JOURNAL OF

THE TRANSACTIONS

OF

The Victoria Institute,

OR,

Philosophical Society of Great Britain.

EDITED BY THE HONORARY SECRETARY,
CAPTAIN F. W. H. PETRIE, F.G.S., &c.

VOL. XXX.

LONDON:
(Published by the Institute, 8, Adelphi Terrace, Charing Cross, W.C.)
DAVID NUTT, 270, STRAND.

1898.

ORDINARY MEETING.*

DAVID HOWARD, ESQ., D.L., IN THE CHAIR.

LIFE MEMBER :—Rev. David Gregg, D.D., LL.D., United States.

MEMBER :—Rev. S. C. Logan, D.D., LL.D., United States.

ASSOCIATES :—George Monro, Esq., London ; Rev. T. Ralph Price, M.A., Surrey.

The following paper was then kindly read by Mr. J. W. Slater, F.E.S., F.C.S., in the author's unavoidable absence :—

THE CLASSIFICATION OF THE VERTEBRATA.

By Professor JOHN CLELAND, M.D., LL.D., F.R.S., Professor of Anatomy in the University of Glasgow.

THE Council of the Victoria Institute having done me the honour of asking me to contribute a Paper, I have adopted a suggestion, for which I am indebted to your esteemed Honorary Secretary, Captain Petrie, and venture to lay before you a defence of the views which I am known to hold, in accordance with those of Cuvier and Owen, but in opposition to the prevailing fashion of the last thirty years, with regard to the classification of that large and most important division of animals, the Vertebrata.

Cuvier divided the Vertebrata into four groups, viz., Fishes, Reptiles, Birds and Mammals. But the zoologists of the present day prefer to break up the Reptilia of Cuvier into two quite distinct groups, erecting his Batrachia into a group by itself and confining the term Reptilia to the remainder, including the serpents, turtles, lizards, and

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* 9th of 31st Session.

crocodiles. This change becomes all the more accentuated when we consider that Owen looked on Cuvier's two lower groups as deserving to be united in one great division of Hæmatocrya or cold-blooded animals, contrasting with the warm-blooded Birds and Mammals; while Huxley joined together the Fishes and Amphibians under the name of Ichthyopsida and in like manner joined the restricted Reptiles with the Birds under the name of the Sauropsida, while he kept the Mammals completely separate.

It will be perceived that the Reptilia of Cuvier is the group which has specially been the subject of different opinions. One authority, recognising it as justly to be regarded as a coherent whole, considered it as separated widely from Birds, while another agreeing with him, as every one will do, on the question of the nearness of Fishes to Amphibians, looked on the whole remainder of Cuvier's Reptilia as far removed from them and having a near affinity to Birds.

To consider such a dispute rightly it must be remembered that classification has in recent times had quite a different function and importance from what it once had. The first attempts at classification are liable to be of a highly artificial kind like the Linnean system in Botany, principally useful in enabling large numbers of species to be easily distinguished. But inevitably a natural arrangement makes itself known where there is an abundance of species with great numbers of important characters in common. More and more mere resemblance of analogy is distinguished from integral unity of structure, and the idea of affinity becomes clearly defined. This is the stage which in Zoology became thoroughly established under the auspices of Cuvier, but up to a much later date the majority of zoologists and even anatomists gave little regard to other than adult forms. In Cuvier's day the knowledge of the real constitution of organs was comparatively limited owing to the circumstance that Embryology was in its infancy, and generally supposed, on account of the great difficulties which beset it, to be a study unlikely to throw much light on questions of relationship of different animals. It is in this way that one must largely account for what is, perhaps, the greatest error that Cuvier fell into, the formation of the great division Radiata, objected to even by his devoted admirer Owen, in language quoted from Rudolphi, as "a chaotic group."

Once the idea of development gets its legitimate place

the way is paved for the appreciation of the whole animal kingdom as a unity, consisting of beings derived from ova, by comparable stages, exhibiting as one may say an orderly evolution both in its totality and in the individuals of each species. In fact such a conception was in a certain fashion elaborated by Oken and his follower Carus, not to mention others, even at the time when Cuvier seemed to be carrying all before him. But such views do not necessarily touch on the question of the mode by which different kinds of animals have made their appearance, though they must naturally lead in the long run to the raising of that question. Nowadays it may be said that naturalists generally have yielded to the doctrine which was most distinctly elaborated at first by Lamarck, that the assumption of the immutability of species was a mistake. In the early part of the century Lamarck and Geoffroy St. Hilaire got thus far, but it was not till Darwin wrote that it was generally recognised that the doctrine of immutability had been accepted on grounds other than scientific, and that there was a great body of evidence in favour of common ancestry of forms widely separate. It has flowed from this change of view that the degrees of genetic relationship or possible consanguinity of distant forms of life are sought for and taken into account in classification.

I do not think that consanguinity can be actually proved between animals far asunder, but willingly grant that it has overwhelming probability in its favour, provided always it is understood that Natural Selection has nothing whatever to do with Evolution, and that to produce any single step of elevation in the scale of animality, there is something necessary of an inherent and not an environing description, something acting not only in the life of individual organisms, but on the totality of animal existence from the commencement till now—a something much more akin to the Mathesis of Oken than to Natural Selection. Evolution in organic nature is ever an evolution toward a determinate goal, not the product of "survival of the fittest. The fittest or ablest to survive in the struggle for existence will survive no doubt, but the question is if that principle is sufficient to account for evolution. Is it a main factor, if factor at all, in evolution as distinguished from variation? Confining ourselves to the Vertebrata, can it be worked so as to throw the smallest light on the ancestry of a mammal from a primitive fish or the ancestry of man from any other kind of mam-

mal? No such demonstration has been seriously attempted. On the other hand the determinate nature of Evolution is in my humble opinion quite demonstrable. To that extent Oken was in the right; but the conception which he termed Mathesis is better laid hold of, though still incompletely, by using the word Design.

It is such considerations as these which invest the classification of the Vertebrata with general interest. Let us examine dispassionately how vertebrate animals differ from others, before we proceed to make a rapid survey of the characters of its great divisions. The fundamental characteristic is to be found not really in the vertebral column so much as in that structure which the column protects and supports, namely, the great nervous centre, the cerebro-spinal axis. This centre, though divisible into brain and spinal cord, forms a single continuous structure, beginning in the region of the head and extending away from it originally in the embryo, placed at first superficially, but soon converted into a continuous tube with skeletal surroundings. It at no period presents the appearance of a gangliated chain, though it gives off the nerves in pairs. Its position is dorsal, while the heart is on the ventral side of the alimentary canal. The whole vertebrate body presents a segmented arrangement, that is to say, a serial chain of repeated parts, a phenomenon no doubt pervading many of the Invertebrates; but that which distinctly characterises the vertebrate segmentation is that it is one in which the outgrowths of the cerebro-spinal axis take prominent part, and is a segmentation of the specially animal sphere, not of the visceral systems, although these exhibit a certain serial repetition of a more or less independent kind.

It is now twenty-two years since, in a popular text-book on the structure and functions of the human body, I referred to the relationship which had been pointed out between Vertebrata and Tunicata; stating that the constant ganglion of the latter might fairly be considered as homologous with the anterior or præsophageal ganglion of Articulata, and that it was probable that the cerebro-spinal axis of vertebrates was "a highly developed structure corresponding with that one ganglion." At that time the idea which had been put forward and greatly favoured was that from the Tunicata ascent took place through Amphioxus to the Vertebrates proper; the part played by the notochord in that supposed evolution being much insisted on. It has since occurred very justly to the

generality of zoologists that a structure like the tunicate notochord, present in the larva and liable afterwards to disappear, conforms rather to the characters of a vestigial than of a new structure, so that the evidence favours the speculation that the Tunicates are degenerate Vertebrata rather than precursors of that highest primary division of animals. But what I wrote still remains true, and we are in the position that if we believe that the Vertebrata proceeded from an invertebrate form, the only hypothesis which seems in harmony with the evidence is, that the invertebrate ancestor was devoid of a ventral chain of ganglia, and had a supra-œsophageal ganglion of which our whole cerebro-spinal axis is the grand evolution. This being the case it is plain that there is much to be said in favour of the name given on Okenite principles by Carus to the Vertebrata, namely, Cephalozoaries; but even more is to be said in favour of the appreciations of Lamarck when he divided animals into two grand divisions, viz., Vertebrata and Invertebrata. For the whole scope of the animal kingdom is the evolution of consciousness and of volition, and it is not till the Vertebrata are reached that the organ of consciousness subordinates to it the whole body, and particularly the animal sphere.

A word may be added here with reference to the limbs of the Vertebrata. They are in certain instances suppressed altogether, and in other instances there is only one pair of them developed, but nevertheless no one will cavil at its being considered a characteristic of all the main divisions, from fishes to mammals, that they have two pairs of limbs, always homologous. It is true that there is a plausible theory current among biologists as to the origin of the limbs, that they are remains of two lateral fins which in some ancestor ran the whole length of the body, and were comparable with the mesial fins still existing in fishes. But we may remark first, that there is no evidence of any weight in favour of this theory; it is a *Deus ex machina*. Secondly, there is no explanation offered as to how it came about that it is always the same fore and hind limbs which make their appearance as the evolutions of the assumed lateral fin, and thirdly, the former existence of such a continuous fin, in the days when the assumed forms, bridging the gap between Invertebrates and Vertebrates, were the highest as yet brought into being, does not interfere with the importance of the two pairs of limbs as a characteristic vertebrate feature.

We now come to the classification of Vertebrata, and

I maintain that Cuvier's division into four is correct, not merely on the grounds which Cuvier stated, but for other and it seems to me more important reasons as well. But I am confronted at once with the argument that Vertebrates are divisible into those which have an amnion and those which have none; that the amnion is a structure of very early occurrence in individual development, and that the division which its presence or absence effects must therefore be of primary importance. That argument is founded altogether on a misconception. If, in the early development of one embryo, a part of its blastema is differently developed from what it is in another, that difference will affect every structure derived from the portion of blastema implicated. That is so obvious that every one can understand it, and a very slight study of abnormal developments is sufficient to bring it home to every anatomist. But the amnion is not a part of the embryo, it is not a part of the future animal, it is an envelope round it, a mere complication of part of the germinal membrane outside the embryo proper: and this enormously diminishes its value for purposes of classification. No one would ever think of classifying the animal kingdom according to the characters of the ova from which different kinds of animals spring. Such a procedure would break up the Vertebrata altogether. The limited Reptiles, that is to say, Reptilia as distinguished from Amphibia, would indeed form in conjunction with Birds, a coherent group under such an arrangement, but the group would be sundered as far from the other vertebrates as from any of the invertebrata. The fact is that the peculiarity of the ova of the so-called Sauropsida does not in the least affect their morphological constitution, and that is just what is true also of the presence or absence of the amnion. It is notable in passing that, to whatever cause we may attribute the appearance of an amnion, it must be considered one of those structures which have appeared independently in two different stems, if it be true as is generally held that Mammals are not derived from Birds or the restricted Reptilia, but from an amphibian or pre-amphibian ancestry, a doctrine which I am not disposed to object to. I merely mention the circumstance at the present moment because it is one of those facts which support the doctrine of determinate evolution.

I know of no other argument worthy of serious consideration, besides that derived from the absence of an amnion, for separating the Batrachia of Cuvier from the Reptiles and

raising them to a group of equal value, as is now done under the name of Amphibia; a word which I may mention was used previously by Stannius to include the whole Reptilia of Cuvier; and Stannius was a good judge of the affinities of Batrachia to other Reptilia, for the last edition of his work exhibits enormously more knowledge of their anatomy than has been shown in any book since published.

Let us now look at some of the differences which make a gap between the Fishes and, not the Amphibia only, but all the animals above them. In the first place all fishes and only the Fishes among Vertebrates possess a heart consisting of one series of chambers, a simple heart, receiving the blood into a single auricle and propelling it thence into a ventricle, which sends it into a single trunk, whence every drop has to pass successively through two sets of capillaries. Moreover, although the swimming bladder is undoubtedly homologous with the lungs of other vertebrates, there is no pulmonary artery carrying venous blood. But as soon as we leave the Fishes we come to a construction common to all the Cuvierian Reptiles in so far that while the heart is more complex, the work of circulation and respiration is not more effectively accomplished. A continuous structural evolution is seen which beginning in the Amphibia rises in the crocodiles to complete duplicity of the heart, similar to that of the warm-blooded animals, but prevented from serving the purposes of a completely double circulation by complications in the arterial trunks. I repeat here what I wrote in the text-book already alluded to, that this complexity "though in a manner accounted for as being a stage of progression towards a more perfect organ found in higher animals, might have been difficult to explain if it could have been noted by an observer before birds and mammals appeared on the earth.' This illustrates one of the characters of determinate evolution, viz., that it cannot be appreciated till it is completed.

A second great distinction between fishes and all other vertebrates is to be found in the characters of their limbs. There is great difficulty in making in detail the comparison between the different parts of the limbs of fishes and those of other vertebrates. The conclusions usually held at the present time are not founded, I venture to assert, on anything like a complete investigation of the subject; but this is not the place to enter on that question, and it will serve my present purpose sufficiently to point out that, when the limbs are developed in the non-piscine

vertebrates, they each spring from a girdle, present a single boned upper part, constituting the arm or thigh, and beyond this a forearm or leg as the case may be, which is followed by a hand or foot of complex structure, exhibiting usually more or less distinctly the multipartite carpus or tarsus surmounting digits not more than five. That general description is applicable to Amphibia but not to Fishes; limbs easily compared in detail with our own exist in the former, but not in the latter. Also there is no ascertained correspondence of the nerve-supply of the limbs of fishes with those of other vertebrates, and it may indeed be stated that the ventral fins or hind limbs are obviously supplied by different nerves in different fishes, while any one can appreciate the similarity of the nerve-supply of the limbs of amphibians, reptiles, birds and mammals.

Thirdly, the auditory organ of fishes consists simply of an internal ear, and when accessory apparatus is developed to regulate the pressure on the internal ear it is in the form of a modification of the swimming bladder and of ribs and transverse processes of vertebræ. Not so when we leave the fishes. Accessory apparatus becomes the rule instead of the exception, and it is still more important to observe that the regulation of pressure is always effected by an element termed columella in amphibians, reptiles and birds,—the stapes of mammals.

Fourthly, it is already in the Amphibia that we see for the first time the head separated from the trunk by a neck, or region intervening between the head in front and the heart and lungs behind.

In all these particulars of large structural or functional importance the amphibia cohere to the reptiles and are separated from the fishes.

Turn to the Birds. They are so separated from Mammals that biologists have ceased to look for a direct link uniting them with these, and the only question is as to the importance of the gap dividing them from Reptiles. Now it is certainly a thing not to be overlooked or made little of, that the separation of the pulmonary from the systemic circulation, toward which the reptiles show so many stages of anatomical progress, is complete in all birds and mammals; and not unconnected with this is the circumstance that these alone are the warm-blooded animals. Further they differ from reptiles in having their integuments protected by horny epidermal growths each founded on a single papilla, for this

is the character common to hairs and feathers, and distinguishes both distinctly from other horny growths. But it will be said that certain fossil reptiles, such as the Iguanodon and the Pterodactyle, make near approach to birds, and that the early fossil birds, such as Hesperornis, make in important respects an approach to reptiles. This is undoubtedly true, just as it is true that the Lepidosyren approaches the Amphibia, and the Monotremata approach Birds; but just as in neither of these cases is the approximation the remains of a bridge of transition, so also there is no sufficient evidence that birds are descended from allies of either Iguanodons or Pterodactyles. In particular there does not appear to have been yet found a fossil reptile with a skull in the least like that of a bird.

The fishes which appear to be nearest to any possible genetic link with other vertebrates are the Elasmobranchs, of which the sharks furnish an example. Those on the other hand which are far more numerous at the present time and may be said to exhibit distinct piscine character in the highest degree, are the osseous fishes, which present remarkable modifications, sundering them far from any possible transition to the other vertebrates. The restricted Reptilia most probably took origin from Amphibia, in which case the Cuvierian Reptilia may be looked on as consisting of a primitive batrachian stem, breaking up into numerous branches of which the serpents, turtles, lizards and crocodiles alone survive; and I may be allowed to doubt if, from any of the extinct branches known, it is possible that birds any more than mammals have descended.

This isolation of mammals is not so remarkable if evolution be determinate, like the development of an embryo into an adult, since in that case the stages of evolution of an organ are not necessarily advantageous. Like the stages of development of such an organ as the eye in the vertebrate embryo, they may be functionally useless. Therefore, one would expect transitional forms to disappear, and also that the character of the whole evolution would not be fully brought out till it was complete or near completion; and in favour of this I have already said something in speaking of the circulatory organs.

The restricted Reptilia, Aves and Mammalia may all three have originated from the Amphibia, but the Amphibia do not differ more from serpents, turtles, lizards and crocodiles than these groups differ one from another. If that be so, there

are only four primary divisions of the Vertebrata. Let us glance at their relationship as regards typical character.

In fishes there are seen all those characters which led Carus to call the Vertebrata Cephalozoa, but so far from the great regions of head and cervico-thorax and abdomen, afterwards met with, being distinct in fishes they cannot be said yet to exist; the viscera are crowded forwards, while the spinal cord, and muscular and osseous segments, are produced backwards, till at last in one great modern group. that to which the haddock and cod belong, not only is the shoulder-girdle attached to the head, but the ventral fins adhere to the shoulder-girdle and the cloaca is immediately behind the ventral fins.

But on leaving the Fishes, there is a more distinct separation of regions having a peculiar relation to the whole animal structure. The body-cavity remains devoted to the structures of the vegetal sphere. The head contains the highest and most characteristic organs of the animal sphere. The neck is the original seat of development of the central vascular system, though the heart is adventitiously pushed in its later development into the part of the trunk which becomes modified for its reception, namely, the thorax.

The Vertebrata, higher than the Fishes, are groups deriving their general facies and characteristic development from the abdomen, the cervico-thorax and the head respectively. The Cuvierian reptiles may be fairly regarded as abdominal Vertebrata; the activity and domination of the circulatory and respiratory system gives character to the Birds; the development of the brain is the characteristic feature of Mammals.

I may be permitted to add what I pointed out to the British Association at Exeter in 1869, viz., that the limbs are not developments of the individual segments of the body, but belong each pair to a region, and that the mandibular arch is the limb arch of the head, while the opercular bone of the fish, the stapes or columella of other vertebrates, is the radiation or limb proper of that arch. To prove this in detail would involve entering so largely on the whole structure of the skull that I acknowledge I have never had time to publish the proof *in extenso*. I shall merely state that if this view is correct, then the Vertebrata have three pairs of limbs, a pair for each region, also that the limb of a region may be perfected in inverse proportion to the central part. Thus the mandibular arch

and limb are at their maximum of development in Fishes while the brain is at its minimum. In Birds, which have remarkable thoracic development in connection with circulation and respiration, the pectoral limb has the hand abortive, and in Mammals, which are specially remarkable for the advance in size and structure of the brain, the quadrate bone which hitherto supported the lower jaw is reduced to an ossicle of the ear, and the jaw itself is simpler than in any other vertebrates.

The CHAIRMAN (D. HOWARD, Esq., D.L.).—We have to thank Dr. Cleland for bringing before us a paper of very great interest, and Mr. Slater for so kindly reading it.

The whole question of Comparative Anatomy, as we used to call it and to which the word evolution may be very fairly applied, especially in the sense used by Dr. Cleland, is one of the most fascinating studies that it is possible to pursue.

It requires an expert to speak on such a difficult subject. But to any of us who have not the privilege of being experts it is simply one of those fields of study which always afford great interest. I would venture to call attention to the very clear and decided way in which the author of the paper defines two things which are so often seriously confused.

The idea of Evolution cannot, I think, be studied without seeing that it is absolutely essential clearly to distinguish between a process of evolution and the cause of that evolution; to many people's minds, the idea of evolution is solely confined to evolution by Natural Selection. Here we have the writer of the paper, who has certainly grasped the relations of different organisms as few have, and yet has no doubt, in his own mind, about the conceivable possibility of mere accident or environment having been the governing factor in that remarkable chain of events.

J. HUTCHINSON, Esq., M.D., F.R.S.—I see a statement in the paper that there is no similarity between the skulls of birds and of reptiles. Is it not forgotten that there is a remarkable similarity in the case of the single occipital condyle?

Inspector-General J. D. MACDONALD, F.R.S.—I have spent more

time on the Invertebrata than the Vertebrata, but still, as an anatomist, I feel much interested in the question that has been so ably brought up. I think there still remains great doubt about the internal evidence of evolution apart from a superintending cause in which skill and design have been most clearly carried out, and no question of environment or any other condition of life in connection with the survival of the fittest will explain evolution. It has been stated so in this paper.

Though the mammal and the bird are very distinct in their leading characteristics, the transitional forms are few and far between: the *Ornithorhynchus* is, perhaps, the most striking example. Again, in the passage from the reptile to the bird we have the *Pterodactyle*, and it is impossible to ignore the construction of the beak and fins of the turtle as exhibiting at least a representative relationship to the corresponding parts of the Penguin.

The reptiles and fishes are singularly connected by the *Lepidosiren*, and it is usual to trace fishes from the *Amphioxus lanceolatus* and the latter from the *Tunicata*, to the exclusion of the *Mollusca* proper, so that whatever had been gained or achieved by Nature in the magnificent eye of the Cuttlefish does not appear to be turned to account in the *Amphioxus*, which has either no eye at all or a rudimentary eye speck.

A good lesson may be derived from the study of the development of the circulatory system, which is so much consulted in reference to classification. It is at first purely ciliary (*Lingula*), and the outgoing and returning currents course along opposite sides of the same vessels. Secondly, without any valvular mechanism the whole round of the circulation in the *Tunicate* sweeps alternately in opposite directions. Thirdly, in the true *Mollusca* the supply of valves determines an irreversible course to the circulation, and the whole organization is as perfect as we find it in any fish.

Finally, with gills or lungs the circulation is either branchial or pulmonic as well as systemic.

J. W. Slater, Esq., F.E.S., F.C.S.—I have presumed to put down a few of my own remarks as comments on Professor Cleland's address, which may not, perhaps, be without interest at the present juncture. Those who, like myself, have had the advantage of studying Professor Cleland's thoughtful and suggestive paper on "Terminal forms of Life" will be exceptionally in a position to appreciate the paper with which we have just been favoured. It is plain

that, like Owen, but unlike certain more recent classifiers, he lays a full and just weight on the distinction between the cold-blooded and the warm-blooded group of animals. This distinction is smothered if we, with Huxley, join together a part of the reptiles with the birds under the name of "Sauropsida." I have ventured to point out that cold-blooded animals alone secrete physiological venous pigments and textile fibres, whilst in the warm-blooded group (birds and mammals) the energy which would be required for the elaboration of such products serves for maintaining the heat of the system. It is a very interesting fact that in the *Ornithorhynchus*, the only mammal which has the power of secreting a true venom, the temperature of the blood is about 15 degrees lower than that of other mammals. Hence, to write cold-blooded and warm-blooded animals in one and the same group seems to be a grave error. It is very satisfactory to find the author giving his opinion that Natural selection has nothing to do with Evolution, and that to raise an organism to a higher plane we must have some inherent power and not any mere external agency. Whether such agency is temperature, atmospheric pressure, moisture, diet, or the rivalry of co-existing species, is not the capital point. Many writers do not distinguish between evolution and variation. I am much gratified to find to what extent Professor Cleland does justice to the late Professor Owen, whom I have the honour of considering as my old master, some of whose contemporaries and successors, though they may have spoken lightly of his attainments, have been reaping the harvest of what he sowed, although, at times, failing to accord him credit for what he has done.

The meeting was then adjourned.

COMMUNICATIONS RECEIVED IN REGARD TO THE PRECEDING PAPER.

Professor H. W. PARKER (United States) writes :—

I have read carefully and with much interest the paper by Professor Cleland—with the more interest because for ten years I used his compact manual of animal physiology as a text-book in my college classes. It seems to me that he makes out his case convincingly, even in so brief a discussion ; and it is refreshing to find that scientific progress with its attendant Babel of classification has not really fused and confused the four vertebrate

classes. I note also what is said of the degenerate Tunicates, which, in a new edition of the zoological text-book most widely adopted in this country, are set up as a group co-ordinate with the other great branches of the animal kingdom. The chief interest of the paper, to me, is its bearing on "determinate evolution," well illustrated by such facts as the development of the heart and circulation from Amphibia upward to the crocodile, and prophetic of perfection and use in the higher vertebrates.

WALTER A. KIDD, Esq., M.D., writes :—

It is well that the Institute should have the opportunity, through the able paper of Professor John Cleland, of contributing to that reaction towards the views of Cuvier and Owen (called by Professor Huxley "The British Cuvier"), which last year a writer in the *Quarterly Review* discerned in the scientific horizon. The Classification of Vertebrata proposed commends itself more to the mind not prepossessed with evolutionary doctrines than, for example, that of Ray Lankester, who places among the Vertebrate Phylum—

(1) The Craniata (or Cuvierian Vertebrata).
(2) Cephalocorda (represented alone by Amphioxus).
(3) Urochorda (Tunicata).
(4) Hemichorda (Balanoglossus, alone).

Doubtless the divisions (2) (3) (4) possess those three structures characteristic of Vertebrata—noto-chord, gill-slits, and dorsal nerve-plates; but the evidence brought forward by Professor Cleland that the Tunicata are degenerate vertebrates, and the opinion of Professor Alleyne Nicholson that the Amphioxus is usually regarded as a degraded type of the Fishes, make it more correct to place No. (2) No. (3) and (4) in special divisions of the animal kingdom. The late Professor Milnes Marshall admitted that the Tunicata or Ascidians are degenerate animals, but refused to allow this of Amphioxus, saying that it "merely stops at what is an early stage in the development of the higher forms." He is not anxious to claim Tunicata, Balanoglossus, or even Amphioxus, as direct links between the Invertebrate and Vertebrate sub-Kingdoms, but that, of all living animals, Amphioxus most nearly represents the Common Ancestor of Vertebrata. No such half-hearted claims are strong enough for Mr. Edward Clodd in the *Primer of Evolution*, who boldly claims Amphioxus, Tunicata, and Balanoglossus as interesting links between the two

kingdoms! Considering the various opinions held as to these intrinsically unimportant and not very numerous groups of animals, it seems more judicial in men of science to continue the Classification of Vertebrata, as proposed by Professor Cleland, into Fishes, Reptiles in the extended sense, Birds, and Mammals.

The doctrine of Dohrn on Degeneration may have yet much to say on the question of the origin of great divisions of animals, and may seriously weaken the theory of evolution, as usually understood, *at certain of its points*, as in this borderland between the Vertebrate and Invertebrate sub-Kingdoms. To take two higher steps in the supposed ladder of the Ascent of Man, the Monotremata may be no more than degenerate Marsupialia, and not links in the chain between Reptiles and Marsupialia. The distribution of the Monotremata and Marsupialia in the Australian Province would harmonize with this, and the fact that the Ornithorhynchus in early life has teeth and loses them when full grown, and that the other type, the Echidna, has none, would tend to place them among Marsupialia of a degraded type, and to show that they have developed by "progressive simplification from their more elaborate ancestors." Again, the small mammals known as Insectivora, which are looked upon as links between certain Placental Mammals and Lemurs, may reasonably be considered to be degenerate Rodents, having become degenerate through their nocturnal subterranean and hybernating habits.

Indeed, when the doctrine of Degeneration is given full play, and the question of *varieties* fully borne in mind, a considerable weakening of the theory of organic evolution as usually understood may be anticipated. The determinate evolution of Professor Cleland is a much more philosophic conception than that in which Natural Selection, with its indispensable partner, accidental variation, is the main factor. Determinate evolution at any rate harmonizes with the undeniable teleology which stares the observer in the face, let the "naturalist" say what he will as to secondary or natural causes. Here, whether one accepts the view or not, supernatural causation of the life-history of the plants and animals which people the earth finds expression.

THE AUTHOR'S REPLY.

April, 1898.

Permit me to thank the Institute, and especially the members who took part in the discussion, for the kindly reception of my communication.

With regard to remarks made on the subject of birds and reptiles, I am constrained to mention that the similarity of a turtle's skull to a bird's consists mainly in the circumstance that modern turtles and birds are both edentulous. Both have beaks, but in cranial osteology they are widely different. It is true that Huxley used the single occipital condyle as a character to link together the parts of his group sauropsida. But I have pointed out the close relationship of the two-condyled amphibia to the single-condyled reptiles, and it will be readily admitted that the single mesial condyle is produced in a very simple way by constriction of the basi-occipital bone and fusion of two articular cavities. Curiously, no one till now has attracted attention to the fact that the seal has the two atlanto-occipital articulations run into one, and yet is as thoroughly mammalian in its skull as in every other part. The greatest care must be taken not to confuse between homology and analogy. Wings are analogous structures, but those of the pterodactyle, the bird and the bat present totally different variations of the skeleton of the pectoral limb, of which they all three are modifications. Homologically a penguin's wings are allied to those of other birds, and are as different as can well be conceived from the fins of a turtle. Questions of this sort are purely anatomical, and those who are familiar with reptilian and avian osteology cannot and do not for a moment admit the possibility of any doubt with regard to them. I have much pleasure in expressing my agreement with Dr. Kidd as to the origin of the Monotremata.