Natural History.

ZOOLOGY.

ORIGIN OF SPECIES.

It was to the section D. (Zoology and Botany, including Physiology, a sub-section attached for the last-named subject) that the chief interest attached at the late meeting of the British Association, in consequence of the popularity at the present moment of discussion as to the Origin of Species. After a Report by Dr. Ogilvie, intimating the little that had been done, in consequence of the tempestuous weather and the early meeting of the Association, by the Dredging Committee for the North and East Coasts of Scotland, and a very interesting communication by the Rev. P. P. Carpenter, on the Progress of Natural Science in the United States and Canada, Dr. Daubeny led off in the great question of the day, by a paper on the Final Causes of the Sexuality of Plants, with particular reference to Mr. Darwin's work on the *Origin of Species by Natural Selection.*

Dr. Daubeny began by pointing out the identity between the two modes by which the multiplication of plants is brought about, the very same properties being imparted to the bud or to the graft as to the seed produced by the ordinary process of fecundation, and a new individual being in either instance equally produced. We are therefore led to speculate as to the final cause of the existence of sexual organs in plants, as well as in those lower animals which can be propagated by cuttings. One use, no doubt, may be the dissemination of the species; for many plants, if propagated by buds alone, would be in a manner confined to a single spot. Another secondary use is the production of fruits which afford nourishment to animals. A third may be to minister to the gratification of the senses of man by the beauty of their forms and colours. But as these ends are only answered in a small proportion of cases, we must seek further for the uses of the organs in question; and hence the author suggested that they might have been provided in order to prevent that uniformity in the aspect of Nature which would have prevailed if plants had been multiplied exclusively by buds. It is well known that a bud is a mere counterpart of the stock from whence it springs, so that we are always sure of obtaining the very same description of fruit by merely grafting a bud or cutting of a pear or apple tree upon another plant of the same species. On the other hand, the seed never produces an individual exactly like the plant from which it sprang; and hence, by the union of the sexes in plants, some variation from the primitive type is sure to result. Dr. Daubeny remarked, that if we adopt in any degree the views of Mr. Darwin, with respect to the origin of species by natural selection, the creation of sexual organs in plants might be regarded as intended to promote this specific object. Whilst, however, he gave his assent to the Darwinian hypothesis, as likely to aid us in reducing the number of existing species, he

wished not to be considered as advocating it to the extent to which the author seems disposed to carry it. He rather desired to recommend to naturalists the necessity of further inquiries, in order to fix the limits within which the doctrine proposed by Mr. Darwin may assist us in distinguishing varieties from species.

Professor Huxley deprecated any discussion on the general question of the truth of Mr. Darwin's theory. He felt that a general audience, in which sentiment would unduly interfere with intellect, was not the public before which such a discussion should be carried on. Dr. Daubeny had brought forth nothing new to demand or require remark. Mr. R. Dowden, of Cork, mentioned, first, two instances in which plants had been disseminated by seeds, which could not be effected by buds, first, in the introduction of *Senicio squalida,* by the late Rev. W. Hincks ; and, second, in the diffusion of chicory, in the vicinity of Cork, by the agency of its winged seeds. He related several anecdotes of a monkey, to show that however highly organized the Quadrumana might be, they were very inferior in intellectual qualities to the dog, the elephant, and other animals. He particularly referred to his monkey being fond of playing with a hammer; but although he liked oysters as food, he never could teach him to break the oysters with his hammer as a means of indulging his appetite. Dr. Wright stated that a friend of his, who had gone out to report on the habits of the gorilla—the highest form of monkey —had observed that the female gorilla took its young to the seashore for the purpose of feeding them on oysters, which they broke with great facility.

Professor Owen said that he wished to approach this subject in the spirit of the philosopher, and expressed his conviction that there were facts by which the public could come to some conclusion with regard to the probabilities of the truth of Mr. Darwin's theory. Whilst giving all praise to Mr. Darwin for the courage with which he had put forth his theory, he felt it must be tested by facts. As a contribution to the facts, by which the theory must be tested, he would refer to the structure of the highest Quadrumana as compared with man. Taking the brain of the gorilla, it presented more differences, as compared with the brain of man, than it did when compared with the brains of the very lowest and most problematical form of the Quadrumana. The differences in cerebral structure between the gorilla and man were immense. The posterior lobes of the cerebrum in man presented parts which were wholly absent in the gorilla. The same remarkable differences of structure were seen in other parts of the body; yet he would especially refer to the structure of the great toe in man, which was constructed to enable him to assume the upright position; whilst in the lower monkeys it was impossible, from the structure of their feet, that they should do so. He concluded by urging on the physiologist the necessity of experiment. The chemist, when in doubt, decided his questions by experiments; and this was what is needed by the physiologist. Professor Huxley begged to be permitted to reply to Professor Owen. He denied altogether that the difference between the brain of the gorilla and

man was so great as represented by Professor Owen, and appealed to the published dissections of Tiedemann and others. From the study of the structure of the brain of the Quadrumana, he maintained that the difference between man and the highest monkey was not so great as between the highest and the lowest monkey. He maintained also, with regard to the limbs, that there was more difference between the toeless monkeys and the gorilla than between the latter and man. He believed that the great feature which distinguished man from the monkey was the gift of speech.

This subject was resumed another day by a paper "on the Intellectual Development of Europe, considered with Reference to the Views of Mr. Darwin and others, that the Progression of Organisms is determined by Law," by Professor Draper, M.D., of New York. The object of this paper was to show that the advancement of man in civilization does not occur accidentally or in a fortuitous manner, but is determined by immutable law. The author introduced his subject by recalling proofs of the dominion of law in the three great lines of the manifestation of life. First, in the successive stages of development of every individual, from the earliest rudiment to maturity; secondly, in the numberless organic forms now living contemporaneously with us, and constituting the animal series; thirdly, in the orderly appearance of that grand succession which in the slow lapse of geological time has emerged, constituting the life of the earth, showing therefrom not only the evidences, but also proofs of the dominion of law over the world of life. In those three lines of life he established that the general principle is, to differentiate instinct from automatism, and then to differentiate intelligence from instinct. In man himself three distinct instrumental nervous mechanisms exist, and three distinct modes of life are perceptible, the automatic, the instinctive, the intelligent. They occur in an epochal order, from infancy through childhood to the more perfect state. Such holding good for the individual, it was then affirmed that it is physiologically impossible to separate the individual from the race, and that what holds good for the one holds good for the other too; and hence that man is the archetype of society, and individual development the model of social progress, and that both are under the control of immutable law: that a parallel exists between individual and national life in this, that the production, life, and death of an organic particle in the person, answers to the production, life, and death of a person in the nation.

Turning from these purely physiological considerations to historical proof, and selecting the only European nation which thus far has offered a complete and completed intellectual life, Professor Draper showed that the characteristics of Greek mental development answer perfectly to those of individual life, presenting philosophically five well-marked ages or periods,—the first being closed by the opening of Egypt to the Ionians; the second, including the Ionian, Pythagorean, and Eleatic philosophies, was ended by the criticisms of the Sophists; the third, embracing the Socratic and Platonic philosophies, was ended by the doubts of the Sceptics; the fourth,

ushered in by the Macedonian expedition, and adorned by the splendid achievements of the Alexandrian school, degenerated into Neoplatonism and imbecility in the fifth, to which the hand of Rome put an end. From the solutions of the four great problems of Greek philosophy, given in each of these five stages of its life, he showed that it is possible to determine the law of the variation of Greek opinion, and to establish its analogy with that of the variations of opinion in individual life. Next, passing to the consideration of Europe in the aggregate, Professor Draper showed that it has already in part repeated these phases in its intellectual life. Its first period closes with the spread of the power of Republican Rome, the second with the foundation of Constantinople, the third with the Turkish invasion of Europe; we are living in the fourth. Detailed proofs of the correspondence of these periods to those of Greek life, and through them to those of individual life, are given in a work now printing on this subject, by the author, in America. Having established this conclusion, Professor Draper next briefly alluded to many collateral problems or inquiries. He showed that the advances of men are due to external and not to interior influences, and that in this respect a nation is like a seed, which can only develop when the conditions are favourable, and then only in a definite way; that the time for psychical change corresponds with that for physical, and that a nation cannot advance except its material condition be touched,—this having been the case throughout all Europe, as is manifested by the diminution of the blue-eyed races thereof; that all organisms, and even man, are dependent for their characteristics, continuance, and life, on the physical conditions under which they live; that the existing apparent invariability presented by the world of organization is the direct consequence of the physical equilibrium; but that if that should suffer modification, in an instant the fanciful doctrine of the immutability of species would be brought to its proper value. The organic world appears to be in repose because natural influences have reached an equilibrium. A marble may remain motionless for ever on a level table, but let the table be a little inclined, and the marble will quickly run off; and so it is with organisms in the world. From his work on Physiology, published in 1856, he gave his views in support of the doctrine of the transmutation of species; the transitional forms of the animal to the human type; the production of new ethnical elements, or nations; and the laws of their origin, duration, and death.

The announcement of this paper attracted an immense audience to the section, which met in the library of the New Museum. The discussion was commenced by the Rev. Mr. Cresswell, who denied that any parallel could be drawn between the intellectual progress of man and the physical development of the lower animals. So far from Professor Draper being correct with regard to the history of Greece, its masterpieces in literature—the *Iliad* and *Odyssey*—were produced during its national infancy. The theory of intellectual development proposed was directly opposed to the known facts of the history of man. Sir B. Brodie stated he could not subscribe to the hypothesis of

Mr. Darwin. His primordial germ had not been demonstrated to have existed. Man had a power of self-consciousness—a principle differing from anything found in the material world—and he did not see how this could originate in lower organisms. This power of man was identical with the Divine Intelligence; and to suppose that this could originate with matter, involved the absurdity of supposing the source of Divine power dependent on the arrangement of matter. The Bishop of Oxford stated that the Darwinian theory, when tried by the principles of inductive science, broke down. The facts brought forward did not warrant the theory. The permanence of specific forms was a fact confirmed by all observation. The remains of animals, plants, and man, found in those earliest records of the human race, the Egyptian catacombs, all spoke of their identity with existing forms, and of the irresistible tendency of organized beings to assume an unalterable character. The line between man and the lower animals was distinct; there was no tendency on the part of the lower animals to become the self-conscious intelligent being, man; or in man to degenerate and lose the high characteristics of his mind and intelligence. All experiments had failed to show any tendency in one animal to assume the form of the other. In the great case of the pigeons, quoted by Mr. Darwin, he admitted that no sooner were these animals set free than they returned to their primitive type. Everywhere sterility attended hybridism, as was seen in the closely allied forms of the horse and the ass. Mr. Darwin's conclusions were an hypothesis, raised most unphilosophically to the dignity of a causal theory. He was glad to know that the greatest names in science were opposed to this theory, which he believed to be opposed to the interests of science and humanity. Professor Huxley defended Mr. Darwin's theory from the charge of its being merely an hypothesis. He said it was an explanation of phenomena in Natural History, as the undulating theory was of the phenomena of light. No one objected to that theory because an undulation of light had never been arrested and measured. Darwin's theory was an explanation of facts; and his book was full of new facts, all bearing on his theory. Without asserting that every part of the theory had been confirmed, he maintained that it was the best explanation of the origin of species which had yet been offered. With regard to the psychological distinction between man and animals, man himself was once a monad—a mere atom; and nobody could say at what moment in the history of his development he became consciously intelligent. The question was not so much one of a transmutation or transition of species, as of the production of forms which became permanent. Thus the short-legged sheep of America were not produced gradually, but originated in the birth of an original parent of the whole stock, which had been kept up by a rigid system of artificial selection.

Admiral Fitzroy regretted the publication of Mr. Darwin's book, and denied Professor Huxley's statement, that it was a logical arrangement of facts.

Dr. Beale pointed out some of the difficulties with which the

Darwinian theory had to deal, more especially those vital tendencies of allied species which seemed independent of all external agents. Mr. Lubbock expressed his willingness to accept the Darwinian hypothesis in the absence of any better. He would, however, express his conviction, that time was not an essential element in these changes. Time alone produced no change.

Dr. Hooker being called upon by the President to state his views of the botanical aspect of the question, observed that the Bishop of Oxford having asserted that all men of science were hostile to Mr. Darwin's hypothesis, whereas he himself was favourable to it, he could not presume to address the audience as a scientific authority. As, however, he had been asked for his opinion, he would briefly give it. In the first place, his lordship, in his eloquent address, had, as it appeared to him, completely misunderstood Mr. Darwin's hypothesis. His lordship intimated that this maintained the doctrine of the transmutation of existing species one into another, and had confounded this with that of the successive development of species by variation and natural selection. The first of these doctrines was so wholly opposed to the facts, reasonings, and results of Mr. Darwin's work, that he could not conceive how any one who had read it could make such a mistake—the whole book, indeed, being a protest against that doctrine. Then, again, with regard to the general phenomena of species, he understood his lordship to affirm that these did not present characters that should lead careful and philosophical naturalists to favour Mr. Darwin's views. To this assertion Dr. Hooker's experience of the vegetable kingdom was diametrically opposed. He considered that at least one-half of the known kinds of plants were disposable in groups, of which the species were connected by varying characters common to all in that group, and sensibly differing in some individuals only of each species; so much so, that if each group be likened to a cobweb, and one species be supposed to stand in the centre of that web, its varying characters might be compared to the radiating and concentric threads, when the other species would be represented by the points of union of these; in short, that the general characteristics of orders, genera, and species amongst plants differed in degrees only from those of varieties, and afforded the strongest countenance to Mr. Darwin's hypothesis. As regarded his own acceptation of Mr. Darwin's views, he expressly disavowed having adopted them as a creed. He knew no creeds in scientific matters. He had early begun the study of natural science under the idea that species were original creations; and it should be steadily kept in view that this was merely another hypothesis, which in the abstract was neither more nor less entitled to acceptance than Mr. Darwin's; neither was it, in the present state of science, capable of demonstration, and each must be tested by its power of explaining the mutual dependence of the phenomena of life. For many years he had held to the old hypothesis, having no better established one to adopt; though the progress of botany had in the interim developed no new facts that favoured it, but a host of most suggestive objections to it. On the

other hand, having fifteen years ago been privately made acquainted with Mr. Darwin's views, he had during that period applied these to botanical investigations of all kinds in the most distant parts of the globe, as well as to the study of the largest and most different floras at home. Now, then, that Mr. Darwin had published it, he had no hesitation in publicly adopting his hypothesis, as that which offers by far the most probable explanation of all the phenomena prescribed by the classification, distribution, structure, and development of plants in a state of nature and under cultivation; he should therefore continue to use this hypothesis as the best weapon for future research, holding himself ready to lay it down should a better be forthcoming, or should the now abandoned doctrine of original creations regain all it had lost in his experience.

The subject has been discussed with kindred interest in America.

The American Academy of Arts and Sciences reports the following summary of the argument of Professor Asa Gray, the distinguished botanist, who criticised in detail several of the positions taken at the preceding meeting by Mr. Lowell, Professor Bowen, and Professor Agassiz respectively; premising that he had no doubt that variation and natural selection would have to be admitted as operative in nature, but were probably inadequate to the work which they had been put to. Professor Gray maintained—

1. That varieties abundantly occur in nature, at least among plants, and that very few of them can be of hybrid origin; that hybridation gives rise to no new features, but only mingles, and, if continued, blends the characters of sorts before separate; and that a hybrid origin was entirely out of the question in species which had no congeners, or none in the country to which they were indigenous; yet that such species diverged into varieties as readily as any other. As to the general denial, 1, that there is any such thing as natural selection, and 2, that there is any variation in species for natural selection to act upon, he could not yet conceive how such denial was to be supported; but to answer its purpose it would have to be carried to the length of denying that the individuals of a species ever have anything which they did not inherit;—slight variations, accumulated by inheritance, being just what the theory in question made use of,—taking little or no account of more salient and abrupt variations, though instances of the latter kind could certainly be adduced.

2. In opposition to the view that such variations as cultivation or domestication so copiously affords are of no account in the discussion, and have no counterpart in nature, Professor Gray maintained, that the varieties of cultivation afforded direct evidence of the essential variability of species; that no domesticated plant had refused to vary; that those of recent introduction, such as Californian annuals, mostly began to sport very promptly, sometimes even in the first or second generation; man having done nothing more than to sow the seed here instead of in California, perhaps in no better soil. Here the variations were as natural as those of the wild plant in its native soil. Man produces no organic variation, but merely directs a power which he did not originate, and by selection and close breeding preserves the incipient variety which else would probably be lost, and gives it a choice opportunity to vary more. Consider, he remarked, how small the chance of the survival of any variety when originated in its native habitat, surrounded by its fellows,—when not one seed out of a hundred or a thousand ever comes to germinate, and not a moiety of these ever succeed in becoming a plant,—and when, of those that do grow up and blossom, the danger is imminent that the flowers may be fertilized by the pollen of some of its abundant neighbours of the unvaried type,—and it will be easy to understand why plants vary so promptly in our gardens, mostly raised from a small quantity of seeds to begin with, probably all from the same stock, where they are almost

sure to self-fertilize in the first generation,—where every desirable variation is watched for and cared for, and kept separate; and it may be confidently inferred that they vary in cultivation, at first, much as they would have varied in the wild state, if such favourable opportunity had there occurred. Continued cultivation under artificial selection would of course force some of these results to an extreme never reached in nature, giving to long-cultivated varieties a character of their own. Yet they may not deviate more widely from the wild type than do some of the wild varieties of many plants of wide geographical range. Moreover, Professor Gray maintained that there occur in nature the same kinds of variation as those to which we owe our improved fruits, &c.; that such originate not rarely in nature, and develope to a certain extent, enough to show the same cause operating in free as in controlled nature; enough to have shown the cultivator what he should take in hand; enough to render it likely that most of our cultivated species of fruit began their career of improvement before man took them in hand. Instances of such variations in the wild state were adduced from our hawthorns, especially *Cratægus tomentosa*, from our Wild Red Plum, Wild Cherries, and especially from our Wild Grapes and Hickories.

3. The view taken by Mr. Lowell, and especially by Professor Bowen, that the indefinitely long periods of time which the theory acquired and assumed was practically equivalent to infinity, and therefore rendered the theory "completely metaphysical in character," Professor Gray animadverted upon, mainly to remark that the theory in question would generally be regarded as too materialistic and physical, rather than too metaphysical in character; and that *à fortiori*, physical geology and physical astronomy would on the principle be metaphysical sciences.

4. Exceptions were taken against the assumption of such a wide distinction, or of any sharply drawn distinction at their confines, between the animal and the vegetable kingdoms, and especially against the view that instinct sharply defines the animal kingdom from the vegetable kingdom on the one hand, and from man on the other, and which denies to the higher brutes intelligence, and to man instinct.

5. Also, against the view that the psychical endowments of the brute animals, whether instinct or other, are invariable and unimproveable; and a variety of instances were adduced, as recorded in the works of Pritchard and of Isidore St. Hilaire, as well as some from personal observation, in which acquired habitudes or varied instincts were transmitted from the parents to their offspring. That such acquirements, once inherited, would be likely to continue heritable, was argued to be the natural consequence of the general law of inheritance, the most fundamental law in physiology; that it is actually so, Professor Gray insisted was well known to every breeder of domestic animals.

6. For decisive instances of the perpetuity by descent or fixity, under interbreeding, of altered structure, Professor Gray adduced Manx cats and Dorking fowls; and he alluded to well-known cases of six-digited people, and the like, transmitting the peculiarity to more than half of their children, and even grandchildren; showing that the salient peculiarity tended to be more transmissible than the normal state at the outset; so that, by breeding in and in, it was likely that *hexadactyles* could soon be made to come as true to the breed as Dorkings.

7. As to the charge that the theory in question denies permanence of type, Professor Gray remarked that, on the contrary, the theory not only admitted persistence of type, as the term is understood by all naturalists, but was actually built upon this admitted fact as one of its main foundations; that, indeed, one of the prominent advantages of this very theory was, that it accounted for this long persistence of type, which upon every other theory remained scientifically unaccounted for.

8. Finally, as to the charge that the hypothesis in question repudiated design or purpose in nature and the whole doctrine of final causes, Professor Gray urged:—1. That to maintain that a theory of the derivation of one species or sort of animal from another through secondary causes and natural agencies negatived design, seemed to concede that whatever in nature is accomplished through secondary causes is so much removed from the sphere of design, or that only that which is supernatural can be regarded or shown to be designed;—which no theist can admit. 2. That the establishment of this particular theory by scientific evidence would leave the doctrines of final cause, utility, special design, or whatever other teleological view, just where they were before its promulgation,

in all fundamental respects; that no new kind of difficulty comes in with this theory, *i.e.*, none with which the philosophical naturalist is not already familiar. It is merely the old problem as to how persistence of type and morphological conformity are to be reconciled with special design (with the advantage of offering the only scientific, though hypothetical, solution of the question), along with the wider philosophical question, as to what is the relation between orderly natural events and intelligent efficient cause, or Divine agency. In respect to which, we have only to adopt Professor Bowen's own philosophy of causation,—viz., "That the natural no less than the supernatural, the continuance no less than the creation of existence, the origin of an individual, as well as the origin of a species or a genus, can be explained only by the direct action of an intelligent cause,"—and all special difficulty in harmonizing a theory of the derivation of species with the doctrine of final causes will vanish.

At the Royal Institution, on Feb. 6, Professor Huxley (who at the Oxford meeting subsequently appeared as the champion of Mr. Darwin's theory) read a paper "On Species and Races, and their Origin." After some preliminary remarks, in the course of which the speaker expressed his obligation for the liberality with which Mr. Darwin had allowed him to have access to a large portion of the MSS. of his forthcoming work, the phenomena of species in general were considered—the horse being taken as a familiar example. The distinctions between this and other closely allied species, such as the asses and zebras, were considered, and they were shown to be of two kinds, structural or morphological, and functional or physiological. Under the former head were ranged the callosities on the inner side of the fore and hind limbs of the horse—its bushy tail, its peculiar larynx, its short ears, and broad hoofs: under the latter head, the fact that the offspring of the horse with any of the allied species is a hybrid, incapable of propagation with another mule, was particularly mentioned. Leaving open the question whether the physiological distinction just mentioned is, or is not, a universal character of species, it is indubitable that it obtains between many species, and therefore has to be accounted for by any theory of their origin. The species *Equus caballus*, thus separated from all others, is the centre round which a number of other remarkable phenomena are grouped. It is intimately allied in structure with three other members of the existing creation, the hyrax, the tapir, and the rhinoceros; and less strait, though still definite bonds of union connect it with every living thing. Going back in time, the horse can be traced into the Pliocene formation, and perhaps it existed earlier still; but in the newer Miocene of Germany it is replaced by the hippotherium, an animal very like a true equus, but having the two rudimental toes in each foot developed, though small. Further back in time, in the Eocene rocks, neither equus nor hippotherium has been met with, nor rhinoceros, tapirus, or hyrax; but instead of them, a singular animal, the palæotherium, which exhibits certain points of resemblance with each of the four existing genera, is found. The speaker pointed out that these resemblances did not justify us in considering the palæotherium as a more generalized type, any more than the resemblance of a father to his four sons justifies us in considering his as of a more generalized type than theirs. The geographical distribution of

the equidæ was next considered, and the anomalies and difficulties it offers were pointed out; and lastly the variations which horses offer in their feral and their domesticated condition, were discussed. The questions thus shown to be connected with the species horse, are offered by all species whatever; and the next point of the discourse was the consideration of the general character of the problem of the origin of species of which they form a part, and the necessary conditions of its solution. So far as the logic of the matter goes, it was proved that this problem is of exactly the same character as multitudes of other physical problems, such as the origin of glaciers, or the origin of strata of marble; and a complete solution of it involves—1. The experimental determination of the conditions under which bodies having the characters of species are producible; 2. The proof that such conditions are actually operative in nature. Any doctrine of the origin of species which satisfies these requirements must be regarded as a true theory of species; while any which does not is, so far, defective, and must be regarded only as a hypothesis whose value is greater or less according to its approximation to this standard.

It is Mr. Darwin's peculiar merit to have apprehended these logical necessities, and to have endeavoured to comply with them. The pigeons called pouters, tumblers, fantails, &c., which the audience had an opportunity of examining, are in his view the result of so many long-continued experiments on the manufacture of species; and he considers that causes essentially similar to those which have given rise to these birds are operative in nature now, and have in past times been the agents in producing all the species we know. If neither of these positions can be upset, Mr. Darwin's must be regarded as a true theory of species, as well based as any other physical theory; they require, therefore, the most careful and searching criticism. After pointing out the remarkable differences in structure and habits between the carrier, pouter, fantail, tumbler, and the wild *Columba livia*, the speaker expressed his entire agreement with Mr. Darwin's conclusion, that all the former domesticated breeds had arisen from the last-named wild stock; and on the following grounds—1. That all interbreed freely with one another. 2. That none of the domesticated breeds presents the slightest approximation to any wild species but *C. livia*, whose characteristic markings are at times exhibited by all. 3. That the known habits of the Indian variety of the rock pigeon (*C. intermedia*) render its domestication easily intelligible. 4. That existing varieties connect the extremest modifications of the domestic breeds by insensible links with *C. livia*. 5. That there is historical evidence of the divergence of existing breeds, *e.g.*, the tumbler, from forms less unlike *C. livia*.

Mr. Huxley then analysed the process of selection by which the domesticated breeds had been produced from the wild rock pigeon; and he showed its possibility to depend upon two laws which hold good for all species, viz., 1. That every species tends to vary; 2. That variations are capable of hereditary transmission. The second law is well understood; but the speaker adverted to the miscompre-

hension which appears to prevail regarding the first, and showed that the variation of a species is by no means an adaptation to conditions in the sense in which that phrase is commonly used. Pigeon-fanciers, in fact, subject their pigeons to a complete uniformity of conditions; but while the similarly used feet, legs, skull, sacral vertebræ, tail feathers, oil gland, and crop undergo the most extraordinary modifications; on the other hand the wings, whose use is hardly ever permitted to the choice breeds, have hitherto shown no sign of diminution. Man has not as yet been able to determine a variation; he only favours those which arise spontaneously, *i.e.*, are determined by unknown conditions. It must be admitted that, by selection, a species may be made to give rise experimentally to excessively different modifications; and the next question is, Do causes adequate to exert selection exist in nature? On this point, the speaker referred his audience to Mr. Darwin's chapter on the struggle for existence, as affording ample satisfactory proof that such adequate natural causes do exist. There can be no question that just as man cherishes the varieties he wishes to preserve, and destroys those he does not care about; so nature (even if we consider the physical world as a mere mechanism) must tend to cherish those varieties which are better fitted to work harmoniously with the conditions she offers, and to destroy the rest. There seems to be no doubt, then, that modifications equivalent in extent to the four breeds of pigeons, might be developed from a species by natural causes; and therefore, if it can be shown that these breeds have all the characters which are ever found in species, Mr. Darwin's case would be complete. However, there is as yet no proof that, by selection, modifications having the physiological character of species (*i.e.*, whose offspring are incapable of propagation *inter se*) have ever been produced from a common stock. No doubt the numerous indirect arguments brought forward by Mr. Darwin to weaken the force of this objection are of great weight; no doubt it cannot be proved that all species give rise to hybrids infertile *inter se*; no doubt (so far as the speaker's private conviction went), a well-conducted series of experiments very probably would yield us derivatives from a common stock, whose offspring should be infertile *inter se*; but we must deal with facts as they stand, and at present it must be admitted that Mr. Darwin's theory does not account for all the phenomena exhibited by species; and, so far, falls short of being a satisfactory theory.

RECURRENT ANIMAL FORM.

Dr. Collingwood has read to the British Association a paper "On Recurrent Animal Form, and its Significance in Systematic Zoology." The object of this communication was to call attention to the frequent recurrence of similar forms in widely separated groups of the animal kingdom, similarities, therefore, which were unaccompanied by homologies of internal structure. These analogies of form had greatly influenced the progress of classification, by attracting the attention of systematizers while as yet structural homologies