

ART. VIII.—DARWIN ON THE ORIGIN OF SPECIES.

On the Origin of Species, by means of Natural Selection; or the Preservation of favoured Races in the Struggle for Life.
By CHARLES DARWIN, M.A. London. 1860.

MR. DARWIN'S long-standing and well-earned scientific eminence probably renders him indifferent to that social notoriety which passes by the name of success; but if the calm spirit of the philosopher have not yet wholly superseded the ambition and the vanity of the carnal man within him, he must be well satisfied with the results of his venture in publishing the "Origin of Species." Overflowing the narrow bounds of purely scientific circles, the "species question" divides with Italy and the Volunteers the attention of general society. Everybody has read Mr. Darwin's book, or, at least, has given an opinion upon its merits or demerits; pietists, whether lay or ecclesiastic, decry it with the mild railing which sounds so charitable; bigots denounce it with ignorant invective; old ladies, of both sexes, consider it a decidedly dangerous book, and even savans, who have no better mud to throw, quote antiquated writers to show that its author is no better than an ape himself; while every philosophical thinker hails it as a veritable Whitworth gun in the armoury of liberalism, and all competent naturalists and physiologists, whatever their opinions as to the ultimate fate of the doctrines put forth, acknowledge that the work in which they are embodied is a solid contribution to knowledge and inaugurates a new epoch in natural history.

Nor has the discussion of the subject been restrained within the limits of conversation. When the public is eager and interested, reviewers must minister to its wants, and the genuine *littérateur* is too much in the habit of acquiring his knowledge from the book he judges—as the Abyssinian is said to provide himself with steaks from the ox which carries him—to be withheld from criticism of a profound scientific work by the mere want of the requisite preliminary scientific acquirement; while, on the other hand, the men of science who wish well to the new views, no less than those who dispute their validity, have naturally sought opportunities of expressing their opinions. Hence it is not surprising that almost all the critical journals have noticed Mr. Darwin's work at greater or less length, and so many disquisitions, of every degree of excellence, from the poor product of ignorance, too often stimulated by prejudice, to the fair and

thoughtful essay of the candid student of nature, have appeared, that it seems an almost hopeless task to attempt to say anything new upon the question.

But it may be doubted if the knowledge and acumen of prejudged scientific opponents, or the subtlety of orthodox special pleaders, have yet exerted their full force in mystifying the real issues of the great controversy which has been set afoot, and whose end is hardly likely to be seen by this generation; so that at this eleventh hour, and even failing anything new, it may be useful to state afresh that which is true, and to put the fundamental positions advocated by Mr. Darwin in such a form that they may be grasped by those whose special studies lie in other directions; and the adoption of this course may be the more advisable, because notwithstanding its great deserts, and indeed partly on account of them, the "Origin of Species" is by no means an easy book to read—if by reading is implied the full comprehension of an author's meaning.

We do not speak jestingly in saying that it is Mr. Darwin's misfortune to know more about the question he has taken up than any man living. Personally and practically exercised in zoology, in minute anatomy, in geology; a student of geographical distribution, not on maps and in museums only, but by long voyages and laborious collection; having largely advanced each of these branches of science, and having spent many years in gathering and sifting materials for his present work, the store of accurately registered facts upon which the author of the "Origin of Species" is able to draw at will is prodigious.

But this very superabundance of matter must have been embarrassing to a writer who, for the present, can only put forward an abstract of his views, and thence it arises, perhaps, that notwithstanding the clearness of the style, those who attempt fairly to digest the book find much of it a sort of intellectual pemmican—a mass of facts crushed and pounded into shape, rather than held together by the ordinary medium of an obvious logical bond: due attention will, without doubt, discover this bond, but it is often hard to find.

Again, from sheer want of room, much has to be taken for granted which might readily enough be proved, and hence, while the adept, who can supply the missing links in the evidence from his own knowledge, discovers fresh proof of the singular thoroughness with which all difficulties have been considered and all unjustifiable supposition avoided, at every reperusal of Mr. Darwin's pregnant paragraphs, the novice in biology is apt to complain of the frequency of what he fancies is gratuitous assumption.

Thus while it may be doubted if, for some years, any one is likely to be competent to pronounce judgment on all the issues

raised by Mr. Darwin, there is assuredly abundant room for him, who, assuming the humbler, though perhaps as useful, office of an interpreter between the "Origin of Species" and the public, contents himself with endeavouring to point out the nature of the problems which it discusses; to distinguish between the ascertained facts and the theoretical views which it contains; and finally, to show the extent to which the explanation it offers satisfies the requirements of scientific logic. At any rate, it is this office which we propose to undertake in the following pages.

It may be safely assumed that our readers have a general conception of the nature of the objects to which the word "species" is applied; but it has, perhaps, occurred to few, even of those who are naturalists *ex professo*, to reflect, that, as commonly employed, the term has a double sense and denotes two very different orders of relations. When we call a group of animals, or of plants, a species, we may imply thereby either, that all these animals or plants have some common peculiarity of form or structure; or, we may mean that they possess some common functional character. That part of biological science which deals with form and structure is called Morphology—that which concerns itself with function, Physiology—so that we may conveniently speak of these two senses or aspects of "species"—the one as morphological, the other as physiological. Regarded from the former point of view, a species is nothing more than a kind of animal or plant, which is distinctly definable from all others, by certain constant and not merely sexual, morphological peculiarities. Thus horses form a species, because the group of animals to which that name is applied is distinguished from all others in the world by the following constantly associated characters. They have 1. A vertebral column; 2. Mammæ; 3. A placental embryo; 4. Four legs; 5. A single well-developed toe in each foot provided with a hoof; 6. A bushy tail; and 7. Callosities on the inner sides of both the fore and the hind legs. The asses again, form a distinct species, because, with the same characters, as far as the fifth in the above list, all asses have tufted tails, and have callosities only on the inner side of the fore legs. If animals were discovered having the general characters of the horse, but sometimes with callosities only on the fore legs, and more or less tufted tails; or animals having the general characters of the ass, but with more or less bushy tails, and sometimes with callosities on both pairs of legs, besides being intermediate in other respects—the two species would have to be merged into one. They could no longer be regarded as morphologically distinct species, for they would not be distinctly definable one from the other.

However bare and simple this definition of species may appear

to be, we confidently appeal to all practical naturalists, whether zoologists, botanists, or paleontologists, to say if, in the vast majority of cases, they know, or mean to affirm, anything more of the group of animals or plants they so denominate than what has just been stated. Even the most decided advocates of the received doctrines respecting species admit this.

"I apprehend," says Professor Owen,* "that few naturalists now-a-days, in describing and proposing a name for what they call 'a new species,' use that term to signify what was meant by it twenty or thirty years ago, that is, an originally distinct creation, maintaining its primitive distinction by obstructive generative peculiarities. The proposer of the new species now intends to state no more than he actually knows; as for example, that the differences in which he finds the specific character are constant in individuals of both sexes, so far as observation has reached; and that they are not due to domestication or to artificially superinduced external circumstances, or to any outward influence within his cognizance; that the species is wild, or is such as it appears by nature."

If we consider, in fact, that by far the largest proportion of recorded existing species are known only by the study of their skins, or bones, or other lifeless exuvia; that we are acquainted with none, or next to none, of their physiological peculiarities, beyond those which can be deduced from their structure, or are open to cursory observation; and that we cannot hope to learn more of any of those extinct forms of life which now constitute no inconsiderable proportion of the known Flora and Fauna of the world; it is obvious that the definitions of these species can be only of a purely structural or morphological character. It is probable that naturalists would have avoided much confusion of ideas if they had more frequently borne these necessary limitations of our knowledge in mind. But while it may safely be admitted that we are acquainted with only the morphological characters of the vast majority of species—the functional or physiological peculiarities of a few have been carefully investigated, and the result of that study forms a large and most interesting portion of the physiology of reproduction.

The student of nature wonders the more and is astonished the less, the more conversant he becomes with her operations; but of all the perennial miracles she offers to his inspection, perhaps the most worthy of admiration is the development of a plant or of an animal from its embryo. Examine the recently laid egg of some common animal, such as a salamander or a newt. It is a minute spheroid in which the best microscope will reveal nothing but a structureless

* On the Osteology of the Chimpanzees and Orangs. Transactions of the Zoological Society, 1858.

sac, enclosing a glairy fluid, holding granules in suspension. But strange possibilities lie dormant in that semifluid globule. Let a moderate supply of warmth reach its watery cradle, and the plastic matter undergoes changes so rapid and yet so steady and purpose-like in their succession, that one can only compare them to those operated by a skilled modeller upon a formless lump of clay. As with an invisible trowel, the mass is divided and subdivided into smaller and smaller portions, until it is reduced to an aggregation of granules not too large to build withal the finest fabrics of the nascent organism. And, then, it is as if a delicate finger traced out the line to be occupied by the spinal column, and moulded the contour of the body; pinching up the head at one end, the tail at the other, and fashioning flank and limb into due salamandrine proportions, in so artistic a way, that, after watching the process hour by hour, one is almost involuntarily possessed by the notion, that some more subtle aid to vision than an achromatic would show the hidden artist, with his plan before him, striving with skilful manipulation to perfect his work.

As life advances, and the young amphibian ranges the waters, the terror of his insect contemporaries, not only are the nutritious particles supplied by its prey, by the addition of which to its frame growth takes place, laid down, each in its proper spot, and in such due proportion to the rest, as to reproduce the form, the colour and the size, characteristic of the parental stock; but even the wonderful powers of reproducing lost parts possessed by these animals are controlled by the same governing tendency. Cut off the legs, the tail, the jaws, separately or all together, and, as Spallanzani showed long ago, these parts not only grow again, but the reintegrated limb is formed on the same type as those which were lost. The new jaw or leg is a newt's, and never by any accident more like that of a frog. What is true of the newt is true of every animal and of every plant; the acorn tends to build itself up again into a woodland giant such as that from whose twig it fell; the spore of the humblest lichen reproduces the green or brown incrustation which gave it birth; and at the other end of the scale of life, the child that resembled neither the paternal nor the maternal side of the house would be regarded as a kind of monster.

So that the one end to which in all living beings the formative impulse is tending—the one scheme which the Archæus of the old speculators strives to carry out, seems to be to mould the offspring into the likeness of the parent. It is the first great law of reproduction, that the offspring tends to resemble its parent or parents, more closely than anything else.

Science will some day show us how this law is a necessary consequence of the more general laws which govern matter; but for

the present, more can hardly be said than that it appears to be in harmony with them. We know that the phenomena of vitality are not something apart from other physical phenomena, but one with them; and matter and force are the two names of the one artist who fashions the living as well as the lifeless. Hence living bodies should obey the same great laws as other matter—nor, throughout nature, is there a law of wider application than this, that a body impelled by two forces takes the direction of their resultant. But living bodies may be regarded as nothing but extremely complex bundles of forces held in a mass of matter, as the complex forces of a magnet are held in the steel by its coercive force; and since the differences of sex are comparatively slight, or, in other words, the sum of the forces in each has a very similar tendency, their resultant, the offspring, may reasonably be expected to deviate but little from a course parallel to either, or to both.

Represent the reason of the law to ourselves by what physical metaphor or analogy we will, however, the great matter is to apprehend its existence and the importance of the consequences deducible from it. For things which are like to the same are like to one another, and if, in a great series of generations, every offspring is like its parent, it follows that all the offspring and all the parents must be like one another; and that, given an original parental stock with the opportunity of undisturbed multiplication, the law in question necessitates the production, in course of time, of an indefinitely large group, the whole of whose members are at once very similar and are blood relations, having descended from the same parent, or pair of parents. The proof that all the members of any given group of animals, or plants, had thus descended, would be ordinarily considered sufficient to entitle them to the rank of physiological species, for most physiologists consider species to be definable as "the offspring of a single primitive stock."

But though it is quite true that all those groups we call species *may*, according to the known laws of reproduction, have descended from a single stock, and though it is very likely they really have done so, yet this conclusion rests on deduction and can hardly hope to establish itself upon a basis of observation. And the primitiveness of the supposed single stock, which, after all, is the essential part of the matter, is not only a hypothesis, but one which has not a shadow of foundation, if by "primitive" be meant "independent of any other living being." A scientific definition, of which an unwarrantable hypothesis forms an essential part, carries its condemnation within itself; but even supposing such a definition were, in form, tenable, the physiologist who should attempt to apply it in nature would soon find himself involved in great, if not inextricable difficulties. As we have said, it is indubitable that offspring *tend* to resemble the parental organism,

but it is equally true that the similarity attained never amounts to identity, either in form or in structure. There is always a certain amount of deviation, not only from the precise characters of a single parent, but when, as in most animals and many plants, the sexes are lodged in distinct individuals, from an exact mean between the two parents. And, indeed, on general principles, this slight deviation seems as intelligible as the general similarity, if we reflect how complex the co-operating "bundles of forces" are, and how improbable it is that, in any case, their true resultant shall coincide with any mean between the more obvious characters of the two parents. Whatever be its cause, however, the co-existence of this tendency to minor variation with the tendency to general similarity, is of vast importance in its bearing on the question of the origin of species.

As a general rule, the extent to which an offspring differs from its parent is slight enough; but, occasionally, the amount of difference is much more strongly marked, and then the divergent offspring receives the name of a Variety. Multitudes, of what there is every reason to believe are such varieties, are known, but the origin of very few has been accurately recorded, and of these we will select two as more especially illustrative of the main features of variation. The first of them is that of the "Ancon," or "Otter" sheep, of which a careful account is given by Colonel David Humphreys, F.R.S., in a letter to Sir Joseph Banks, published in the Philosophical Transactions for 1813. It appears that one Seth Wright, the proprietor of a farm on the banks of the Charles River, in Massachusetts, possessed a flock of fifteen ewes and a ram of the ordinary kind. In the year 1791, one of the ewes presented her owner with a male lamb, differing, for no assignable reason, from its parents by a proportionally long body and short bandy legs, whence it was unable to emulate its relatives in those sportive leaps over the neighbours' fences, in which they were in the habit of indulging, much to the good farmer's vexation.

The second case is that detailed by a no less unexceptionable authority than Reaumur, in his "Art de faire eclorre les poulets." A Maltese couple, named Kelleia, whose hands and feet were constructed upon the ordinary human model, had born to them a son, Gratio, who possessed six perfectly moveable fingers on each hand and six toes, not quite so well formed, on each foot. No cause could be assigned for the appearance of this unusual variety of the human species.

Two circumstances are well worthy of remark in both these cases. In each, the variety appears to have arisen in full force, and, as it were, *per saltum*; a wide and definite difference appearing, at once, between the Ancon ram and the ordi-

nary sheep; between the six-fingered and six-toed Gratio Kelleia and ordinary men. In neither case is it possible to point out any obvious reason for the appearance of the variety. Doubtless there were determining causes for these as for all other phenomena; but they do not appear, and we can be tolerably certain that what are ordinarily understood as changes in physical conditions, as in climate, in food, or the like, did not take place and had nothing to do with the matter. It was no case of what is commonly called adaptation to circumstances; but, to use a conveniently erroneous phrase, the variations arose spontaneously. The fruitless search after final causes leads their pursuers a long way; but even those hardy teleologists, who are ready to break through all the laws of physics in chase of their favourite will-o'-the-wisp, may be puzzled to discover what purpose could be attained by the stunted legs of Seth Wright's ram or the hexadactyle members of Gratio Kelleia.

Varieties then arise we know not why; and it is more than probable that the majority of varieties have arisen in the spontaneous manner, though we are, of course, far from denying that they may be traced, in some cases, to distinct external influences, which are assuredly competent to alter the character of the tegumentary covering, to change colour, to increase or diminish the size of muscles, to modify constitution, and, among plants, to give rise to the metamorphosis of stamens into petals, and so forth. But however they may have arisen, what especially interests us at present is, to remark that, once in existence, varieties obey the fundamental law of reproduction that like tends to produce like, and their offspring exemplify it by tending to exhibit the same deviation from the parental stock as themselves. Indeed, there seems to be, in many instances, a pre-potent influence about a newly-arisen variety which gives it what one may call an unfair advantage over the normal descendants from the same stock. This is strikingly exemplified by the case of Gratio Kelleia, who married a woman with the ordinary pentadactyle extremities, and had by her four children, Salvator, George, André, and Marie. Of these children Salvator, the eldest boy, had six fingers and six toes, like his father; the second and third, also boys, had five fingers and toes, like their mother, though the hands and feet of George were slightly deformed; the last, a girl, had five fingers and toes, but the thumbs were slightly deformed. The variety thus reproduced itself purely in the eldest, while the normal type reproduced itself purely in the third, and almost purely in the second and last: so that it would seem, at first, as if the normal type were more powerful than the variety. But all these children grew up and intermarried with normal wives and husbands, and then, note what took place: Salvator had four children, three of

whom exhibited the hexadactyle members of their grandfather and father, while the youngest had the pentadactyle limbs of the mother and grandmother; so that here, notwithstanding a double pentadactyle dilution of the blood, the hexadactyle variety had the best of it. The same pre-potency of the variety was still more markedly exemplified in the progeny of two of the other children, Marie and George. Marie (whose thumbs only were deformed) gave birth to a boy with six toes, and three other normally formed children; but George, who was not quite so pure a pentadactyle, begot, first, two girls, each of whom had six fingers and toes; then a girl with six fingers on each hand and six toes on the right foot, but only five toes on the left; and lastly, a boy with only five fingers and toes. In these instances, therefore, the variety, as it were, leaped over one generation to reproduce itself in full force in the next. Finally, the purely pentadactyle Andrè was the father of many children, not one of whom departed from the normal parental type.

If a variation which approaches the nature of a monstrosity can strive thus forcibly to reproduce itself, it is not wonderful that less aberrant modifications should tend to be preserved even more strongly; and the history of the Ancon sheep is, in this respect, particularly instructive. With the "cuteness" characteristic of their nation, the neighbours of the Massachusetts farmer imagined it would be an excellent thing if all his sheep were imbued with the stay-at-home tendencies enforced by nature upon the newly-arrived ram; and they advised Wright to kill the old patriarch of his fold, and instal the Ancon ram in his place. The result justified their sagacious anticipations, and coincided very nearly with what occurred to the progeny of Gratio Kelleia. The young lambs were almost always either pure Ancons, or pure ordinary sheep.* But when sufficient Ancon sheep were obtained to interbreed with one another, it was found that the offspring was always pure Ancon. Colonel Humphreys, in fact, states that he was acquainted with only "one questionable case of a contrary nature." Here, then, is a remarkable and well-established instance, not only of a very distinct race being established *per saltum*, but of that race

* Colonel Humphreys' statements are exceedingly explicit on this point:—"When an Ancon ewe is impregnated by a common ram the increase resembles wholly either the ewe or the ram. The increase of the common ewe impregnated by an Ancon ram follows entirely the one or the other, without blending any of the distinguishing and essential peculiarities of both. Frequent instances have happened where common ewes have had twins by Ancon rams, when one exhibited the complete marks and features of the ewe, the other of the ram. The contrast has been rendered singularly striking, when one short-legged and one long-legged lamb, produced at a birth, have been seen sucking the dam at the same time."—"Philosophical Transactions," 1813, Pt. I., pp. 89, 90.

breeding "true" at once, and showing no mixed forms, even when crossed with another breed.

By taking care to select Ancons of both sexes, for breeding from, it thus became easy to establish an extremely well-marked race, so peculiar that even when herded with other sheep, it was noted that the Ancons kept together, and there is every reason to believe that the existence of this breed might have been indefinitely protracted; but the introduction of the Merino sheep, which were not only very superior to the Ancons in wool and meat, but quite as quiet and orderly, led to the complete neglect of the new breed, so that, in 1813, Colonel Humphreys found it difficult to obtain the specimen whose skeleton was presented to Sir Joseph Banks. We believe that, for many years, no remnant of it has existed in the United States.

Gratio Kelleia was not the progenitor of a race of six-fingered men, as Seth Wright's ram became a nation of Ancon sheep, though the tendency of the variety to perpetuate itself appears to have been fully as strong in the one case as in the other. And the reason of the difference is not far to seek. Seth Wright took care not to weaken the Ancon blood by matching his Ancon ewes with any but males of the same variety, while Gratio Kelleia's sons were too far removed from the patriarchal times to intermarry with their sisters; and his grandchildren seem not to have been attracted by their six-fingered cousins. In other words, in the one example a race was produced, because, for several generations, care was taken to *select* both parents of the breeding stock, from animals exhibiting a tendency to vary in the same direction, while in the other no race was evolved, because no such selection was exercised. A race is a propagated variety, and as, by the laws of reproduction, offspring tend to assume the parental form, they will be more likely to propagate a variation exhibited by both parents than that possessed by only one.

There is no organ of the body of an animal which may not, and does not, occasionally, vary more or less from the normal type; and there is no variation which may not be transmitted, and which, if selectively transmitted, may not become the foundation of a race. This great truth, sometimes forgotten by philosophers, has long been familiar to practical agriculturists and breeders: and upon it rest all the methods of improving the breeds of domestic animals, which for the last century have been followed with so much success in England. Colour, form, size, texture of hair or wool, proportions of various parts, strength or weakness of constitution, tendency to fatten or to remain lean, to give much or little milk, speed, strength, temper, intelligence, special instincts; there is not one of these characters

whose transmission is not an every-day occurrence within the experience of cattle-breeders, stock-farmers, horse-dealers, and dog and poultry fanciers. Nay, it is only the other day that an eminent physiologist, Dr. Brown Sequard, communicated to the Royal Society his discovery that epilepsy, artificially produced in guinea-pigs, by a means which he has discovered, is transmitted to their offspring.

But a race, once produced, is no more a fixed and immutable entity than the stock whence it sprang; variations arise among its members, and as these variations are transmitted like any others, new races may be developed out of the pre-existing ones *ad infinitum*, or, at least, within any limit at present determined. Given sufficient time and sufficiently careful selection, and the multitude of races which may arise from a common stock is as astonishing as are the extreme structural differences which they may present. A remarkable example of this is to be found in the rock-pigeon, which Mr. Darwin has, in our opinion, satisfactorily demonstrated to be the progenitor of all our domestic pigeons, of which there are certainly more than a hundred well-marked races. The most noteworthy of these races are, the four great stocks known to the "fancy" as tumblers, pouters, carriers, and fantails; birds which not only differ most singularly in size, colour, and habits, but in the form of the beak and of the skull; in the proportions of the beak to the skull; in the number of tail-feathers; in the absolute and relative size of the feet; in the presence or absence of the uropygial gland; in the number of vertebræ in the back; in short, in precisely those characters in which the genera and species of birds differ from one another.

And it is most remarkable and instructive to observe, that none of these races can be shown to have been originated by the action of changes in what are commonly called external circumstances, upon the wild rock-pigeon. On the contrary, from time immemorial, pigeon fanciers have had essentially similar methods of treating their pets, which have been housed, fed, protected and cared for in much the same way in all pigeonries. In fact, there is no case better adapted than that of the pigeons, to refute the doctrine which one sees put forth on high authority, that "no other characters than those founded on the development of bone for the attachment of muscles" are capable of variation. In precise contradiction of this hasty assertion, Mr. Darwin's researches prove that the skeleton of the wings in domestic pigeons has hardly varied at all from that of the wild type; while, on the other hand, it is in exactly those respects, such as the relative length of the beak and skull, the number of the vertebræ, and the number of the tail-feathers,

in which muscular exertion can have no important influence, that the utmost amount of variation has taken place.

We have said that the following out of the properties exhibited by physiological species would lead us into difficulties, and at this point they begin to be obvious; for, if, as a result of spontaneous variation and of selective breeding, the progeny of a common stock may become separated into groups distinguished from one another by constant, not sexual, morphological characters, it is clear that the physiological definition of species is likely to clash with the morphological definition. No one would hesitate to describe the pouter and the tumbler as distinct species, if they were found fossil, or if their skins and skeletons were imported, as those of exotic wild birds commonly are—and, without doubt, if considered alone, they are good and distinct morphological species. On the other hand, they are not physiological species, for they are descended from a common stock, the rock-pigeon.

Under these circumstances, as it is admitted on all sides that races occur in nature, how are we to know whether any apparently distinct animals are really of different physiological species, or not, seeing that the amount of morphological difference is no safe guide? Is there any test of a physiological species? The usual answer of physiologists is in the affirmative. It is said that such a test is to be found in the phenomena of hybridization—in the results of crossing races as compared with the results of crossing species.

So far as the evidence goes at present, individuals, of what are certainly known to be mere races produced by selection, however distinct they may appear to be, not only breed freely together, but the offspring of such crossed races are also perfectly fertile with one another. Thus, the spaniel and the greyhound, the dray-horse and the Arab, the pouter and the tumbler, breed together with perfect freedom, and their mongrels, if matched with other mongrels of the same kind, are equally fertile.

On the other hand, there can be no doubt that the individuals of many natural species are either absolutely infertile, if crossed with individuals of other species, or, if they give rise to hybrid offspring, the hybrids so produced are infertile when paired together. The horse and the ass, for instance, if so crossed, give rise to the mule, and there is no certain evidence of offspring ever having been produced by a male and female mule. The unions of the rock-pigeon and the ring-pigeon appear to be equally barren of result. Here, then, says the physiologist, we have a means of distinguishing any two true species from any two varieties. If a male and a female, selected from each group, produce offspring, and that offspring is fertile with others pro-

duced in the same way, the groups are races and not species. If, on the other hand, no result ensues, or if the offspring are infertile with others produced in the same way, they are true physiological species. The test would be an admirable one, if, in the first place, it were always practicable to apply it, and if, in the second, it always yielded results susceptible of a definite interpretation. Unfortunately, in the great majority of cases, this touchstone for species is wholly inapplicable.

The constitution of many wild animals is so altered by confinement that they will not even breed with their own females, so that the negative results obtained from crosses are of no value, and the antipathy of wild animals of different species for one another, or even of wild and tame members of the same species, is ordinarily so great, that it is hopeless to look for such unions in nature. The hermaphroditism of most plants, the difficulty in the way of ensuring the absence of their own, or the proper working of other pollen, are obstacles of no less magnitude in applying the test to them. And in both animals and plants is superadded the further difficulty, that experiments must be continued over a long time for the purpose of ascertaining the fertility of the mongrel or hybrid progeny, as well as of the first crosses from which they spring.

Not only do these great practical difficulties lie in the way of applying the hybridization test, but even when this oracle can be questioned, its replies are sometimes as doubtful as those of Delphi. For example, cases are cited by Mr. Darwin, of plants which are more fertile with the pollen of another species than with their own; and there are others, such as certain *fuci*, whose male element will fertilize the ovule of a plant of distinct species, while the males of the latter species are ineffective with the females of the first. So that, in the last-named instance, a physiologist, who should cross the two species in one way, would decide that they were true species; while another, who should cross them in the reverse way, would, with equal justice, according to the rule, pronounce them to be mere races. Several plants, which there is great reason to believe are mere varieties, are almost sterile when crossed; while both animals and plants, which have always been regarded by naturalists as of distinct species, turn out, when the test is applied, to be perfectly fertile. Again, the sterility or fertility of crosses seems to bear no relation to the structural resemblances or differences of the members of any two groups.

Mr. Darwin has discussed this question with singular ability and circumspection, and his conclusions are summed up as follows at page 276 of his work:—

“First crosses between forms sufficiently distinct to be ranked as

species, and their hybrids, are very generally, but not universally, sterile. The sterility is of all degrees, and is often so slight that the two most careful experimentalists who have ever lived have come to diametrically opposite conclusions in ranking forms by this test. The sterility is innately variable in individuals of the same species, and is eminently susceptible of favourable and unfavourable conditions. The degree of sterility does not strictly follow systematic affinity, but is governed by several curious and complex laws. It is generally different, and sometimes widely different, in reciprocal crosses between the same two species. It is not always equal in degree in a first cross, and in the hybrid produced from this cross.

In the same manner as in grafting trees, the capacity of one species or variety to take on another is incidental on generally unknown differences in their vegetative systems, so in crossing, the greater or less facility of one species to unite with another is incidental on unknown differences in their reproductive systems. There is no more reason to think that species have been specially endowed with various degrees of sterility to prevent them crossing and breeding in nature, than to think that trees have been specially endowed with various and somewhat analogous degrees of difficulty in being grafted together, in order to prevent them becoming inarched in our forests.

“The sterility of first crosses between pure species, which have their reproductive systems perfect, seems to depend on several circumstances; in some cases largely on the early death of the embryo. The sterility of hybrids which have their reproductive systems imperfect, and which have had this system and their whole organization disturbed by being compounded of two distinct species, seems closely allied to that sterility which so frequently affects pure species when their natural conditions of life have been disturbed. This view is supported by a parallelism of another kind; namely, that the crossing of forms only slightly different is favourable to the vigour and fertility of the offspring; and that slight changes in the conditions of life are apparently favourable to the vigour and fertility of all organic beings. It is not surprising that the degree of difficulty in uniting two species, and the degree of sterility of their hybrid offspring should generally correspond, though due to distinct causes; for both depend on the amount of difference of some kind between the species which are crossed. Nor is it surprising that the facility of effecting a first cross, the fertility of hybrids produced from it, and the capacity of being grafted together—though this latter capacity evidently depends on widely different circumstances—should all run to a certain extent parallel with the systematic affinity of the forms which are subjected to experiment; for systematic affinity attempts to express all kinds of resemblance between all species.

“First crosses between forms known to be varieties, or sufficiently alike to be considered as varieties, and their mongrel offspring, are very generally, but not quite universally, fertile. Nor is this nearly general and perfect fertility surprising, when we remember how liable we are to argue in a circle with respect to varieties in a state of nature; and when we remember that the greater number of varieties have been

produced under domestication by the selection of mere external differences, and not of differences in the reproductive system. In all other respects, excluding fertility, there is a close general resemblance between hybrids and mongrels" (pp. 276—8).

We fully agree with the general tenor of this weighty passage, but forcible as are these arguments, and little as the value of fertility or infertility as a test of species may be, it must not be forgotten that the really important fact, so far as the inquiry into the origin of species goes, is, that there are such things in nature as groups of animals and of plants, whose members are incapable of fertile union with those of other groups; and that there are such things as hybrids, which are absolutely sterile when crossed with other hybrids. For if such phenomena as these were exhibited by only two of those assemblages of living objects, to which the name of species (whether it be used in its physiological or in its morphological sense) is given, it would have to be accounted for by any theory of the origin of species, and every theory which could not account for it would be, so far, imperfect.

Up to this point we have been dealing with matters of fact, and the statements which we have laid before the reader would, to the best of our knowledge, be admitted to contain a fair exposition of what is at present known respecting the essential properties of species, by all who have studied the question. And whatever may be his theoretical views, no naturalist will probably be disposed to demur to the following summary of that exposition:—

Living beings, whether animals or plants, are divisible into multitudes of distinctly definable kinds, which are morphological species. They are also divisible into groups of individuals, which breed freely together, tending to reproduce their like, and are physiological species. Normally, resembling their parents, the offspring of members of these species are still liable to vary, and the variation may be perpetuated by selection, as a race, which race, in many cases, presents all the characteristics of a morphological species. But it is not as yet proved that a race ever exhibits, when crossed with another race of the same species, those phenomena of hybridization which are exhibited by many species when crossed with other species. On the other hand, not only is it not proved that all species give rise to hybrids infertile *inter se*, but there is much reason to believe that, in crossing, species exhibit every gradation from perfect sterility to perfect fertility.

Such are the most essential characteristics of species. Even were man not one of them—a member of the same system and subject to the same laws—the question of their origin, their causal connexion, that is, with the other phenomena of the uni-

verse, must have attracted his attention, as soon as his intelligence had raised itself above the level of his daily wants.

Indeed history relates that such was the case, and has embalmed for us the speculations upon the origin of living beings, which were among the earliest products of the dawning intellectual activity of man. In those early days positive knowledge was not to be had, but the craving after it needed, at all hazards, to be satisfied, and according to the country, or the turn of thought of the speculator, the suggestion that all living things arose from the mud of the Nile, from a primeval egg, or from some more anthropomorphic agency, afforded a sufficient resting-place for his curiosity. The myths of Paganism are as dead as Osiris or Zeus, and the man who should revive them, in opposition to the knowledge of our time, would be justly laughed to scorn; but the coeval imaginations current among the rude inhabitants of Palestine, recorded by writers whose very name and age are admitted by every scholar to be unknown, have unfortunately not yet shared their fate, but, even at this day, are regarded by nine-tenths of the civilized world as the authoritative standard of fact and the criterion of the justice of scientific conclusions, in all that relates to the origin of things, and, among them, of species. In this nineteenth century, as at the dawn of modern physical science, the cosmogony of the semi-barbarous Hebrew is the incubus of the philosopher and the opprobrium of the orthodox. Who shall number the patient and earnest seekers after truth from the days of Galileo until now, whose lives have been embittered and their good name blasted by the mistaken zeal of Bibliolaters? Who shall count the host of weaker men whose sense of truth has been destroyed in the effort to harmonize impossibilities—whose life has been wasted in the attempt to force the generous new wine of science into the old bottles of Judaism, *compelled by the outcry of the same strong party?*

It is true that if philosophers have suffered, their cause has been amply avenged. Extinguished theologians lie about the cradle of every science as the strangled snakes beside that of Hercules, and history records that whenever science and dogmatism have been fairly opposed, the latter has been forced to retire from the lists, bleeding and crushed, if not annihilated; scotched, if not slain. But orthodoxy is the Bourbon of the world of thought. It learns not, neither can it forget; and though at present bewildered and afraid to move, it is as willing as ever to insist that the first chapter of Genesis contains the beginning and the end of sound science, and to visit with such petty thunderbolts as its half-paralysed hands can hurl, those who refuse to degrade nature to the level of primitive Judaism.

Philosophers, on the other hand, have no such aggressive ten-

dencies. With eyes fixed on the noble goal to which "per aspera et ardua" they tend, they may, now and then, be stirred to momentary wrath by the unnecessary obstacles with which the ignorant, or the malicious, encumber, if they cannot bar, the difficult path; but why should their souls be deeply vexed? The majesty of Fact is on their side, and the elemental forms of matter are working for them. Not a star comes to the meridian at its calculated time but testifies to the justice of their methods—their beliefs, are "one with the falling rain and with the growing corn." By doubt they are established, and open inquiry is their bosom friend. Such men have no fear of traditions however venerable, and no respect for them when they become mischievous and obstructive; but they have better than mere antiquarian business in hand, and if dogmas, which ought to be fossil but are not, are not forced upon their notice, they are too happy to treat them as non-existent.

The hypotheses respecting the origin of species, which profess to stand upon a scientific basis, and, as such, alone demand serious attention, are of two kinds. The one, the "special creation" hypothesis, presumes every species to have originated from one or more stocks, these not being the result of the modification of any other form of living matter—or arising by natural agencies—but being produced, as such, by a supernatural creative act.

The other, the so-called "transmutation" hypothesis, considers that all existing species are the result of the modification of pre-existing species and those of their predecessors, by agencies similar to those which at the present day produce varieties and races, and therefore in an altogether natural way; and it is a probable, though not a necessary consequence of this hypothesis, that all living beings have arisen from a single stock. With respect to the origin of this primitive stock or stocks, the doctrine of the origin of species is obviously not necessarily concerned. The transmutation hypothesis, for example, is perfectly consistent either with the conception of a special creation of the primitive germ, or with the supposition of its having arisen, as a modification of inorganic matter, by natural causes.

The doctrine of special creation owes its existence very largely to the supposed necessity of making science accord with the Hebrew cosmogony; but it is curious to observe that, as the doctrine is at present maintained by men of science, it is as hopelessly inconsistent with the Hebrew view as any other hypothesis.

If there be any result which has come more clearly out of geological investigation than another, it is, that the vast series of extinct animals and plants is not divisible, as it was once supposed to be, into distinct groups, separated by sharply marked boundaries.

There are no great gulfs between epochs and formations—no successive periods marked by the appearance of plants, of water animals, and of land animals, *en masse*. Every year adds to the list of links between what the older geologists supposed to be widely separated epochs; witness the crags linking the drift with the older tertiaries; the Maestricht beds linking the tertiaries with the chalk; the St. Cassian beds exhibiting an abundant fauna of mixed mesozoic and paleozoic types, in rocks of an epoch once supposed to be eminently poor in life; witness, lastly, the incessant disputes as to whether a given stratum shall be reckoned devonian or carboniferous, silurian or devonian, cambrian or silurian.

This truth is further illustrated in a most interesting manner by the impartial and highly competent testimony of M. Pictet, from whose calculations of what per-centage of the genera of animals existing in any formation lived during the preceding formation, it results that in no case is the proportion less than *one-third*, or 33 per cent. It is the triassic formation, or the commencement of the mesozoic epoch, which has received this smallest inheritance from preceding ages. The other formations not uncommonly exhibit 60, 80, or even 94 per cent. of genera in common with those whose remains are imbedded in their predecessor. Not only is this true, but the subdivisions of each formation exhibit new species characteristic of, and found only in, them, and in many cases, as in the lias for example, the separate beds of these subdivisions are distinguished by well marked and peculiar forms of life. A section, a hundred feet thick, will exhibit at different heights a dozen species of ammonite, none of which passes beyond its particular zone of limestone or clay into the zone below it or into that above it; so that those who adopt the doctrine of special creation must be prepared to admit, that at intervals of time, corresponding with the thickness of these beds, the Creator thought fit to interfere with the natural course of events for the purpose of making a new ammonite. It is not easy to transplant oneself into the frame of mind of those who can accept such a conclusion as this, on any evidence, short of absolute demonstration; and it is difficult to see what is to be gained by so doing, since, as we have said, it is obvious that such a view of the origin of living beings is utterly opposed to the Hebrew cosmogony. Deserving no aid from the powerful arm of bibliolatry, then, does the received form of the hypothesis of special creation derive any support from science or sound logic? Assuredly not much. The arguments brought forward in its favour all take one form: If species were not supernaturally created, we cannot understand the facts x, or y, or z; we cannot understand the structure of animals or plants, unless we suppose they were contrived

for special ends ; we cannot understand the structure of the eye, except by supposing it to have been made to see with ; we cannot understand instincts, unless we suppose animals to have been miraculously endowed with them.

As a question of dialectics, it must be admitted that this sort of reasoning is not very formidable to those who are not to be frightened by consequences. It is an *argumentum ad ignorantiam*—take this explanation or be ignorant. But suppose we prefer to admit our ignorance rather than adopt a hypothesis at variance with all the teachings of nature ? Or suppose for a moment we admit the explanation, and then seriously ask ourselves how much the wiser are we ? what does the explanation explain ? Is it any more than a grandiloquent way of announcing the fact, that we really know nothing about the matter ? A phenomenon is explained, when it is shown to be a case of some general law of nature ; but the supernatural interposition of the Creator can by the nature of the case exemplify no law, and if species have really arisen in this way, it is absurd to attempt to discuss their origin.

Or, lastly, let us ask ourselves whether any amount of evidence which the nature of our faculties permits us to attain, can justify us in asserting that any phenomenon is out of the reach of natural causation. To this end it is obviously necessary that we should know all the consequences to which all possible combinations, continued through unlimited time, can give rise. If we knew these, and found none competent to originate species, we should have good ground for denying their origin by natural causation. Till we know them, any hypothesis is better than one which involves us in such miserable presumption.

But the hypothesis of special creation is not only a mere specious mask for our ignorance ; its existence in Biology marks the youth and imperfection of the science. For what is the history of every science but the history of the elimination of the notion of creative, or other interferences, with the natural order of the phenomena which are the subject-matter of that science ? When Astronomy was young “the morning stars sang together for joy,” and the planets were guided in their courses by celestial hands. Now, the harmony of the stars has resolved itself into gravitation according to the inverse squares of the distances, and the orbits of the planets are deducible from the laws of the forces which allow a schoolboy’s stone to break a window. The lightning was the angel of the Lord ; but it has pleased Providence, in these modern times, that science should make it the humble messenger of man, and we know that every flash that skimmers about the horizon on a summer’s evening is determined by ascertainable conditions, and that its direction and brightness

might, if our knowledge of these were great enough, have been calculated.

The solvency of great mercantile companies rests on the validity of the laws, which have been ascertained to govern the seeming irregularity of that human life which the moralist bewails as the most uncertain of things; plague, pestilence, and famine are admitted, by all but fools, to be the natural result of causes for the most part fully within human control, and not the unavoidable tortures inflicted by wrathful Omnipotence upon his helpless handiwork.

Harmonious order governing eternally continuous progress—the web and woof of matter and force interweaving by slow degrees, without a broken thread, that veil which lies between us and the Infinite—that universe which alone we know, or can know;—such is the picture which science draws of the world, and in proportion as any part of that picture is in unison with the rest, so may we feel sure that it is rightly painted. Shall Biology alone remain out of harmony with her sister sciences?

Such arguments against the hypothesis of the direct creation of species as these are plainly enough deducible from general considerations, but there are, in addition, phenomena exhibited by species themselves, and yet not so much a part of their very essence as to have required earlier mention, which are in the highest degree perplexing, if we adopt the popularly accepted hypothesis. Such are the facts of distribution in space and in time; the singular phenomena brought to light by the study of development; the structural relations of species upon which our systems of classification are founded; the great doctrines of philosophical anatomy, such as that of homology, or of the community of structural plan exhibited by large groups of species differing very widely in their habits and functions.

The species of animals which inhabit the sea on opposite sides of the isthmus of Panama are wholly distinct; the animals and plants which inhabit islands are commonly distinct from those of the neighbouring mainlands, and yet have a similarity of aspect. The mammals of the latest tertiary epoch in the Old and New Worlds belong to the same genera, or family groups, as those which now inhabit the same great geographical area. The crocodilian reptiles which existed in the earliest secondary epoch were similar in general structure to those now living, but exhibit slight differences in their vertebræ, nasal passages, and one or two other points. The guinea pig has teeth which are shed before it is born, and hence can never subserve the masticatory purpose for which they seem contrived, and, in like manner, the female dugong has tusks which never cut the gum. All the members of the same great group run through similar conditions in their

development, and all their parts, in the adult state, are arranged according to the same plan. Man is more like a gorilla than a gorilla is like a lemur. Such are a few, taken at random, among the multitudes of similar facts which modern research has established; but when the student seeks for an explanation of them from the supporters of the received hypothesis of the origin of species, the reply he receives is, in substance, of oriental simplicity and brevity—Mashallah! it so pleases God! There are different species on opposite sides of the isthmus of Panama, because they were created different on the two sides. The pliocene mammals are like the existing ones, because such was the plan of creation; and we find rudimental organs and similarity of plan, because it has pleased the Creator to set before himself a "divine exemplar or archetype," and to copy it in his works; and somewhat ill, as those who hold this view imply, in some of them. That such verbal hocus-pocus should be received as science will one day be regarded as evidence of the low state of intelligence in the nineteenth century, just as we amuse ourselves with the phraseology about Nature's abhorrence of a vacuum, wherewith Torricelli's compatriots were satisfied to explain the rise of water in a pump. And be it recollected that this sort of satisfaction works not only negative but positive ill, by discouraging inquiry, and so depriving man of the usufruct of one of the most fertile fields of his great patrimony, Nature.

The objections to the doctrine of origin of species by special creation which have been detailed, must have occurred with more or less force to the mind of every one who has seriously and independently considered the subject. It is therefore no wonder that, from time to time, this hypothesis should have been met by counter hypotheses, all as well, and some better, founded than itself; and it is curious to remark that the inventors of the opposing views seem to have been led into them as much by their knowledge of geology as by their acquaintance with biology. In fact, when the mind has once admitted the conception of the gradual production of the present physical state of our globe, by natural causes operating through long ages of time, it will be little disposed to allow that living beings have made their appearance in another way, and the speculations of De Maillet and his successors are the natural complement of Scilla's demonstration of the true nature of fossils.

A contemporary of Newton and of Leibnitz, sharing therefore in the intellectual activity of the remarkable age which witnessed the birth of modern physical science, Benoît de Maillet spent a long life as a consular agent of the French Government in various Mediterranean ports. For sixteen years, in fact, he held the office of Consul-General in Egypt, and the wonderful phenomena

offered by the valley of the Nile appear to have strongly impressed his mind, to have directed his attention to all facts of a similar order which came within his observation, and to have led him to speculate on the origin of the present condition of our globe and of its inhabitants. But, with all his ardour for science, De Maillet seems to have hesitated to publish views which, notwithstanding the ingenious attempts to reconcile them with the Hebrew hypothesis contained in the preface to "Telliamed" (and which we recommend for Mr. MacCausland's perusal), were hardly likely to be received with favour by his contemporaries.

But a short time had elapsed since more than one of the great anatomists and physicists of the Italian school had paid dearly for their endeavours to dissipate some of the prevalent errors; and their illustrious pupil, Harvey, the founder of modern physiology, had not fared so well, in a country less oppressed by the benumbing influences of theology, as to tempt any man to follow his example. Probably not uninfluenced by these considerations, his Catholic majesty's Consul-General for Egypt kept his theories to himself throughout a long life, for "Telliamed," the only scientific work which is known to have proceeded from his pen, was not printed till 1735, when its author had reached the ripe age of seventy-nine; and though De Maillet lived three years longer, his book was not given to the world before 1748. Even then it was anonymous to those who were not in the secret of the anagrammatic character of its title, and the preface and dedication are so worded as, in case of necessity, to give the printer a fair chance of falling back on the excuse that the work was intended for a mere *jeu d'esprit*.

The speculations of the supposititious Indian sage, though quite as sound as those of many a "Mosaic Geology" which sells exceedingly well, have no great value if we consider them by the light of modern science. The waters are supposed to have originally covered up the whole globe; to have deposited the rocky masses which compose its mountains by processes comparable to those which are now forming mud, sand, and shingle; and then to have gradually lowered their level, leaving the spoils of the animal and vegetable inhabitants embedded in the strata. As the dry land appeared, certain of the aquatic animals are supposed to have taken to it, and to have become gradually adapted to terrestrial and aerial modes of existence. But if we regard the general tenor and style of the reasoning in relation to the state of knowledge of the day, two circumstances appear very well worthy of remark. The first, that De Maillet had a notion of the modifiability of living forms (though without any precise information on the subject), and how such modifiability might account for the origin of species; the second, that he very clearly apprehended

the great modern geological doctrine, so strongly insisted upon by Hutton, and so ably and comprehensively expounded by Lyell, that we must look to existing causes for the explanation of past geological events. The following passage of the preface indeed, in which De Maillet is supposed to speak of the Indian philosopher Telliamed, his *alter ego*, might have been written by the most philosophical uniformitarian of the present day.

“Ce qu’il y a d’étonnant, est que pour arriver à ces connoissances il semble avoir perverti l’ordre naturel, puisqu’au lieu de s’attacher d’abord à rechercher l’origine de notre globe il a commencé par travailler à s’instruire de la nature. Mais à l’entendre, ce renversement de l’ordre a été pour lui l’effet d’un génie favorable qui l’a conduit pas à pas et comme par la main aux découvertes les plus sublimes. C’est en décomposant la substance de ce globe par une anatomie exacte de toutes ses parties qu’il a premièrement appris de quelles matières il était composé et quels arrangemens ces mêmes matières observaient entre elles. Ces lumières jointes à l’esprit de comparaison toujours nécessaire à quiconque entreprend de percer les voiles dont la nature aime à se cacher, ont servi de guide à notre philosophe pour parvenir à des connoissances plus intéressantes. Par la matière et l’arrangement de ces compositions il prétend avoir reconnu quelle est la véritable origine de ce globe que nous habitons, comment et par qui il a été formé.”—(Pp. xix., xx.)

But De Maillet was before his age, and as could hardly fail to happen to one who speculated on a zoological and botanical question before Linnæus, and on a physiological problem before Haller, he fell into great errors here and there; and hence, perhaps, the general neglect of his work. Robinet’s speculations are rather behind than in advance of those of De Maillet, and though Linnæus may have played with the hypothesis of transmutation, it obtained no serious support until Lamarck adopted it, and advocated it with great ability in his “*Philosophie Zoologique*.”

Impelled towards the hypothesis of the transmutation of species, partly by his general cosmological and geological views; partly by the conception of a graduated, though irregularly branching scale of being, which had arisen out of his profound study of plants and of the lower forms of animal life, Lamarck, whose general line of thought often closely resembles that of De Maillet, made a great advance upon the crude and merely speculative manner in which that writer deals with the question of the origin of living beings, by endeavouring to find physical causes competent to effect that change of one species into another which De Maillet had only supposed to occur. And Lamarck conceived that he had found in nature such causes, amply sufficient for the purpose in view. It is a physiological fact, he says, that organs are increased in size by action, atrophied by inaction; it is another

physiological fact that modifications produced are transmissible to offspring. Change the actions of an animal, therefore, and you will change its structure, by increasing the development of the parts newly brought into use and by the diminution of those less used; but by altering the circumstances which surround it you will alter its actions, and hence, in the long run, change of circumstance must produce change of organization. All the species of animals, therefore, are in Lamarck's view the result of the indirect action of changes of circumstance upon those primitive germs which he considered to have originally arisen, by spontaneous generation, within the waters of the globe. It is curious, however, that Lamarck should insist so strongly* as he has done, that circumstances never in any degree directly modify the form or the organization of animals, but only operate by changing their wants, and consequently their actions; for he thereby brings upon himself the obvious question, how, then, do plants, which cannot be said to have wants or actions, become modified? To this he replies, that they are modified by the changes in their nutritive processes, which are effected by changing circumstances; and it does not seem to have occurred to him that such changes might be as well supposed to take place among animals.

When we have said that Lamarck felt that mere speculation was not the way to arrive at the origin of species, but that it was necessary in order to the establishment of any sound theory on the subject, to discover by observation or otherwise, some *vera causa*, competent to give rise to them; that he affirmed the true order of classification to coincide with the order of their development one from another; that he insisted on the necessity of allowing sufficient time, very strongly; and that all the varieties of instinct and reason were traced back by him to the same cause as that which has given rise to species, we have enumerated his chief contributions to the advance of the question. On the other hand, from his ignorance of any power in nature competent to modify the structure of animals, except development of parts, or atrophy of them, in consequence of a change of needs, Lamarck was led to attach infinitely greater weight than it deserves to this agency, and the absurdities into which he was led have met with deserved condemnation. Of the struggle for existence, on which as we shall see Mr. Darwin lays such great stress, he had no conception; indeed, he doubts whether there really are such things as extinct species, unless they be such large animals as may have met their death at the hands of man; and so little does he dream of there being any other destructive causes at work, that, in discussing

* See *Phil. Zoologique*, vol. i. p. 222, et seq.

the possible existence of fossil shells, he asks, "Pourquoi d'ailleurs seroient-ils perdues dès que l'homme n'a pu opérer leur destruction?"—"Phil. Zool.," vol. i. p. 77.) Of the influence of selection Lamarck has as little notion, and he makes no use of the wonderful phenomena which are exhibited by domesticated animals, and illustrate its powers. The vast influence of Cuvier was employed against the Lamarckian views, and as the untenability of some of his conclusions was easily shown, his doctrines sank under the opprobrium of scientific as well as of theological heterodoxy. Nor have the efforts made of late years to revive them, tended to re-establish their credit in the minds of sound thinkers acquainted with the facts of the case; indeed it may be doubted whether Lamarck has not suffered more from his friends than from his foes.

Two years ago, in fact, though we venture to question if even the strongest supporters of the special creation hypothesis had not, now and then, an uneasy consciousness that all was not right, their position seemed more impregnable than ever, if not by its own inherent strength, at any rate by the obvious failure of all the attempts which had been made to carry it. On the other hand, however much the few, who thought deeply on the question of species, might be repelled by the generally received dogmas, they saw no way of escaping from them, save by the adoption of suppositions, so little justified by experiment or by observation, as to be at least equally distasteful. The choice lay between two absurdities and a middle condition of uneasy scepticism; which last, however unpleasant and unsatisfactory, was obviously the only justifiable state of mind under the circumstances.

Such being the general ferment in the minds of naturalists, it is no wonder that they mustered strong in the rooms of the Linnæan Society, on the first of July of the year 1858, to hear two papers by authors living on opposite sides of the globe, working out their results independently, and yet professing to have discovered one and the same solution of all the problems connected with species. The one of these authors was an able naturalist, Mr. Wallace, who had been employed for some years in studying the productions of the islands of the Indian Archipelago, and who had forwarded a memoir embodying his views to Mr. Darwin for communication to the Linnæan Society. On perusing the essay Mr. Darwin was not a little surprised to find that it embodied some of the leading ideas of a great work which he had been preparing for twenty years, and parts of which, containing a development of the very same views, had been perused by his private friends fifteen or sixteen years before. Perplexed in what manner to do full justice both to his friend and to himself, Mr. Darwin placed the matter in the hands of Dr. Hooker and Sir Charles

Lyell, by whose advice he communicated a brief abstract of his own views to the Linnæan Society, at the same time that Mr. Wallace's paper was read. Of that abstract, the work on the "Origin of Species" is an enlargement, but a complete statement of Mr. Darwin's doctrine is looked for in the large and well-illustrated work which he is said to be preparing for publication.

The Darwinian hypothesis has the merit of being eminently simple and comprehensible in principle, and its essential positions may be stated in a very few words: all species have been produced by the development of varieties from common stocks, by the conversion of these, first into permanent races and then into new species, by the process of *natural selection*, which process is essentially identical with that artificial selection by which man has originated the races of domestic animals—the *struggle for existence* taking the place of man, and exerting, in the case of natural selection, that selective action which he performs in artificial selection.

The evidence brought forward by Mr. Darwin in support of his hypothesis is of three kinds. First, he endeavours to prove that species may be originated by selection; secondly, he attempts to show that natural causes are competent to exert selection; and thirdly, he tries to prove that the most remarkable and apparently anomalous phenomena exhibited by the distribution, development, and mutual relations of species, can be shown to be deducible from the general doctrine of their origin, which he propounds, combined with the known facts of geological change; and that, even if not all these phenomena are at present explicable by it, none are necessarily inconsistent with it.

There cannot be a doubt that the method of inquiry which Mr. Darwin has adopted is not only rigorously in accordance with the canons of scientific logic, but that it is the only adequate method. Critics exclusively trained in classics or in mathematics, who have never determined a scientific fact in their lives by induction from experiment or observation, prate learnedly about Mr. Darwin's method, which is not inductive enough, not Baconian enough, forsooth, for them. But even if practical acquaintance with the process of scientific investigation is denied them, they may learn, by the perusal of Mr. Mill's admirable chapter "On the Deductive Method," that there are multitudes of scientific inquiries, in which the method of pure induction helps the investigator but a very little way.

"The mode of investigation" (says Mr. Mill) "which from the proved inapplicability of direct methods of observation and experiment remains to us as the main source of the knowledge we possess, or can acquire, respecting the conditions and laws of recurrence of the more

complex phenomena, is called, in its most general expression, the deductive method, and consists of three operations: the first, one of direct induction; the second, of ratiocination; and the third, of verification."

Now, the conditions which have determined the existence of species are not only exceedingly complex, but, so far as the great majority of them are concerned, are necessarily beyond our cognisance. But what Mr. Darwin has attempted to do is in exact accordance with the rule laid down by Mr. Mill; he has endeavoured to determine certain great facts inductively, by observation and experiment; he has then reasoned from the data thus furnished; and lastly, he has tested the validity of his ratiocination by comparing his deductions with the observed facts of nature. Inductively, Mr. Darwin endeavours to prove that species arise in a given way. Deductively, he desires to show that, if they arise in that way, the facts of distribution, development, classification, &c., may be accounted for, *i. e.* may be deduced from their mode of origin, combined with admitted changes in physical geography and climate, during an indefinite period. And this explanation, or coincidence of observed with deduced facts, is, so far as it extends, a verification of the Darwinian view.

There is no fault to be found with Mr. Darwin's method, then; but it is another question whether he has fulfilled all the conditions imposed by that method. Is it satisfactorily proved, in fact, that species may be originated by selection? that there is such a thing as natural selection? that none of the phenomena exhibited by species are inconsistent with the origin of species in this way? If these questions can be answered in the affirmative, Mr. Darwin's view steps out of the ranks of hypotheses into those of proved theories; but so long as the evidence at present adduced falls short of enforcing that affirmation, so long, to our minds, must the new doctrine be content to remain among the former—an extremely valuable, and in the highest degree probable, doctrine, indeed the only extant hypothesis which is worth anything in a scientific point of view; but still a hypothesis, and not yet the theory of species.

After much consideration, and with assuredly no bias against Mr. Darwin's views, it is our clear conviction that, as the evidence stands, it is not absolutely proven that a group of animals, having all the characters exhibited by species in nature, has ever been originated by selection, whether artificial or natural. Groups having the morphological character of species, distinct and permanent races in fact, have been so produced over and over again; but there is no positive evidence at present that any group of animals has, by variation and selective breeding, given rise to

another group which was even in the least degree infertile with the first. Mr. Darwin is perfectly aware of this weak point, and brings forward a multitude of ingenious and important arguments to diminish the force of the objection. We admit the value of these arguments to their fullest extent; nay, we will go so far as to express our belief that experiments, conducted by a skilful physiologist, would very probably obtain the desired production of mutually more or less infertile breeds from a common stock, in a comparatively few years; but still, as the case stands at present, this "little rift within the lute" is not to be disguised or overlooked.

In the remainder of Mr. Darwin's argument our own private ingenuity has not hitherto enabled us to pick holes of any great importance; and judging by what we hear and read, other adventurers in the same field do not seem to have been much more fortunate. It has been urged, for instance, that in his chapters on the struggle for existence and on natural selection, Mr. Darwin does not so much prove that natural selection does occur, as that it must occur; but, in fact, no other sort of demonstration is attainable. A race does not attract our attention in nature until it has, in all probability, existed for a considerable time, and then it is too late to inquire into the conditions of its origin. Again, it is said that there is no real analogy between the selection which takes place under domestication, by human influence, and any operation which can be effected by nature, for man interferes intelligently. Reduced to its elements, this argument implies that an effect produced with trouble by an intelligent agent must, *à fortiori*, be more troublesome, if not impossible, to an unintelligent agent. Even putting aside the question whether nature, acting as she does according to definite and invariable laws, can be rightly called an unintelligent agent, such a position as this is wholly untenable. Mix salt and sand, and it shall puzzle the wisest of men with his mere natural appliances to separate all the grains of sand from all the grains of salt; but a shower of rain will effect the same object in ten minutes. And so while man may find it tax all his intelligence to separate any variety which arises, and to breed selectively from it, the destructive agencies incessantly at work in nature, if they find one variety to be more soluble in circumstances than the other, will inevitably in the long run eliminate it.

A frequent and a just objection to the Lamarckian hypothesis of the transmutation of species is based upon the absence of transitional forms between many species. But against the Darwinian hypothesis this argument has no force. Indeed, one of the most valuable and suggestive parts of Mr. Darwin's work is that in which he proves, that the frequent absence of transitions is a necessary consequence of his doctrine, and that the stock whence

two or more species have sprung, need in no respect be intermediate between these species. If any two species have arisen from a common stock in the same way as the carrier and the pouter, say, have arisen from the rock-pigeon, then the common stock of these two species need be no more intermediate between the two than the rock-pigeon is between the carrier and pouter. Clearly appreciate the force of this analogy, and all the arguments against the origin of species by selection, based on the absence of transitional forms, fall to the ground. And Mr. Darwin's position might, we think, have been even stronger than it is if he had not embarrassed himself with the aphorism, "*Natura non facit saltum*," which turns up so often in his pages. We believe, as we have said above, that nature does make jumps now and then, and a recognition of the fact is of no small importance in disposing of many minor objections to the doctrine of transmutation.

But we must pause. The discussion of Mr. Darwin's arguments in detail would lead us far beyond the limits within which we proposed, at starting, to confine this article. Our object has been attained if we have given an intelligible, however brief, account of the established facts connected with species, and of the relation of the explanation of those facts offered by Mr. Darwin to the theoretical views held by his predecessors and his contemporaries, and, above all, to the requirements of scientific logic. We have ventured to point out that it does not, as yet, satisfy all those requirements; but we do not hesitate to assert that it is as superior to any preceding or contemporary hypothesis, in the extent of observational and experimental basis on which it rests, in its rigorously scientific method, and in its power of explaining biological phenomena, as was the hypothesis of Copernicus to the speculations of Ptolemy. But the planetary orbits turned out to be not quite circular after all, and grand as was the service Copernicus rendered to science, Kepler and Newton had to come after him. What if the orbit of Darwinism should be a little too circular? what if species should offer residual phenomena here and there not explicable by natural selection? Twenty years hence naturalists may be in a position to say whether this is, or is not, the case; but in either event they will owe the author of "*The Origin of Species*" an immense debt of gratitude. We should leave a very wrong impression on the reader's mind if we permitted him to suppose that the value of that work depends wholly on the ultimate justification of the theoretical views which it contains. On the contrary, if they were disproved to-morrow, the book would still be the best of its kind—the most compendious statement of well-sifted facts bearing on the doctrine of species that has ever appeared. The chapters on Variation, on the Struggle for Existence, on Instinct, on Hybridism, on the Imperfection of the Geological Record, on

Geographical Distribution, have not only no equals, but, so far as our knowledge goes, no competitors, within the range of biological literature. And viewed as a whole, we do not believe that, since the publication of Von Baer's *Researches on Development*, thirty years ago, any work has appeared calculated to exert so large an influence, not only on the future of Biology, but in extending the domination of Science over regions of thought into which she has, as yet, hardly penetrated.

SCRIPTURE INSPIRATION: THE REV. J. MACNAUGHT.

Everton, Liverpool, Feb. 21, 1860.

To the Editor of the Westminster Review.

SIR,—In your article on *Christian Revivals*, last month, it is asserted that “the Rev. J. Macnaught renounces Scripture Inspiration.”

Now, Sir, I beg most respectfully, but most distinctly, to contradict this assertion; and, as you and the reviewer are, doubtless, anxious to propagate only that which is true and just, I request that you will give as much publicity to this disclaimer as was given to the error above quoted.

I have always *maintained* the Inspiration of Holy Writ, and in support of that doctrine, have shown that, as the grace of God's Spirit, sanctifying a believer, is by no means held to confer *moral perfection* or impeccability; similarly, the grace of *the same* Divine Spirit, enlightening a Scripture-writer, need and ought by no means to be regarded as conferring *intellectual perfection* or infallibility.

Thus, I believe the saint to be inspired, but not without sin; and the Bible to be inspired, but not without error.

In support of this disclaimer I may refer to all that I have ever spoken, written, or published, and, especially, to my work on “*The Doctrine of Inspiration*.”

I have the honour to be, Sir, your obedient servant,
 JOHN MACNAUGHT, M.A.,
 Incumbent of St. Chrysostom's.

We are sorry if, by our assertion, that Mr. Macnaught “renounces Scripture inspiration,” we have in any degree misrepresented his teaching; but we confess that though, in his letter, he seems to draw a distinction between his views and our presentation of them, we find it difficult to discern more than a difference of words. If, in his opinion, the Bible is “inspired, but not without error,” and if, as we presume, he possesses no infallible test by which the error may be distinguished from the truth, how does he become assured that any part of it is truth only? It appears to us that believing the Bible “to be inspired, but not without error,” Mr. Macnaught is compelled, *practically*, to “renounce Scripture inspiration;” whether the hold by which he still clings to it *theoretically* justifies him in contradicting our assertion we leave to the judgment of our readers.—EDITOR.