain the prevalent convention.

It is needless to labour the point. A subdivision of pure science into concrete and abstract is inapplicable, because, however interesting, it has nothing in common with the actual facts of the case.

It is the same with pure and applied science. Pure sciences have developed and produced applied sciences; thus chemistry has produced analytical chemistry and metallurgy; or they may receive direct application in some other science (*e.g.* biological chemistry), or in some art (*e.g.* agricultural chemistry). And chemistry itself was originally an applied science; for the chemistry of the Egyptians was but a trade, while alchemy at first pursued a purely practical end. Medicine is admittedly an applied science. Anthropology on the contrary, although a daughter of medicine, is a pure science. It is medicine also that has given rise to embryology, physiology, human anatomy, and even comparative anatomy, and many other pure sciences. Why, then, separate all these sciences from the mother science? Because it was and is still, an applied science? No! the sciences are bound together by ties of origin, by ties of relationship,—they are children one of another.

The supposed divisions ought to give place to a more natural classification, denoting, by generic terms, both the various families of sciences, and the groups of minor sciences into which they have split up.

Before developing this idea further, we must ask—what is philosophy and what is science? An answer by definitions is of little value. We simply ask if it is possible to draw a line of demarcation between science and the philosophy of science. I think not. It seems to me that any branch of speculation, at a given moment, is capable of scientific verification; and that any science, originally a speculation, only passed by degrees into the region of the positive; that in fact the sciences sprang from philosophy.

On the other hand every science contains a set of observations centreing round a subject, or pursuing a definite end; they all tend towards perfect knowledge and the comprehension of some enigma. But as one never stops at facts, but rather seeks to explain them and to unveil the mystery, so the sciences as they develop lead us back to philosophy. Considered generally, science forms a means of investigation of nature and natural phenomena, a sort of analysis, all serving to form a basis for philosophic speculation.

But many sciences, aiming too high at first, have had to create auxiliary sciences to furnish them with the necessary materials. Thus geology has created palæontology, and there is in the history of geology a long period during which its votaries were absorbed in the study and classification of fossil organisms; and it was only when these palæontological researches were sufficiently advanced that stratigraphy could re-assert its importance. Finally, in our own days, we have been able to attack with success other problems more directly geological, such as the origin of mountains, *i.e.* dynamics of the earth-crust. The questions treated of to-day are old, they were discussed before geology was a science; and it is only because we philosophised over these questions of a general nature that we were led to record observations, add comments and so, as it were, to build up a science. The needs of geology have likewise created petrology, which in turn has sought assistance from optics, and as that does not suffice, we now see it seeking assistance from chemistry. Shortly we shall hear of synthetic petrology. But this science will take further time to develop properly, because chemistry cannot furnish all that the synthesis of rocks requires.

Thus it is that a science, having become positive, serves another science. Mathematics is an example. Physics another, which at the present time, being of current application in chemical, physiological and other investigations, renders services both by its methods of investigation and by its laws and other acquired influences.

But on the other hand many well-defined sciences, becoming relatively far advanced in their development, fuse, making place for higher investigations where philosophy already impinges on science. Physico-chemistry, which in these latter days has developed in Germany to a considerable extent, which has already numerous methods of investigation, and which is sufficiently well-characterised as a distinct science, is a good example. And thus we see sciences, coming from one stock, dividing up in the course of development, and then fusing again to pursue ideas still greater than before, to synthetise the knowledge acquired in some more general law, and in this way, by the intermediary of purely scientific investigation, one is led to the philosophy of one or the other great branches of human knowledge. Thus, close bonds unite positive philosophy to scientific investigation, and it would be very difficult to draw a clear line of demarcation between science and philosophy.

All things meet, but all are not confused. Order exists; let us look for it. Let us first see the historical beginning of a science. Origins are vague. One can always push the origin of a science back as far as one will; but in reality the science has a history only from the time when it is individualised, the day it ceases to be a part of an assemblage of subjects till then confounded. Generally its birth is due to some great thinker, who has specialised in the branch. Such an one, by inventing methods of experiment, or by co-ordinating his observations, has individualised the science, characterised it, and given it free development. The history of oceanography is an example.

Sniadecki in his remarkable work on the theory of dissolution (1808), said that "sciences are born or become perfect by the discovery of their origins, *i.e.* of some observations embracing a whole group of natural phenomena." And this is true, because if certain sciences have had their founder, others have been called to life by some special discovery, made in some earlier science. A simple observation, well made, may be the germ of numerous discoveries connected with a field of study absolutely unexplored. It may lead to the explanation of phenomena hitherto inaccessible. Bacteriology is a case in point.

Thus then, although their origins are vague, sciences are born, and each has a date of birth; and if all sciences are not founded in the same manner, if there are no general laws governing their mode of origin as well as their development, we are yet able to affirm of all that they never figure in history except after one more or less definite date which can be determined. There are young sciences and old sciences; some are still in embryo (still incorporate with the mother science and developing through her); others have died, but their ruins still serve and will serve always. Sciences, then, are born. And this fact alone gives to each a definite relationship, which may be discovered by the study of its history.

A general and philosophical history of sciences, such as is yet unwritten, would therefore be of the highest utility. Houzeau, in one of his admirable addresses to the Academy of Belgium, has shown that the history of science reveals to us a regular evolution of the human mind. For sciences have evolved like everything else. But more than that. Sciences live; live in humanity just as languages do, or as beings live in nature. The sciences, in the same way as languages, have a genealogical tree. They also are differentiated. The history of the development of science is like that of life, and results in a tree with branches and twigs, of which each twig represents one science. The branches are the mother sciences. The historian of science should begin his work by the study of this tree; and only when it has been traced and explained thoroughly, shall we be able to say we have a history of science.

Order therefore exists—natural order. Natural order is the genealogical tree.[1] Classification therefore must be preceded by a

[1] In his introduction to the Encyclopædia (1781) d'Alembert after having characterised the different sciences, adds "Après le détail où nous sommes entrés sur les différentes parties de nos connaissances, et sur les caractères qui les distinguent, il ne nous reste plus qu' à former un *arbre généalogique ou encyclopédique qui les rassemble sous un même point de vue, et qui serve à marquer leur origine et les liaisons qu' elles ont entre elles.* . . . . Quoique l' histoire philosophique que nous venons de donner de l' origine de nos idées, soit fort utile pour faciliter un pareil travail, il ne faut pas croire que l' Arbre encyclopédique doive ni puisse même être servilement assujetti à

history; for no classification can be rational unless it is founded on that genealogical tree which the history of science ought to furnish. The progress of science dates only from the middle of last century. It was then only that the knowledge of mathematicians and philosophers became grouped in distinct sciences. The history of science then is recent, because, if need be, we are able to begin with the encyclopædists. And from our point of view, the more modern part of this history interests us the most.

A classification to be useful to a bibliographer should be complete. The natural classification that we demand from history should go further, in the dismemberment of subjects of study, than do the sciences themselves. And it is modern history which tells us how each science is subdivided into sections, embracing each subject of particular study. The history of each science considered separately can go into great detail; it can give numerous data, and as the annals of the sciences are very precise, this history can give a classification down to the minutest details desirable. Thus then each science may be represented by a bundle of branching twigs, upon which we can group with certainty the material to be classified. And in this way we can collect everything that ought to be collected, and classify it, without there being any particular difficulty in avoiding confusion.

But all is not to do, for that which is done persists. Imperishable monuments have been raised to science. Houzeau and Lancaster's Bibliography of Astronomy is one of them. It is to be noted that this Bibliography is based on history; this is more than a series of biographies, it is a history full of observation, a model.

Others than Houzeau have written on similar lines. Each science perhaps has its historians.[1] The Germans and the English have written more than one history of the mathematics, of physics, of chemistry, and other sciences. It is not for us to criticise them; but one may remark that, though precious, they are of no use for our pur-

cette histoire. Le système général des sciences et des arts est une éspèce de labyrinthe, de chemin tortueux, où l' esprit s' engage sans trop connaître la route qu' il doit tenir. . . . . On pourrait former l' arbre de nos connaissances en les divisant, soit en naturelles et en révélées, soit en utiles et agréables, soit en speculatives et pratiques. . . . . Nous avons choisi une division qui nous a paru satisfaire tant à la fois le plus qu' il est possible à l' ordre encyclopédique de nos connaissances et à leur ordre généalogique. Nous devons cette division à un auteur célèbre (Bacon) . . . . nous avons pourtant cru y devoir faire quelques changements." Further on we read: "Ces trois facultés (mémoire, raison, imagination) forment d' abord les trois divisions générales de notre système et les trois objets généraux des connaissances humaines, l'Histoire qui se rapporte à la mémoire; la Philosophie qui est le fruit de la raison, et les Beaux Arts que l'imagination fait naître." That is how d'Alembert was brought to put the history of minerals and plants by the side of civil history, and to unite metaphysics with mathematics. But his errors concern us little; he did the best for his time. These extracts have not been made to criticise him, but to render him his due. The idea of a classification of sciences based on such a genealogical tree was clearly formulated in 1781.

[1] See Jahresberichte der Geschichtswissenschaft,—§70 of the last few volumes especially.

pose. A good history of chemistry, for example, has yet to be written, and the work done on the lines here suggested would be more interesting. The history of geology also might contain interesting pages, but this history does not yet exist.

We may be told that there are many sciences, and that if we must wait for the history of each before pushing onwards, we shall always remain in one spot, and nothing will ever be done. Yes and no. The Germans are capable of furnishing us this kind of work in a short time, and it would be done conscientiously. The Munich Academy has already published a general history of science. We cannot use it; but never mind, the example is there. And examples of this kind abound. The Germans know how to combine to work on a given plan. Why then not ask of them a general history of sciences, prepared by the most competent men? The thing is possible and its realisation does not offer any special difficulty; sooner or later therefore it will be realised. With such a history of the sciences we should have a sure guide for classification.

There is, however, a real difficulty before us. What do we mean by a science? For example, is statistics a science? Evidently not. It is only a method which can be applied in many sciences. Yet certain sciences can equally well be applied as methods of verification. There lies one difficulty.

Let us take another example. Is graphology a science? Its founder considered it as such. It has a certain aim, the subject is well defined; but nevertheless we cannot admit that it is really a science, because to make scientific observations you must have a method; and graphological observations are made without method, they are conventional. Graphology is an art (founded on observation) as alchemy was an art (founded on experiment). But when experimental psychology shall lay hold of this art, it will give it a rational basis and make a science of it. From the historical point of view, graphology does not come to us from any science; it has a date of birth, but no scientific parentage, no genealogy.

Sociology which is still in embryo, is quite different. It is a true science and very abstract, entirely general, having need of very diverse positive knowledge. It is a result of other sciences, and of very diverse studies, which it summarises. It is a name applied to the general results of a great branch of science connected with the study of humanity, and is in a way the philosophy of all these studies.

Thus, we see from the example chosen, the difficulties are very great. Sciences are so little comparable to one another, that one cannot on *a priori* grounds eliminate any one or admit any new science into the general domain without preliminary study, and without considering the history of its development. The classification of sciences demands, as does that of organisms, a comparative anatomy and an embryological study of each individual member. This ought to be made, but it should be made by specialists. It is for those who devote themselves to science, to study the mode of development of each branch of human investigation, because they alone are competent to dissect each science, to enumerate and define them, to trace their histories, and to make a classification.

The principle of a natural classification here proposed being admitted, one must not fail to meet the question and discuss it properly. The difficulties presented will be overcome by work. History will elucidate relationships, and it will put our knowledge in order. The genealogical tree will appear. The classification of science will be complete.

Bibliographers must not infer from all this that they have nothing to do while waiting. Far from it, the work they ought to furnish is immense. The complete index of scientific works will demand great efforts before its termination. This catalogue under authors will take many years; and it is only when the list is complete that it will be worth while to pull it to pieces to make a scientific and systematic bibliography. But for this grand preparatory work, detached indices suffice, granted that the sum of all the indices is complete. There is no need to recommence anew the work already accomplished, it is only necessary to complete what has already been compiled. The first step in this direction will be to make a Bibliography of scientific Bibliographies. The second step will be to hunt up and point out what has not been ransacked. A third will be to do what remains to be done.

It is this way that I have set to work. By means of detached indices I wish to complete the compilations of the Polish bibliographers, by adding to their work materials for a bibliography of scientific papers published in our language. I have invited my compatriots to aid me in this work, because it is not given to me to complete the work begun.

The notes collected up to the present, for a bibliography comprising the years 1800–1873, that is to say all the period which preceded the foundation of the Cracow Academy, suffice to give us an idea of the progress of the scientific activity of Poland. Progress can be expressed in figures, and these can be rendered graphically. By taking years as abscissæ, and the annual amount of scientific publication in Poland as ordinates, we obtain a curve which represents the varying scientific activity of the nation. This curve, which I have constructed, is very interesting though sad. It begins by a rapid rise —the influence of the brothers Sniadecki and of Staszic, three men who did much for their country. In 1805 the curve falls; then it grows by successive jumps to 1829, when it attains a high maximum. The "Society of Friends of the Sciences of Warsaw" is now the focus of activity. Then comes the year of the revolution: the curve falls abruptly, then breaks; in 1832 it is at zero. The Warsaw Society, founded by Staszic is no more; but that of Cracow has survived and will prosper. The curve rises again, gently at first, then more rapidly and with a co-efficient of growth greater than in the homologous section of the first years. In 1841 and in 1844 we have two maxima greater than those of 1829: again we are on the eve of revolution. 1846: fatal year for Poland, and fall of the curve. It does not rise till after 1848, and attains a maximum in 1851. Then comes the Crimean War. The losses sustained by Russia lead her to exploit the Poles afresh; want of money causes a cessation of publication, and it is only after several years of fluctuation that the curve begins again to rise, again sharply till the eve of the revolution of 1863. But this fall is not so severe as the preceding, and from this moment the curve grows more and more rapidly; it has the form of the limb of a hyperbola.

I regret that I cannot prolong this curve for Poland to the present day. I also regret that I have not the necessary data to trace the curve of progress of scientific literature in Russia and Bohemia. But however that may be, the facts allow me to charge Alphonse de Candolle[1] with injustice. In speaking of the development of science in Poland and Russia, he only considers insufficient evidence, and, without taking the trouble to seek for the causes, he begins to philosophise. Now to philosophise it is necessary to have far better evidence than that now at our disposal, for even basing our arguments on Bibliography, on the Catalogue of Scientific Papers for example, we should equally arrive at entirely erroneous conclusions.

It is absolutely necessary that scientific men of all nations should take part in bibliographic work in order to obtain positive data. The great bibliography of scientific work—this Bibliography of the 20th century—must be really international, because it must be a monument to science, and not to the scientific activity of that group of nations which to-day are found at the head of civilisation. To arrive at a successful conclusion it is necessary before all not to deceive ourselves as to the aid that may be given by governments. And yet we do—unfortunately.

At the London Congress we saw a crowd of government representatives. English scientific literature was represented by the governments of the British Colonies, even of those that have as yet no literature of their own. Slavonic literature on the contrary had no government representative. Shall we be better off at future gatherings? We want to know also whether the Germans will cease their convention of considering German works published in Vienna, Prague or elsewhere, as German literature? Whether Finnish literature and the German literature of the Baltic Provinces will become, by similar convention, Russian literature? Whether the Poles will permit their literature to be divided among the governments on which they are dependent, and if, also by international convention,

[1] "Histoire des Sciences et des Savants," pp. 238–241.

the publications of the "Polish Society of Science of Paris," ought to be considered by the French *savants* as belonging to French literature? These questions appear ridiculous. Yes they are foolish, and yet they were not raised, so far as I remember, at the London Congress.

The fact is that the representatives of the governments have nothing to say and nothing to do at a congress on the bibliography of science. Science is for men of science, not for governments. It is to the interest of philosophers and of the learned societies to have this bibliography. But those who have no understanding of the matter take no interest in it: and governments are in this position. Governments are only migratory institutions, which science may exploit, but on which she must never depend, because science, which marches in great strides, must be free. The example of Poland will suffice. To-day still the Russian Government, far from aiding, does its best to choke intellectual development and culture in Poland; and though, in spite of all, science progresses on the banks of the Vistula as elsewhere in the civilised world, it is because science belongs to the world and not to the governments.

To conclude. If we must combine, it is for men of science to do it; for they alone can understand the principle of international solidarity, they, who, with sure and continuous step, march in the front of progress.

HENRYK ARÇTOWSKI.

London, *February* 15, 1897.

V.

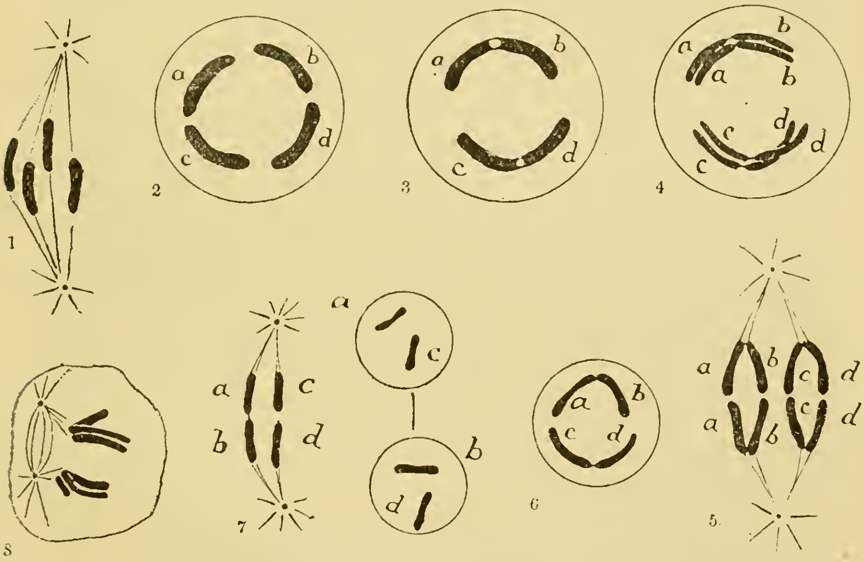
The Facts of Chromosome-reduction *versus* the Postulates of Weismann.[1]

It is advisable that any new observations which bear upon the data supporting a popular theory should be widely known. The present article calls attention to the consequences which arise from the latest contribution to our knowledge of the maturation processes in sexual cells. As is well known, Weismann in the "germplasm" theory regarded the nuclear chromatin as the hereditary substance, while the chromosomes which always appear during nuclear division, in numbers that are fixed for each species, were supposed to contain equal quantities and qualities of hereditary units—"ids." In consequence of this hereditary equivalence of the cells, it is necessary that some reduction in the chromatin, the hereditary substance, should take place before fertilization can occur, since the essential process in fertilization is generally regarded as an interchange of half the hereditary substance of each of the conjugating cells. Weismann (1) was thus led to postulate as necessary the existence of peculiar divisions, in which this chromosome-reduction, or halving of the hereditary substance, could be brought about. The "Reductions-theilungen" or reduction-divisions as Weismann conceived them, consisted of final nuclear divisions during the formation of the sexual elements, in which half the original chromosomes passed unsplit directly to the poles, as in fig. 1.

Now, although it has been subsequently shown that such a process, if it exists at all, is not of general occurrence either among animals or plants, it is still maintained by Häcker (2) that the reduction postulated by Weismann may really be carried out, but in another way. It is agreed now among all observers, zoologists and botanists alike, that a halving of the *number* of chromosomes does actually take place, apparently universally before the maturation of the sexual elements. But this merely numerical reduction is not brought about by any nuclear division, it always follows a remarkable resting-

[1] Readers unfamiliar with the terms used in this paper will find an explanation of them, as well as a statement of the facts then known, in Mr. M. D. Hill's articles on "Cell-Division" in Natural Science, vol. iv., pp. 38 and 417, especially p. 425 (Jan. and June, 1894).—Ed. Nat. Sci.

stage in the maturation of the sexual cells, elsewhere termed by me the "synaptic rest" because this rest is universally followed by the disappearance of the regular number of somatic chromosomes and the evolution of half that number of chromatic rings (heterotype chromosomes). The succeeding heterotype division is generally followed by another, in which the same reduced number of chromosomes is retained. Now Häcker, together with Vom Rath (3) and Rückert (5), has maintained that this last division differs from the heterotype and all other mitoses, in that here the chromosomes split transversely, and not longitudinally as in every other observed case. If this is so, and if we assume as Häcker does that the synaptic rest results in the fusion of pairs of chromosomes end to end, then it will be seen that if we represent, say four chromosomes, typical of the



Figures 1–8.—Methods of Chromosome-reduction.

somatic divisions of any species, by the letters *a*, *b*, *c*, *d* (fig. 2), the effect of the synapsis will be to unite these chromosomes together in pairs as in fig. 3. In the ensuing division these double chromosomes ("plurivalent chromosomes" of Häcker) split longitudinally to form the double V's ("tetrads" of Häcker) as represented in fig. 4. These bodies subsequently divide, in the manner represented in fig. 5, to form two new cells each with two compound chromosomes, as in fig. 6; and according to Häcker, the final division is brought about by these compound chromosomes becoming disunited from one another and passing unsplit into each daughter-cell. It is thus possible to imagine that in this way a distribution of the chromosomes may be brought about exactly similar to that which Weismann originally conceived, and that this final transverse splitting may be really what Weismann's postulate requires.

It is, however, obvious that the probability of this or of any other process representing in reality a true reduction, depends absolutely on the universality of its occurrence. If, therefore, the process now put forward by Häcker, Rückert, and Vom Rath, as the "Reductions-theilung" can be proved to be in no sense universal, then we are not only without assurance that the process described by these authors *is* a "Reductions-theilung" at all, but there arises the greatest probability that, the final phenomena of maturation being thus conclusively demonstrated to be diverse, the theoretical value of reduction is annihilated altogether.

In 1894 the work which directly substantiated this new type of reduction was almost if not entirely restricted to observations made upon arthropods by the above-mentioned three authors. In 1894, however, Vom Rath (4) published a paper on the spermatogenesis of *Salamandra maculosa*, which not only flatly contradicted the previous careful work of Flemming (6), but which, if true, went far to establish the wide-spread and possibly universal occurrence of what we may call "the new reduction" in animal cells. It was the most important paper that the supporters of this type of reduction had produced, since in it Vom Rath unhesitatingly declared that the processes previously described in Arthropoda, also occurred in Amphibia in full.

It was impossible for me at the time to investigate the subject anew; but I subsequently (7) became quite convinced that so far as the maturation process in elasmobranchs was concerned, tetrad formation and reduction in Häcker's sense did not exist. It is hardly probable on the face of it that the cardinal points in the spermatogenesis of amphibians and elasmobranchs differ widely each from each; and the diametrically opposed character of Vom Rath's work and my own, led naturally to the enquiry whether one or other set of observations was not altogether wrong.

The answer to this question appears in a recent paper by Meves (8), who, with the full concurrence of Flemming, refutes almost the whole of Vom Rath's work. He shows in the first place that Vom Rath has fallen into the grossest error concerning the number of post-synaptic generations, that tetrad formation is in Amphibia, as Flemming had long ago ascertained, a rare abnormality, generally associated with degeneration phenomena. Lastly, and most important of all, he shows that the final division is a true example of longitudinally split, not transversely separating, chromosomes (fig. 8)—being in fact the very type from which Flemming originally made his description of the homotypic form of division. There is moreover, as Meves himself points out, the closest agreement between the processes I describe in elasmobranchs and the spermatogenesis of Amphibia. We have thus two more types which rank with Brauer's observations upon *Ascaris* (9), as directly contradicting the universality of the "Reductions-theilung" described by Häcker. Such being the case, it is obvious that these authors can no longer maintain the universal occurrence of this process, and consequently the theoretical value of the remaining observations of Rückert and Häcker has in this respect vanished altogether.

Naturally I have no wish to question the accuracy of Rückert and Häcker's work on the Arthropoda. If the process they describe really occurs in these animals, it is certain that it simply exemplifies a diversity in the phenomena of maturation, which at once abolishes all the theoretical importance of reduction, in the sense in which these authors originally employed it. In fact it cannot be too clearly stated at the present time, that Häcker's, no less than Weismann's, attempt to support the theory of reduction on a basis of observed facts, has altogether broken down.

The state of our knowledge on this subject being then in the unedifying condition I have just described, it is not a little unfortunate that, in his work "The cell in development and inheritance" Professor Wilson should have given the by no means impartial description of these processes that is to be found in the chapters dealing with reduction.[1] As this book is popularly written, it is likely to be widely read, and the incorporation of erroneous observations in support of the theoretical conclusions it contains can only perpetuate that obscuration of the facts which the reiterated assertion of the universal occurrence of the new reduction has already produced.

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[1] On page 189 Professor Wilson also attributes to me the assertion that the ring chromosomes *do not* arise by a longitudinal splitting of a primary chromatin rod. Had he taken the trouble to read page 289 of my paper "On the . . . . . Spermatogenesis of Elasmobranchs (*Quart. Journ. Micro. Sci.* vol. xxxviii., Part II, pp. 275-313, 1895) before misquoting it, he would have found the following passage describing the origin of the heterotype rings: "The polarised thread-work is disposed throughout the nucleus in long parallel loops, the free ends of which, if they exist, are difficult to discern. After a time the threads begin to show longitudinal splittings, the double ropes thus formed dividing into equal segments, eventually giving rise to twelve thick loops, which form the twelve ring chromosomes, typical of the division of the second spermatogenetic period."

[*For Literature, see over.*

LITERATURE REFERRED TO.

1. **Weismann, A.**—"The Germ-Plasm." Pp. 235–236 of English translation. London, 1893.
2. **Häcker, V.**—"Die heterotypische Kerntheilung im Cyklus der generativen Zellen." *Ber. naturf. Ges. Freiburg*, vi., pp. 160–193, pls. x.–xii. 1892.
3. **Vom Rath, O.**—"Zur Kenntniss der Spermatogenese von *Gryllotalpa vulgaris* Latr." *Arch. für. mikr. Anat.*, xl., pp. 102–132, pl. v. 1892.
4. ————————.—"Beiträge zur Kenntniss der Spermatogenese von Salamandra." *Zeitschr. Wiss. Zool.*, lvii., pp. 97–140, pl. vii. 1894.
5. **Rückert, J.**—"Die Chromatinreduktion bei der Reifung der Sexualzellen· *Ergebn. d. Anat, u. Entwick.*, iii, pp. 517–583. 1893 (1894).
6. **Flemming, W.**—"Weitere Beobachtungen über die Entwicklung der Spermatosomen bei *Salamandra maculosa*." *Arch. für mikr. Anat.*, xxxi., pp. 71–97, pl. vii. 1888.
7. **Moore, J. E. S.**—"On the Structural Changes in the Reproductive Cells during the Spermatogenesis of Elasmobranchs." *Quart. Journ. Micr. Sci.*, n.s., xxxviii, pp. 275–313, pls. xiii–xvi. 1896.
8. **Meves, F.**—"Ueber die Entwicklung der männlichen Geschlechtszellen von *Salamandra maculosa*." *Arch. für mikr. Anat.*, xlviii, pp. 1–83, pls. i–v. 1897.
9. **Brauer, A.**—"Zur Kenntniss der Spermatogenese von *Ascaris megalocephala*." *Arch. für mikr. Anat.*, xlii, pp. 153–213, pls. xi-xiii. 1893.

VI.

Oscar Hertwig on Mechanics and Biology.[1]

THE first volume of Professor Hertwig's "Biological Problems of Today," that dealing with Preformation and Epigenesis, was summarised in these pages by its subsequent translator, Mr. P. Chalmers Mitchell, in August, September, and October of 1894 (NATURAL SCIENCE, vol. v., pp. 132–134, 184–194, and 292–297). A review of the English translation was published in our volume ix., pp. 270–271. In the second volume, which has recently been issued, Professor Hertwig has taken "Mechanics and Biology" as his theme, and treats the relation between these two branches of science from a very critical standpoint. The whole book is an energetic protest against the tendency of a certain school of writers, among whom Wilhelm Roux ranks as pre-eminent, to exalt mechanics as the science of the future, and to regard all biological, and especially embryological, phenomena as the outcome of purely mechanical processes.

The general question is discussed by Professor Hertwig in his introductory chapter, and the rest of the book deals more particularly with various attempts to elevate the so-called "mechanics of development" into a new and separate science. The goal to which this science of the future tends, and its methods, are discussed under separate headings, while a lengthy appendix, occupying nearly half the book, is devoted to a more detailed consideration of the laws of development according to Roux, or the "causal morphology" for which he claims such a high place.

Professor Hertwig is doing good service in leading a crusade against the over-glorification of mathematics, and the attempt to force biology into posing as one of the exact sciences. Apart from questions relating to pressures upon the skeleton and similar problems, mechanics in the strict sense have comparatively little to do with biology, and the introduction into zoological writings of words coined for another branch of science, and bearing therein a different connotation, can only lead to mystification. This is, however, an error only too prevalent among modern scientific writers of all nations. Our author draws attention to Roux's present use of the word "mechanics,"

[1] ZEIT-UND STREIT-FRAGEN DER BIOLOGIE. HEFT. II., MECHANIK UND BIOLOGIE. By Professor Dr. Oscar Hertwig. 8vo. Pp. iv., 211. Jena: G. Fischer, 1897. Price 4 marks.

not in its physical, but in a broad philosophical sense, and points out that the new-fangled word "Mechanik," used in this way, has no advantage over the old-fashioned term "Lehre." The latter covers the same ground, and at least cannot mislead. Now Lotze, in 1842, and Roux in 1880, both used the word "mechanics" to express widely-differing ideas. Lotze used it in the stricter physical sense, in opposition to certain writers whose vitalistic theories were supposed to explain all otherwise inexplicable phenomena of life; but Roux, while drawing attention to the work of Lotze, uses his word with a much wider and less definite meaning, in fact in several senses.

With regard to the newness and importance of Roux's doctrine of the causes of organisation, Professor Hertwig considers that his opponent deceives himself on both points. Seeing that Lotze in 1851 wrote of the mechanical explanation of life as a task to which more and more workers were then addressing themselves, it can hardly be reckoned as one of the latest developments of modern science. Kant, Wolff, and Schwann all had, in their turn, the same end in view. According to Kant, every science is a mechanical one, but to tack on the word "mechanics" to zoology and re-christen it "zoomechanics" in a general philosophical sense is not to create a new science. It is, however, with the methods of the "causal morphologists" that Professor Hertwig is most inclined to quarrel. Roux claims that zoomechanical methods are of primary importance, that those of the descriptive zoologists only prepare the way for causal investigators, and are of little or no intrinsic value: their very facts are scarcely worth having. Perhaps, if descriptive zoologists confined themselves, as Roux appears to think they do, to details of external anatomy, his strictures might have greater truth, but Professor Hertwig does not seem to consider Roux necessarily the best judge as to the value of facts supplied by the methods the latter depreciates. "To me," says our author, "nature is at least as trustworthy a teacher as the experimental anatomist." The methods upon which Roux lays most stress are those which he calls "analytical causal thought" and "intellectual anatomy," instruments of investigation which Professor Hertwig does not think will, unaided, be found very advantageous. Embryology is the branch of biology which can least be studied by analytical or mathematical methods, and the tendency of the "causal morphologists" to bring mainly these processes to bear upon biological—and especially embryological—problems, and to reject direct and constant observation in favour of purely experimental work, Professor Hertwig regards as mischievous. Experiment and observation should, he considers, go hand-in-hand, and there would be small chance of original discoveries of lasting value if experiment, instead of being a means to an end, came to be regarded as an end in itself. Probably many who think that merely statistical experiments are too often so regarded in these days will be inclined to agree with him. The great zoological discoveries of the age have certainly been due more to the patient collection of facts and the stringing together of conclusions based upon observed sequences of these, than to experimental work alone. On p. 75, Professor Hertwig gives an instance from Roux's own work of the way in which one-sided experimental work may lead to erroneous or unsound conclusions; while he draws attention to the work of Dareste, Gerlach, Weismann, Boveri, Wilson, Morgan, and many others, both as a proof that he does not underrate experimental work in itself and that the best results are attained when observation and experiment are not divorced.

The last pages of the volume are devoted to a series of critical studies of Roux's laws of mechanical development, and deal with (1) the mosaic theory, (2) the so-called "copulatory track," (3) certain definitions, and (4) cytotropism.

The first study treats of Roux's view that the development of the gastrula in the frog is, from the first segmentation-furrow onwards, a piece of mosaic work (in that a perfect whole arises out of various independently differentiated parts), and, further, of the problem whether there is, or is not, a constant relation between the direction of the first plane of segmentation and the direction of the median plane in the resulting embryo. Professor Hertwig regards Roux's conclusion that there is such a relation as a preconceived theory, into which Roux has, in some Mikado-like manner, contrived to make his results "fit." In the case in question the results arrived at may be valuable or the reverse, but we think Professor Hertwig deviates somewhat from the path of fairness in the view he takes of them. Surely it is too much to expect of any investigator that he should start without a hypothesis at all; this would be the very experimenting "in's Geradewohl" which Professor Hertwig, in his neatly-chosen quotation from Johannes Müller, so deprecates.

In the second study Roux's theory as to the influence of the process of fertilization in determining the direction of the first segmentation-furrow is discussed, and somewhat severely handled.

The third study calls for little special mention, many of the questions therein raised having been already more fully dealt with in Part I. of this work, and already described in NATURAL SCIENCE.

The last chapter is devoted to a newly-discovered phenomenon described by Roux as "cytotropism," which Hertwig understands to mean a process, partially chemiotactic in nature, by which neighbouring cells in a segmenting ovum so react upon one another as to draw each other gradually into closer contact. Professor Hertwig makes a careful survey of the facts which have led to these conclusions, and believes that the former should bear a different interpretation. His comment is somewhat ironically expressed in a quotation from Roux's own writings, in which it is said to be "easier to establish a new fact than to arrive at a true estimate of its significance." In conclusion, Professor Hertwig sums up his own position with regard to these theories in three pithy paragraphs.

"(1) The direction of the first plane of segmentation and the planes of the embryo's body are independent of, and have no causal relation to, each other.

"(2) The process of segmentation does not result in the division of nuclear material into qualitatively dissimilar portions which determine the development of different parts of the body.

"(3) The fact that, in many cases, the planes of segmentation coincide more or less with the axes of the embryonic body, and that individual primitive organs are developed from definite areas on the surface of the ovum, is simply a consequence of the structure of the ovum itself, of its form, and of the distribution of protoplasm and reserve-material."

The book is written throughout in the author's usual lucid and readable style; the appendix is illustrated by a few diagrammatic figures which render the somewhat involved statements of Roux more intelligible; and there is an ample bibliographical list of works to which reference is made, as well as some additional notes.

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SOME NEW BOOKS.

Newton's Birds.

A Dictionary of Birds. By Alfred Newton, assisted by Hans Gadow, with contributions from R. Lydekker, C. S. Roy, and R. W. Shufeldt. Part IV., 8vo. London: A. & C. Black. 1896. Price 7s. 6d. nett.

Professor Newton's Dictionary of Birds is just completed by the issue of Part IV. now before us. Viewing the book as a whole, one can gather a better idea of its scope, depth, and aims, as well as of its relative value as a work of reference, when contrasted with other ornithological works which have appeared lately. To estimate this book at its true worth is difficult. Its merits, perhaps, were never more justly appreciated than by the reviewer of the first part in the pages of Natural Science, vol. iii., p. 146, who described it as "marking an epoch" in the history of ornithology. None will realise the truth of this better than the serious student of ornithology, who has so many and such humiliating opportunities of sounding the depths of his ignorance. Such will ever feel grateful to the Professor and his colleagues for the vast amount of labour and research which they have expended upon their several contributions.

The present part contains the Preface and Introduction. This last occupies some 120 pages, wherein is passed in review the whole history of the rise and progress of ornithology from the days of Aristotle onward to the present time. It is really the Encyclopædia Britannica article recast and brought up-to-date. It should suffice then to say that it preserves all the charm and graceful ease of expression that characterised the original article.

Perhaps the most interesting of Professor Newton's contributions to this part are those on the Solitaire, Song, and Variation. In the latter the author subscribes to the view that those variations in intensity of colour usually regarded as instances of protective coloration—*e.g.*, the pallid colour of desert forms—are really due to climatic influences. The remarkable discontinuous colour-variations which occur amongst certain owls, night-jars, &c., are dealt with under the heading of Dimorphism, or better, as is suggested, Dichromatism. Finally, we are treated to a few brief remarks on abnormal colour-variations, such as Albinism, Melanism, Xanthochroism, and Erythrism; these are collectively grouped together under the head of Heterochrosis.

Dr. Gadow's articles maintain their high standard to the end. Indeed in some cases matter will be found which does not find a place in his larger works. In one instance, however, statements are made which certainly may justly demand further qualification. Thus, in describing the skull, certain membrane bones, to wit, the maxilla, pre-maxilla, quadrato-jugal, and jugals are described as modifications of the first visceral arch. This is surely heresy. Accompanying the description is an analytical diagram of the visceral arches, which, if it is to be made intelligible to the majority of the readers of this book, needs some sort of explanation. As it stands the pre-maxilla and maxilla appear as if each represented a separate arch, though they are described as forming part of the first arch.

As touching the homologies of the spina pubica (pectineal process) and pubis of birds, Mehnert's view will probably find more favour than Miss Johnson's with the majority of those who have given the matter their attention. In describing the tectrices or wing-coverts, it is a pity that Wray's terms, Major, Median, Minor and Marginal coverts were not adopted, instead of "Greater, Middle, and Lesser," since in the plan the marginal coverts are left unnoticed. The Striges and Accipitres should be included with those groups in which the Median (Middle) and Minor (Lesser) coverts overlap *proximally*, and not, as is done, with those having a distal overlap. These, however, are not grave faults, and can easily be remedied should a second edition be called for—which is more than probable.

Nothing now remains but to offer, in the name of ornithologists generally, our hearty congratulations and deep gratitude to Professor Newton and his colleagues for this invaluable work, which will long remain the guide, philosopher, and friend of all who seek its help.

W. P. Pycraft.

Butterflies.

Die Schmetterlingsfauna von Hildesheim. Von A. Radcliffe Grote A.M. Theil I.: Tagfalter. 8vo. pp. 44, pls. 4. (Mittheilungen aus dem Roemer-Museum, Nr. 8.) Hildesheim, 1897. Price 6 marks.

Under this modest title, suggesting a contribution to a local fauna, Mr. Grote has given us a valuable study on the classification and affinities of butterflies. Recently, various writers discussed in Natural Science the question, "Are the Arthropoda a Natural Group?" and several authorities were found to support the view that the animals now classed together as arthropods must be regarded as having reached their present organisation by development along two more or less parallel lines of descent. In the work before us, Mr. Grote puts forward a similar opinion with regard to the butterflies, which are perhaps the most familiar and most attractive of all arthropods. From a study of the wing-neuration in the different families, he concludes that while the Papilionidæ and Parnasiidæ have been derived from forms related to the geometers and notodonts, the rest of the butterflies are descended from *Castnia*-like ancestors. In the Papilios and their allies, there is always to be found the lowest of the sub-median nervures running from the root of the forewing to a point a short distance along the inner margin, while the middle sub-median nervure is always absent. In the rest of the butterflies, on the contrary, the lowest sub-median is always wanting, while the middle one is generally present. The principal types of neuration to be found among the butterflies are well shown in the four plates accompanying Mr. Grote's work. These plates are indeed entirely devoted to illustrations of wing-neuration.

It may be doubted whether Mr. Grote's radical division of the butterflies, founded as it is only on the arrangement of the wing-nervures, will meet with general acceptance among lepidopterists. In the search for the affinities of animal groups, weight must be given to a combination of characters, and in a dual division of the butterflies from any other point of view, the Hesperiidæ, and not the

Papilionidæ, would be set over against all the other families, as retaining in imaginal, pupal, and larval stages, a number of peculiar primitive features. But except for the position of the Papilionid group, it is of great interest to observe how closely Mr. Grote's view of the relationships of the families of butterflies agrees with the scheme, founded on pupal structure, lately put forward by Dr. Chapman.[1] Both authors reject Bates' arrangement founded entirely on the development of the front legs of the imago, and instead of considering the Lycænidæ and Lemoniidæ intermediate between the Pieridæ and Nymphalidæ, regard them as representing an offshoot not far above the Hesperiid stem; while they both place the Pieridæ much closer to the Nymphalidæ and allied families than to the Papilionidæ with which they were classed by Bates.

In the faunistic portion of his work, Mr. Grote enumerates eighty species of butterfly to be found in the neighbourhood of Hildesheim—ten more than occur in the whole of the British Isles.

G. H. C.

The Mosses of North America.

Analytic Keys to the Genera and Species of North American Mosses. By C. R. Barnes and F. D. Heald. *Bull. University of Wisconsin.* Pp. x., 157–368. Madison. December, 1896. Price 1 dol.

This is an official publication of the University of Wisconsin and forms a section of Vol. I. of its *Bulletin* (Science series). It consists of three parts—a Key to the Genera (now in its third edition), a Key to the Species (second edition), and a bulky Appendix containing 603 descriptions of species and varieties, which have been added to the North American Moss-flora between the date of publication of Lesquereux and James's "Manual of the Mosses of North America" (Boston, 1884), and the end of the year 1895. The Keys are intended to be employed in combination with the Manual, and in this function they make good what was a serious omission from that book—an omission which was not unnatural in view of the misfortunes which prematurely put an end to the work of the projectors of the Manual. Professor Barnes's Keys have been in public use for some years and have been of material assistance for the rapid determination of species. In their new and extended form they have gained much in value, and have almost been brought into line with our present knowledge of the North American moss-flora. Since the publication of the Manual copious additions have been made to the total of 900 species described in that work. Of these additions Mr. Heald has collected the descriptions and placed them in systematic order in the appendix, for which kindly office he deserves the gratitude of all who are interested in North American mosses, but especially of those who have not access to the periodicals in which these descriptions are scattered. It is probable that many of the species recently described will upon further investigation be found untenable. Indeed the work of destruction has already begun. In the genus *Dicranum*, for instance, seventeen out of eighteen species described as new in a Canadian catalogue by a European bryologist have been reduced. This is an extreme case no doubt, but it serves to shew that there is no great demand for new species of mosses in the United States, whatever may be the case with new species of other organisms. But be that as it may, the Analytic Keys have been carefully and intelligently constructed, and are well

[1] *Entom. Record*, vol. vi., pp. 150–2, 1895.

calculated to fulfil the purpose of the authors—to encourage and facilitate the study of the North American Moss-flora, and to lead to more accurate information as to the distribution and the degree of variability of the species. A.G.

"The Whole World Kin."

The New Charter: a discussion of the rights of men and the rights of animals. Edited by Henry S. Salt. 8vo. Pp. xii., 156. London: G. Bell & Sons, 1896. Price 1s.

"The six addresses included in this volume were delivered . . . before the Humanitarian League. . . . Each of the writers has . . . treated the subject from a quite independent standpoint. . . . The result has been a practically unanimous acceptance of the humanitarian principle." This Review is not concerned with "The Humanitarian View" presented by J. C. Kenworthy, with "The Church's View" by Rev. A. L. Lilley, with "The Ethical View" by Frederic Harrison, with "The Secularist View" by G. W. Foote, or with "The Theosophical View" by C. W. Leadbeater; but Mr. Salt will doubtless expect us to say something about "The Scientific View" as it is expounded to ordinary humanity by Josiah Oldfield, distinguished from the other writers by the addition of letters (M.A., B.C.L.) to his name. "*It is not scientific*," says Mr. Oldfield in italics, "*to take an unscientific dictum as the foundation for a scientific superstructure.*" Here are some dicta on which he bases the conclusion that "there is a true brotherhood of all that lives":—"If there is one fact of embryology more certain than another, it is in the practical identity of the history of all embryos. . . . The human only becomes human after passing anew and in rapid transit through all the histories of its past. . . . Thus is it that comparative embryology points out the title deeds to philogeny [*sic*] or the kinship of all that lives. Through the reptilic stages the germ of the infant life threads its mystic way." But it "does not stop there. It has an inherited gnosis which shows it how to pass higher still, and so through stages whence fish and bird branch off, until it reaches the higher vertebrates and mammals, and finally, it puts on the human form." Our readers can decide for themselves whether these dicta are scientific or unscientific; but to Mr. Salt and the Humanitarian League it is simple kindness to say: they are not science, they are bosh. Most scientific men, we venture to say, hold views more nearly coincident with those of Mr. Frederic Harrison; and, sympathising thus far with the objects of the League, they will regret to see that body accepting the turgid and misleading rhetoric of this Mr. Oldfield as representative of the support that Science, if seriously approached, might give.

Fish-Making and Fish-Taking.

La Piscifacture Marine. Par M. le Dr. Marcel Baudouin. 8vo. Pp. iv., 52. Paris: Institut International de Bibliographie Scientifique. 1897.

Northumberland Sea Fisheries Committee, 1896. Report of the Scientific Results of the Trawling Expeditions during 1896. By Alexander Meek. 8vo. Pp. 26 & i. pl. 1897.

Dr. Baudouin plainly states in his "Introduction" that he has approached his problem as a scientific journalist and a "vulgarisateur modeste," but he has so mastered the details of the subject that the result is a connected Report upon the Rise and Present Position of the Artificial Propagation of Marine Fishes, which cannot fail to be understood by any educated layman, and which, on the other hand, has points of interest for the ichthyologist.

Starting at Gloucester, Mass., the pioneer station of the movement, the author takes his readers a tour of inspection through the "hatcheries" of Wood's Holl (Mass.), Dildo (Newfoundland), Flœdevig (Norway), and Dunbar (Scotland). Each of these institutions in turn is graphically illustrated by views, bird's-eye-views, and plans, and their special work is alluded to.

There is one statement in the account of the Gloucester station (p. 12), upon which we would have desired more light. "Dès 1879 et 1880, on pêchait déjà, dit-on, dans la baie, des alevins de morue, qu'on reconnut à certains signes pour être nés au laboratoire en 1878 et les marins du pays leur donnèrent aussitôt la dénomination caractéristique et très honorifique de 'Morues de la Commission.'" The seafaring man is so often instinctively right that statements like these should be carefully investigated.

Perhaps the point which is best brought out, in this little report, is the comparative ease, combined with small pecuniary outlay, with which it is possible to hatch, and place in the sea, hundreds of millions of larval food-fishes, a fact of prime economical importance. Dr. Baudouin quotes the authority of such specialists as Mr. Dannevig and Mr. Nielsen to show that the 'hatcheries' are justifying the labour spent upon them, by a marked improvement in "the harvest of the sea." The author is an enthusiast, and, whilst more or less reconciled to the progressive strides of America, he would like to see his country abreast of Norway and Scotland, in an official attempt to recuperate the home fisheries.

If Dr. Baudouin's Report does not go far to bring about the desired result, it will not be due to the doubtful nature of its conclusions or the want of graphic exposition of the whole subject.

We have also received the Scientific Report of the Northumberland Sea Fisheries Committee, a little volume which is full of promise for the future. After a short account of the local fisheries, the results of trawling excursions are given. The most useful and interesting part of this is undoubtedly that dealing with the food of the fisheries, especially the local weakness of plaice for *Donax trunculus*. We would warn the author against a habit which is unfortunately only too prevalent with labourers amongst the eggs and larvæ of fishes. We refer to the issuing of plates illustrating the development of types which have already been fully worked out by others. The waste of money is not so important as the multiplication of literature involved. Mr. Meek is to be congratulated on the results of his labours, and we hope soon to hear of the successful establishment in his district of a marine laboratory with its attendant facilities for research.

We cannot pass from this subject without quoting the following delicious paragraph from the *Scientific American*, which, we hasten to add, disclaims any responsibility for its accuracy:

"In China the hen is kept constantly busy. When not engaged in hatching her own brood, she is compelled to hatch fish eggs. The spawn of fish are placed in an eggshell, which is hermetically sealed and placed under the unsuspecting hen. After some days the eggshell is removed and carefully broken, and the spawn, which has been warmed into life, is emptied into a shallow pool, well warmed by the sun. Here the minnows that soon develop are nursed until strong enough to be turned into a lake or stream."

The Strata of the Earth.

Congrès Géologique International. 6e Session, Zurich, 1894. Chronographe Géologique. 12 grands tableaux en couleurs (gamme internationale). Texte explicatif suivi d'un répertoire stratigraphique polyglotte par E. Renevier .... 8vo. Lausanne (Bridel & Cie). Mars, 1897. Compte Rendu du Congrès, pp. 523-695. Issued separately at 10 fr., or the tables without the text at 6 fr.

This chart of the sedimentary formations was first published by Professor Renevier in 1873 and 1874 in the *Bulletin de la Société Vaudoise des Sciences Naturelles*, volume xii., and consisted of nine tables. It is now enlarged to twelve tables, and published as a part of the Report of the Sixth International Geological Congress. The tables give a bird's-eye view of the homotaxy of the formations in different countries, and one can see at a glance the general correspondence of beds of similar age in different localities. The text explains the tables, and is followed by an extremely valuable "Répertoire Stratigraphique," which is in reality an alphabetical list of the names given to general or to local deposits in all parts of the world, followed in most cases by the authority for the name, and the date when it was founded. This alphabetical list will, we venture to think, be as indispensable to the geologists as de Margerie's Bibliography, noticed in our last number; and the veteran Swiss Professor will have the satisfaction of knowing that he has helped the internationality of geology to a very considerable extent by his indefatigable labours.

The Glastonbury Lake-village.

In 1893 (November) we referred to the Lake Dwellings discovered by Mr. Bulleid at Glastonbury, quoting Prof. Boyd Dawkin's address to the British Association. The excavations have been systematically carried out so far as the funds available have permitted, Mr. Edward Bath, the owner of the five acres, having generously given the site over for systematic exploration. An interesting and well-illustrated pamphlet, recently published at one shilling, gives an account of the work up to the end of 1896, and can be obtained of Messrs. Barnicott and Pearce, Fore Street, Taunton, or Mr. E. V. P. Barker, Glastonbury, the treasurer of the Excavation Fund, who will also be glad to receive any contributions towards the further exploration of this early British Settlement.

Scraps from Serials.

The Bucharest *Anuaru al Biuroului Geologicu* has changed its title with the publication of the volume for 1894, which is just to our hands. It now reads *Anuarulŭ Museului de Geologia şi de Paleontologia.* At the same time there were published four sheets of the Geological Map of Roumania. This *Anuarulŭ* contains descriptions of fossil Camel and Dinothere found in the district, and of some rock-specimens from the Carpathians, etc. All praise is due to Gregoriu Stefanescu for his energy and perseverance in keeping science in evidence in Roumania.

Another of Mr. F. Enock's delightful and well-illustrated articles appears in *Knowledge* for May. It is called "The Insects of a London back-garden," and the first instalment deals with the willow saw-fly, *Nematus gallicola*, and its parasites. The same number contains articles on the progress of biology and geology during the Queen's reign, by R. Lydekker and Grenville Cole respectively. Mr. T. A. G. Strickland's photographs of animals in the Zoological Gardens are no doubt good enough, but the reproductions of them are excruciatingly inartistic owing to the blocking out of all ground-tone, a feature on which Mr. Strickland seems to pride himself.

We have received volume V. of *The Journal of Malacology*, 1896, carefully bound up with the wrappers of the respective parts, as was to be expected of a periodical that devotes so much attention to bibliography. Besides the excellent annotated list of current literature, the volume contains communications from G. W. Chaster, E. A. Smith, A. S. Kennard, W. Moss, F. Stephenson, E. R. Sykes, and Professor T. D. A. Cockerell. Mr. W. M. Webb, the editor, is certainly to be congratulated on the appearance of his volume, printed by Mr. J. Dutton of Chelmsford.

New Serials.

A. Priber, of Berlin, announces the *Zeitschrift für Criminal-Anthropologie*, under the editorship of Dr. Walther Wenga.

La radiographie, edited by Dr. Paulin-Mery, deals with the medical and surgical applications of the Röntgen rays, and is published in Paris.

Der Deutscher Tierfreund is a monthly, edited by Dr. R. Klee, and published at Leipzig.

Further Literature Received.

The Forcing-book, L. H. Bailey; Experimental Morphology, C. B. Davenport; Rocks, Rock-weathering and Soils, G. P. Merrill: Macmillan, New York. Through a Pocket Lens, H. Scherren: Relig. Tract Soc. Handbook to the Birds of Great Britain, vols. iii. and iv., R. B. Sharpe: Allen. First Stage Physiography, A. M. Davies: Clive. Naturalists' Directory: L. Upcott Gill. Problems of Nature, Gustav Jaeger, Ed. and trans. H. G. Schlichter: Williams and Norgate. Genesis and Matrix of the Diamond, H. C. Lewis: Longmans. Young Beetle-Collector's Handbook, E. Hofmann: Sonnenschein. The Vertebrate Skeleton, S. H. Reynolds: Cambridge Univ. Press. Prehistoric Problems, R. Munro: Blackwood. Das Ellenbogengelenk, J. W. Hultkrantz; Lehrbuch der Zoologie, 1897, R. Hertwig: Fischer, Jena. Crustacea of Norway, vol. ii., Isopoda, pts. iii. and iv., G. O. Sars: Bergens Mus. Geological Structure of the Hill Slopes around Naini Tal, T. H. Holland: Geol. Surv. India.

Note on Computing Diffusion, G. F. Becker: *Amer. Journ. Sci.* Artificial Production of Certain Organic Forms, G. Rainey: *Rep. St. Thomas's Hospital.* Geological Evidence regarding the Age of the Earth, J. G. Goodchild: *Proc. Roy. Phys. Soc. Edinburgh.* Crater Lake, Oregon, J. S. Diller: *Nat. Geogr. Mag.* Hornblende-Basalt in Northern California, J. S. Diller: *Amer. Geol.* Mesial Fins of Ganoids and Teleosts, T. W. Bridge: *Journ. Linn. Soc. Zool.* Geographical Distribution of Dragonflies, G. H. Carpenter: *Proc. Roy. Soc. Dublin.* Distribution and habits of Puget Sound Invertebrates, N. R. Harrington and B. B. Griffin; *Rep. Trans. N. Y. Acad. Sci.* Deep-Sea Crustacea from South West of Ireland; On the Genus Anaspides, W. T. Calman: *Trans. Roy. Irish Acad.* Two New Species of Pterophyllum, J. Shirley: *Proc. Roy. Soc. Queenlsand.* Ratzel's History of Mankind, pts., 18 and 19. Considerazioni ed Appunti sul Cane cirneco, M. Migneco. Effects of the Weather upon Vegetation, J. Clayton.

Shooting Times, April 17, May 15; Amer. Geol., April, May; Scot. Geogr. Mag., May; Amer. Journ. Sci., May; Rep. Albany Mus., 1896; Amer. Nat., April, May; Victorian Nat., March–April; Journ. School Geogr., April; Botan. Gazette, April; Chivers' New Book List, May; Aeronautical Journ., April; Feuille des jeunes Nat., May; Illinois Wes. Mag., April; Irish Nat., May; Westminster Rev., May; Journ. Essex Tech. Lab., vol. ii. (1895–96), March–April; Journ. Malac., May; Proc. Biol. Soc. Washington, vol. xi.; Knowledge, May; Literary Digest, April 10, 17, 24, May 1, 8, Index; Naturæ Novit., April; Nat. Sci. Journ., April; Naturaleza (Madrid), viii., Nos. 11, 12, 13; Naturaleza (Mexico), 12; Jersey Times, May 12; Naturalist, May; Nature, April 15, 22, 29, May 6; Nature Notes, May; Naturen, April; Bull. U.S. Dept. Agric., No. 8; Photogram, May; Psychol. Rev., Index (1896), May; Revue Scient., April 17, 24, May 1, 8, 15; Science, April 9, 16, 30, May 7; Science Gossip, May; Scientific Amer., April 10, 17, 24, May 1, 8; Scot. Med. and Surg. Journ., May; Ann. Rep. New York Zool. Soc., 1.

OBITUARY.

ABRAHAM DEE BARTLETT.

BORN 1812. DIED MAY 7, 1897.

THE Zoological Society has sustained a severe loss by the death of the superintendent of the Gardens, aged 85. Mr. Bartlett had occupied that position since so long ago as 1859, having before that been head of the Natural History Department at the Crystal Palace. Few men had so great a practical acquaintance with animals as had Mr. Bartlett, and the Council of the Society will find it by no means an easy matter to appoint his successor. His contributions to zoological literature are mainly to be found in the *Proceedings* of the Zoological Society, and the most important of these deal with birds. He was the first to note the presence of powder down patches in that singular bird the Kagu (*Rhinochetus*), of New Caledonia, and also in the African *Balaeniceps*. He drew attention to certain struthious characters exhibited in the incubation and by the young of the tinamous—facts which tended to support the current views as to the low position of these birds among the Carinatæ. In the moulting of the penguin, Mr. Bartlett emphasised the interesting fact that the feathers of the wing, so scale-like in appearance, are shed almost in one piece as is the skin of a snake. He commented also upon the singular sac-like structures thrown up by the hornbill, and made a number of other contributions to our knowledge of the habits and structure of birds. The main part of his work, however, lay in the practical management of the Regent's Park Gardens, and the excellent condition of those gardens was in no small degree due to his industry and capabilities.

SIR EDWARD NEWTON, K.C.M.G., who died at Lowestoft on April 25, aged 64, availed himself of his opportunities as Colonial Secretary of Mauritius, as member of a mission to Madagascar, and as Lieutenant-Governor of Jamaica, to increase our knowledge of the natural history, and especially the ornithology of those islands. He was one of the founders of the British Ornithologists' Union, and was joint author of various scientific papers with Dr. A. Günther, Dr. H. Gadow, and his elder brother, Professor Alfred Newton.

THERE are also announced the deaths of:—MAX SINTENIS, Entomologist, at Kupferberg, Silesia; FILLIPPO TOGNINI, Curator of the Botanical Institute at Pavia University; on December 1, the explorer LUDWIG KÄRNBACH, aged 34; in January, at Libreville, M. THOLLON, a botanist and chief of exploration on the French Congo; on January 17, Professor AUGUSTO PALUMBO, at Castelvetrano, Sicily, aged 54; J. B. BARLA, head of the Natural History Museum at Nizza; Professor BERTHAUD, of Lille, geologist; G. GERCKE, of Hamburg, dipterologist.

WE are compelled by the exceptional pressure on our space in this number to hold over a large number of names.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

THE following appointments are announced :—

Dr. J. Y. Mackay, Professor of Anatomy, to be Principal of University College, Dundee; Dmitri Klementz, to be Curator at the Museum of the Academy of Sciences, St. Petersburg; Dr. K. v. Buchka, to be Director of the Department of Scientific Enquiry at the Sanitary Office in Berlin; C. H. Townsend, to be Chief of the Division of Fisheries, and Dr. H. M. Smith, chief of the Division of Scientific Enquiry, U.S. Fish Commission.

Dr. J. Ritchie, to be Lecturer in Pathology at Oxford; Robert B. Yound, to be Assistant Biologist in the U.S. Department of Agriculture; Dr. Jensen, to be Privat-Docent in Physiology in the University of Halle; Dr. Max Siegfried, to be Professor-Extraordinarius of Physiology at Leipzig; Dr. Fritz Noll, to be Professor of Physiology at Heidelberg; Dr. Wladislaw Szymonowicz, of Cracow, to be Professor-Extraordinarius of Histology and Embryology at Lemberg; Dr. Polydore Francotte, to be Professor of Embryology at Brussels; Dr. Ludwig Heim, to be Professor-Extraordinarius of Bacteriology at Marburg; W. B. Pillsbury, to be Instructor of Experimental Psychology at Cornell University; J. F. Crawford, to be Demonstrator in Experimental Psychology at Princeton University; Dr. Simmara, to be Professor of Physiological Psychology in the Government School of Science, Madrid.

F. F. Blackman, to be Lecturer in Botany at Cambridge University; Dr. E. Fischer, to be Professor of Botany and Director of the Botanical Gardens at Berne; Dr. Victor Folgner, to be Assistant at the Botanical Institute of the German University, Prague; Anton Heinz, to be Professor of Botany at Agram; Dr. Noll to be Professor of Botany at Bonn.

Dr. Velain to be Professor of Physical Geography at the University of Paris; Dr. Mijat Kishpatitch to be Professor of Mineralogy at Agram; Dr. Siedentopf, of Göttingen, to be Assistant in Mineralogy at Greifswald University; Dr. Beckenkamp, of Mulhausen, to be Professor of Mineralogy at Würzburg; Dr. E. A. Wülfing, to be Professor-Extraordinarius of Mineralogy at Tübingen; J. H. Pratt, of Yale University, to be Mineralogist to the N. Carolina Geological Survey; C. H. Warren, to be Instructor in Mineralogy in the Sheffield Scientific School of Yale University; Dragutin Gorjanovitch Kramberger, to be Professor of Geology and Palæontology at Agram; Dr. F. Pompeckj, to be Curator of the Palæontological Collection in the State Museum at Munich; T. A. Rickard, mining engineer of Denver, to continue as State Geologist of Colorado; G. W. Stone, lawyer and politician, to be Mine Inspector of Kentucky; C. H. Crantz, steamboat agent, to be State Geologist of Illinois and Curator of the State Museum.

Nature for April 29 contains a statement made by the Duke of Devonshire, as Chancellor of Cambridge University, to the effect that the University is by no means the wealthy body it is often supposed to be. Agricultural depression continues to reduce the income of the University, while many of the museums, laboratories, and libraries, urgently demand extension, and new branches of study call in vain for recognition. Such a sad state of affairs likewise obtains at Oxford; and there is no doubt that a time has arrived when those with money to spare may well consider the re-endowment of our ancient seats of learning on a broader and firmer basis.

DR. NANSEN has sent a cheque for £50 to the Vice-Chancellor of the University of Cambridge, asking to be allowed to make a contribution towards the teaching of geography in the University. It is stated that Dr. Nansen is going to Australia to give fifty lectures; but it is not stated whether he intends to go *viâ* the South Pole.

It is proposed to start a laboratory of experimental psychology at University College, London. For the present, instruction would be given for one term in each year. Subscriptions, to be devoted to the purchase of apparatus and the payment of a teacher, may be sent to the Hampstead Branch of the London and South Western Bank (a/c Psychological Laboratory, University College).

We learn from the *Scottish Medical Journal* that the Universities Commission has instituted a Chair of Public Health and Sanitary Science in the University of Edinburgh, to be known as the "Bruce and John Usher Chair of Public Health," in recognition of bequests from Messrs. Usher and A. L. Bruce. The salary of the Professor will be not less than £600.

Sir Donald Smith, who is Chancellor of McGill University, has provided funds for the establishment of a chair of Zoology.

Political changes in America have hitherto left the state universities untouched; but now *Science* announces with regret that the Populists, on securing a majority in the Board of Regents of the Kansas State Agricultural College, have dismissed a President who had served for eighteen years, to make room for a young man 'in harmony with the fundamental principles of the administration,' and have removed other members of the faculty and employees. In this connection the last two items in our list of appointments are full of interest.

Mrs. Stanford, widow of the railway millionaire, has taken out an insurance policy with the Mutual Life of New York, by which, if she dies within the next ten years, the Leland Stanford University will receive one million dollars, but if she survives that period, the sum will be two millions. The annual premium is 170,000 dollars, and the age of Mrs. Stanford is 71 years.

Prof. W. B. Scott of Princeton University, is taking a party of students to study the geology of S. Dakota, and to make collections for the University.

We learn from *Science* that the building of the College of Agriculture at Berkeley Cal., has been burned.

The United States Geological Survey has for some time been preparing a number of duplicate sets of 156 typical rock-specimens, for educational purposes. These are now ready for distribution to the chief universities and colleges of the country.

A Departmental Committee, consisting of Sir E. Hamilton, Sir A. Godley, and Mr. G. H. Murray, has been appointed to inquire into the organisation, pay, and duties of the staff of the British Museum, including the system under which the staff is recruited and the reasons for or against competitive examination, either limited or otherwise; the classification, scale of salary, and hours of attendance required to insure the efficient and proper discharge of the duties of the establishment, and whether it is practicable to assign clerical and routine work wholly or in part to clerks of the second division; and, generally, any matters connected with the Museum establishment in regard to which they may be of opinion that alteration of existing regulations is desirable.

The town-council of Chelmsford has decided to commemorate Her Majesty's long reign by raising funds for a free library, museum, and technical institute.

A LARGE addition is being made to the Bergen Museum, from plans by the architect, Mr. Sparre. To the expected cost of £8,500, the Norwegian government has contributed half, while smaller sums have been given by the municipality of Bergen and by private persons. The number of visitors to this museum shows a steady increase, being 52,015 for 1896, of whom nearly four-fifths came on Sundays.

PROFESSOR ACHILLE TELLINI has published a long account of the Museum of Natural History in the R. Istituto tecnico "Antonio Zanon" in Udine, with a note on other collections in the city of Friuli. The notice appears in the *Annali* of the Institute, series 2, volume xiv., 1896 (1897). The museum was founded in 1866, and contains the Bartolomeo Gastaldi collection, and that of Giovanni Michelotti, beside many specimens given by Pirona, Taramelli, Zucchari, Brodmann, Bicknell, and Gropplero. The other collections to be found in Friuli, and shortly described in the pamphlet, are: the Museum of the "Jacopo Stellini" Lyceum; the Museum of Natural History of the Udine Archiepiscopal Seminary; the Provincial Museum of Natural History; those of the Gymnasium, and of the "Scuole reali" of Gorizia; and the Museum of Natural History at Trieste. Mention is also made of many private collections in the district.

AN Educational Museum was opened at the State House, Boston, U.S.A., on May 1. It includes, says *Science*, the exhibits of the Massachusetts schools at the Columbian Exposition, together with the work of other schools, school appliances, and a pedagogical library. Among educational appliances one might well include methods of museum installation; and such an exhibit would be of interest to many, whether at Boston or elsewhere.

THE Field Columbian Museum, Chicago, has purchased from the widow of Dr. Arthur Schott, the plants collected by him in Campeche, Tabasco, Upper Mexico, Hungary, and elsewhere.

THE following fifteen candidates are recommended for election by the Council of the Royal Society :—Dr. R. Bell, geographer of Canada; Sir W. H. Broadbent, the physician; Dr. C. Chree, of Kew observatory; Mr. H. J. Elwes, the entomologist; Dr. J. S. Haldane, the physiologist; Prof. W. A. Haswell, the zoologist, of Sydney; Prof. G. B. Howes, of the Royal College of Science, zoologist; Dr. F. S. Kipping, chemist, of the Central Technical College; Prof. G. B. Mathews, mathematician, of Bangor; Mr. G. R. M. Murray, Keeper of the Department of Botany in the British Museum; Mr. F. H. Neville, physicist, of Sidney Coll., Cambridge; Prof. H. A. Nicholson, of Aberdeen, invertebrate palæontologist; Prof. J. Millar Thomson, chemist; Dr. F. T. Trouton, physicist, of Trinity Coll., Dublin; and Mr. H. H. Turner, professor of astronomy at Oxford.

THE gold medal of the Linnean Society has been awarded to the renowned phycologist, Prof. Jacob Georg Agardh, of Lund University, Sweden.

AT the anniversary meeting of the Royal Geographical Society on May 17, the Founder's Medal was presented to Dr. G. M. Dawson, director of the Geological Society of Canada, an account of whose work, from the pen of Dr. G. J. Hinde, appears in the *Geological Magazine* for May last. The Murchison Grant of the same society is awarded to Lieut. S. Vandeleur, for surveying work in Somaliland, along the Abyssinian frontier, and in the Niger region. The Boeck Grant was presented to Lieut. Ryder, of the Danish Navy, and the Cuthbert Peek Grant to Dr. Thorwald Thoroddsen. Mr. C. E. Douglas received the Gill Memorial for his explorations of the New Zealand Alps.

THE 68th Annual Report of the Council of the Zoological Society, recently issued, shows that the number of Fellows on Jan. 1 was 3,098, an increase of 71 over the previous year, and that the income for 1896 was £27,081, being more than

£3,000 above the ordinary expenditure. This enabled a new house to be built, for the storks and cranes in one division of it, with 16 compartments, and the struthious birds in the other, with 12 compartments. The portion for the ostriches is kept at a higher temperature, and the arrangements enable the birds to remain on view in the same house all the year round. The number of animals in the Gardens at the close of 1896 was 2,473, an increase of 104 over the previous year; the difference was partly due to a decrease in the number of deaths, 986 as against 1,092 in 1895. Amongst the chief additions of the year were: A pair of a rather scarce species of Duiker antelope from West Africa, a silver-backed fox from Cape Colony, a young male manatee from the Rio Purus, a fine young female gorilla from French Congoland, and also a very fine and large series of reptiles from Burma, collected by Mr. W. G. Bligh, and embracing specimens of fifteen species new to the collection.

The American Association for the Advancement of Science is to meet at Detroit on Monday, August 9. The President was Prof. Cope, who is succeeded by the senior Vice-President, Prof. T. Gill; his address will take the form of an obituary of Prof. Cope. In place of the late Brown Goode, L. O. Howard has been nominated President of the Zoological section, while S. C. Chandler is elected auditor to replace yet another deceased officer, B. A. Gould. The Council of the British Association having specifically invited members of the American Association to attend the Toronto meeting on payment of a five dollar subscription, the American Association has replied by pointing out that members of all foreign scientific societies, are welcomed as honorary members of the American Association. "This is called the Retort Courteous."

The British Medical Association is to meet in Montreal from August 30 to September 3, under the presidency of Dr. T. G. Roddick, Professor of Surgery at McGill University.

The German Zoological Society will hold its 7th annual meeting at Kiel, under the presidency of Prof. O. Bütschli, on June 9–11.

The Hayden medal has been awarded by the Philadelphia Academy of Natural Sciences to Prof. A. Karpinski, head of the Russian Geological Survey.

The Hull Scientific Club had successful excursions at Easter in the neighbourhood of Flamborough. The botanists added *Cerastium tetandrum*, the mouse-eared duckweed, to the list of the East Riding flora. To judge from the report sent to us, this Club is in a condition of very healthy activity. Its members are helping in the formation of a collection of living British wild plants, which are being arranged in the Hull Parks.

The Agricultural Society of Austria will hold an exhibition of agriculture and forestry in Vienna, from May 7 to October 9, 1898, The following sections will be of an international character: Machinery and implements for agriculture and forestry; dairy machinery and appliances: fertilisers, feeding stuffs, and chemical products for agricultural and forest purposes; veterinary science; agricultural improvements, building and engineering; agricultural and forest education, research work, statistics and literature.

At the annual meeting of the National Academy of Sciences at Washington (20-22 April), the following papers were read, among others: Adelaide W. Peckham, "An experimental study of the influence of Environment upon the biological processes of the various members of the Colon group of Bacilli"; T. C. Mendenhall, "The energy involved in recent Earthquakes"; A. Agassiz, "Recent borings in Coral-reefs"; and T. Gill, "The position of the Tarsiids and relationships to the

Phylogeny of Man." This last should interest readers of Dr. C. Earle's recent paper in NATURAL SCIENCE. During the meeting Sir A. Geikie was formally introduced to the Academy.

Science states that the physiological library of the late Professor Du Bois-Reymond has been bought by Dr. Nicholas Senn, and presented by him to the Newberry Library, Chicago. On the other hand, according to *Die Natur*, these same books have been bought by the Prussian Government on behalf of the Physiological Society of Berlin.

THE twenty-fifth anniversary of the foundation of the Zoological Station at Naples was celebrated on April 14, when addresses were presented to Dr. Anton Dohrn from learned bodies in all parts of the world, and one signed by some two thousand naturalists of Europe. Dr. Dohrn was admitted to the freedom of the city of Naples, and received the Grand Order of the Crown of Italy.

THE annual report for 1896 of the Millport Marine Biological Station, informs us that the good seed planted by Dr. David Robertson is bearing fruit. The mason work of the main building is completed, and the structure was to be finished and opened in May. Unfortunately, Dr Robertson has not been spared to see the crown of his long and active life. The funds in hand have sufficed for the construction, but another £300 will be wanted for fittings, etc. Since 1894, "The Ark," lent by Dr. John Murray, has formed a temporary station, and has proved useful to a small number of investigations, and it is hoped that the completion of the permanent structure will see a large influx of scientific workers, for the possibilities of Cumbrae are perfectly well known to all those who have followed Dr. David Robertson's life-work.

NOTWITHSTANDING rather boisterous weather, the usual Easter expeditions of the Liverpool Marine Biology Committee, were carried on with success, and the Port Erin Biological Station has never been so full of workers as it was during April. During the actual Easter vacation, the rather limited accommodation was more than fully occupied. The Lancashire Sea-Fisheries Steamer, has been at Port Erin during the Easter week, carrying on her trawling investigations of the district; and several dredging expeditions have been made in her. Spawning fish have been procured to the West of the Isle of Man, and the tanks in the Biological Station now contain developing lemon soles, and witches, and a cross between *Myxine* and the cod.

There is to be an "international" fisheries exhibition at Bergen, from May 16 to Sept. 30, 1898. And there is to be a "national" fisheries congress at Tampa, Florida, on Jan. 19, 1898.

MR. A. GIBB MAITLAND, late of the Geological Survey of Queensland, has been appointed government geologist of West Australia, and is reorganising the staff with the view of making a proper geological survey of the mining fields of the colony, and publishing maps of the same. As a preliminary to this a topographical survey is being prepared with the assistance of a topographer. An important addition to the staff is Mr. Torrington Blatchford, of the Sydney School of Mines, who has had much practical experience of mining geology. Applications have also been invited by the Government for the position of assistant geologist; while as mineralogist and assayer there has been appointed Mr. Simpson of the Sydney School of Mines, late chief assistant assayer to the Mount Morgan Co., Queensland. A lithographic draughtsman and a clerk complete the not over-large staff, which has a large and important task before it.

PROF. W. B. DWIGHT of Vassar College, Poughkeepsie, has invented a machine for cutting sections of rocks, minerals, and fossils. The object can be held so as to produce a cut in exactly the desired direction. Sections of from eight to twelve square inches, with a thickness of one-fiftieth to one-hundredth of an inch, can be cut with true smooth surfaces.

At the Tennessee Exposition, which opened on May 1, the U. S. Government has interesting scientific exhibits; those illustrating the work of the Department of Agriculture are specially complete. The Bureau of Ethnology has concentrated its energies on a representation of a Kiowa camping circle.

The progress of skiagraphy in medicine and surgery is rapid. A very complete installation was sent out to the seat of war in Greece, by the *Daily Chronicle* ambulance fund, and was of great help in localising bullets. A laboratory for the purpose has been founded at the Trousseau Hospital by the Municipal Council of Paris.

According to *Science*, Prof. Lawrence Bruner, of the University of Nebraska, sailed on April 27 for Buenos Ayres, where he will spend a year investigating the injurious locusts which have of late increased enormously in three of the eastern provinces of the Argentine Republic. The expenses are paid by a committee of Buenos Ayres business men.

On his forthcoming Arctic Expedition, Lieut. Peary intends to take with him Esquimaux, not only men but women. The Esquimau is a pronounced family-man, and prefers his wife to the North Pole.

The administration of the grants to schools in Scotland for science and art is to be transferred to the Scotch Education Department. The details of the transfer will be matter of departmental arrangement.

We learn from our always interesting contemporary, the *Shooting Times*, that General Sir Julius Raines has decided to bequeath to the nation his fine collection of Japanese masks made of fossil mammoth ivory.

A severe earthquake took place in the West Indies on April 29. The island of Montserrat seems to have been about the centre of the disturbance; but most loss of life occurred in Guadeloupe, while Antigua also suffered severely. An earthquake also occurred at Lima at 2 a.m. on May 1.

The chapter-house at Canterbury, which has been magnificently restored under the direction of Sir Arthur Blomfield, will be re-opened by H.R.H. the Prince of Wales on Saturday, May 29. Dean Farrar asks us to state that at 2 p.m. on May 31, Sir Henry Irving will read Tennyson's "Becket" therein, for the benefit of the Thirteenth Centenary Fund. St. Thomas Becket entered the chapter-house on the evening of December 29, 1170, only a few minutes before his murder. Reserved seats for the reading may be procured, at one guinea each, by letter to Mr. Crow, Mercery Lane, Canterbury.

Under the title of the Kent Coal Exploration Co., Limited, a company has just been floated to explore for minerals, especially coal and iron, by means of deep borings, in the County of Kent and elsewhere in the South of England. There is already a Kent Coal Fields Syndicate, Limited, with which the present company will co-operate. It is proposed to enter into conditional agreements with landowners to explore for the Coal Measures, in consideration of the grant of leases of minerals at royalties. All the borings may not be immediately successful in finding coal, but each will throw fresh light on the structure of the country, and the details will be eagerly followed by geologists no less than by those in search of dividends.

CORRESPONDENCE.

The Presidency of the Chemical Society.

In the May number of Natural Science you have called attention to the recent contest for the presidency of the Chemical Society, and the extraordinary character of the two circulars put forward by prominent members of the official party in the Society has been very justly commented upon by you.

I shall regard it as a favour if you will permit me to supplement your notice with a statement of the facts which have come to light.

The total number of votes recorded at the anniversary meeting was about 380, and the number of votes for Dewar has been officially declared to be 166. It is, therefore, quite clear that Dewar did not receive the votes of a majority of the voters. There were about 214 votes, cast by voters, who certainly indicated their will that Dewar should not occupy the presidency,

Inasmuch as the names of both Dewar and Ramsay had been, in the most formal manner, proposed for the presidency, the 214 must be taken as votes for Ramsay. I am informed, however, that, on the first Thursday in this month the statement made from the chair at the meeting of the Society was that the votes for Ramsay were 152, and that no mention was made of the 62 votes, which apparently were regarded as void.

J. Alfred Wanklyn.

11 May, 1897, Laboratory, New Malden, Surrey.

[We have verified Dr. Wanklyn's statement, which indicates a more serious attempt on the part of the Council to override the will of the Fellows than we had suspected. According to the Charter of the Society, the President must be elected by a majority of those voting; this condition has not been complied with. There is nothing in the Charter or Bye-laws of the Society to justify the rejection of votes which, though of clear intention, did not absolutely comply with all the requirements of a cumbrous ballot-paper. We conclude that Professor William Ramsay is legally the President of the Chemical Society, and we hope that the Fellows will see their will enforced.—Ed. Nat. Sci.]

The Numerical Variations in *Ranunculus repens*.

Upon closer examination of my figures, I find, that the whorls of *Ranunculus repens* do *not* vary independently as previously stated (Nat. Sci., May, 1897, p. 325), only 39 per cent. of the possible variations being recorded. As I am about to proceed with a second series, I will reserve further particulars for a future occasion.

115 Richmond Road, London, N.E. John H. Pledge.

[Others than Mr. Pledge will see with pleasure that the *Revue Scientifique*, after devoting a column to an account of his paper, concludes: " Mr. Pledge's work is conscientious; in such matters there is need of care and a spirit of accuracy, and this is undoubtedly an example that might be followed by many with profit to science."—Ed. Nat. Sci.]

Polychæta in the Cambridge Natural History.

It would be a pity if Mr. Goodrich's "gratitude" and "wonder" were to lead him to use the book he esteems so highly as the basis of future researches on the Polychæta. If this were, as Mr. Goodrich says it "undoubtedly" is, "the best general account" of the group "in any single work, in any language," I should feel sorry indeed for all who are intending to devote their energies to this particular field of zoology. Nor do I think that the author himself would claim such high rank for his work. No one who is at all familiar with the admirable monograph of Eisig—

which, though it professes to deal with a single family only, really deals in a most comprehensive and suggestive manner with the morphology of the whole group to which that family belongs,—could hesitate for a moment in declaring it far and away the best "*modern attempt* to deal in a broad, yet thorough manner with the group Polychæta." And, even were it not for Eisig amongst others, Dr. Benham could hardly be described as the "*first*" to make the attempt in modern times when we remember the names of Ehlers, Grube, Von Marenzeller, Hatschek, Saint-Joseph, Gravier, and Racovitza, all of whom, in the last twenty years, have not only made the attempt, but have been by no means unsuccessful in so doing. I am well aware that Eisig does not deal in detail with the appendages of the head and the minute forms of the chætæ in the different families, but I should have thought that no one claiming to be a "student of the anatomy of the Polychæta" and to have been so, I believe, for some years, could fail to have discovered how little reliance can be placed on such external features, and to perceive that it is to the internal anatomy that we must look for the solution of the many puzzling problems in the morphology of these worms which may help us to a "clear understanding of their classification."

It seemed to me that your reviewer erred rather on the side of leniency than otherwise, in that he chose a few small points to find fault with when there were many more important statements in the book, the incompleteness of which would lead one who knew nothing about the subject to infer the exact opposite of the truth. And it is, to my mind, no excuse to say that in some cases (it would not be true to say "in all") the statement may, if looked for, be found elsewhere in the book accurately made and with its necessary qualifications, but rather the admission of a second fault, for space might have been saved with great advantage, and room left for important facts which find no place in the work, by stating a fact but once. The author, for instance, might have made up his mind *either* to draw comparisons after dealing with each organ (or system of organs) in his type genus, *or* to devote a separate chapter to comparisons. It was not necessary or desirable to do both.

You would probably not allow me space to point out some of the many "misleading statements" (as I will call them for Mr. Goodrich'se dification) which seem to me of more consequence than some of those mentioned by your reviewer. But there is one point which I feel, in the interest of all future students of the Polychæta, should not be passed over without comment. The "Cambridge Natural History Series is intended," we were originally told by the publishers, "in the first instance for those who have not had any special scientific training." Now I imagine that any such person, after perusal of the chapters we are here considering, would conclude that the classification given on p. 258 and dealt with more fully in chapter xii., was the one generally accepted by authorities on the Polychæta. I venture, however, to think that no one of the great authorities of the group would for a moment admit a classification which (amongst other things):

(1) Separates such closely allied families as the Capitellidæ, Opheliidæ, and Maldanidæ by placing them in two distinct suborders, while leaving such totally dissimilar families as *e.g.* the Aphroditidæ, Syllidæ, Tomopteridæ, Glyceridæ, and Typloscolecidæ in one suborder;

(2) Nowhere gives any indication as to the relationship between the families in a suborder (unless perchance the order they are placed in is meant to have some such significance), nor of one suborder to another;

(3) Takes no account of such important genera as *Staurocephalus* and *Spiochætopterus*, to say nothing of *Palmyra* which, though important, is not common, and of *Gattiola* (*Amblyosyllis*) and *Sabellides*, which, though fairly common, are of rather less classificatory importance;

(4) Leaves such genera as *Spinther*, *Euphrosyne*, and *Amphinome* in one family while making two distinct families of the Spionidæ and Polydoridæ;

(5) Places families which pass almost imperceptibly into one another in distinct suborders (Spionidæ and Cirratulidæ), or even Orders (Spionidæ and Amphicorinidæ, though here we must remember Mr. Goodrich's information that Dr. Benham does not agree with those who have most carefully studied the subject).

One is unaccustomed now-a-days to see such a purely artificial classification introduced as an improvement upon others, and it is scarcely fair to impose on the untrained scientific mind by giving, as though there were no question about its sufficiency, an entirely new classification which has not been confirmed and accepted by men who are competent to form an opinion on the subject. It is true that, as Dr. Benham points out, he had published his classification before (though with one Suborder in a different Order, and two families in a different Suborder than they are now!); but would any authority on the Polychæta, even had the brief notice in the *Report of the British Association* come to his notice, have thought it worth while to confute a classification with so few facts given to support it, and with apparently no account taken of the main difficulties which every one who attempts to classify the Polychæta has to encounter? Silence in this case cannot fairly be taken for consent. To anyone who has made a study of the Polychæta there is only one thing which Dr. Benham's classification shows, and that is which of the forms have been particularly studied by him either directly or indirectly. It is quite evident that, with the aid of Eisig, he knows a good deal about the Capitellidæ, and so much is he impressed with their importance that he wonders (p. 303) whether he should not have given the family an even more prominent position than he has, presumably thinking of making it a separate order on a level with the Oligochæta or Myzostomaria, as opposed to all other Polychæta. Had only Eisig written as elaborate a monograph on every family of the Polychæta, it is probable that Dr. Benham would have allowed each one as important a position in the system as he now assigns to the Capitellidæ As it is, Dr. Benham has had to make out what he can about the other families for himself, and his opportunities for doing so have not been all that could be desired. He has evidently studied some of the British forms most carefully, and has carried his patriotism so far as to assume that they may be allowed to give the key-note to the whole classification, forgetting that it is in warmer seas than ours that these annelids occur in greatest variety.

Those who have made no special study of the Polychæta will accept such a classification unhesitatingly, in the same way as they will accept other statements in the book, though by no means established facts, the author going so far in one case as to assume that investigations of his own as yet unpublished may be taken as evidence against the most careful researches of other writers, a course which his ardent admirer seems to uphold.

Had it not been for the excessiveness of the eulogy bestowed upon the work by this admirer, I would not have felt bound to dwell upon nothing but the shortcomings in the fulfilment of a task, the difficulty of which no one can appreciate more fully than myself. Although my own opinion is that it is better not to publish anything, and especially not for the untrained scientific mind, until all the facts that *are* known are mastered and digested, I am well aware that the risk is thereby run of never publishing at all knowledge which may have taken years to acquire, and that others may think they are furthering science more by publishing than by withholding their preliminary halting-places on the road (which may or may not be the direct road) to knowledge.

University College, London. F. Buchanan.

The Extermination of the Golden Eagle.

Referring to your remarks at pp. 303-4, I should like to be allowed to say that Mr. Joseph Collinson will do good service to British ornithology, and especially to Yorkshire faunists, if he can substantiate the two records of golden eagles said by him to have been killed in that county within three months; the two Scotch instances are unhappily too likely to be correct, and are much to be deplored, as they were probably breeding birds. As for the Yorkshire specimens, I incline to Mr. Murdoch's opinion, and my experience has been long and extensive, that the species is incorrectly reported. Of the many reputed golden eagles which I have traced, not one has proved to be of that species, and south of the Tweed all reports of the occurrence of this bird should be received with the greatest caution. Messrs. Clarke and Roebuck in their Vertebrate Fauna of Yorkshire enumerate seven instances of the occurrence of the golden eagle in Yorkshire since the year 1804, all of which I do not think they would like to vouch for; Mr. Cordeaux has not one to report for Lincolnshire, and the only claim Norfolk can advance to this species is founded upon the remains of one found upon a salt marsh at Stiffkey in December, 1868. The fact is that the so-called English "golden eagles" are the young of Continental bred white-tailed eagles, and these are not at all uncommon along the east coast during the autumn migration, but the adult birds are extremely rare, and I do not know of a single mature individual having been met with in the counties named, and of only one which had made considerable approach to maturity. Mr. Collinson's "sentiments" may be very admirable, but they certainly are not judicious, nor are they likely to be fully shared by the Home Secretary who will require more weighty evidence than he adduces before making the drastic order suggested; nor does Mr. Collinson seem to be aware what measure of protection is being *voluntarily* exercised by certain proprietors in Scotland and the Isles. I think I am justified in saying that the golden eagle is in no immediate danger of extermination in the northern division of the kingdom, but is probably even increasing in numbers. I am an advocate for persuasion rather than coercion in such matters, and would prefer to trust to the friendly action of those who have the power to protect these birds—and I do not think on the matter being fairly laid before them this is very difficult to procure —rather than to the doubtful enforcement of a possibly obnoxious law. Personally I am strongly in favour of judicious protection of most birds during the breeding season, and I have persistently written and worked to that end; but if we stigmatise a man as a "butcher" for killing out of the close time a passing migrant, which under no circumstances could be expected to take up its residence with us, however much we may regret the event, I fear we are more likely to harden his heart than to convert him. I have seen but one of the leaflets of the Humanitarian League (Leaflet No. iv.), and in that the snowy owl is spoken of as one of the birds "rapidly disappearing" in this country. Such questionable ornithology is, I fear, not calculated to add weight to the Society's advocacy.

Norwich, May 3rd, 1897. Thomas Southwell.

"Buffaloes" in North America.

It must have surprised many readers of Natural Science to notice in an editorial comment in last month's number (p. 304) the common but incorrect application of the name "buffalo" to the American Bison. The "*Scientific*" *American* ought to be aware that there are no true buffaloes in the whole American continent. It is a still greater shock to find Natural Science taking up and perpetuating such an error in animal nomenclature.

I hope this will not be regarded as a merely pedantic plea for the correct use of words. When animals have well-known English names, it is most desirable that they should be used on all possible occasions, if they be used correctly, If not, they become highly misleading, giving rise to utterly false views of geographical distribution. Within the last few months in the course of miscellaneous reading I have met with North American "buffaloes," South American "baboons," and South African "deer" and "wolves."

Science and Art Museum, Dublin, May 11, 1897. G. H. Carpenter.

Erratum.

P. 333, line 7 from bottom, for "Walker" read "Walter."

SPECIAL NOTICE.

To Contributors.—*In future all communications must be addressed to the* Editor *of* Natural Science, *at* 67 St. James's Street, London, S.W.

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