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PHYSICAL THEORIES OF THE PHENOMENA OF LIFE.

PART I.

QUESTIONS relating to the nature and origin of the phenomena of life, especially when extended to man as an intellectual and spiritual being, naturally present themselves under two distinct aspects,—first, in the direct and immediate relation which they may bear to the existence and attributes of an Omnipotent Creator and All-wise Governor of the universe; and secondly, in their relation to the secondary causes to which we refer the more ordinary phenomena of inorganic matter. The first may be called the *religious* or *theological* aspect, the second the *scientific* aspect of such questions. It has too frequently happened that men of pious minds and strong religious convictions have regarded this latter view of such subjects as inconsistent with the reverence due to the Deity, and have sometimes, moreover, been ready to brand with hard names those who entertain such views; while the latter, in their turn, have often shown little sympathy with the feelings of reverence and piety in which the opinions of their opponents may have originated. This is deeply to be regretted, and we are anxious to express at once our respect for the sentiments of piety in which we believe opposition to science to have frequently arisen, and our unequivocal belief that all the phenomena which nature presents to us constitute legitimate objects of scientific investigation. At the same time, the subjects to which we are here more especially alluding, require that such investigations be conducted with modesty and reverence, and not with that presumption which would claim even for some of the vaguest conclusions of man's intellect a weight and authority beyond that which may be assigned to higher sources of our knowledge. But at all events, if any one believe that the phenomena of life can be accounted for by the same laws as those which govern the combinations of inorganic matter, let him feel that the subject is freely open to his researches; but while we concede to him entirely the right of conducting his investigations on the same principles as we recognise in all investigations of the laws of inorganic matter, let him also understand that we exact from him in the support of his theories the same logical reasoning and the same kind of general evidence as we demand before we yield our assent to more ordinary physical theories. While we admit the same principles of research, we cannot admit different principles of interpretation, and yield our assent to the naturalist on evidence which we should utterly reject in the physicist. Let the investigator of the causes of vital phenomena state his evidence in support of his theories; it is then for us to test the claim of such theories to our belief by instituting an impartial comparison between the weight of the evidence adduced, and that which we demand in the theories respecting inorganic matter; and moreover, if his theories necessarily involve materialistic or pantheistic conclusions, we are bound to weigh his evidence in the balance against those general considerations which are opposed to materialism and pantheism, and to

reject it if found wanting. It is impossible, we repeat, to admit laxity of reasoning to the naturalist, while we insist on rigorous proof in the physicist. He who appeals to Cæsar must be judged by Cæsar's laws.

Let us, then, consider the logical course of research which will thus be prescribed to the naturalist who would enter with the true spirit of inductive philosophy into what may be termed the *physical* investigations of his subject. The first step will be the generalization of the observed phenomena, which, that we may speak of them the more simply and definitely, we may suppose to be restricted not merely to organic matter, but to animal life. It will embrace the classification of animals, their structural organization, the correlation of their various organs, the adaptation of those organs to the functions which they are required to perform, and, in short, the grouping together of the particular phenomena in such a manner as to indicate any laws by which they may be connected. In every physical investigation the first step is a precisely analogous one. In crystallography, for instance, it consists, of the recognition of the various forms of crystals, and the geometrical laws by which those forms are connected, together with any other characteristic phenomena which may belong to crystalline bodies. In astronomy, the analogous first step (restricting ourselves to the solar system) consists in the observations of the motions of the planets and satellites, with any other phenomena which may present themselves, followed by the grouping together of such observed facts as seem to be connected by general laws. Thus Kepler, after years of observation on the planetary motions, deduced from the observed phenomena the three great laws which characterize those motions—that of elliptic motion, of the uniform description of areas, and that which expresses the relation between the periodic times of the planets and the longest axes of their orbits. Again, in the science of optics, all the phenomena of reflexion, refraction, polarization, &c., were observed, and the corresponding laws of reflexion, refraction, &c., were deduced from them; and so we might enumerate every other branch of physical science. Now, it will be observed that in the three sciences specified above, the laws immediately deduced from the grouping of the observed facts are entirely independent of any theory respecting the *physical causes* to which the phenomena may be referable. They admit for the most part of being enunciated in the language of pure geometry, and are frequently termed the *geometrical* laws of the phenomena, in contradistinction to the laws by which the physical causes of those phenomena may be governed.

It is manifest that these geometrical laws in the physical sciences above specified, are exactly analogous to those which may be inferred from the generalizations of the phenomena of animal life. Cuvier, as is well known, made an enormous advance in these generalizations, especially in basing them not merely on external characters, but on anatomical structure. Hence he was enabled to deduce his most important law respecting the correlation of different organs, and their individual and combined adaptation to meet the peculiar necessities of each class of the animal kingdom. He thus became the strongest advocate of the doctrine of Final Causes. But before the close of Cuvier's life, another school of zoologists and anatomists arose, who were not satisfied with what they regarded as the comparatively narrow limits of his generalizations. They sought to discover something that might lead us nearer to the cause and origin of those organs which Cuvier had regarded chiefly in subservience to the animal wants which they were intended by a beneficent Creator to supply. It was a fundamental idea of this school of transcendental anatomy, as it was called (of which Geoffroy St. Hilaire was one of the principal leaders), that

the whole animate creation was constructed according to a *uniform plan*, and that consequently there must be some form of organization which presented a general *type* of animal structure, every individual form being only a particular modification of this typical one. The attempts to discover this typical form led to the assertion of resemblances in some cases, which could only be recognised by a wild imagination, and exposed these researches to the charge of great extravagance. They led, moreover, in many instances to the adoption of pantheistic views, and the utter rejection of the doctrine of final causes, and of the belief in a Supreme Intelligence and personal Governor of the universe. Independently, however, of all such collateral views, the search for a typical form of animal organization, as a higher order of generalization than had been previously arrived at, was, in a philosophical point of view, strictly legitimate, and perfectly in accordance with the rules of inductive philosophy. Restricted as it appears to have been of late to the vertebrate skeleton, it has led to a generalization of structural characters which, though it may not in any one precise form command the universal assent of naturalists, must be recognised as an important step in the subject.

Let us now consider the next logical step to be taken with the view of establishing a physical theory of these vital phenomena, and in accordance with the process of investigation which is deemed imperative in forming our theories respecting inanimate matter. In the most perfect physical sciences, the observed laws of the phenomena (their geometrical laws) have suggested some physical cause to which the phenomena may be due, or if the mode of expression be preferred, some higher generalization. Then, assuming the truth of our hypothetical cause, we must calculate or investigate by the best means which the subject may supply, the necessary consequences which must result from the action of the cause assigned under such conditions as the problem presents to us. The results thus obtained must then be compared with the observed phenomena, and the proof that our assumed cause is the true one will consist, first, in the accuracy with which we can determine its necessary consequences; and, secondly, on the degree of accordance which we can establish between those consequences and the existing observed phenomena. These two last-mentioned particulars are the important ones of the proof; for whether the hypothesis of the assigned cause be suggested by a careful consideration of the previously observed phenomena, or by any *à priori* considerations, is manifestly of no importance so long as we can establish the accordance between the essential consequences of our hypothetical cause and existing facts.

All this is so well known to scientific men, that these detailed remarks might almost seem to require an apology were it less certain than it is, that however well the abstract philosophical rules of theorizing in physical subjects may be known, many people will still theorize as if Bacon and Newton had never lived. Our special object, too, is to inquire how far those naturalists who have aspired to this higher order of generalization, and have sought to ascertain the physical causes of the origin and nature of vital phenomena, have complied with the essential rules we have indicated, before they claim our acquiescence in their theories. And in order to establish a standard to which the amount of evidence in favour of any particular theory may be referred, let us briefly consider the evidence on which we base our convictions of the truth of the theory of gravitation as applied to the phenomena of the solar system, and that of the Undulatory theory of light, these being the two most perfect physical theories which we possess.

Newton was led (whether by the falling apple or more profound considerations is of little importance) to assign the mutual gravitation of

all particles of matter towards each other as the physical cause which governs the motion of the moon, and, by an extension of the hypothesis, the motions also of all the bodies of the solar system. Moreover, it was assumed that the intensity of this mutual gravitation or force of attraction varies with the distance between the mutually attracting particles according to a determinate law—that of the inverse square of the distance between the particles. In this hypothesis there is no ambiguity. On the contrary, it is so determinate that mathematicians have been able to deduce with the most refined accuracy innumerable results as the necessary consequences of this assumed gravitation; while, on the other hand, astronomers have been able to observe with equally refined accuracy the motions of the various bodies of the solar system. Hence we are in a position to compare the results of calculation with those of observation, and their exact accordance in all particulars affords the most perfect proof we can conceive of the truth of a physical theory. It is the case to which we must ever recur to understand the degree of conclusiveness of which physical reasoning is capable, and the amount of conviction which may be derived from it. Such conviction can scarcely be said to be less than that derivable from the demonstrations of the properties of numbers and of space.

Again, let us turn to the theory of light, or Physical Optics. We have an immense number of carefully observed and accurately defined optical phenomena depending on the reflexion, refraction, polarization, double refraction, &c., of light, which any theory of light is imperatively called upon to account for. This is accomplished for an immense majority of these phenomena by the Undulatory theory. In this theory the fundamental hypotheses are extremely simple, and constitute a beautiful generalization. A self-luminous body is assumed to be so by virtue of the extremely rapid and minute vibrations of its constituent molecules, as a body becomes sonorous in a state of similar though much larger and less rapid vibrations. All free space and the pores of all transparent bodies are assumed to be occupied by a highly subtle and refined elastic medium, to which the vibrations of luminous bodies are communicated, and by which they are propagated from one point of space to another, as sound is propagated through the atmosphere; and finally, the effect of light is assumed to be produced by the action of the vibrating molecules of this ethereal medium on our organs of vision, in a manner similar to that in which the aerial vibrations produce the effect of sound by the action of the contiguous particles of the air on our organs of hearing. Now from these simple hypotheses we can deduce by mathematically accurate reasoning a great variety of consequences which are found exactly to agree with the corresponding observed optical phenomena. This theory, however, is less perfect than that of gravitation, since we cannot calculate all the consequences of our hypotheses with the same certainty as in the latter theory, nor can we assert an equally complete agreement in every case between the results of calculation and those of observation. It is a case of somewhat more imperfect evidence in support of a physical theory; but still the accordance between theory and observation is so great that it might be difficult to draw a line between the conviction we feel of the truth of the Undulatory theory, and that perfect conviction with which we regard the theory of gravitation.

Other physical theories are for the most part far inferior to the two above mentioned in the conclusiveness of the evidence on which they rest. The theories of heat, electricity, and magnetism, for instance, are not yet reduced to the same simple and certain elements as those of gravitation and light; and in all such cases, whatever he may think of the preponderance of probabilities, a sound

philosopher will hold his opinions with a certain degree of reticence and reserve, till more perfect evidence shall be adduced. Again, in those branches of science in which he must seek for the causes of the observed phenomena in molecular attractions, we can scarcely be said to have advanced beyond that stage of investigation in which we group and arrange observed phenomena according to their more obvious resemblances, and deduce from them what I have termed in physical astronomy and physical optics, *geometrical laws*, in contradistinction to the *physical causes* to which in those sciences the phenomena are ultimately referred. To take a definite case, the crystallographer has ascertained with great exactness the various crystalline forms and their relations to each other. He has thus done for his science what Kepler did for the astronomy of the solar system. The step analogous to that which Newton made in referring the lunar and planetary motions to gravitation, would consist in the determination of the nature and laws of those molecular attractions from which the phenomena of crystallization would result as necessary consequences. Some attempts have been made to investigate the nature of molecular action, but without any considerable success. In all cases the great difficulty lies in demonstrating that the observed phenomena are the necessary results of the physical causes to which they are attributed. As we have already intimated, the theory of gravitation is the only one in which this has been perfectly effected. But with respect to molecular attractions and their effects the task is infinitely more difficult. Newton and succeeding mathematicians have had to determine the motions of the heavenly bodies as depending on the mutual attractions of the finite and comparatively small number of bodies which compose the solar system. These are of finite magnitudes, and at finite distances from each other; whereas in determining the effects of molecular actions we have to regard them as the results of an infinite number of infinitely small material particles acting on each other at infinitely small distances. Considering the subject with respect merely to the phenomena of crystallization, as among the best defined and apparently most simple results of molecular forces, the mathematicians who have been able to vanquish all the difficulties of the theory of gravitation have made scarcely any progress in overcoming the far more complex difficulties of the theory of molecular actions. And if such be the difficulties in the simplest branch of the subject, what must they be in the infinitely more complicated branches of the different departments of chemistry. Nearly all that has been effected in that extensive science, must be considered as the generalizations of phenomena equivalent to the geometrical laws above spoken of in astronomy and optics, and still remaining to be referred, in some indefinitely more advanced state of the science, to molecular actions as their physical cause.

It has been our object in these remarks to point out, in the first place, as distinctly as possible, the modes of investigation by which our most perfect physical theories have been established, and by which alone other physical theories possessing a corresponding degree of generalization, can hereafter be established on foundations sufficiently stable to secure general assent to their truth; and, secondly, to indicate the enormous difficulties which present themselves in the application of a similar mode of research to those sciences which involve the action of molecular forces. Without following the investigations of the effects of these forces in some of the simpler cases in which they have been attempted, into the intricate details and mathematical complexities into which they lead us, it is perhaps impossible to realize these difficulties; and if they be so great in reference to the phenomena of mere form and structure, as in crystalline bodies, it seems highly probable

that they must be far greater still in reference to all those phenomena which depend on the qualitative properties of bodies, phenomena with which the science of chemistry deals so largely.

In examining the process of reasoning and investigation above described, we observe that the great difficulty in every instance is in the exact determination, whether by mathematical or other accurate modes of reasoning, of the consequences which would necessarily result from the action of the physical causes hypothetically assigned as the true causes of the observed phenomena. In all cases where the phenomena depend on the molecular actions of the constituent particles of matter, this difficulty has been really found invincible; for even in the simplest cases, which have reference only to form and geometrical structure, it has been very partially, if at all, overcome; and in all that infinite variety of cases in which the qualitative properties of bodies are concerned, it has not even been approached. Now if such be the difficulties which beset the completion of our theories of inorganic matter, what must they be with reference to organic matter; and not only organic but animate matter, and further still, with reference to the connexion of mind with matter? When we consider the array of these enormous cumulative difficulties, we cannot but smile at the thought that we should at present hope to arrive at even the most remote approximation to the real solution of the problem of vital phenomena.

But, it may be objected, this is not the solution of the problem, or the mode of effecting it which has ever been contemplated. We believe it. Still it is not less the only complete and demonstrative solution which it admits of; and our especial object in thus clearly and explicitly indicating it, is that we may the more correctly estimate the value of the vague solutions which have been really attempted. We doubt whether many of those who have believed in such solutions have ever formed a distinct conception of what the complete solution is—complete, we mean, in the sense in which Newton's solution of the problem of the planetary motions is complete, for any completeness of solution at which man can arrive must necessarily be relative.* We suspect, for instance, that those earlier transcendentalists who sought for this solution in the *unity of type* for the whole animate creation, fell into a confusion of ideas similar to that into which we should fall in confounding Kepler's geometrical laws with Newton's higher generalization, or physical solution of the problem of the planetary motions. At all events, no determination of the nature of vital forces, nor any explanation which they may afford of vital phenomena, can be rendered complete and demonstrative except by the process of reasoning and investigation we have described; and it is only by an appeal to the solution of the problem which would be thus obtained, that we can judge of the incompleteness of other solutions, and of the degree of confidence to which they may be entitled. We shall proceed to examine certain theories which have been offered, and more particularly in the sequel, that put forth by Mr. Darwin in his recent work on *The Origin of Species*, with the view of estimating their value on the principle here stated.

The first theory we have to notice is that of Lamarck, a distinguished French naturalist at the end of the last and beginning of the present century. It is fully developed in his *Zoologie Philosophique*, published in 1809. It embraces the origin of life from inorganic matter, and comprises man, regarded not merely as an animate, but as an intellectual being. We consider his theory of the origin of life altogether worthless as a physical theory, but it serves well to

* A more complete solution than Newton's, for instance, might determine the origin and cause of *gravity* in some more simple and elementary property of matter.

elucidate the point in which all such theories utterly fail; and this, as well as other parts of his general theory, seems to us to afford a curious proof of the union of great acuteness of observation and large views in the grouping and classification of natural objects and phenomena, with the want of that judicial power by which a man is enabled to estimate the true weight of the evidence laid before him, and the degree of confidence with which it ought to inspire us. Is it that the power and habit of minute external observation is so far opposed to the power and habit of abstract reasoning that they are rarely found coexistent in the same mind? Assuredly we should not often expect to find in the abstract philosopher the best observer of external objects; and it might perhaps be equally unreasonable to expect generally in the acute observer the higher powers of abstract reasoning.

According to Lamarck, all animate matter is composed of, or immediately results from, a *cellular tissue*, forming the substance of all vital organs, or surrounding and investing every separate portion of them. It consists of an aggregation of minute cells filled with a more or less compound fluid incapable of permeating the membranous walls of the cells, the whole constituting a soft and flexible mass. But this mass would not be a *living* mass unless acted on by some exciting cause which puts the contained fluids in motion and gives to the mass its properties of animate existence. This exciting cause is supposed to be found in some circumambient ethereal fluid pervading all space in which animate matter can exist. The principal of these *fluids* (as such they are spoken of) are supposed to be heat and electricity, with a certain degree of humidity, these being in fact, as we well know, the convenient agencies by which a bold philosopher in those days overcame the most formidable obstacles which might beset him. These are assumed to produce and maintain a certain *irritability* in the cellular membranes, and also to act upon the contained fluid, thus calling forth the vital actions of the mass.

The above general description of the masses which constitute animal organs would not, we conceive, be materially objected to by physiologists of the present day; and regarding vital phenomena as the result altogether of physical causes, Lamarck committed no violation of the strictest rules of philosophic reasoning in the assumption that the exciting cause—that which gives real vitality to the cellular mass—was some such circumambient medium as above described. He had the same right to assume this as any other philosopher has to assume the existence of that refined ethereal medium by means of which, as already intimated, light is supposed to be propagated from one point of space to another, or to assume that gravity is a universal property of matter and the cause of the planetary motions. But then comes the next step in the proof of any physical theory, always the most difficult, and that in which every false or imperfect theory necessarily fails. It consists in the investigation of the necessary consequences of the cause assigned, acting under probable conditions, and in showing the accordance of the results thus obtained with existing phenomena. Philosophers have not established the theory of gravitation or the Undulatory theory of light by asserting merely their *belief* that the causes assigned according to the fundamental hypotheses of those theories, are capable of producing the phenomena thus referred to them. They are not content to say that it *may* be so, and thus to build up theories based on bare possibilities. They *prove*, on the contrary, by modes of investigation which cannot be wrong, that phenomena exactly such as are observed would *necessarily*, not by some vague possibility, result from the causes hypothetically assigned, thus demonstrating those causes to be the true causes. Lamarck, on the contrary, and those who reason like him, content themselves with

the mere *assertion* that phenomena exactly according with existing phenomena will, or rather may, result from the physical causes to which their theories refer them. Thus, Lamarck asserts that from some gelatinous or mucilaginous mass, nature, in some way or other, forms the cellular tissue which becomes a living mass when acted on by the supposed circumambient medium. This constitutes, according to his theory, the *origin of life* or an act of *direct* or *spontaneous generation*, the power of subsequent generation being supposed to be given to the mass in common with the other vital powers thus communicated to it. But here we have nothing beyond bare assertion. There is not an attempt to define the intimate nature of the physical causes assigned or their mode or laws of action, much less to explain even the possibility of their producing the simplest phenomena of life. Where the philosopher who forms his theories of the physical phenomena of inorganic matter feels himself imperatively called upon for *demonstration*, the philosophical naturalist, while he appeals to the same principles of investigation, is here content to offer us his simple *assertion*. And yet the great naturalist of whose theories we are speaking remarks with the most perfect complacency, '*On ne sauroit donc douter** que des portions de matières inorganiques appropriées, et qui se trouvent dans un concours de circonstances favorables, ne puissent, par l'influence des agens de la nature, dont la chaleur et l'humidité sont les principaux, recevoir dans leurs parties cette disposition qui ébauche l'organisation cellulaire, de là, consequemment, passer à l'état organique le plus simple, et dès lors jouir des premiers mouvemens de la vie.' It is to the total absence of even an attempt at anything resembling demonstration that we would here especially direct the attention of our readers. The same fault characterizes all theories of vital phenomena. It will be our object to point out similar defects in such other theories as we propose to examine.

It has been too much the custom, we fear, among the opponents of views like these above mentioned, to brand their authors indiscriminately with the names of pantheists and atheists. In too many cases such appellations may have been well merited, but certainly not in all, whatever may be the necessary tendency of these doctrines. Lamarck constantly recognises a Supreme Intelligence and great Author and Source of all things; but he maintains that in the act of creation the Creator impressed on matter such properties, and subjected it to such laws as might not only be necessary, but also sufficient for the future maintenance of the universe, and for the production of all subsequent natural phenomena, without any further exertion of His creative or controlling power. At the same time the manner in which he extends his physical theory of life to man as a moral and intellectual being is manifest materialism.

We now come to that part of Lamarck's theory in which he professes to account for the continued organic development of a mass which he supposes to have acquired the properties of vitality in the manner above described. It is this part of his theory which is exactly parallel to, and so much resembles, that recently proposed by Mr. Darwin. The latter author declines entering on any theory, like that above mentioned, of the origin of life, nor does he, like Lamarck, attempt to follow out his views to their legitimate consequences in their application to man as an intellectual and spiritual being. He restricts himself to the explanation of the manner in which he conceives the organization, whether of plants or animals, to have advanced from its lower to its higher stages, thus producing by a persistent advance all their peculiarities of organization. Lamarck also, in the corresponding

* The italics are our own.

part of his theory, endeavours to explain how, according to his views, the same changes have taken place. In examining these theories we shall first point out the common physical hypothesis on which Mr. Darwin's theory and this corresponding part of Lamarck's may be considered to depend, and likewise to indicate the distinction between these and certain other theories which might be erroneously regarded equally as physical theories.

And here it is necessary to have a distinct understanding of the meaning we attach to the word *species*, which has frequently been used by naturalists in two senses, widely different in respect to the points immediately under our discussion. The two meanings may be distinguished by the terms *natural* and *artificial*. By a natural species we mean a group of organic beings which can only have been derived by descent from beings similar to themselves, which possess certain external and anatomical characters distinguishing them from every other group, and whose descendants must necessarily inherit the same distinctive characters. Artificial species consist of groups which are equally distinguished by particular characters, but are such that, so far as the definition is concerned, they may not have been derived from each other, or from some common original stock. In the former case the grouping is formed by nature; in the latter it is arbitrary, and only used in subservience to the convenience of classification, though generally made to coincide, as nearly as our knowledge will allow, with a perfectly natural classification. Every natural species must by definition have had a separate and independent origin, so that all theories—like those of Lamarck and Mr. Darwin—which assert the derivation of all classes of animals from one origin, do, in fact, deny the existence of natural species at all. The classifications adopted by those who hold the opposite opinion vary in some measure from each other, and therefore are liable to the charge of error in deviating from the natural lines of demarcation which they believe nature to have drawn. But this is the manifestly necessary consequence of the imperfection of our knowledge, and affords not the slightest argument against the existence of natural species, though some naturalists have appealed to it as such. They might as well contend that the solar spectrum does not consist of various colours, because we are unable to define the exact lines which separate them.

Lamarck's theory, as we have already intimated, if it could be established, would be more complete than Mr. Darwin's, since the latter does not profess to give any account of the origin of vital phenomena. In the former the physical cause assigned for the production of these phenomena must still be considered active, according to the theory, in all the phenomena of the continued existence and progressive advance of animal life. From the utter failure of the author's attempts, however, to establish the most remote relation between the real phenomena of life and his hypothetical cause of circumambient fluids, of heat, electricity, &c., we may altogether dismiss his original physical hypothesis, in the consideration of that part of his theory which embraces the same phenomena as Mr. Darwin's. With this restriction, it may be enunciated for both theories in common, that they refer all the phenomena of the varied organization in the animal kingdom to the continuous operation of the ordinary natural causes to which we ascribe the growth of each individual animal, or of the organs which compose it, and the propagation of its species, but without further consideration of the intimate nature of those causes. They equally maintain that the highest organisms in nature have been derived from the lower ones by some *continuous* and unbroken line of descent. These theories, then, are distinguished by the character of *linear continuity* which they assign to the chain of beings which have

connected each existing organism with the simple one from which, according to these theories, it has proceeded. The theory commonly received asserts, on the contrary, the existence of *natural species*, each of which, since by hypothesis they are incapable of being derived from each other, must have had an independent origin. Moreover, it has been usually supposed that each of these species originated in an independent act of the Creator, towards whom the adoption of any other theory has frequently been regarded as an act of irreverence. Thus, as we have already remarked, the religious or theological aspect of the question has been placed in antagonism with its scientific aspect. We have already expressed our regret that such should be the case; and to escape the risk of it at present we shall endeavour to avoid even the phraseology of the theological view, and to state in the language of science that view of the subject which may be regarded as antagonistic to such theories as above described, and which would appear to be our obvious alternative in the case of our rejection of them.

By an act of creation considered with reference to scientific investigation, we mean a result due to some cause beyond and above those secondary causes to which the ordinary phenomena of nature are considered to be due. And we should define the difference between ordinary and extraordinary phenomena to consist in this—that in the former there exists a certain character of *continuity*, and in the latter there exists an equally distinct character of *discontinuity*. To elucidate our meaning, we may remark that the motions of the heavenly bodies are continuous, inasmuch as they are subject only to gradual or continuous variations. If, on the contrary, the velocities of these bodies, or the directions in which they move, or the orbits which they describe, were subject to sudden, instantaneous changes, the phenomena of their motions would have an obvious character of discontinuity. Similar remarks would apply to any other ordinary natural phenomena of inorganic matter. Again, with respect to organic matter, each plant or animal is gradually and continuously developed, and propagates descendants of the same character as itself. Such phenomena have a character of continuity; if the growth, on the other hand, were suddenly arrested or accelerated at particular stages, or if each organic being produced offspring totally unlike itself, the phenomena would be discontinuous. Now, it will be at once admitted that any such discontinuities in the existing phenomena must necessarily indicate a corresponding discontinuity in the action of the physical causes producing them, or in the laws according to which those causes act. But in the phenomena which we all agree to refer to natural or secondary causes, we observe no such discontinuities as those above-mentioned, and we consequently conclude that every cause which we ordinarily recognise as belonging to secondary natural causes, must be supposed to act according to some *continuous law*. This conclusion has probably been tacitly assumed, as it has been by Lamarck, by every one who has speculated on the natural causes to which the phenomena of either the organic or inorganic world are to be attributed. Mr. Darwin thus recognises it in the continuity of the changes which he supposes animals and plants to have undergone in the gradual development of one specific form from another.

Hence, then, the fundamental distinction between the theories we have been discussing, and that which asserts the independent creation and existence of natural species, regarded as a physical theory, and under its merely scientific aspect, is this—that the former recognise only those *continuous* physical causes which produce the ordinary phenomena of nature; whereas the latter, in addition to these causes, recognises a higher order of causation, acting according to some law which, in our ignorance of its nature, we are obliged to describe

as *discontinuous*. Moreover, this discontinuous causation may be supposed to act in accordance with the continuous causation before mentioned, and not altogether independent of it. We are of course utterly ignorant of the relation which may exist between these two sets of physical causes, and make no hypothesis respecting it. We are, in fact, equally ignorant of the intimate nature of all the causes, whether continuous or discontinuous, with which we are now concerned. And here it may be remarked, that there is nothing new in the idea of organic matter being under the dominion of two sets of forces, the continuous and the discontinuous. We have in like manner two distinct sets of forces acting on all matter: that of gravitation, producing its effects at finite distances; and molecular forces, which produce their effects only at distances which are infinitely small. These two sets of forces may probably be in some way related to each other; but so far as we are able to contemplate their effects, they must act according to widely different laws. The only difference between this and the above case is that physical action, according to any discontinuous law, is restricted to organic matter.

Let it not be supposed that we are professing to enunciate a new theory. We have only done what might have been done by saying that new species, or any such discontinuous phenomena, are due to separate acts of creative power, but that those acts have a certain reference to previously existing phenomena, according to some self-imposed law in the Divine Mind. We have merely translated some such enunciation as this into the more ordinary language of science.

In order that a theory may have a just claim to be called a *physical* theory, it must assign some determinate physical cause to which the phenomena involved in it are considered to be due. The two theories above-mentioned belong to this class, although, as above stated, they do not define the nature and mode of action of the physical causes referred to by them, except by assuming them to be the same as those which produce the ordinary phenomena of nature. Other views, however, which have been offered ought not to be regarded in the same light. The theory of *polarity* suggested by the late Professor Edward Forbes asserts that up to certain epochs in geological time (as, for instance, the termination of the palæozoic period), the development of *generic* forms gradually decreased from some previous period, and then began to increase; or, as it may be stated, this development increased in going forward in the order of time, or upwards in that of the geological formations from the end of the palæozoic period, and also increased in proceeding from the same epoch backwards in regard to time, and downwards in regard to geological formations. This he termed *polarity*. But if we regard it merely as a statement of a law which characterizes observed phenomena, it is manifestly not a physical theory in the same sense in which that of gravitation, or those of Lamarck and Mr. Darwin are such, for it asserts, as we are now supposing, no *physical cause* for the phenomena of which it speaks. It is, in this sense, a *generalization* similar to those of which we have spoken as *geometrical* generalizations in the theories of gravitation, physical optics, and crystallography. In these physical theories the proofs of their being true must consist in demonstrating a necessary relation between the observed facts and some physical cause to which those facts are assigned; but in this theory of polarity the proof must consist in determining by simple observation whether the phenomena do or do not follow the law ascribed to them. If, however, the term *polarity* is intended, on the contrary, to convey the idea of some particular physical cause to which the phenomena are due, the theory becomes a physical theory, which we estimate, as such, at the lowest value, since so far from tracing the action of some definite physical cause, it does not even

assign such cause in any comprehensible terms.

We may instance another case, which has more the character of a geometrical generalization than of a physical theory. We allude to Mr. Wallace's views respecting the law which has regulated the introduction of new species. The author thus enunciates the law here alluded to—'Every species has come into existence coincident both in time and space with a pre-existing closely allied species.' Mr. Wallace has put forth this view in a clear and striking manner, so far as it is represented as a generalization of observed facts, which show a juxtaposition in time and space of allied species; but to convert it into a physical theory in the proper sense of the expression, some physical cause should be assigned, from the action of which this law of the phenomena would result. But there is no allusion to such cause. And therefore it is that we cannot approve of the assertions that the most singular peculiarities of anatomical structure are explained by it, and that many of the most important facts in nature are almost as necessary deductions from it, as are the elliptic orbits of the planets from the law of gravitation. There appears to us to be some confusion in these modes of expression, between the *law by which phenomena may be characterized*, and *the physical causes to which they may be due*, and which account for the law. We submit that the above assertions are no more correct than the assertion would be that the elliptic motion of the older known planets explains and accounts for that of Neptune or of the tribe of minor planets recently discovered. These latter motions are *in accordance* with the laws of elliptic motions, but are *explained* by the action of mutual gravitation; and the phenomena of organic matter appealed to by Mr. Wallace would, in like manner, be properly explained and accounted for by a theory which should prove the law here stated to be the necessary consequence of some determinate physical cause. We may also add that Mr. Wallace's theory cannot supersede that of polarity, as the author seems to suppose; for no physical cause is assigned which would be necessarily inconsistent with the law of polarity; nor, regarding both theories merely as generalizations of observed phenomena, can Mr. Wallace's law supersede that of Professor Forbes, since it is manifest that the truth of the one does not necessarily involve either the truth or falsehood of the other.

Much of what we have here said may possibly be deemed hypercritical. We think otherwise; for we believe that there is no inconsiderable amount of confusion of language and thought on the higher speculative subjects of which we have been speaking; and we are convinced that we can neither theorize philosophically ourselves on such subjects, nor judge rightly of the theories of others, without a clear and logical conception of those distinctions which we have been desirous of indicating.

Let us now recur to Mr. Darwin's theory and the corresponding part of Lamarck's. Having pointed out the hypothesis on which they may be considered in common to rest—viz., that all vital phenomena are due to the action of ordinary secondary causes—we may proceed to examine how they attempt to deduce from these causes the phenomena in question, but more especially those which relate to the varied forms of animate existence. In the absence of all knowledge of the real nature and laws of vital forces, it is impossible to investigate directly the consequences of those forces under any assigned conditions, as the mathematician calculates all the effects of gravitation; and this constitutes an enormous difference between our best physical theories and the theories of vital phenomena. In the latter, instead of the above direct and perfect mode of investigation, other indirect and far more imperfect modes must be substituted. In the two theories before us, the authors lay down certain principles by which they conceive ordinary natural causes to be regulated in the

production of the phenomena referred to them. The principles thus laid down by Lamarck are involved in the three following assumptions:—

1. That any considerable and permanent change in the circumstances in which a race of animals is placed, superinduces in them a real change in their wants and requirements.

2. That this change in their wants necessitates new actions on their part to satisfy those wants, and that finally new habits are thus engendered.

3. That these new actions and habits necessitate a greater and more frequent use of particular organs already existing, which thus become strengthened and improved; or the development of new organs when new wants require them; or the neglect of the use of old organs, which may thus gradually decrease and finally disappear.

The principle on which Mr. Darwin's reasoning rests is that of *Natural selection*. An immensely greater number of animals must be born than can possibly live to what may be regarded as the natural term of their lives. A 'struggle for existence' (to use our author's phrase) must therefore necessarily ensue; and in this struggle the stronger will vanquish the weaker, and live to transmit their species to future generations. Thus nature is supposed to 'select' the best variety* of any existing species, however it may have arisen, for the propagation of the race, as a breeder of domestic animals selects the best of those he may be cultivating, to breed from; and as he dooms to more immediate slaughter the inferior portion of his flocks and herds, and thus year by year improves his stock, so nature abandons to early destruction and final extermination those inferior portions of any race of animals which are least able to protect themselves in the 'struggle for existence,' and thus improves the general character of succeeding generations, to which every improvement is supposed to be transmitted by descent. Mr. Darwin does not appear to consider that much influence is due *directly* to the external conditions of climate, food, &c., in the advance of any race of animals. He remarks (p. 85), 'We must not forget that climate, food, &c., probably produce some slight and direct effect. It is, however, far more necessary to bear in mind that there are many unknown laws of correlation of growth, which, when one part of the organization is modified through variation, and the modifications are accumulated by natural selection for the good of the being, will cause other modifications of the most unexpected nature.' He also intimates his opinion that the causes which produce varieties may likewise affect the generative powers of any existing race, so as to enable it to propagate descendants of improved organization.

The great similarity of the notions on which the reasoning of these two theories is based, is obvious; but Lamarck insists more on the direct influence of ordinary external conditions, and Mr. Darwin more especially on that of natural selection. But this difference is perhaps as much in appearance as in reality; for if Lamarck could have been asked whether he included the struggle for existence among the conditions which might influence the wants and habits of animals, and consequently their organization, he would necessarily have answered in the affirmative; and on the other hand, we understand Mr. Darwin to affirm that there could be no natural selection without varieties, and it seems difficult to understand how natural selection could operate in perpetuating one variety rather than another, except with reference to those circumstances which, according to Lamarck's view, would modify the wants and constitution of existing species. Again, while

* The accurate distinction between *varieties* and *species* consists in this—the former, when crossed, always produce fertile offspring; the latter either do not admit of being crossed at all, or when crossed they produce sterile offspring.

Lamarck believed that any modification of the organs of an animal which its altered wants might lead it to desire, would be accorded to it, and thus lead to its indefinitely progressive improvement, Mr. Darwin concludes that any improvement in the form of a variety will be transmitted to future generations, and by a cumulative process, elevate a species in the course of time, from the lowest to the highest order of organization. We confess that we find some difficulty in recognising any very essential difference in the fundamental hypotheses on which these two theories rest as physical theories. In Lamarck's an animal is incited to exertion by some kind of external circumstances, and obtains an improved organization as the reward of his efforts; according to Mr. Darwin's theory the same advantage is gained, but (if we may use the expression) rather by an accident of birth. We are disposed to think the less aristocratic process somewhat the preferable one. However, both are supposed to be equally cumulative, and to lead equally to a continuous slow advance of the race from the lower to the higher organizations. They both, consequently, deny the existence of groups of animals which have descended only from progenitors like themselves. In other words, they equally ignore the existence of natural species, and only recognise specific groups as convenient for the purposes of classification.

Mr. Darwin, in his *Origin of Species*, has entered much more fully than Lamarck into the detail of facts bearing on the subject. In our discussion of it we shall chiefly follow the work just mentioned. And here we would especially remark, that whatever opinion may be formed of the conclusiveness of our author's reasoning, every reader will acknowledge the great value of his observations respecting many of the most interesting facts and speculations which natural history presents to us. It is a work which will doubtless be found in the hands of every professed naturalist, while many others, without necessarily subscribing at all to its general theory, will probably find that its perusal has greatly enlarged their views on the subject, and given them a more adequate conception of the various questions of interest which it presents to the general reader. We are anxious to bear our most cordial testimony to Mr. Darwin's work on these points, as well as our full participation in the high respect in which the author is universally held, both as a man and a naturalist; and the more so, because in the remarks which will follow in the second part of this Essay we shall be found to differ widely from him as regards many of his conclusions and the reasonings on which he has founded them, and shall claim the full right to express such differences of opinion with all that freedom which the interests of scientific truth demand, and which we are sure Mr. Darwin would be one of the last to refuse to any one prepared to exercise it with candour and courtesy.

WILLIAM HOPKINS.

only true mode is a skilful course of cultivation adapted to the nature of the soil. In acclimatizing plants there is a limit of heat and cold beyond which you cannot advantageously pass. If the plant is not killed by a temperature contrary to its nature, it becomes stunted in its growth, sickly in its foliage, and poor in its colour. It is the same with animals. An Arctic animal may live, but cannot flourish beyond the circle, and as he is moved southward he droops and dies. In like manner a denizen of the tropics perishes from cold. If a sort of artificial climate is prepared in both cases, the animals may exist, but piningly. So it is with moral or intellectual qualities. The true wisdom is to cultivate the natural bent of the mind in which nature will second effort; for though it may perhaps be possible to induce a sort of artificial character in an opposite direction, it will only be available on common or indifferent occasions. When the real strain comes, the artificial character will break down, not only failing at the time when it is most wanted, but perhaps after causing artificial difficulties which would not have arisen had the man been left to his natural bent.

W. W.

PHYSICAL THEORIES OF THE PHENOMENA OF LIFE.

Part II.

WE proceed to further remarks which we have to offer on theories of vital phenomena, more especially on that of Mr. Darwin in his *Origin of Species*.

Mr. Darwin states that he has numerous facts which he still holds in reserve for future publication, but it is not our intention to follow him even into the details which he has already given. It would suit neither our space nor our purpose to do so. Besides, many details are always apt to perplex the mind and to draw it off from general principles and real arguments, to which it is here especially our wish to direct the attention of our readers.

The principal arguments, for both Mr. Darwin's theory and Lamarck's, are drawn from the influence of domestication on the specific characters of animals, and that of cultivation on those of plants. All species must be subject to greater or less variation, for it is all but inconceivable that any two animals should be exactly alike; and the peculiar circumstances under which domestic animals are placed, as compared with those in a state of nature, has doubtless been one great cause of their presenting so much greater variations than we observe in wild animals. This variability enables man to modify our domestic breeds still further by his power of selection, and of thus cultivating any particular variation which he may wish to promote. 'Like breeds like,' is the great principle of all breeders who have most improved our domestic animals, though it is in fact by the variations from this rule that they are enabled to effect their improvements. The principle itself asserts the transmission by descent of any acquired property, and therefore the general likeness between the parents and their offspring; but the power of man to improve a race, with reference to any particular property it may possess, depends on his being able to select those particular individuals of each successive generation in which this property is more especially developed, in order that they may transmit it in its improved form to their posterity. It has been by this means that breeders have been able within the last three-quarters of a century to effect such marvellous improvements, especially in cattle and sheep. Bakewell, who resided at Dishley, near Loughborough, in Leicestershire, was the first to recognise and apply

scientifically this principle to the cultivation of those animals. Breeding became in his hands an art, which has since exercised the most important influence on the science of agriculture in our own country, and is now widely extended in America and France, and, in fact, in all countries where the importance of improving those animals on which man is so largely dependent for the supply of his necessities and enjoyments, is fully recognised. Horses and dogs are also races of animals to which this principle of selection has been largely applied; and pigeons are especially referred to by our author, on account of the curious and extensive modifications which have been effected in them by the continued application of the same art. These and other similar cases are appealed to by Mr. Darwin in proof of the influence which man possesses, by his power of selection, in extending the variations of which any race of animals under his constant control may be susceptible. They are the facts which have suggested our author's fundamental idea of *natural selection*, and form the very corner-stone of his theory. If man has done so much in so short a period of time, what may not nature have effected, he asks, during the illimitable ages in which she has been exercising her powers?

In discussing this question, we must consider whether nature and man act in the same or in different ways. Now it appears to us that man only exercises his influence in modifying organic forms, by counteracting the laws of nature, or rather by altering the conditions under which nature would otherwise act; and that, consequently, we have no right whatever to assume that nature will *necessarily* produce such effects at all when left to her own unobstructed operations, as those which she produces under man's interference. It is not man who is operating, but nature; in the one case under ordinary conditions, in the other, when those conditions are interrupted; and the above assumption is equivalent to the assertion that in either case effects of the same kind will be the necessary result. An elucidation may perhaps occur to the mind of a geologist, of the manner in which man is able to counteract some of the slowly operating though apparently almost irresistible agencies of nature, by the power which he possesses of resisting the ravages of the ocean waves on the shores on which they are incessantly breaking. Man, by means of works which he is able to construct, can not only withstand in many cases the efforts of the waves to encroach on the land, but can, when so minded, reclaim land from the dominion of the sea; and it might be said that if man can do so much in a finite time, what may not nature accomplish in rescuing dry land from the ocean during the enormous period in which her agencies have been at work? This question, we say, might be asked in this case as well as in the actual case before us, did we not know that the inevitable general tendency of the sea is to swallow up the dry land, with little disposition to disgorge it again. It is here, strictly speaking, as in the case of man's influence over the modifications of organic characters, an error of language to say that *man* recovers dry land from the ocean; it is the ocean that produces this effect, while acting under the artificial conditions which man is able to impose upon it. If in this case we should infer that natural causes, acting under ordinary conditions, would produce effects of the same kind as those produced under certain artificial conditions, we should commit a manifest error; and in like manner we may commit an error in asserting that nature, under ordinary conditions, will produce modifications in the essential characters of organic beings, such as she may produce under the artificial conditions to which our domestic animals are subjected. Certain it is that Leicester sheep and Durham short-horns were never produced by unaided nature.

But our author asserts that nature is always ready to render any variation permanent when it is for

the good of the individual and its race. We quite believe that this is one way in which the beneficence of the Creator may be displayed. But what *is good* for an animal? Is it necessarily good that a sponge or a polyp should be promoted to the rank of an oyster, that the ambition of the oyster should aspire to the position of a cephalopod, or a cephalopod to that of a fish, and so on? We should have thought it far more likely that the advantage which may thus be accorded to any organic being was intended for its good in the sphere in which it was born, but not to raise it above that sphere. At all events, we are sure that there can be no such probability on the side of our author's view as to constitute any positive *à priori* argument in its favour. He has a perfect right, however, to make such an hypothesis; but we maintain, on the ground of what has been advanced in this and the preceding paragraph, that, admitting the existence of natural selection, there is no more reason *à priori* for believing that it has the power of raising an organic being from a lower to a higher stage of organic life, than there is for supposing its power restricted to those limits which the distinctive characters of natural species would impose upon it. The onus of proving the existence of such power must rest with him who asserts it.

It is the opinion of some naturalists that there is a tendency in every variety to return to the original characters of the stock whence it has proceeded, and this tendency Mr. Darwin has designated as the *principle of reversion*, considering it not as depending on crossing with other varieties of the same species, but as an inherent property of every variety, though all intercrossing be prevented. In this sense he does not consider the principle established by any adequate experiment. But this is not the tendency (allowing it really to exist) which we should regard as most effectively antagonistic to that of natural selection. The real antagonistic tendency is that which exists to return to the form of the original stock by the intercrossing of different varieties. It is by the prevention of this process that man imposes artificial conditions so effectively on the operations of nature, and encourages the production of varieties. Remove all these artificial restrictions from any of our domestic races, and there can be no doubt, we conceive, of their returning rapidly through a series of mongrel breeds to a common type, modified only by ordinary local conditions of climate, food, &c., and the struggle for existence with other animals of the same or different species. If it could be proved that there is a predominant tendency in the more perfect and robust of each species to pair with individuals like themselves for the transmission of their kind, then the necessary existence of natural selection as an operative cause must be admitted, and the only question would be as to the extent of its operation. But we have no reason to believe that nature would thus favour the ambition of rising indefinitely in the scale of organization. The despotic will of one of the most despotic monarchs that ever lived could not call forth a race of grenadiers taller than nature chose to furnish him with. The elective affinities were against him, and short women would league with tall men, as if it were on purpose to frustrate the designs of the monarch who presumed to interfere with wills which were stronger in vindicating the law of nature than his own to violate it. Moreover, the experience of breeders of our domestic animals, with the more matured knowledge which later years have given them, indicates, we conceive, a distinct limit to the improvement which they may be able to effect in any direction. We have had considerable opportunities of witnessing the improvements which have been wrought, more especially in our sheep and cattle, and there can be no doubt but that each step in advance, in whatever direction it may take place, becomes more and more difficult, and the greater the perfection obtained, the greater is

the attention required to prevent regression towards some inferior type. There are in fact two antagonistic causes at work, producing a tendency on the one hand to ascend, and on the other to descend. The ascent becomes more difficult as we advance, while the tendency to descend increases under the same circumstances; and thus we conceive it to be most probable that these two tendencies come at a certain point to counteract each other, and that therefore no advance can be made beyond that point. We are not asserting this as conclusive against Mr. Darwin's view of the continued advance due to natural selection; but we do assert it as proving that he does not establish *à priori* any necessary probability in its favour. If this cause be really operative to the extent he imagines, the fact must be entirely established by inductive reasoning, and a careful comparison of the consequences which must necessarily flow from the operation of this cause, with the phenomena which nature presents to us. We are not denying our author's right to reason upon his hypothesis as the foundation of his theory. He has an unquestionable right, according to the laws of all philosophical reasoning, to do so. We merely maintain that the soundness of his theory is not to be judged by this foundation, but by the superstructure which can be raised upon it. Let us proceed, then, to consider what must necessarily result from the truth of this hypothesis.

It is manifest that every change which takes place according to Mr. Darwin's theory, and equally so according to Lamarck's, must be strictly *continuous*, so that if we possessed specimens of each organic form which, since the first existence of organic life, has been derived from an immediately preceding form, the series would constitute an unbroken chain. It is not that organic forms of every kind would lie in one and the same unbroken line of descent. There may be an indefinite number of these continuous lines, all springing from the same origin, but branching out in different directions, either from that origin or subsequently from other points, precisely in the same manner as the branches of a tree diverge from its trunk, secondary branches from the primary ones, and so on till we come to the finest twigs and leafstalks. Mr. Darwin has given a diagram fully illustrative of this idea. But in accepting this illustration we must carefully mark the limitation implied by it as to the nature and extent of the law of continuity by which organic forms are connected. This continuity is not general, but what may be termed *linear*, in the sense in which each branch of a tree is continuous in itself without having any immediate connexion with the neighbouring branches. And as the branches of a tree, after their divergence from each other, do not again unite together, so according to this analogy the organic beings which have descended along different lines will be incapable of amalgamating with each other and thus producing intermediate forms. Admitting this kind of linearity, the discontinuity here spoken of would be a necessary result of this law, just as the separation of the ultimate branches of a tree is a necessary consequence of its mode of growth. But here it should be observed that, as the branches of the tree separate entirely at their common point of divergence, so, according to this hypothesis of linearity of descent, organic beings, even in their initial state of divergence from each other, and therefore while nearly identical, must still be incapable of amalgamation, either from their nature or the external conditions to which they may be subjected; for otherwise it is manifest that their union would destroy the separation of the two distinct lines of descent, and thus produce a *general* and not merely a *linear* continuity between all existing organic forms.

It has sometimes been said that, according to any theory like Mr. Darwin's, all different groups of animals must merge into each other, and all distinct lines of

demarcation be obliterated. It is manifest, however, that such an objection is not valid if we admit the linearity of development above described; for there might be any amount of difference between two organic forms situated on two distinct lines of descent, though diverging from the same stock. But again, it may be said, that at least along these separate lines of development we ought to find in organic forms now existing, or in those which have existed during the historic period, indications of this gradual transition from one form to a consecutive one. But this argument also, if restricted to the evidence afforded by the historic period, is not necessarily a valid one, for it may be asserted that the variation of organic characters has been so slow as to be altogether inappreciable within that period. Thus we may conceive the horse, the camel, or the crocodile, for instance, to have varied within the last three thousand years in accordance with the law of continuous development, but still so slowly as to render the amount of progression insensible to us. Hence it follows that neither Lamarck's theory nor Mr. Darwin's, nor any other requiring only a linear and not a general continuity of development, is necessarily inconsistent with those differences which naturalists recognise as constituting generic or specific distinctions. Passing over, then, these objections as untenable, let us proceed first to consider the proof required for the establishment of the fundamental hypothesis—that natural selection does so act as to produce different developments diverging from the same organism.

Varieties, as already stated, are distinguished from natural species by their perfect fertility *inter se*. According to Mr. Darwin's theory, these varieties are supposed to diverge till they become species, *i.e.*, till they can no longer unite with other organic beings which have diverged from the same stock, so as to produce fertile offspring. It might be supposed that this kind of negative property of sterility was acquired directly, according to the theory, by the assumed process of natural selection. This, however, is not our author's view. He supposes other qualities to be acquired by that means, and sterility to result from them as secondary effects, according to the laws of the correlation of growth. In his chapter on Hybridism he remarks (p. 245):—

> The importance of the fact that hybrids are very generally sterile has, I think, been much under-rated by some late writers. On the theory of natural selection the case is especially important, inasmuch as the sterility of hybrids could not possibly be of any advantage to them, and therefore could not have been acquired by the continued preservation of successive profitable degrees of sterility. I hope, however, to be able to show that sterility is not a specially acquired or endowed quality, but is incidental on other acquired differences.

After these observations the author's object is to break down the line of demarcation between *varieties* and *species*, founded on the perfect fertility of the former when crossed with each other, and the sterility or very imperfect fertility of the latter when similarly crossed. But in doing this he confounds artificial with natural species, to which alone the above distinction from varieties is, of course, asserted. In the chapter just mentioned he says, in speaking of the sterility of species when crossed, or that of their hybrid offspring (p. 246):—

> It is impossible to study the works of these two conscientious and admirable observers, Kölreuter and Gärtner, who almost devoted their lives to this subject, without being deeply impressed with the high generality of some degree of sterility. Kölreuter makes the rule universal; but then he cuts the knot, for in ten cases in which he found two forms, considered by most authors as distinct species, quite fertile together, he unhesitatingly ranks them as varieties.

Kölreuter is here accused of cutting the knot of a difficulty, but unjustly. The universality of the rule which he asserts is with reference to *natural* and not *artificial* species, and his definition of the term 'species' rendered it imperative upon him to rank the ten ex-

ceptional cases as varieties. Again, Mr. Darwin says (p. 267):—

> It may be urged, as a most forcible argument, that there must be some essential distinction between species and varieties, inasmuch as varieties, however much they may differ from each other in external appearance, cross with perfect facility, and yield perfectly fertile offspring. I fully admit that this is almost invariably the case. But if we look to varieties produced under nature, we are immediately involved in hopeless difficulties; for if two hitherto reputed varieties be found in any degree sterile together, they are at once ranked by most naturalists as species. For instance, the blue and red pimpernel, the primrose and cowslip, which are considered by many of our best botanists as varieties, are said by Gärtner not to be quite fertile when crossed, and he consequently ranks them as undoubted species. If we thus argue in a circle, the fertility of all varieties produced under nature will assuredly have to be granted.

It is a grave and very uncomplimentary charge against any one who assumes to himself the possession of a reasoning faculty, to say that he has exercised it only within the narrow limits of a vicious circle. In the above cases, however, Gärtner and the naturalists who have adopted similar views are easily justified. They define species as *natural* species, and propose a classification strictly consistent with their definition. The distinction to be drawn is not, as already remarked, between varieties and *artificial* species, or what may, in the imperfection of their knowledge, have been conventionally regarded as species by naturalists, but between varieties and *natural* species. There is here no reasoning in a circle. Kölreuter and Gärtner both assert the absolute generality of this distinction in reference to the vegetable kingdom, adopting their own proper definition of 'species;' and we must deny that the one weakens the force of his conclusion by cutting the knot of a difficulty, or the other by any slip in his reasoning.

In fact we have nothing here to do with artificial lines of distinction between species, but with those which are real or natural; nor does the validity of our reasonings depend on an exact knowledge of the positions of these latter lines, but only on their existence.

But it may be asked, How do we know that such lines actually exist at all? And how do we know, it may be asked in return, that natural selection is actually operative at all in nature? Both these questions are altogether irrelevant. In answer to the latter it would of course be said, and justly, that it is not an asserted truth, but a fundamental hypothesis, which in the building up of the theory founded upon it must be reasoned upon as if it were an essential truth. And so the existence of the distinctions above spoken of is a fundamental hypothesis of any theory which asserts that different groups of organic forms have originated in successive creations; and consequently such distinctions must be reasoned upon as if their existence had been completely proved. Should any such theory be hereafter established, these distinctions will then be recognised as demonstrated truths; but if, on the contrary, any antagonistic theory should be established, though these distinctions might co-exist with lines of continuous descent, they must be given up in the more general sense in which the doctrine of successive creations asserts their existence. To deny *à priori* their existence would be an absurdity in the abstract reasoning of the subject; for it would be to assume at once the truth of some antagonistic theory which it is the object of our reasonings to establish or refute. The ultimate object of the discussion is to prove or disprove the existence of *natural* species, distinguished from varieties as above stated. With *artificial* species we have no concern.

It may be stated as a fundamental proposition of Mr. Darwin's theory, *that varieties distinguished by the property of perfect fertility, when crossed, may so far diverge from each other in the process of time, as to be incapable of producing fertile offspring when similarly crossed.* This is the proposition to be esta-

blished by experiment and observation. The animals on which we should naturally expect experiments of this kind to be made are those of our domestic breeds, on which man has in fact ever been experimenting, consciously or unconsciously. But of the infinite number of such experiments, we are not aware of one which affords the slightest positive proof of the above proposition. If we continue to breed *in and in*, as Mr. Darwin has somewhere remarked, the variety may in turn dwindle away and disappear; but on being crossed with another variety of the same stock, it seems immediately to regain its pristine vigour and fertility, in exact opposition to the above proposition. If we allow full weight to all our author's arguments in his chapter on Hybridism, we only arrive at the conclusion that natural selection *may* possibly have produced changes of organization, which *may* have superinduced the sterility of species; and that therefore the above proposition *may* be true, though not a single positive fact be adduced in proof of it. And it must be recollected that this is no proposition of secondary importance—a mere turret, as it were, in our author's theoretical fabric—but the chief corner-stone which supports it. We confess that all the respect which we entertain for the author of these views has inspired us with no corresponding feeling towards this *may be* philosophy, which is content to substitute the merely possible for the probable, and which, ignoring the responsibility of any approximation to rigorous demonstration in the establishment of its own theories, complacently assumes them to be right till they are rigorously proved to be wrong. When Newton in former times put forth his theory of gravitation, he did not call on philosophers to believe it, or else to show that it was wrong, but felt it incumbent on himself to prove that it was right. And this must be the rule for the philosopher who advances his theories on the phenomena of life, equally with him who speculates on the properties of inanimate matter. We know that, in the case before us, no investigation can at present be founded on the action of the molecular forces which govern the combinations of animate matter, on account of our total ignorance of such forces; and so far these theories must be comparatively imperfect; but what we require is some proof of fundamental propositions, founded on observation or experiment, as above stated. Till this or something equivalent to it be done, we maintain that no theory like Mr. Darwin's can have a real foundation to rest upon, or can ever secure the general convictions of philosophers in its favour.

But let us pass to another point, one also of the first importance in any theory involving continuity of development. We have already pointed out that no unanswerable argument against any such theory is deducible from the absence of evidence to prove that any appreciable change has taken place in existing organisms during the historic period, simply because it may require a longer period to effect such change. But the question assumes a very different aspect when considered with reference to the geological period, during which such important changes in animal organization have undoubtedly taken place. It is, then, in the records of geology that we must seek for the evidence of that continuity by which, according to the theories we are considering, these changes must have been characterized. Now, all are agreed that palæontology affords no evidence of this continuity; and it is in the absence of this evidence that we meet with one of the gravest objections to any theory of continuous organic variation. Mr. Darwin has stated this difficulty with all that fairness and impartiality which so eminently distinguish his work in the statement of facts opposed to his views. He allows that the number of organic forms which must have existed intermediate to those which are known must have been enormous, and admits that it may naturally be asked (p. 280),

'Why, then, is not every geological formation and every stratum full of such intermediate links? Geology surely does not reveal any such finely graduated organic chain; and this, perhaps, is the most obvious and gravest objection which can be urged against my theory.' He then proceeds in his ninth chapter to develop his reasons for believing that the answer to this difficulty is to be found in the imperfection of the geological record.

In the first place we would remark that we entirely agree with our author as to the immense lapse of time indicated by geological phenomena, more especially by the enormous mass of fossiliferous strata which must have been transported by water from one locality and deposited in another, and by a process which must have been so slow that the whole work done must have required an almost inconceivable length of time for its accomplishment; and Mr. Darwin has well remarked, that no one, perhaps, who is not a practical geologist, can adequately realize this impression of the lengths of geological periods. Independently of this conclusion, there is little in this chapter from which we do not gravely dissent, as arguments for destroying almost entirely our confidence in the records of geology. If the discredit which it is here attempted to throw on these records be admitted, we cannot conceive what value can attach to any of the general conclusions of palæontology; and that science which has already effected so immense a revolution in our opinions on some of the most interesting speculative subjects which can occupy the thoughts of men, must become comparatively valueless.

Mr. Darwin's purpose is to account for the want of such an immense number of the links of his chain of graduated organisms, by showing that either these intermediate forms have not been preserved at all in the sedimentary strata, or, if they ever were thus entombed, the record has been obliterated by subsequent denudation. But to arrive at satisfactory conclusions on this subject would essentially require a far more ample discussion of the phenomena of elevation, denudation, and deposition than our author has entered upon. Our space will allow us to make only a few general remarks upon it.

Mr. Darwin observes that a small portion only of animals existing at any past period can have been embedded and preserved in sedimentary beds. No perfectly soft organisms could be thus preserved, and many others thus entombed may have been subsequently destroyed by percolation of water or other solvents. This is undoubtedly true, but it is not the question. We are here essentially concerned with those animals which are best calculated to be thus preserved; and the question is, why, in these particular cases, and not in those in which preservation is comparatively rare, so few of the links of a continuous chain of organisms should have been preserved, when an immense number of contiguous links must have been equally capable of preservation. 'But,' our author further remarks, 'the imperfection of the geological record mainly results from another and more important cause than any of the foregoing—namely, from the several formations being separated from each other by wide intervals of time.' There are doubtless many instances of this kind, as indicated either by an extensive unconformability of stratification, or an entire change in the general character of the organic contents; but these are not the places in which we should look for the continuity of the organic chain; and when no such proofs of discontinuity of deposition on the scale here contemplated are observed, we do not admit any probability of its having occurred. Where two successive strata are perfectly conformable, a difference of mineral character is certainly no necessary or probable indication of a long interval between the periods of their deposition. Our author talks, too, of the frequent

and entire destruction of sedimentary beds. Now we believe the entire destruction of any sedimentary bed of considerable horizontal extent to have been of rare occurrence. All the more important denudations of which we have any evidence have been preceded by large upheavals, by which the strata have been tilted; and thus while those portions of each stratum which have been most elevated may have been exposed to enormous denudation, those portions which have been least elevated, or perhaps depressed, have been thus kept out of the reach of the denuding agencies. The entire obliteration of a stratum would require in general that it should be upheaved in such a manner as never to deviate sensibly from a horizontal position. In fact, this approximation to horizontality must be closer than it frequently may be during the time of deposition, for the smallest dip in an extensive stratum would place it in a condition as to denudation similar to that above described as due to large upheavals. The higher portions might be denuded while the lower remained untouched. The Weald affords one of the best elucidations of denudation accompanied only by the partial destruction of strata. We have no reason to suppose that a single stratum has been obliterated by this denudation, which, while it has left scarcely a remnant of the removed beds in the central portion of the district, has left portions of them untouched on its borders, where they dip beneath the existing surface. The same remarks apply to the oolitic beds of this country, as well as to the enormous denudations of Wales, or the northern parts of England, and in fact to all the great denudations of which geology affords us a record. It is for these reasons that we entertain a strong conviction of the permanence of some portions of all sedimentary beds of considerable horizontal extent. Beds of limited extent may of course have been so deposited as to admit of their subsequent entire obliteration; but with respect to those of larger extent, it is certain that if all the areas of existing land were denuded to the present level of the surface of the ocean, scarcely a single sedimentary bed would be obliterated, simply because there is probably no such bed some part of which does not lie lower than that level. This may elucidate the extreme difficulty of getting entirely rid of any extensive sedimentary bed after it has once been deposited.

During the periods of denudation there could, of course, have been no preservation of organic remains over the denuded area; but deposition of the matter transported from such an area must necessarily have taken place simultaneously with the denudation, and *at no remote distance;* for it is certain that none but the smallest portion of the transported sediment could float to any great distance, in either deep or shallow water. Denudation and deposition must not only be connected by contemporaneity in time, but by proximity in space. Thus deposition of sediment will always be proceeding within areas which may probably have been small compared with those over which the organic forms of any particular kind, existing at that time, may have extended; and in this manner, likewise, the preservation of organic remains may probably have proceeded without intermission, when considered with reference to the whole of one of these areas, though it may have been intermittent in any particular part of it. Hence we maintain that within one of these areas there must be a far more continuous record of its whole geological history than Mr. Darwin admits. Consequently, we cannot but suppose that a far greater number of the intermediate links of his unbroken chain of organisms must have been preserved than those of which we have at present the smallest evidence. Moreover, they must in number have exceeded immensely those which have been discovered, and must have been equally capable of preservation; and since it is admitted that each must have existed a long period of

time, there would be the greater chance of some individuals being preserved as monuments of the former existence of their race. It should also be observed that the number of individuals thus preserved may be almost infinitely small compared with all those of the same kind which may have existed; and also, that our argument only requires this evidence of continuity (if Mr. Darwin's theory be true) in those species which best admitted of preservation.

But again, it has been contended that so small a portion of any stratum has been examined, that we can know little of what it really contains; and this sometimes seems to be stated, as if the value of our knowledge of the organic contents of a given formation ought to be estimated by the proportion which the part of it actually examined bears to the whole formation. If such were the case, it would indeed be a cheerless prospect for the palæontologist, for assuredly the value of his science would be so small as to be scarcely appreciable for centuries to come. But the fact is, that men will have their irresistible convictions in defiance of opinions adduced in support of particular theories. At the last meeting of the British Association, Sir Roderick Murchison asserted his belief, on strong stratigraphical evidence, that a certain bed in the north of Scotland belonged to the old red sandstone of that country. But it was found that this same bed contained reptilian remains; and Professor Huxley showed that nothing similar to these remains had been found except in strata considerably higher in the geological scale, and never near the level here assigned to them; and therefore contended that the stratigraphical evidence must be at fault. Now, if palæontological evidence were to be taken at Mr. Darwin's valuation, this reasoning would have been little better than an unworthy quibble; and yet it was actually deemed so important by all the geologists and palæontologists who were present, as to balance the stratigraphical evidence, though the latter, independently of some strong antagonistic view, would have been deemed conclusive. It is true that every question of this kind must be a question of probabilities, and an opinion formed upon it to-day may be upset to-morrow; but that does not lessen the existing probability, on which alone our present convictions (subject, it may be, to a certain reservation) must be founded. We do not, in fact, derive our convictions at all, respecting either the composition or contents of the sedimentary strata, from any attempt at accurate comparison between the portions of them which have and those which have not been examined. How do we know that in every part of the chalk formation calcareous matter enormously preponderates? or that argillaceous matter predominates in the gault, and siliceous in the new red sandstone? On what portion of the whole mass of these formations has the eye of man ever rested, except the almost infinitesimally small portions at their external surfaces? And yet our convictions are almost as strong on these points as those which we feel respecting the demonstrated properties of number and space. The reason is obvious to any one who gives a moment's thought to the subject. It is by a process—sometimes perhaps an unconscious one—of induction, by which we conclude the whole of these masses to have respectively the same characters as those which we invariably see at so many points of their surfaces. And so it is with respect to organic remains. When we find the same distinctive organisms in any formation, in a sufficient number of different localities, we conclude that those organisms are characteristic of the formation in general, and that those organic forms which are absent in every locality examined, are generally absent in other parts of the formation, just as we believe the mineral substances characteristic of any one of the above-named formations to be generally absent from the other two. And these are valid conclu-

sions according to any theory of probabilities by which their validity can be tested.

From these considerations, which we cannot now stay to develop, we regard the probability of the present existence of these intermediate forms in the fossil state, or their former existence as living beings, to be indefinitely small. A great number may yet be found, and still leave the continuity of the series nearly as incomplete as at present; and therefore, without adding in any appreciable degree to the evidence in favour of Mr. Darwin's theory, or of any other equally involving a continuity of change in organic forms.

Mr. Darwin has also attempted to meet the argument against such theories as his own, founded on the sudden appearance of whole groups of allied species. According to his theory, these organic beings must have had their line of ancestors, and the difficulty lies in the total want of evidence of their former existence. He at once affirms that they must have existed in some distant locality, where their remains are now buried far beneath our reach. Thus, in speaking of the first appearance of the group of teleostean fishes in the early period of the chalk, comprising a large majority of existing fishes, he says,

> Assuming that the whole of them did appear, as Agassiz believes, at the commencement of the chalk formation, the fact would certainly be highly remarkable; but I cannot see that it would be an insuperable difficulty on my theory, unless it could likewise be shown that the species of this group appeared suddenly and simultaneously throughout the world at the same period. . . . Some few families of fish now have a confined range; the teleostean fish might formerly have had a similarly confined range, and after having been largely developed in some sea, might have spread widely.

It is thus by a vague hypothesis, entirely unsupported by facts, that our author meets a difficulty which appears to us, as it has appeared to many others, to be of the gravest magnitude. He does not himself venture to say more than that he does not think it altogether insuperable. This kind of argument demands our assent to a proposition, because it is not impossible. It is to found a theory, not on our knowledge, but on our ignorance. Nor is this the only instance in which he seems to have adopted similar reasoning. In discussing the question of the sudden introduction of groups of allied species into the lowest known fossiliferous strata, our author remarks (p. 306)—

> Most of the arguments which have convinced me that all the existing species of the same group have descended from one progenitor, apply with nearly equal force to the earliest known species. For instance, I cannot doubt that all the silurian trilobites have descended from some one crustacean, which must have lived long before the silurian age, and which probably differed greatly from any known animal.

And again (p. 307)—

> If my theory be true, it is indisputable that before the lowest silurian stratum was deposited, long periods elapsed, as long as, or probably far longer than, the whole interval from the silurian age to the present day; and that during these vast yet quite unknown periods of time, the world swarmed with living creatures.

We confess ourselves to have been somewhat astonished at this bold manner of disposing of difficulties. It may be that our freedom of thought has been shackled by an habitual adherence to the principles of philosophising in such matters. We had not dreamt that because the objections to a theory could not be proved to be absolutely insuperable, we were called upon to accept it as true. We had fancied that the laws of reasoning in such matters, to which Newton and Laplace, Fresnel and Faraday, in their mathematical or experimental investigations, have bowed in reverential obedience, were still in force; and that every one who proposed a physical theory acknowledged the obligation of supporting it by positive reasons, which should at least put the balance of probabilities in its favour. We had imagined, too, that the facts reasoned upon ought to be real, and not hypothetical. Our author is perplexed with the existence of

trilobites, comparatively highly-organized animals, in almost the earliest recognised fossiliferous strata, and to make the fact square with his theory, he at once creates a hypothetical world of indefinite duration for the due elaboration of the ancestral dignity of these intrusive crustaceans. If this supposition were one of the requirements of a theory resting on foundations which could not be moved, we might be obliged to admit it; but we confess that the adoption of such conclusions, unsupported by any positive and independent evidence, merely on the demand of an unproved theory, appears to us little consistent with the sobriety and dignity of philosophical investigation.

Tant pis pour les faits, is a taunt which has frequently been thrown out against philosophers whose boldness has somewhat exceeded their discretion; and we cannot help thinking that Mr. Darwin must be prepared to submit to it largely. It must be recollected, too, that the geological evidence, which he has so entirely set at nought, is precisely that by which alone any theory like his can be adequately tested, since it is that evidence which bears directly on the great, if not the only, distinctive inference which can be drawn from such theories—the linear continuity of specific characters. The two great points which require to be established (as already intimated) are those which we have discussed in the preceding paragraphs of this part of our essay. First, it is essential to show by experiment that varieties characterized by fertility, may diverge into species distinguished by sterility, in the sense already explained; and secondly, to prove by palæontological evidence that linear continuity, which must necessarily have existed, if any theory of continuous development be true. To us, however, it appears that no approximation has been made to the establishment of these fundamental points; and we can no more believe that the elaborate theory which Mr. Darwin has endeavoured to construct can rest securely on the base on which he has placed it, than we should believe that a massive tower rising above the surrounding mist, really rested on the vapour which surrounded its base. We venture to assert, without fear of contradiction, that any physical theory of inorganic matter which should rest on no better evidence than the theory we are considering, would be instantly and totally rejected by every one qualified to form a judgment upon it.

Among the physical theories of vital phenomena, there is one which we have not yet noticed—that brought forward some years ago in the *Vestiges of Creation*. It is well known that the more highly organized animals, in the course of their embryonic development, pass through a series of states resembling in some measure those through which the lower animals pass, only that the development of the latter is arrested, as it were, at some earlier stage than that of the former. This forms the basis of the theory in question. Each specific organism is supposed to be connected with another immediately preceding it by the fact of fœtal development having been carried on during gestation an additional step from one specific form to the next. This theory, like those already spoken of, requires the assumption of linear and diverging development, if, at least, each successive step be supposed to be the least which observed specific differences will admit of; for without this linear divergency it would not account for those larger differences which may be supposed to exist between animals whose lines of descent have been different. Thus, then, we should have chains of organisms as in the other theories; but here they might be *discontinuous*, the cause producing these finite steps in advance being supposed to act discontinuously at long intervals of time, and not necessarily by slow and continuous efforts, like the ordinary causes in nature. It will thus be seen that this theory gives a determinate form to the discontinuous

physical action which we have above insisted on, in some form or other (vol. lxi. p. 748), as necessary to account for the discontinuities presented to us by the specific differences in nature. So far as regards the effects resulting from it, the theory will be identical with Lamarck's or Mr. Darwin's, if we suppose the successive advancing steps in organization to be *indefinitely small*, in which case they would constitute continuous chains of organic forms.

The great defect of this theory is the want of all positive proof, a defect, however, which it only shares in common with the other theories we have been discussing. The facts of embryonic development asserted as the foundation of the theory, are exaggerations of those recognised by physiologists; nor do we possess the smallest indication of any new species ever having been produced in the manner supposed. But authors of theories, it would seem, are far less sensitive to the want of proof in their own speculations than in those of others. The absence of proof in his own theory did not prevent the author of the *Vestiges* from speaking contemptuously of Lamarck's as one of the follies of the wise; and at present his own theory is generally, we suppose, regarded as the folly of one who was *not* wise—not wise, we mean, as tested by the only means which, in the preservation of his *incognito*, he has afforded the world of judging of his wisdom. There is a strange mutability in the fate of vague and doubtful theories. That of the *Vestiges* attracted a large share of public attention, and is now almost as much forgotten as Lamarck's. Mr. Darwin's is unquestionably of the same kind. The history of those which have preceded it appears to us to be ominous of its future destiny.

In our preceding remarks we have designedly restricted ourselves as much as possible to those parts of the subject which have a more or less immediate reference to the theories discussed, as *physical* theories. Lamarck's work and the *Vestiges* are a good deal restricted in the same way; but Mr. Darwin's work contains much that will be read, as we have already intimated, with great interest, independently of the theory which it is its great purpose to establish, both by the general reader, and those who may be following the subject as a special object of study. It indicates a large acquaintance with the facts of natural history, and still larger stores of knowledge in reserve. In the statement of facts, the author is uniformly impartial. It is difficult to conceive a fairer advocate. But when, in his judicial capacity, he comes to the discussion of facts in their theoretical bearings, we recognise a want of strict adherence to philosophical and logical modes of thought and reasoning. There is one great and plausible error of this kind which pervades nearly his whole work. He constantly speaks of his theory as explaining certain phenomena, which he represents as inexplicable on any other theory. We altogether demur to this statement. A theory resting on unproved hypotheses can explain little except in that vague hypothetical sense in which a theory that really explains little may often be made apparently to explain much. There is a remarkable ductility about theories unshackled by the rigours of demonstrative proofs. A phenomenon is properly said to be *explained*, more or less perfectly, when it can be proved to be the necessary consequent of preceding phenomena, or more especially, when it can be clearly referred to some recognised cause; and any theory which enables us to do this may be said, in a precise and logical sense, to explain the phenomenon in question. But Mr. Darwin's theory can explain nothing in this sense, because it cannot possibly assign any necessary relation between phenomena and the causes to which it refers them. A great number of facts are mentioned as being only explicable on this theory, and might thus appear to an inattentive reader to constitute a large

amount of inductive evidence. But all that is attempted to be done is to *assert*, not to *prove*, that the facts are consistent with the theory; and so far from being explicable, in some imperfect sense, by Mr. Darwin's theory alone, they are certainly equally so, in general, by the other theories we have mentioned. In fact, those of Lamarck and of the *Vestiges* immediately resolve themselves into Mr. Darwin's, so far as regards the effects produced by the causes severally assigned by those theories, except that the author of the *Vestiges* would account for the discontinuities between species which Mr. Darwin's does not admit. Hence the phenomena in question are in no way distinctive in proof of the theory of the latter writer more than of the others. The more express object, however, in Mr. Darwin's book, is here to show that his theory will account for phenomena of which the doctrine of successive creations renders no account at all—*i.e.*, no scientific account; since it is supposed to refer the phenomena to the arbitrary will of the Creator. But we do not admit that this is the correct representation of the doctrine, as appears at once when enunciated and explained, as in a preceding part of this essay (vol. lxi. p. 749), where it is pointed out that the discontinuous physical cause there assumed must be supposed to act, not arbitrarily, but in a certain accordance with the ordinary causes with which it is conjoined; or, adopting the more usual phraseology, the creative power is not to be supposed to act arbitrarily, in the strict sense of the term, but in a certain accordance with ordinary causes, and in a certain relation to pre-existing organisms. But, in fact, the doctrine of successive creations, any more than that of final causes, does not pretend to be a *physical theory*, since it pretends not to define the nature of any physical cause to which certain extraordinary phenomena of nature may be referred. It may be regarded as a mode of expressing our disbelief in the assertion that such phenomena are, like ordinary phenomena, due solely to the action of ordinary natural causes, together with our belief that there is some higher order of causation in nature than that which is usually recognised in secondary causes, whether that higher causation is to be sought in properties originally impressed on matter by the Creator, or is rather to be regarded as a more immediate emanation from the Divine mind. It professes to be a negation of other theories rather than a theory of itself, and therefore cannot be called upon to account for phenomena at all in the physical sense in which we necessarily call upon a definite physical theory to account for them. The attempt, therefore, to establish a comparison between the modes in which a phenomenon may be accounted for according to the doctrine of successive creations, and by any definite physical theory, is equivalent to the attempt to compare two things of which the properties and qualities are totally different. We do not object to the assertion that all natural phenomena must be referred to *natural causes*, provided always we give a proper interpretation to this last expression; but we maintain that neither Mr. Darwin nor any other writer, has yet given the slightest proof that the recognised discontinuities in specific forms can be accounted for by the action of ordinary secondary causes. Indeed, our author makes at any time but little use of the verb 'to prove,' in any of its inflections. His formula is 'I am convinced,' 'I believe,' and not 'I have proved.' We are not finding fault with these more modest forms of expression; but we may be allowed, perhaps, to remark, that they are the formulæ of a creed, and not of a scientific theory. Views like his rest, in fact, on no demonstrative foundation, but, as we conceive, on *à priori* considerations, and on what appears to us a restricted, instead of an enlarged, view of the physical causes, operations, and phenomena which constitute what we term Nature.

A large portion of the phenomena which our author declares to be inexplicable on the theory of successive creations, are alluded to in the latter part of his work, under the heads of 'Geographical Distribution,' 'Classification,' 'Morphology,' 'Embryology,' &c. We have explained the incorrectness of this view, and beyond it the facts spoken of have no direct bearing on the real establishment of this particular theory. We decline, therefore, entering into any of their details, though they present many interesting subjects of discussion, which, independently of the author's particular theory, we cannot too strongly recommend for perusal.

We have not yet alluded to the most difficult and most important point connected with any theory which asserts the continuous variation of all organic forms from the lowest to the highest, including man as the last link in the chain of being. It is the transition in passing up to man from the animals next beneath him, not to man considered merely as a physical organism, but to man as an intellectual and moral being. Lamarck and the author of the *Vestiges* have not hesitated to expose themselves to a charge of gross materialism in deriving mind from matter, and in making all its properties and operations depend on our physical organization. Mr. Darwin, on the contrary, seems carefully to have shunned all allusion to the immediate point in question. He has said much on the subject of animal instinct and the successive derivation of its higher from its lower forms, but as far as we recollect, has not even alluded to that infinitely more difficult transition from the instinct of the brute to the noble mind of man. This, however, is the great difficulty to be grappled with, especially in theories which involve the necessity of continuous change in both the material and immaterial portions of animal life, and include man in common with the lower animals. We know not precisely how far our author would extend his theory with respect to man, for he has been reserved on this point. But it appears to us inevitable that it should be so extended by most of those who adopt it; and we would gladly therefore know something more of this mysterious process of metamorphosis from the Quadrumana to the Bimana, according to any theory of continuous development. It may be said perhaps that man may have existed long before the term indicated by any known records of his existence, and that he began his earthly career under a character much more approximate to that of the ape than that which he has now acquired by the progressive advancement of his faculties. But then, where are the missing links in the chain of intellectual and moral being? What has become of the aspirants to the dignity of manhood whose development was unhappily arrested at intermediate points between the man and the monkey? It will not be doubted, we presume, that there exists at present an enormous gap between the intellectual capabilities of the lowest race of men and those of the highest race of apes; and if so, we ask again, why should the creatures intermediate to them—exalted apes or degraded men—have been totally exterminated, while their less worthy ancestors have successfully struggled through the battle of life? But there are other questions which seem to us far more difficult to answer than these. We believe that man has an immortal soul, and that the beasts of the field have not. If any one deny this, we can have no common ground of argument with him. Now we would ask, at what point of his progressive improvement did man acquire this spiritual part of his being, endowed with the awful attribute of immortality? Was it an 'accidental variety' seized upon by the power of 'natural selection' and made permanent? Is the step from the finite to the infinite to be regarded as one of the indefinitely small steps in man's continuous progress of development, and effected by the operation

of ordinary natural causes? We can scarcely suggest these questions without an apprehension of their being deemed irreverent. But they force themselves irresistibly upon us in considering these theories in regard to their legitimate and almost necessary extension to man. The difficulty of passing, according to any theory of development, from the finite to the infinite—from the mortal to the immortal—cannot be avoided by any advocate of such theories, except by denying the immortality of man or admitting that of a sponge or a polyp. We have stated emphatically our opinion that all natural phenomena are subjects for scientific investigation, so far as such investigation can be applicable to them; but what answer can science possibly give to such questions as those above suggested? It is at this stage of his scientific researches, where the reasonings and methods of science are no longer applicable, that the candid and earnest investigator of truth will turn to any other source from which he conceives that further knowledge is to be derived. He is thus driven as it were, not merely as a man of certain religious convictions, but as a philosopher, to regard the subject under its religious aspect, and to compare the conclusions at which he may thus arrive with those to which his physical theories may have conducted him. We are not pretending here to say what ought to be the result of this comparison—that forms no part of the task we have undertaken in this essay; but we do assert that it must be made. We must look upon this picture and upon that, and decide on their respective claims. We know not how the difficulties arising from the spiritual nature of man are answered by the advocates of any unrestricted theory of continuous development, nor is it our desire to question too curiously any one's abstract religious views in relation to the physical theories he may advocate on vital phenomena. Considerations may present themselves to other minds which have not occurred to our own, tending to lessen difficulties which to us appear so insurmountable; still, we cannot but feel a strong conviction that many minds are so constituted that it would be difficult or impossible for them to hold such theories as we have been discussing, without being led to doubt some of the highest hopes and aspirations which it hath entered into the heart of man to conceive respecting his future destiny. Surely, then, we are bound to scrutinize such theories with all possible care, and in doing so, not merely the religious dogmatist, but every true philosopher, will give full weight to his faith in spiritual matters, and feel that the convictions which he derives from it are true elements in his decision for the acceptance or rejection of these unrestricted theories of development.

Mr. Darwin, it appears, expects converts to his views not among experienced naturalists, but among the young and rising ones. 'It is so easy,' he remarks, 'to hide our ignorance under such expressions as "plan of creation," "unity of design," &c., and to think that we give an explanation when we only re-state a fact,' (p. 482.) We believe that this has frequently been true. But if the sentence just quoted be intended as a warning to younger naturalists, and not merely as an explanation of the pertinacious adherence of older ones to preconceived opinions, the warning might perhaps be strengthened by the insertion of another phrase as one which may not be without its self-deceptive influences —'natural selection.' Assuredly the theory founded on this notion does not give explanations, in any rigorous sense of the term, of many things which it professes to explain, but really only asserts. Our author also remarks that all who believe in the mutability of species will do good service by the conscientious expression of their convictions. We heartily agree with him respecting the value of a conscientious expression of opinion; but we would remind the young naturalist that no convictions on

scientific subjects can be conscientious unless arrived at by accurate inquiry, and careful and continued thought; and that of all such subjects the one we have been discussing is the most mysterious and difficult. We would also further remind him that the philosophical naturalist must not only train the eye to observe accurately, but the mind to think logically; and the latter will often be found the harder task of the two. With respect to all but the exact sciences, it may be said that the highest mental faculty which they call upon us to exert is that by which we separate and appreciate justly the *possible*, the *probable*, and the *demonstrable*. It is the exercise of this faculty which, in our opinion, renders the natural sciences, when pursued in a proper spirit, an excellent mental training, and a corrective to that which is derived from the too exclusive study in the exact sciences of the *demonstrable* alone. But let not the young student suppose that this faculty comes by intuition. It can only be acquired by the habit of patient, continuous, and impartial thought on the more difficult questions on which he may have to form his opinions, and will often be found more especially difficult for those whose minds have had none of the rigorous training which the study of the exacter sciences affords. Of all things, too, we would warn the young man of science against prematurely pledging himself to the adoption of doubtful theories. Let him be careful lest, while he imagines that he is only liberating himself from former prejudice, and exercising a proper freedom of thought, he may be imposing shackles on himself for some future time, when the desire to maintain his own consistency may stand opposed to that higher freedom of thought which may become necessary when enlarged knowledge and more matured judgment may possibly suggest large modifications of preconceived opinions. It may be difficult to avoid the influence exerted over us by the prejudices of others, but it is infinitely more difficult to rid ourselves of those which may attach to our own early and immature conclusions. A philosophical reservation of opinion on doubtful points will not diminish the zeal of an earnest lover of nature's truths. Let him pursue his researches, recollecting that Biological science requires at present its Keplers rather than its Newtons—the discovery of the more obvious laws according to which its phenomena may be arranged, rather than attempts at that higher generalization which may account for such laws by the operation of physical causes. The naturalist who carries on his researches on this principle, not necessarily without reference to, but without the professed adoption of, vague and uncertain theories, will preserve himself in the best temper and frame of mind for the impartial investigation of truth, and the final adoption of those theories which may hereafter appear to be best established.

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