ZOOLOGY AT THE BRITISH ASSOCIATION.

SECTION D met on the afternoon of Thursday, September 17, in the Zoology theatre, University College, the President's address having been given in the Arts theatre in the morning. The principal feature of the Section was the large number of discussions, these occupying the mornings throughout the meeting, and two of them being in conjunction with the sections of Physiology and Botany (I and K).

Thursday, September 17.—The first paper was by Mr. R. T. Günther, on Roman oyster culture. The author's facts were drawn both from classical writings and, also, from pictorial representations on ancient vases. Some of the latter gave fairly intelligible pictures of the processes used, and showed what means were adopted for attaching and preserving the young oysters. He believed that there was good evidence to show that ropes and other similar substances were used for the former purpose.

Mr. Walter Garstang read a preliminary communication on the utility of specific characters in the Brachyurous Decapods, referring, however, particularly to the crabs. The object of the inquiry was to ascertain the essential meaning of the denticulation of the edges of the frontal area of the carapace. The author first drew attention to the fact that in some crabs the respiratory current was from before backwards—the reverse of that believed to have applied in all cases by Milne Edwards. This led to the inference that the function of the serration was to filter off solid matter from the water entering the branchial chamber from before. This conclusion was supported by the fact that the denticulations were characteristic essentially of the burrowing crabs, in which, as being buried in sand, it was important that some filtration of the respiratory current should be provided for, in order to prevent the otherwise inevitable blocking up of the respiratory chamber by foreign matter. When, owing to the habits of a crab, the serrations should *à priori* be absent, they were, in fact, not found. The conclusion was that the denticulations on the frontal area of the carapace were functionally correlated with the flow of the respiratory current from before backwards, thus confirming the theory of natural selection by proving the utility of specific characters which would otherwise have been concluded as useless. In the discussion which followed, Prof. W. F. R. Weldon remarked how necessary it was to exercise caution before concluding that any specific characters were useless. Dr. C. H. Hurst drew attention to the anterior position of the renal aperture in Crustacea, which he thought implied a forward, and not a backward, flow of the respiratory current. He suggested that this might also be a diagnostic character in determining the direction of the flow, as it would doubtless be essential that the products of the nephridia should be carried away from the gills. The Rev. T. R. R. Stebbing stated that in many Crustacea the position of the renal aperture could not possibly be correlated with the flow of the respiratory current, and that, therefore, its forward position had no significance in this connection.

The following reports of Committees were then presented: (1) "The Zoology of the Sandwich Islands," by Prof. A. Newton. Three papers had been published as the result of the work of the Committee's collector, on the Orthoptera, Slugs, and Earthworms. Prof. Newton emphasised the importance of proceeding with the work as rapidly as possible, as the fauna in some parts was being partially destroyed by animals introduced into the islands. (2) "The Occupation of a Table at the Marine Biological Laboratory, Plymouth." The report dealt with the work of Mr. George Brebner on the Algæ of the Plymouth district. (3) "Zoology and Botany of the West India Islands." Five papers had been published as the result of the work of the Committee, and other papers were in hand on the Isopod Crustacea and Diptera. During the year much work had been done on the flora of the islands, and the Committee required a grant of £50 to aid in working out the collections already made. (4) "The Biological Investigation of Oceanic Islands." The Rev. T. S. Lea and the Rev. Canon Tristram spoke of the important work being done by the Committee, and of the necessity of its being done at once.

Friday, September 18.—The morning was devoted to a discussion on Neo-Lamarckism, Prof. C. Lloyd Morgan having undertaken to open it. Prof. Morgan, after referring at some length to the precise positions taken up by Neo-Lamarckians and Neo-Darwinians, and the difficulty of disproving either belief, said what was wanted was a really crucial case. If they could in some way exclude natural selection in some cases, and allow it to act in others, they would obtain such crucial cases; and if the habit was equally transmitted, whether natural selection was present or not, that would present an exceedingly strong point for the transmissionist. The nearest approach to such a crucial case, from his own observations, was the reaction of young birds to water. There did not seem to be any instinctive reaction to the sight of water, even on the part of ducklings. But as soon as the bill incidentally touched the water, the appropriate drinking response was at once called forth. Why did not a chick or duckling respond instinctively to the sight of something so essential to its existence as water? He had very little doubt that, under natural conditions, the mother-bird taught them to drink, and this implied that the presence of the mother, as a source of instruction, shielded the young from the incidence of natural selection. Now, though the mother could lead her young chicks to peck at the water, she could not suggest the appropriate drinking response. In this matter she did not shield them from the incidence of natural selection, and those which failed to respond to the stimulus would die of thirst, and be eliminated. Thus, when natural selection was excluded, the habit had not become congenitally linked with a visual stimulus, and where natural selection was in operation the habit had become congenitally linked with a touch or taste stimulus. Prof. Morgan concluded by saying that it was the consideration of such cases as these that had induced him to take up the Weismannian position. In the discussion, Prof. C. S. Minot said he could not defend the Neo-Lamarckian position, as the facts of embryology directly negatived it. Prof. W. F. R. Weldon deplored a metaphysical treatment of the subject. These matters could be proved or disproved by observation, and what they wanted were facts and not polemics. Mr. F. A. Bather thought the Ammonites afforded at least some proof of the Neo-Lamarckian doctrine. Prof. Hartog, Dr. Hurst, Mr. McLachlan, Sir Henry Howorth, the Rev. T. R. R. Stebbing, and Mr. E. W. McBride also took part in the discussion.

In the afternoon Mr. F. Enock read a paper on "The Life-History of the Tiger Beetle (*Cicindela campestris*)." The burrows of the larva, and how they were made, were described, and the method which the larva adopted in order to catch its prey was illustrated by some beautiful coloured lantern slides. The various changes undergone by the larva to produce the perfect insect were then outlined, and the method of egg-laying also described. It was pointed out how perfectly the larva was adapted to the conditions under which it lived, both in its anatomy and habits. The author emphasised the importance of studies of this character, and remarked that only very few of the life-histories of these animals were known in any detail. After this paper the Section adjourned to the loan museum, where a series of slides of Eozoön was exhibited by Sir William Dawson. The latter explained that upon the question of the organic or inorganic character of these remains he had still an open mind.

Mr. J. W. Woodall gave an account of Dannevig's Flodevigen hatchery for salt-water fish. This hatchery was erected in 1883, with the object of ascertaining whether it was possible to produce large numbers of the fry of the better class of salt-water fish at a reasonable cost, the decrease in the fisheries, especially the cod-fishing, having at that time been greatly felt. Difficulties had all along been a considerable bar to the work, but between the years 1890–96, 1203 millions of fry had been hatched at a cost of 0·65*d.* per 1000, whilst last season the cost had been one-third of a penny per 1000, with, it was thought, a still further chance of lowering the expenses. The hatchery cost £800, and the annual expenses were about £500. It was claimed that as a result of the operations of this hatchery the cod was rapidly increasing on the south coast of Norway, and especially at those points where the fry had been liberated. In the discussion on this paper, Dr. J. Hjort thought that ocean currents caused either the destruction or the removal of a great number of the fry liberated by the hatchery. The fry could not be kept sufficiently long in the hatchery at a reasonable expense, and if liberated before, the destruction of them must be very great. Prof. W. F. R. Weldon concurred. He wanted more evidence as to the survival of an appreciable number of the young animals when cast into the sea. Mr. Walter Garstang thought it possible that the supply of food fishes had been increased by means of fish-hatcheries. The following two papers were then read:—"On the necessity for a Fresh-water Zoological Station," by Mr. Scourfield; and "On Improvements in Trawling Apparatus," by Mr. J. H. Maclure.

The report on the "Index generum et specierum animalium" was presented by Mr. F. A. Bather. This index is being compiled by Mr. C. D. Sherborn, who has already been occupied for five years on the work, and has registered over 135,000 species. A small grant was asked for in order that this important work might be more quickly proceeded with. Mr. Bather quoted cases that had occurred in his own individual experience showing the importance of the work being carried on by Mr. Sherborn, and the Rev. T. R. R. Stebbing agreed that for systematists a complete index would be of the greatest assistance, and was becoming year by year more indispensable. The following reports were also submitted:—"On the Coccidæ of Ceylon," "On the transmission of specimens by post," and "On zoological bibliography and publication."

Saturday, September 19.—The report and discussion on the migration of birds, presented by Mr. John Cordeaux, occupied the whole of the morning, and attracted the largest meeting of the Section. The report, prepared by Mr. W. Eagle Clarke, of the Museum of Science and Art, Edinburgh, is a digest of the results obtained concerning the migration of birds as observed at lighthouses and lightships of the British Islands during the years 1880–1887 inclusive. The contents of the report had reference only to the facts obtained by the Committee, and its object was not to solve problems connected with the causes of the phenomena, the evolution of the migratory instinct, or other purely theoretical aspects of the general subject. The digest having been made from at least one hundred thousand records, it was claimed that a sufficient basis had been obtained on which a sound and proper conception of many of the phenomena of the migration of British birds could be based. The migration was treated by Mr. Clarke under the three heads of Geographical, Seasonal, and Meteorological, and a very valuable collection of facts is detailed under each section. Prof. A. Newton opened the discussion by pointing out that Mr. Clarke's labours were not by any means at an end, it being his intention to work out the migration of each species of British bird in as much detail as his data allowed. He (Prof. Newton) could wish that their observations were even more numerous, as they were still very far indeed from having exhausted the facts. Rev. Canon Tristram gave the results of some personal observations tending to show that during the day the birds flew nearer the surface and were guided by sight, whilst flying at a higher altitude during night migrations, when the difficulties of direction were evidently greater. Mr. R. M. Barrington did not think that the wind had much influence on migration. Dr. Hewetson and the Rev. E. P. Knubley also took part in the discussion.

Monday, September 21.—The morning was occupied, in conjunction with Section I, in hearing Dr. Gaskell's presidential address on the ancestry of the vertebrates, the discussion on the latter, requested by Dr. Gaskell, but unusual, taking place in the afternoon. Prof. W. F. R. Weldon, after criticising several special points in Dr. Gaskell's address, said that the great difficulty was the substitution of one alimentary canal for another. If this had been done in the way that had been suggested, they would have expected that vertebrate ontogeny would show some evidence of it; but in no vertebrate was the pharynx formed by the coalescence in the mid-ventral line of a series of buds representing arachnid appendages. He was also not all impressed by the so-called thyroid of the Scorpion. It was easy to find such clusters of cells in many animals. Prof. C. S. Minot could not follow Dr. Gaskell with regard to the central nervous system. The formation of a tube was altogether secondary, and the central nervous system must be described as being originally solid. Further, the origin of both the epithelium lining the neural canal, and the surrounding nervous material, was the same, and this would not be the case if Dr. Gaskell's hypothesis were correct. He therefore differed from Prof. Weldon, who had seen no special difficulty in this part of Dr. Gaskell's address. Mr. E. W. McBride pointed out that if the vertebrate alimentary canal was phylogenetically more recent than its nervous system, ontogeny would of necessity bear out Dr. Gaskell's conclusions. This, however, it did not do. The alimentary canal was always formed first, and the nervous system afterwards. Mr. McBride further, in maintaining that the invertebrate and vertebrate alimentary canals were homologous, stated that in the Decapod *Lucifer*, in which segmentation was not affected by yolk, the formation of the alimentary canal was essentially the same as in vertebrates. He maintained that this objection was absolutely fatal to Dr. Gaskell's theory. Mr. Walter Garstang said that two alternative theories of vertebrate ancestry had been mentioned by Dr. Gaskell, but there was also another which required respectful consideration. That was that the vertebrate nervous system had been formed by the coalescence of lateral cords. He maintained that there was considerable evidence in favour of this. Mr. W. E. Hoyle thought that Dr. Gaskell had been misled by the superficial resemblances of adults, and had not attached enough importance to the early stages. Mr. F. A. Bather stated that palæontology afforded no evidence for Dr. Gaskell's theory. It was very extraordinary, if the vertebrates had been preceded by a series of *Limulus*-like animals having a skeleton of the most imperishable substance known, that absolutely no traces should have been left of these animals in the fossiliferous rocks. Dr. H. Gadow, Prof. A. M. Paterson, and Prof. S. J. Hickson, also took part in the discussion.

As having some bearing on the above discussion, Dr. R. H. Traquair gave an account, illustrated by the original specimens, diagrams, and a model, of the remarkable fossil *Palæospondylus Gunni*. He insisted on its Cyclostome affinities, and expressed his belief that Dr. Bashford Dean's pectoral fin did not belong to the fossil at all. He had examined hundreds of specimens, and had seen no traces of it.

Tuesday, September 22.—The Section was occupied in the morning, in conjunction with Section K, with a discussion on the cell theory, an account of which will appear in the report of the Botany Section. Prof. C. S. Minot read a paper "on the theory of panplasm." He agreed with Bütschli in regarding protoplasm as a mixture of two fluids, similar in nature to an emulsion of oil and water. There was no evidence to show that vital functions were localised in small particles, and that each particle in itself was a unit of living material, and with a number of other such particles went to constitute the protoplasm of a single cell. He supposed that all the materials of the cell by their interaction produced living protoplasm, and that therefore the particles were mutually dependent. Hence the name panplasm. Prof. E. Zacharias thought that the study of living protoplasm was one which would produce valuable results, and had been too much neglected. He did not think protoplasm had a fibrillar structure, and such statements usually rested on an insecure basis of fact. Prof. M. M. Hartog then read a communication on the "relation of multiple cell-division to bipartition at the limit of growth," in which Herbert Spencer's explanation of bipartition was criticised and a new view expounded.

In the afternoon Mr. E. W. McBride opened with a paper on "the value of the morphological method in zoology." He stated that for some time back a distrust of the morphological method of studying evolution had been growing up amongst zoologists, and several alternative methods had been proposed. All of these, however, had their drawbacks. The reason of the discontent with the morphological method was that it proved too much, and the most contradictory conclusions were to be drawn from the same premises. Several suggestions were offered as to better ways of dealing with morphological facts. It was a gratuitous assumption that similarity in broad outlines of structures which were adaptive indicated descent from the same species. Structural resemblance indicated not primarily identity of ancestry, but similarity of past environment; and there might be all degrees in this similarity, both in extent and duration. Such a conclusion was tacitly admitted by systematists who made the basis of their system minute and apparently unimportant peculiarities of external form, colour, or arrangement of similar organs. It was, however, the origin and history of adaptations which interested the morphologist, and his task must be not primarily to draw up genealogical trees, but to correlate adaptations as far as possible to the external conditions which had caused them. Mr. F. A. Bather largely followed the conclusions of Mr. McBride. A great deal of misconception had arisen on account of general conclusions having been drawn from the study of specialised types. As an instance of this he cited the case of the Crinoid *Antedon*, which was a most specialised form, and yet had done duty for a primitive type. Morphologists should be more careful in the selection of their types if they wanted to base general conclusions on their results. Prof. F. Y. Edgeworth then read a paper upon the habits of wasps, showing how statistical methods could be utilised with success in the study of the migrations and other movements of animals such as wasps and other insects.

The following business concluded the proceedings for the day: Prof. C. S. Minot read a paper on the morphology of the olfactory

lobe; a report by Mr. J. E. S. Moore was presented on the fauna of the African lakes; Prof. M. M. Hartog read a paper on the Morphology of the Rotifera and the Trochophore larva; and a letter was read by Prof. A. Newton from Dr. Stirling, on *Genyornis Newtoni*, an extinct Ratite bird from Australia allied to the Emu, but with leg-bones like those of the Moa, supposed to belong to the order *Megistanes*.

Wednesday, September 23.—The first paper of the final meeting of the Section was by Mr. A. T. Masterman on "*Phoronis*, the earliest ancestor of the Vertebrates." Mr. Masterman described two diverticula of the gut in the Actinotrocha larva, which he concluded from their structure represented a double notochord. He hence proposed a new group, to be called the Diplochordata. Hence the supposed relationship of *Phoronis* to the primitive vertebrate was confirmed. Mr. E. W. McBride said that there was such a strong tendency to discover ancestors for the Vertebrata, that great caution should be exercised before needlessly adding to the list. He thought that a double notochord was too great a demand upon their credulity, although Mr. Masterman's diverticula might function as a notochord.

Prof. W. A. Herdman then read a report on the Zoology, Botany, and Geology of the Irish Sea (illustrated by the lantern). A very interesting account was given of the work done by the members of the Liverpool Marine Biology Committee and other naturalists, and slides were shown of the Laboratory at Port Erin and its surroundings. The Committee were doing a useful work, and a work which was very far from being complete. The Rev. T. R. R. Stebbing spoke of the admirable faunistic work being done by the members of the Committee, and thought that they were to be congratulated on their report. Mr. W. E. Hoyle thought the results obtained by Prof. Herdman and his colleagues had an important bearing upon questions of general oceanography, and it was to be hoped, therefore, that the work of the Committee would not cease. Prof. Johann Walther testified to the admirable work that had been done in British seas during the last fifty years. This work, which was so important to marine biologists and oceanographists, had been initiated by Edward Forbes, and continued by Prof. Herdman, whom he regarded as Forbes' natural successor. Dr. Hjort and Mr. A. O. Walker also took part in the discussion.

Mr. Masterman read a further paper on "Some Effects of Pelagic Spawning on the Life-Histories of Marine Fishes," in which he maintained that pelagic spawning was more primitive than littoral. This explained many well-known facts in the migration of fishes. Dr. W. B. Benham then read a short paper on the structure of the genital glands of *Apus*, which, he asserted, could not be described as an hermaphrodite. He had recently made some observations on the reproductive organ of a male *Apus*, and showed diagrams of the spermatogenesis. The specimen had not been well preserved, but, except in this respect, he believed he was the first to study the testis of *Apus* according to modern methods. After some remarks by Prof. Hartog, the meeting concluded with a paper on the life-history of the Haddock, by Prof. W. C. McIntosh, communicated by Mr. Masterman.

MECHANICS AT THE BRITISH ASSOCIATION.

THE meetings in Section G—that devoted to mechanical science—at the recent Liverpool meeting of the British Association were generally well attended, and, on the whole, the proceedings compared not unfavourably with those of recent years. But only qualified praise can be given, as for long "G" has fallen short of its vocation. We look back to past times, to the days of Rankine and Froude, when the Section was more constant to its true mission, and sigh over later records. Mechanical science, though only applied science, *is* science; and though the Section must be utilitarian, it need not be a penny-readings or a means of trade advertisement. We think that any one acquainted with the proceedings of later years will agree that both the latter elements have been too much in evidence. With regard to the penny-readings or popular-lecture side of the question, we had more than one example during the recent meeting. There were some most interesting lectures and discourses, illustrated by equally interesting lantern slides, but they could hardly be classed as scientific They were just admirable penny-readings—nothing more.

With regard to the second undesirable feature to which reference has been made, we feel we are on delicate ground. A man having made an invention of a useful nature, and translated it into a machine or a process, naturally wishes to bring it prominently before the world for financial reasons. A cheap and efficacious method of doing so, is by reading a paper before a technical society. That is a perfectly legitimate proceeding, and is thoroughly recognised by the various societies and institutions of this nature; for however much they may strive to pose as scientific, they know well enough they are no more than technical, and founded on commercial bases. Were it not for the hope of advertisement—it is best to call spades, spades—not one half the papers read before engineering societies would ever be written; but that is no reproach to the societies. They do most admirable and useful work, without which the country would not make the progress it does. The morality of technical societies is, as it should be—"If a man has anything new and instructive to tell us, he is entitled to his advertisement, short of introducing purely commercial details."

But the British Association for the Advancement of Science should take higher ground than this, even in Section G. It should not allow a paper to be read on a trade article at the same time that illustrated catalogues and price-lists of the article are distributed amongst the audience. Neither should it allow its officials to distribute among the audience touting circulars asking members present to subscribe to a public company which bears evidence of being a trade association.

There were, however, at the recent meeting one or two good examples of the work Section G ought to do. Mr. Beaumont's paper may be taken. It was an endeavour to account for a somewhat obscure, but well-known, engineering phenomenon by the aid of scientific or physical data. The author may have been wrong in his conclusions, even in his premises, as some speakers during the discussion suggested, but at any rate he had a proper conception of what a British Association paper should be, and some regard for the dignity of the Section. Mr. Wheeler's report on tidal influences was also a piece of good work, which will be useful to those making scientific investigation of the subject; and there were one or two other items in the programme of a character proper to the Section; but we will proceed to details.

This year Sir Douglas Fox was President of the Section, and on Thursday, September 17, the proceedings were opened by his inaugural address. This we have already printed in full. The first paper taken was by Mr. G. F. Lyster, and was on the "Physical and Engineering Features of the River Mersey, and the Port of Liverpool." This was not a contribution of the popular-lecture order, because it was not popular, and it was certainly not a "mechanical science" paper. It could hardly be called an engineering paper, excepting in respect of it being a catalogue of engineering works. It was very long, and its author read it to the bitter end. It is to be printed in the *Proceedings*.

Mr. Beaumont's paper, to which reference has already been made, came next. The following is an abstract of this contribution. The author was of opinion that the failure of any rail, however perfect, is chiefly a question of the number and weight of the trains passing over it. The result of the rolling of the heavily loaded wheels of engines and vehicles is that a gradual compression of the upper part of the rails takes place, and this produces internal stresses which are cumulative and reach great magnitude. That which takes place in the material of a rail head under the action of very heavy rolling loads at high speed, is precisely that which is purposely brought into use every day in ironworks. The effect is, however, obscured by the slowness of the growth and transmission of the forces which are ultimately destructive. It was pointed out, further, that when a piece of iron or steel is subjected to pressures exceeding the limit of elastic compression, by a rolling or hammering action, or by both these combined, the result is spreading of the material and general change of the dimensions. This is equally the case with a plate hammered or rolled on one side while resting on a flat surface. In these cases, the hammering or rolling work done upon the surfaces tends to compress the material beneath it, but being nearly incompressible and unchangeable in density, the material flows, and change of form results. Generally the material thus changed in form suffers permanently no greater stresses than those within its elastic limit of compression or extension. When, however, the material is not free to flow or to change its form in the directions in which the stresses set up