

DOHRN ON THE ORIGIN OF THE VERTEBRATA AND ON THE PRINCIPLE OF SUCCESSION OF FUNCTIONS

THE introduction of the doctrine of Descent into the study of organic phenomena has opened the flood-gates of speculation, of hypothesis, and theory. Probably, with very few exceptions, this is regarded with regret and impatience by zoologists and botanists, even though staunch Darwinian converts, who had made any name in biology in the period anterior to the publication of Mr. Darwin's work on the "Origin of Species." Those were the days of a reaction brought about by the fantastic imaginings of Oken and his school; and the naturalists brought up in those days cannot rid themselves of a dread of speculation which has become as much an organic part of their nervous systems as has the fear of precipices, bricklayers' ladders, and of the mythological personages of their childhood, to most men. It remains for the present and later generations who will be brought up, not to fear, but to use speculation, to turn fully to account the immense engine of research which Mr. Darwin has placed in their hands. We see, in fact, no reason for refusing to welcome any number of hypotheses and theories on biological topics: let every one make his suggestion—the more ingenious and original the better—and let it be taken for what it is worth. If in its author's or another naturalist's hands it should lead to the discovery of new facts—if it should in a more or less modified form be established as true—it will bring thanks and honour to its promoter. If, on the other hand, it should lead to nothing, should be tested and found neither true nor suggestive of truth, it will fall to the ground quietly enough, and do no harm to anybody. This, be it said, applies only to the publication of such hypotheses within the scientific area—a totally different and a very grave responsibility is incurred when an author represents a hypothesis as an established doctrine, and appeals to the support of an uninstructed public. The fact is that we have acquired this freedom of speculation as compared with the proscription of it in the pre-Darwinian period, through the circumstance that biological theory has passed from the theological to the scientific form. Today—no matter who its author—a speculation as to the mode of development of this or that group of animals and the significance of this or that organ, may be verified or rejected; no one will attach undue value to it until this process has been gone through. Formerly it was not possible to test such speculations; we had in fact no link by which organic phenomena were made part of the whole series of phenomena of which science takes cognisance, and biology had no foundation in the so-called experimental sciences. Hence speculations were liable (as in theological discussion) to be launched by authorities, and to be received not as speculation, but as something like *inspiration*, by disciples; and on the other hand to be rigorously and almost puritanically tabooed by a constantly increasing number who, refusing to occupy themselves with these vain imaginings, endeavoured to keep the facts pure and undefiled, waiting for the coming of an interpreter—who was realised in Mr. Darwin. The doctrine of organic evolution as elaborated by Mr. Darwin and his immediate successors has provided us with a proper scientific framework, and we can now proceed to build on that by the legitimate methods of modern inductive science. It will be some time before biology fully emerges from its theological form; at least another generation must pass; and in the mean time we must expect the continuation of special claims on the part of authorities to advance speculative doctrines *ex cathedra*; and on the other hand a lingering antagonism to all speculation, even to that which makes no pretension to authority, on the part of those who have imbibed the

horror of fantastic "Natur-philosophie" and of dogmatic pretensions.

To those who belong to neither of these sections, it is worth while pointing out that even the most careful observation and recording of phenomena in the absence from the observer's mind of some theory or speculation which shall, so to say, sharpen his wits and keep his eyes open, is likely to be of the very smallest value. It cannot be too strongly asserted that in observing a complicated phenomenon—such as an organic structure or series of structures—the investigator is only likely to see what he has already imagined *may be* there; the chances are greatly against his detecting an arrangement or a mode of development of which he had previously no suspicion. Though cases of unforeseen discovery do occur, yet it may be safely stated that, as far as all but the most patent and macroscopic appearances are concerned, the observations of no predecessor should be trusted by an investigator beyond the limit which is given by the hypotheses which are known to have been present to that predecessor's mind. In fact, a man can only expect to get answers from Nature to specific questions; she will not give him unsolicited information, nor make a voluntary statement, however attentive the listener. Hence the value and legitimacy of speculations, even *ad nauseam*, on such matters as the pedigree of animals and plants. When advanced, with due knowledge of ascertained facts, they suggest to the embryologist, to the palæontologist, and the anatomist, a number of possibilities which he holds before him as so many questions to be answered by the material of his studies. It is true that it is desirable in a high degree that the person who frames a hypothesis should also himself be active in using it in a practical way, and indeed if he is not, he may find no one who will take the trouble to bear it in mind. Therefore, one must admit the generosity of those who now-a-days make a present of their speculations to scientific *confrères*, and undertake the part of the profound thinker, whilst assigning to others the more practical task of verification and elaboration. For, since the days of scientific inspiration are past, but little credit will attach to the launchers of hypotheses, and more and more to those who destroy them, either by showing their error or by transubstantiating them, in demonstrating that which was supposed, actually to be. It is Darwin whose name we associate with the doctrine of evolution—not Lamarck's, nor Goethe's, nor Wells', nor Freke's.

These remarks are a necessary prelude to the consideration of the bold speculations with which Dr. Anton Dohrn, the founder of the zoological station of Naples, known also for some interesting observations on the development of Crustacea, has recently astonished the zoological world in his "Ursprung der Wirbelthiere und Princip des Functionswechsels." The necessary sequence of the general acceptance of Darwin's theory of the origin of species by descent and natural selection has been an attempt to establish the pedigree of the animal kingdom, and to indicate the degrees of consanguinity among the different members of it known to us. In the first attempts in this direction no one can doubt that errors and vagaries of all kinds must occur. It is only when naturalists have fairly set themselves to the task and made some few false starts that we can expect to see anything like a just appreciation of the methods to be pursued, of the difficulties to be encountered, and of the fallacies to be avoided. We are obliged to admit that the first attempts in the way of constructing the pedigree have been influenced, as they were likely to be, by the remnants of old notions and by the lack of a perfectly unprejudiced appreciation of the question in hand. The pamphlet of Dr. Dohrn comes opportunely enough to insist upon one or two important considerations which have been neglected; and even though, by an excess of antagonism to prevailing prejudice, Dr. Dohrn may be

led to oppose exaggeration to exaggeration, we cannot the less feel that there is sound sense and truth in the general purport of his views.

In the pre-Darwinian period naturalists looked upon the series of classes and orders of the animal kingdom as a more or less branched ascending series. The effort in nearly all classifications was to distinguish the lower from the higher and to place the groups in their supposed order of merit, as competitors for the highest rank of organisation. This has led—now that Darwinism is accepted—to a tacit assumption that the order of “degree of organisation” which was worked out in the pre-Darwinian era, is necessarily the order of historical development; that consequently the lower forms of any group which are existing to day, are nearer to the ancestral forms of that group than are the more highly organised forms.

Whilst an exception has been made to this unreasoned and unchallenged assumption in favour of the parasitic forms for which the term “retrogressive development” has been coined, it does not appear to have occurred to any prominent naturalist, at any rate it has not been prominently maintained, that the “retrogressive development” which all so readily admit for parasites, may be a very general phenomenon, as widely or more widely diffused as that of “progressive development.” To have insisted on this possibility even to an excess (of which more below) is the merit of Dr. Anton Dohrn. Dr. Dohrn has arrived at an appreciation of the possibilities of degradation or retrogressive development, by divesting himself of all preconceived notions and of all respect for authorities. In his pamphlet he grapples courteously, but fearlessly enough, with Von Baer, Darwin, Haeckel, Gegenbaur, and for the matter of that by implication with almost every zoologist of note.

We claim for him, first of all, full liberty to do this and to launch the hypothesis of general retrogressive development as a competitor with that of universal progressive development. It requires but a few words of explanation and an example, for which Dr. Dohrn has selected the possible relations of the Ascidians to the Vertebrata, to show that retrogressive development is not only a possibility, but *must* be going on and has been going on—on a very large scale—and in any doubtful case is as much entitled to consideration as the hypothesis of progressive development. A less important portion of the pamphlet is that which precedes the development of the author's Hypothesis of Degradation, and illustrates the application of what he calls the “principle” of the Succession of Functions. To put it in the form of a hypothesis it comes to this:—“Organs do not arise *de novo* in organisms, but are formed by the gradual change of function and accompanying change of structure of pre-existing organs.” That this is true, or at any rate that it is the hypothesis which, according to the “principle of uniformity,” must be preferred to its converse, namely, “that organs *are* formed *de novo*” must be admitted by everyone. In fact, most of Dr. Dohrn's readers will feel that there really is not much novelty in this proposition, since it is already involved in the doctrine of homologies to a very large extent. Dr. Dohrn admits this in his pamphlet, but we conceive that his view differs from that implied in the generally recognised doctrine of homologies, in that the latter is not absolute; it would merely assert that *many* or *some* organs do not arise *de novo*, but are formed by the gradual change of function and accompanying change of structure of pre-existing organs. Dr. Dohrn raises this into a hypothesis of *universal* application, and proposes to apply it stringently in speculations as to the genealogical relationships of organisms. He illustrates its application in an attempt to explain the genealogical affinities and mode of development of Ascidians, Amphioxus, Lampreys, and Sharks. We are very much disposed to believe that here, as in his advocacy of

the hypothesis of degradation, Dr. Dohrn has grasped and emphasised a truth which has been floating before the eyes of other people but has not been appreciated at anything like its real importance by them. We believe that the hypotheses of degradation and of continued homologies put before naturalists in the present pamphlet will have a very important and powerful influence on the rapidly progressing reconstruction of the animal pedigree with which so many zoologists are busy.

At the same time it is necessary to point out that the particular speculative conclusions at which Dr. Dohrn arrives as to the new Vertebrate mouth which has replaced the ancestor's mouth as well as the new Ascidian mouth, which has done the same thing—further, the conclusion as to the secondary character of the Vertebrates' anus, and the development of Vertebrate gill-slits from segment organs and of Vertebrate limbs from annelidan gill-supports—all this and more besides is ingenious and healthy hypothesis, but has no value unless Dr. Dohrn or some one else (which is not a thing he should rely upon) will bring it to bear upon the facts and seek to establish it by new observations. We must confess that although we are inclined to entertain some of Dr. Dohrn's suggestions as hypotheses, yet we feel that he has given us rather a large supply, which, in justice to his reputation as an observer, he should hasten to balance by a fair amount of new investigation. Such a speculation as that which he gives us relative to the origin of Vertebrates, can from his hands only be regarded as a sort of programme or announcement of the work which he intends to do during the next decade at the Zoological Station. We shall look most anxiously for the first instalment of results.

Lastly, we shall not shrink from pointing out that Dr. Dohrn urges the hypothesis of degradation to a degree which would be regrettable were it quite evident that he is serious and not merely anxious to engage the attention of his reader by letting imagination have its full swing. Supposing, says Dr. Dohrn, that the Ascidians are the degenerate descendants of a half-worm-half-fish-like ancestor—and the mere consideration of their individual development is enough to make this probable—then we have to admit an amount of degeneration which covers very wide possibilities. For the compound Ascidians, with their various encrusting species, are included in the series; and, moreover, many forms which have ceased in their individual development to give any indication of the affinities which are indicated by the larvæ of other forms. If so large, so abundant, and varied a group can thus take its rise by degeneration, what is to prevent the simpler worms from having originated in the same way? Why may not the Cœlenterata have acquired histological and general simplification in a parallel manner by degeneration accompanying a fixed life? And the Protozoa, the whole series of unicellular animals, why are they not to be considered as degenerated from multicellular forms by a process of simplification? In fact, in a few sentences Dr. Dohrn suggests doubts which land him in a theory which is almost identical with that of Aristotle.

“Thus then,” he says, “the animal kingdom has quite a new aspect for us when we look at it from the point of view developed in this essay. Instead of having before us a large mass of forms which from the first commencement of organic life have made little or no progress, whilst a few favoured stems have developed themselves to the highest perfection, we obtain the conception of one single stem, which bore within itself the germ of all other higher, highest but also lowest forms, whose descendants on the one hand in thought and fancy embrace the universe and recognise themselves within the universe as individualities, whilst others lead a senseless inert existence and give rise to the belief that a non-living nature might be able now or at any time to originate such things.” Finally, the author argues that the development

of this single stem is not to be assigned to either chance or to chemico-physical, but to an "Entwickelungs-gesetz" yet to be discovered. This, we confess, is to us a disappointing termination to a clever and spirited essay. Surely Dr. Dohrn would not expect a scientific man to understand by the word "chance" anything but a periphrasis for the operation of hidden cause. And what can he expect any law of development to be, if not an expression of the operation of chemico-physical causes?

As to the original form under which life made its first appearance, Dr. Dohrn's words would almost lead to the impression that he believes in the creation of a "type-form" something like the Cherubim, with an account of which Archdeacon Freeman favoured Section D of the British Association when it met at Exeter in 1869. His language is, however, sufficiently vague to warrant the supposition that, as an orthodox physical philosopher, he holds the doctrine of the evolution of organic forms subject to the larger doctrine of general evolution, and consequently we may suppose that he would hold that the single stem which has blossomed in man, and from which all other forms have descended by retrograde development, *did* take its origin from simple protoplasm, which had naturally been evolved from carbon compounds. If the animal pedigree did originate from these very simple beginnings, we suppose Dr. Dohrn would say that all trace of them is gone, what is simple *now* in the way of organisms is not the simplicity of the original stock, but a simplicity attained by degeneration. We do not see any reason to accept this hypothesis of *universal* degradation (man alone being excepted from its influence), any more than we can see reason to accept the competing hypothesis of *universal* progress. We are very strongly inclined to think that neither hypothesis can have the whole field to itself. We should expect to find in some directions progress, in others retrogression.

The extent to which each of these processes has gone on in past ages in connection with the family history of the animal kingdom is the great problem for zoological research.
E. R. L.

THE NEW METAL GALLIUM

THE discovery, by M. Lecoq de Boisbaudran, of a supposed new element in a blende from the Pierrefite mine in the Argeles Valley, Pyrenees, was made known in our "Notes" of last week. This element, which the discoverer proposes to name *Gallium*, has revealed itself by the following chemical reactions:—

The oxide, or possibly suboxide, is precipitated by metallic zinc from a solution containing chlorides and sulphates.

In a mixture of the chlorides of the new metal and of zinc, ammonia throws down the new element first if added in a quantity insufficient to precipitate the whole of the metals present. Nearly the whole of the gallium is thus thrown down in the first fraction.

Under conditions competent to peroxidise the new metal, the oxide is soluble in excess of ammonia.

Ammonium sulphhydrate produces a precipitate insoluble in an excess of the reagent. The sulphide appears to be white.

Sulphuretted hydrogen produces a precipitate in presence of ammonium acetate and excess of acetic acid. In presence of zinc salts the new substance concentrates itself in the sulphides first deposited, but six fractional precipitations were requisite to remove the greatest part of the zinc sulphide. In presence of hydrochloric acid no precipitate is formed.

The oxide, like that of zinc, dissolves in excess of ammonium carbonate.

The salts of gallium are readily precipitated in the cold by barium carbonate.

The chloride may be frequently evaporated with great

excess of *aqua regia* without undergoing any loss by volatilisation.

When hydrated zinc chloride containing a trace of the new substance is heated to the point when zinc oxychloride begins to form, the gallium remains in an insoluble condition, possibly as oxychloride.

The quantity of the substance procured was too small to attempt its isolation. Some drops of zinc chloride solution in which the new metal had been concentrated were examined spectroscopically by the electric spark. The spectrum is composed chiefly of a violet line about wavelength 417, and a feeble line about 404.

In his communication to the French Academy, the author states that he obtained the first indications of the new metal on Friday, Aug. 27. It is to be hoped that a good supply of the mineral will be procurable, so that the new element may be isolated, its atomic weight determined, and its reactions studied in detail. This now makes the fifth terrestrial element which the spectroscope has been instrumental in bringing to light.

R. MELDOLA

UNPUBLISHED LETTERS OF GILBERT WHITE

AT the meeting of the Norfolk and Norwich Naturalists' Society, held on the 28th inst., the secretary read an interesting series of ten unpublished letters, written by Gilbert White, of Selborne, to Robert Marsham, F.R.S., of Stratton Strawless, Norfolk, and communicated by the Rev. H. P. Marsham, great-grandson of the latter. The letters, which are dated between August 13, 1790, and June 15, 1793, are excellent examples of Gilbert White's delightfully discursive style, their contents being of a very varied nature. Mr. Marsham, to whom they were addressed, was a great planter, and communicated his experiments on growing trees to the Royal Society; the beauty and great size of the timber at Stratton bear testimony at the present day to his judgment and successful treatment. As might be expected, under these circumstances, a large portion of the correspondence is devoted to forest-trees, the love for which was shared in an almost equal degree by both correspondents. The "Indications of Spring," of which Mr. Marsham left such a remarkable register, and which have been continued by his family, with one slight interruption, from the year 1736 to the present time (see "Philosophical Transactions" for 1789, and the "Transactions" of this Society for 1874-5), of course form an annual topic, as well as the rainfall; but perhaps the most valuable part of the correspondence is the gossip about birds, some of which is of very great interest. On the 30th October, 1792, Marsham writes to White: "My man has just shot me a bird which was flying about my house; I am confident I have never seen its likeness before." On reference to his Willoughby, he declares it to be "the Wall-creeper, or Spider-catcher," and a description, endorsed by him on one of White's letters, as well as a manuscript note in his copy of Willoughby's "Ornithology," still in the possession of the Marsham family, places it beyond doubt that the bird was a veritable *Tichodroma muraria*. White, after saying he is persuaded that the bird is the "very *Certhia muraria*," continues: "You will have the satisfaction of introducing a new bird of which future ornithologists will say, 'Found at Stratton, in Norfolk, by that painful and accurate naturalist, Robert Marsham, Esq.,"—a prophesy which, after an interval of eighty-two years, will at length be fulfilled. Nearly a whole letter is devoted to an extract from an unpublished "Natural History of Gibraltar," by Gilbert White's brother, the Rev. John White, who resided many years on the "Rock." By this it is shown that John White, who went to reside there in 1756, soon discovered the Crag Swallow