THE LAWS OF ORGANIC DEVELOPMENT*

THE discussion of this subject divides itself into two parts, viz.: a consideration of the proof that evolution of organic types, or descent with modification, has taken place; and, secondly, the investigation of the laws in accordance with which this development has progressed.

I.—*On the Proof for Evolution.*

There are two modes of demonstration, both depending on direct observation. One of these has been successfully presented by Darwin. He has observed the origin of varieties in animals and plants, either in the domesticated or wild states, and has shown, what had been known to many, the lack of distinction in the grades of difference which separate varieties and species. But he has also pointed out that species (such, so far as distinctness goes) have been derived from other species among domesticated animals, and he infers by induction that other species, whose origin has not been observed, have also descended from common parents. So far I believe his induction to be justified: but when from this basis evolution of divisions defined by important structural characters, as genera, orders, classes, &c., is inferred, I believe that we do not know enough of the uniformity of nature's processes in the premises to enable us so regard this kind of proof as conclusive.

I therefore appeal to another mode of proving it, and one which covers the case of all the more really structural features of animals and plants.

It is well known that in both kingdoms, in a general way, the young stages of the more perfect types are represented or imitated with more or less exactitude by the adults of inferior ones. But a true identity of these adults with the various stages of the higher has, comparatively, rarely been observed. Let such a case be supposed.

In *A* we have four species whose growth attains a given point, a certain number of stages having been passed prior to its termination or maturity. In *B* we have another series of four (the numbering a matter of no importance), which, during the period of growth, cannot be distinguished by any common, *i.e.*, generic character, from the individuals of group *A*, but whose growth has only attained to a point short of that reached by those of group *A* at maturity. Here we have a parallelism, but no true evidence of descent. But if we now find a set of individuals belonging to one species, and therefore held to have had a common origin or parentage (or still better the individuals of a single brood), which present differences among themselves of the character in question, we have gained a point. We know in this case that the individuals *a*, have attained to the completeness of character presented by group *A*, while others, *b*, of the same parentage, have only attained to the structure of those of group *B*. It is perfectly obvious that the individuals of the first part of the family have grown further, and, therefore, in one sense faster, than those of group *b*. If the parents were like the individuals of the more completely grown, the offspring which did not attain that completeness may be said to have been *retarded* in their development. If, on the other hand, the parents were like those less fully grown, then the offspring which have added something have been *accelerated* in their development.

I claim that a consideration of the uniformity of nature's processes, or inductive reasoning, requires me (however it may affect the minds of others) to believe that the groups of species whose individuals I have never found to vary, but which differ in the same point as those in which I have observed the above variations, are also derived from common parents, and the more advanced have been accelerated or the less advanced retarded, as the case may have been with regard to the parents.

This is not an imaginary case, but a true representation of many which have come under my observation. The developmental resemblances mentioned are universal in the animal and probably in the vegetable kingdoms, approaching the exactitude above depicted in proportion to the near structural similarity of the species considered.

II.—*On the Laws of Evolution.*

Wallace and Darwin have propounded as the cause of modification in descent their law of natural selection. This law has been epitomised by Spencer as the "preservation of the fittest." This neat expression no doubt covers the case, but it leaves the origin of the fittest entirely untouched. Darwin assumes a "tendency to variation" in nature, and it is plainly necessary to do this in order that materials for the exercise of a selection should exist. Darwin and Wallace's law is, then, only restrictive, directive, conservative, or destructive of something already created. Let us, then, seek for the originative laws by which these subjects are furnished—in other words, for the causes of the origin of the fittest.

The origin of new structures which distinguish one generation from those which have preceded it, I have stated to take place under the law of acceleration. As growth (creation) of parts usually ceases with maturity, it is entirely plain that the process of acceleration is limited to the period of infancy and youth in all animals. It is also plain that the question of growth is one of nutrition, or of the construction of organs and tissues out of protoplasm.

The construction of the animal types is restricted to two kinds of increase—the addition of identical segments and the addition of identical cells. The first is probably to be referred to the last, but the laws which give rise to it cannot be here explained. Certain it is that segmentation is not only produced by addition of identical parts, but also by subdivision of a homogeneous part. In reducing the vertebrate or most complex animal to its simplest expression, we find that all its specialised parts are but modifications of the segment, either simply or as sub-segments of compound but identical segments. Gegenbaur has pointed out that the most complex limb with hand or foot is constructed, first, of a single longitudinal series of identical segments, from each of which a similar segment diverges, the whole forming parallel series, not only in the oblique transverse, but generally in the longitudinal sense. Thus the limb of the Lepidosiren represents the simple type, that of the Ichthyosaurus a first modification. In the latter the first segment only (femur or humerus) is specialised, the other pieces being undistinguishable. In the Plesiosaurian paddle the separate parts are distinguished; the ulna and radius well marked, the carpal pieces hexagonal, the phalanges well marked, &c.

As regards the whole skeleton, the same position may be safely assumed. Though Huxley may reject Owen's theory of the vertebrate character of the segments of the cranium, because they are so very different from the segments in other parts of the column, the question rests entirely on the definition of a vertebra. If a vertebra be a segment of the skeleton, of course the skull is composed of vertebræ; if not, then the cranium may be said to be formed of "sclerotomes," or some other name may be used. Certain it is, however, that the parts of the segments of the cranium may be now more or less completely parallelised or homologised with each other, and that as we descend the scale of vertebrated animals, the resemblance of these segments to vertebræ increases, and the constituent segments of each become more similar. In the types where the greatest resemblance is seen, segmentation of either is incomplete, for they retain the original cartilaginous basis. Other animals which present cavities or parts of a solid support are still more easily reduced to a simple basis of segments, arranged either longitudinally (worm) or centrifugally (star-fish, &c.).

Each segment—and this term includes not only the parts of a complex whole, but parts always subdivided, as the jaw of a whale or the sac-body of a mollusc—is constructed, as is well known, by cell-division. In the growing fœtus the first cell divides its nucleus and then its whole outline, and this process repeated millions of times produces, according to the cell theory, all the tissues of the animal organism or their bases from first to last. That the ultimata or histological elements of all organs are produced originally by repetitive growth of simple, nucleated cells with various modifications of exactitude of repetition in the more complex, is taught by the cell theory. The formation of some of the tissues is as follows:—

First Change—Formation of simple nucleated cells from homogeneous protoplasm or the cytoblastema.

Second—Formation of new cells by division of body and nucleus of the old.

Third—Formation of tissues by accumulation of cells with or without addition of intercellular cytoblastema.

A. In connective tissue by slight alteration of cells and addition of cytoblastema.

B. In blood, by addition of fluid cytoblastema (fibrin) to free cells (lymph corpuscles), which in higher animals (vertebrates) develop into blood-corpuscles by loss of membrane, and by cell development of muscles.

* Abstract of paper by Prof. E. D. Cope, read at the Indianapolis meeting of the American Association for the Advancement of Science: reprinted from the *American Naturalist*.

C. In muscles by simple confluence of cells, end to end, and mingling of contents (Kölliker).

D. Of cartilage by formation of cells in cytoblast which break up, their contents being added to cytoblast; this occurring several times, the result being an extensive cytoblast with few and small cells (Vogt). The process is here an attempt at development with only partial success, the result being a tissue of small vitality.

Even in repair-nutrition recourse is had to the nucleated cell. For Cohnheim first shows that if the corner of a frog's eye be scarified, repair is immediately set on foot by the transportation thither of white or lymph or nucleated corpuscles from the neighbouring lymph heart. This he ascertained by introducing aniline dye into the latter. Repeated experiments have shown that this is the history in great part of the construction of new tissue in the adult man.

Now, it is well known that the circulating fluid of the fœtus contains for a period only these nucleated cells as corpuscles, and that the lower vertebrates have a greater proportion of these corpuscles than the higher, whence probably the greater facility for repair or reconstruction of lost limbs or parts enjoyed by them. The invertebrates possess only nucleated blood corpuscles.

What is the relation of cell division to the forces of nature, and to which of them as a cause is it to be referred, if to any? The animal organism transfers the chemism of the food (protoplasm) to correlated amounts of heat, motion, electricity, light (phosphorescence), and nerve force. But cell division is an affection of protoplasm distinct from any of these. Addition to homogeneous lumps or parts of protoplasm (as in that lowest animal, *Protamœba* of Haeckel) may be an exhibition of mere molecular force, or addition as is seen in the crystal, but cell division is certainly something distinct. It looks to me like an exhibition of another force, and though this is still an open question, it may be called for the present *growth force*. It is correlated to the other forces, for its exhibitions cease unless the protoplasm exhibiting it be fed. It is potential in the protoplasm of both protoplasmic animal mass and protoplasmic food, and becomes energetic on the union of the two. So long as cell-division continues it is energetic; when cells burst and discharge the contained cytoblastema, as in the formation of cartilage, it becomes again potential.

The size of a part is then dependent on the amount of cell division or growth force, which has given it origin, and the number of segments is due to the same cause. The whole question, then, of the creation of animal and vegetable types is reduced to one of the amount and location of growth force.

Before discussing the influences which have increased and located growth force, it will be necessary to point out the mode in which these influences must necessarily have affected growth. Acceleration is only possible during the period of growth in animals, and during that time most of them are removed from the influence of physical or biological causes either through their hidden lives or incapacity for the energetic performance of life functions. These influences must, then, have operated on the parents, been rendered potential in their reproductive cells, and become energetic in the growing fœtus of the next generation. However little we may understand this mysterious process, it is nevertheless a fact. Says Murphy, "There is no act which may not become habitual, and there is no habit which may not be inherited." Materialised, this may be rendered—there is no act which does not direct growth force, and therefore there is no determination of growth force which may not become habitual; there is, then, no habitual determination of growth force which may not be inherited; and of course in a growing fœtus becomes at once energetic in the production of new structure in the direction inherited, which is acceleration.

III.—*The Influences Directing Growth Force.*

Up to this point we have followed paths more or less distinctly traced in the field of nature. The positions taken appear to me either to have been demonstrated or to have a great balance of probability in their favour. In the closing part of these remarks I shall indulge in more of hypothesis than heretofore.

What are the influences locating growth force? First, physical and chemical causes; second, use; third, effort. I leave the first, as not especially prominent in the economy of type growth among animals, and confine myself to the two following. The effects of use are well known. We cannot use a muscle without increasing its bulk; we cannot use the teeth in mastication without inducing a renewed deposit of dentine within the pulp-cavity to meet the encroachments of attrition. The hands of the labourer are always larger than those of men of other pursuits. Pathology furnishes us with a host of hypertrophies, exostoses, &c., produced by excessive use, or necessity for increased means of performing excessive work. The tendency, then, induced by use by the parent is to add segments or cells to the organ used. Use thus determines the locality of new repetitions of parts already existing, and determines an increase of growth force at the same time, by the increase of food always accompanying increase of work done, in every animal.

But supposing there be no part or organ to use. Such must have been the condition of every animal prior to the appearance of an additional digit or limb or other useful element. It appears to me that the cause of the determination of growth force is not merely the irritation of the part or organ used by contact with the objects of its use. This would seem to be the remote cause of the deposit of dentine used in the tooth, in the thickening epidermis of the hand of the labourer, in the wandering of the lymph-cell to the scarified cornea of the frog in Cohnheim's experiment. You cannot rub the sclerotica of the eye without producing an expansion of the capillary arteries and corresponding increase in the amount of nutritive fluid. But the case may be different in the muscles and other organs (as the pigment cells of reptiles and fishes) which are under the control of the volition of the animal. Here, and in many other instances which might be cited, it cannot be asserted that the nutrition of use is not under the direct control of the will through the mediation of nerve force. Therefore I am disposed to believe that growth force may be, by the volition of the animal, as readily determined to a locality where an executive organ does not exist, as to the first segment or cell of such an organ already commenced, and that therefore effort is in the order of time the first factor in acceleration.

Effort and use have, however, very various stimuli to their exertion.

Use of a part by an animal is either compulsory or optional. In either case the use may be followed by an increase of nutrition under the influence of reflex force or of direct volition.

A compulsory use would naturally occur in new situations which take place apart from the control of the animal, where no alternatives are presented. Such a case would arise in a submergence of land where land animals might be imprisoned on an island or in swamps surrounded by water, and compelled to assume a more or less aquatic life. Another case which has also probably often occurred, would be when the enemies of a species might so increase as to compel a large number of the latter to combat who would previously have escaped it.

In these cases the structure produced would be necessarily adaptive. But the effect would be most frequently to destroy or injure the animals (retard them) thus brought into new situations and compelled to an additional struggle for existence, as has, no doubt, been the case in geologic history. Preservation, with modifications, would only ensue where the changes should be introduced very gradually. This mode is always a consequence of the optional use. The cases here included are those where choice selects from several alternatives, thus exercising its influence on structure. Choice will be influenced by the emotions, the imagination, and by intelligence.

As examples of intelligent selection the modified organisms of the varieties of bees and ants must be regarded as striking examples of its exercise. Had all in the hive or hill been modified alike, as soldiers, queens, &c, the origin of the structures might have been thought to be compulsory; but varied and adapted as the different forms are to the wants of a community, the influence of intelligence is too obvious to be denied. The structural results are obtained in this case by a shorter road than by inheritance.

The selection of food offers an opportunity for the exercise of intelligence, and the adoption of means for obtaining it still greater ones. It is here that intelligent selection proves its supremacy as a guide of use, and consequently of structure, to all the other agencies here proposed. The preference for vegetable or for animal food determined by the choice of individual animals among the omnivores, which were, no doubt, according to the palæontological record the predecessors of our herbivores, and perhaps of carnivores also, must have determined their course of life, and thus of all their parts into those totally distinct directions. The choice of food under ground, on the ground, or in the trees would necessarily direct the uses of organs in those directions respectively.

Intelligence is a conservative principle, and always will direct effort and use into lines which will be beneficial to its possessor. Thus we have the source of the fittest—*i.e.*, addition of parts by increase and location of growth force directed by the will—the will being under the influence of various kinds of compulsory choice in the lower, and intelligent option among higher animals. Thus intelligent choice may be regarded as the originator of the fittest, while natural selection is the tribunal to which all the results of accelerated growth are submitted. This preserves or destroys them, and determines the new points of departure on which accelerated growth shall build.

Acceleration under the influence of effort accounts for the existence of rudimental characters. Many other characters will follow at a distance, the modifications proceeding in accordance with the laws here proposed, and retardation is accounted for by complementary or absolute loss of growth force.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 18.—"Investigations of the Currents in the Strait of Gibraltar, made in August 1871," by Captain G. S. Nares, R.N., of H.M.S. *Shearwater*, under instructions from Admiral Richards, F.R.S., Hydrographer of the Admiralty.

Geological Society, Jan. 10.—Mr. Joseph Prestwich, F.R.S., president, in the chair. "On Cyclostigma, Lepidodendron, and Knorria from Kiltorkan." By Prof. Oswald Heer. In this paper the author indicated the characters of certain fossils from the yellow sandstone of the South of Ireland, referred by him to the above genera, and mentioned in his paper "On the Carboniferous Flora of Bear Island," read before the Society on November 9, 1870 (see Q. J. G. S. vol. xxvii. p. 1). He distinguished as species *Cyclostigma kiltorkense*, Haught., *C. minutum* (Haught.), *Knorria acicularis*, Göpp. var. *Bailyana*, and *Lepidodendron Veltheimianum*, Sternb.—Mr. Carruthers was glad that he had made the observations which he did on Prof. Heer's former paper, as it had caused the Professor to give the reasons on which his opinions were based. He was doubtful whether the success which had attended Prof. Heer's determination of species from leaves justified the application of the same principles to mere stems. He could not accept the difference in size or distance of leaf-scars as a criterion of species, inasmuch as they were merely the result of the difference in the age and size of the parts of the plants on which they were observed. Even Prof. Heer himself had united together specimens presenting greater differences in this respect than those which he distinguished. He considered *Cyclostigma kiltorkense*, *C. minutum*, and *Lepidodendron Veltheimianum* to be founded on different parts of one species. In the Kiltorkan fossils the outer surface of the original stems was often broken up into small fragments, the phyllotaxy on which proved them to be portions of large stems, and not entire branches. As to *Knorria*, it was certainly the interior cast of the stem of *Lepidodendron*, with casts of the channels through which the vascular bundles passed with some cellular tissue to the leaves; and the specimen figured showed that it belonged to a branch similar to that represented as *C. minutum*. He considered that the four supposed species belonging to three genera were only different forms of the same plant.—"Notes on the Geology of the Plain of Marocco, and the Great Atlas." By Mr. George Maw. The author described first the characters presented by the coast of Marocco, and then the phenomena observed by him in his progress into the interior of the country and in the Atlas Chain. The oldest rocks observed were ranges of metamorphic rocks bounding the plain of Marocco, interbedded porphyrites and the porphyritic tuffs forming the backbone of the Atlas Chain, and the Mica-schists of Djeb Tezah in the Atlas. At many points in the lateral valleys of the Atlas almost vertical grey shales were crossed; the age of these was unknown. Above these comes a Red Sandstone and Limestone series, believed to be of Cretaceous age, and beds possibly of Miocene age, which occupied the valleys of the Atlas and covered the plain of Marocco, where vestiges of them remain in the form of tabular hills. The probable age of these beds was determined on the evidence of fossils. The author noticed the sequence of denuding and eruptive phenomena by which the arrangement and distribution of these rocks has been modified, and described the more recent changes resulting in the formation of enormous boulder beds flanking the northern escarpment of the Atlas plateau, and of great moraines at the heads of the valleys of the Atlas, both of which he ascribed to glacial action. An elevation of the coast line of at least seventy feet was indicated by raised beaches of concrete sand at Mogador and elsewhere, and the author considered that a slight subsidence of the coast was now taking place. The surface of the plain of Marocco was described as covered with a tufaceous crust, probably due to the drawing up of water to the surface from the subjacent calcareous strata and the deposition from it of laminated carbonate of lime. Mr. Ball, as an Alpine traveller who had also visited the Atlas in company with Dr. Hooker and Mr. Maw, offered a few remarks. The plain of Marocco was not, in his opinion, a level, but an inclined plane, rising gradually in height up to the foot of the mountain, so that the base of the boulder ridges was at some height above the level of the plain near Marocco. He did not think that the boulder deposits could be safely attributed to glaciers, but thought rather that they had been carried into and deposited in a shallow sea. He thought also that Mr. Maw had somewhat over-estimated the thickness of some of the boulder deposits; and though there was one instance of an undoubted moraine in one of the higher valleys of the Atlas, yet he could not agree in the view that the glaciation of the Atlas was general. He could not accept such a great thickness of beds as that represented by the vertical shales in Mr. Maw's section. Prof. Ramsay was pleased that the author, though giving so many interesting details, had not assigned any definite age to many of the beds. He agreed with him as to the cause assigned for the great tufaceous coating of the country. He had already assigned the same cause for the existence of certain saline beds, and would attribute the existence of the great coating of gypsum at slight depth below the surface of the Sahara to the same cause. As to the existence of moraines, he was not surprised to find them in the Atlas, as they were already known in the mountains of Granada. As to the escarpments, it was now well known that, as a rule, they assumed a direction approximately at right angles to the dip of the strata; and he felt inclined to consider that the bulk of the mounds at the foot of the escarpment of the Atlas were rather the remains of a long series of landslips from the face of the cliffs than to an accumulation of moraine matter. Mr. D. Forbes commented on the similarity of the rocks to those of the Andes in South America. In the Andes the porphyritic tuffs appeared to belong to the Oolitic age; and the igneous rocks associated with them were of the same date. He thought that, so far as the author's observations had gone, the structure of the Atlas was much the same as that of the Andes. Mr. W. W. Smyth mentioned that in the district to the east of the Sierra Nevada, in the south part of Spain, where there was great summer heat, and also heavy occasional rainfall, the same tufaceous coating as that observed in Marocco was to be found. He had been led to much the same conclusion as to its origin as that arrived at by Mr. Maw. The upper part was frequently brecciated, and the fragments re-cemented by carbonate of lime. Mr. Seeley, though accepting Mr. Etheridge's determination as to the Cretaceous age of the fossils if found in England, could not accept it as conclusive in the case of fossils from Marocco. The genius *Exogyra*, for instance, which ranges through the Secondary to existing seas, might well belong to some other age; and even the fossils presumably Miocene might, after all, date from some other period. Mr. Maw, in reply, stated that he agreed with Mr. Ball as to the rise in the Marocco plain as it approached the Atlas, having taken it in one direction at 400 feet in 25 miles. He pointed out the resemblance between the moraines in the valley of the Rhone and those which he regarded as such on the flanks of the Atlas. As a proof of their consisting of transported blocks, he mentioned the fact that the Red Sandstone rock of which they were composed did not occur in the adjacent escarpments, but was not to be found within seven or eight miles. There was, moreover, a mixture of different materials in the mounds.

Linnean Society, January 18.—Mr. Bentham, president, in the chair. "On the Anatomy of *Limulus polyphemus*," by Prof. Owen (continued). The author resumed and concluded the reading of this memoir. The nervous system of *Limulus* appeared to have occupied most attention, and was described in detail. From the fore part of the œsophageal ring, answering to the brain, were sent off the "ocellar," "ocular," "antennular," and "antennal" nerves; the latter supplying the second pair of articulate limbs—the homologues of the "external antennæ" of higher *Crustacea*. From the post- or sub-œsophageal part of the ring, proceeded large nerves to the four succeeding pairs of limbs; and also smaller nerves, having distinct origins,