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ORDINARY MEETING, MARCH 4, 1867.

THE REV. WALTER MITCHELL, VICE-PRESIDENT, IN THE CHAIR.

The minutes of the previous Meeting were read and confirmed.

The HONORARY SECRETARY announced the names of the following new members and associates.

MEMBERS:—John Griffith, Esq., 6, Hanover Terrace, Regent's Park; W. Castle Smith, Esq., F.R.G.S., M.R.I., 1, Gloucester Terrace, Regent's Park (*Life Members*).

The Hon. Charles Barter, B.C.L. Oxon. (Member of the Legislative Council of Natal), Pietermaritzburg; the Rev. D. A. Beaufort, M.A., Warburton Rectory, Warrington; George Pigot Moodie, Esq., Gov. Surv., J. P., Melsetter, Pietermaritzburg, Natal; P. C. Sutherland, Esq., M.D., M.R.C.S. Edinburgh, F.R.G.S., Surv. Gen. Pietermaritzburg, Natal.

The above Members are on the Foundation List.

MEMBERS for 1867:—R. G. M. Browne, Esq., 9, College Crescent, Hampstead; Thomas Lomas, Esq., H.M. Civil Service, Alma Villas, Windsor, Berks.

ASSOCIATES, 1ST CLASS:—W. F. Browell, Esq., Barrister-at-Law, J.P. for Kent, Broadlands, Tunbridge Wells; 2ND CLASS:—The Rev. Philip Dwyer, A.B., T.C.D., Vicar of Drumcliffe, Bindon Street, Ennis, Ireland; the Rev. Walter S. Grindle, Theol. Assoc. K.C.L., 26, Bessborough Street, Pimlico W. R. Warwick, Esq., M.D., Southend, Essex.

MR. WARINGTON then read the following Paper :—

ON THE CREDIBILITY OF DARWINISM.—By GEORGE WARINGTON, Esq., F.C.S., *Mem. Vict. Inst.*

IT is a rare circumstance for the full explanation of any phenomenon, or series of phenomena, in nature, to be discovered at once. In respect to the most certain, as well as the most uncertain of the interpretations of Science, there has been in nearly every case a period of speculation, of theorizing, in which the view ultimately accepted as true was merely an hypothesis. From the very nature of things it must be so. A certain interpretation is not to be arrived at without a widely-extended series of facts on which it may be based,—facts often requiring long and laborious investigation to accumulate. In such accumulation, carried on with the express purpose of obtaining an explanation, it is impossible but that various hypothetical explanations should suggest themselves to the inquirer, one of which will be almost certain to approve itself to his mind as the most probable. This immediately becomes his theory; to ascertain the truth or falsity of which is henceforth his object. It may be that further investigation disproves it, and it is cast aside; only, however, to be replaced by another, which, so far, stands the test of facts. Or it may be that fuller knowledge merely adds strength and solidity to that first adopted. But in either case it is through hypothesis that truth is ultimately attained. Theoretically, of course, the scientific method is first to obtain a full view of all the facts, and then deduce the explanation. Rarely, if ever, however, is this theory carried out in practice. Nor, indeed, can it be; since how, before any idea of the explanation exists, is it to be known *what* facts especially need to be accumulated and sought after? All that the severest Science can demand is that the result, when offered for acceptance as true, shall be capable of being cast into this theoretical mould; the facts when duly weighed and classified being shown exactly and inevitably to imply the explanation given. But that this should have been the actual course of the investigation—that Science has nothing whatever to do with. In a word, to use Darwinian language, the process by which true explanations are obtained in Science is very much one of Natural Selection. Many hypotheses spring up and struggle together for existence; passing on from hand to hand, they become varied and modified; each variation tending to produce harmony with the conditions of life (*i.e.* the facts of the case) favours prolonged existence; each

variation tending the other way leads to extinction; and so at last, hypothesis after hypothesis dying out, that one is left alone as victor which is found to be most perfectly adapted to the exigencies of the case.

I have been led to these remarks upon the relation of hypotheses to scientific conclusions, 1st, because it is such an intermediate hypothesis which we have this evening to consider; and, 2nd, because it is sometimes said that to spend time in estimating the credibility of an unproved theory is unscientific, we should rather devote ourselves to the investigation of facts. But if the principles laid down above be correct, and it is practically impossible to conduct investigation without hypotheses, then it is clearly a matter of grave importance *what* hypotheses we thus tentatively adopt,—a matter which should of course be determined by the amount of credibility at present belonging to them. Some facts, some arguments, some analogies, bearing upon Darwinism, we have already, albeit confessedly insufficient to demonstrate its truth. The question is, then, do these facts, arguments and analogies afford such an amount of evidence in its favour as to render it a fair working hypothesis for future research? Is Darwinism, in a word, credible? Not, is it the true explanation of the phenomena it seeks to account for; but, is it such an hypothesis as *may* possibly in the end prove to be so? If so—if Darwinism be credible, then it ought to be carefully kept in mind, applied, and tested, in all investigations into the facts which it concerns, that so its truth or falsity may become apparent. If, on the other hand, Darwinism be incredible, then it may be at once rejected as unnecessary to be considered, at all events until fresh evidence in its favour is adduced. This is the practical issue which the present paper proposes to raise.

The tests to which scientific hypotheses are subject in the process of selection by which they pass into certain interpretations, are fourfold,—possibility, adequacy, consistency, and harmony. The precise meaning to be attached to these terms, and the value to be set upon the tests they denote, may be best seen by a simple example. Let us take for this purpose the hypothesis that gravitation is the sole controlling force by which the motions of the planets in the solar system are regulated. To test the credibility of this hypothesis we should have to inquire,—1st, Is gravitation a *real* cause, capable by its action of controlling planetary motion; *i. e.*, is the hypothesis possible? 2nd, Is gravitation a *sufficient* cause to account for all the motions actually observed; *i. e.*, is the hypothesis adequate? 3rd, Are *all* the effects in fact produced which

gravitation must produce if really at work; *i. e.*, is the hypothesis consistent? 4th, Is there any evidence of gravitation being actually at work in any part of the solar system, or other similar sphere, which would afford ground of *analogy* for regarding it as probably at work throughout the whole; *i.e.*, is the hypothesis harmonious? Before any hypothesis can be admitted as certainly true, it must satisfy all these four requirements. Until it does so, it can only be accounted as more or less credible; provided always that it answers the first demand—that it is possible. If this be wanting—if there be no evidence that the cause assumed is a real cause, then the hypothesis is purely fanciful and unworthy of credence. But if it be possible, then so far as it fulfils the other three conditions it is also credible. The degree to which any hypothesis fulfils these conditions will depend primarily of course upon its truth; it will also, however, be affected very seriously by the inevitable limitations of human knowledge. It is quite possible for a true explanation to appear inadequate or inconsistent, simply because of our ignorance. Thus, to take an illustration bearing upon the example just reviewed, the adequacy of gravitation, prior to the discovery of Neptune, appeared at fault, perturbations being observed in the planetary motion, for which gravitation failed to account. The discovery of Neptune, which removed this objection, depended, however, on a property altogether independent of its gravitating influence, the property, namely, of reflecting light. Had Neptune been so constituted as not to reflect (which is perfectly possible), no telescope could have descried it, and gravitation might very likely in consequence have been rejected by some as an inadequate hypothesis, when, in truth, the apparent inadequacy arose entirely from the imperfection of our knowledge. It is plain, then, that no objection to an hypothesis should be regarded as of final weight, for which a possible explanation can be given, not inconsistent with observed facts. Weaken the credibility of the hypothesis such objections can and do, destroy it altogether they cannot. On these principles, then, it is proposed now briefly to discuss "the credibility of Darwinism."

1. Its possibility. Are the elements involved in Mr. Darwin's hypothesis real elements, and are they capable of producing the *kind* of effects he ascribes to them? The elements involved are four:—(1) "Growth with reproduction; (2) Inheritance which is almost implied by reproduction; (3) Variability from the indirect and direct action of the external conditions of life, and from use and disuse; (4) A ratio of increase so high as to lead to a struggle for life."

The result being "Natural selection, entailing divergence of character and the extinction of less improved forms." * Is it possible for these elements, in their mutual action and reaction, to occasion specific differences in living beings? This is our first inquiry; for the solution of which it is manifestly necessary that we should understand clearly what is meant by specific differences—what is meant by a species. We may define it roughly by saying that a species is a race of living beings differing in certain respects from all other races, such differences being steadily transmissible by reproduction, and not being the immediate result of present outward conditions. By this definition are excluded—(1) all mere transient sports, and (2) all apparent varieties dependent directly upon situation, climate, &c. To many it will doubtless seem far too lax a definition, as raising a large number of so-called varieties into the rank of species. Let such afford a better one, equally sufficient and equally free from arbitrary assertion. The great thing we have to beware of is allowing prejudice to lay down a definition which shall beg the question at issue, *e. g.*, that specific differences are those which are permanently invariable; that species are those races which are not bound together with intermediate varieties, or which cannot be traced to a common origin, or which, when crossed, yield sterile hybrids. To discuss the natural origin of species with such definitions as the basis would be as impossible and absurd as to discuss the motions of the fixed stars with the definition given that the fixed stars are those which never move. The only fair definition of a species is a race of living beings possessing common characteristic differences from all others, which differences at the present time are constant and inherent.

That species grow and reproduce, that they pass on their characteristics by inheritance, and that they are liable to variation, is admitted by every one. The point at issue is whether they can *so* pass on and accumulate their variations by inheritance as in the end to bring about specific differences. If they can do so, then the Darwinian hypothesis of the origin of species is, so far, possible; variation and inheritance *could* bring about specific differences.

Our attention must in the first place be directed to the formation of breeds among domesticated animals and cultivated plants. It is notorious that there have been produced by the agency of man distinct races of living beings, having characteristic differences from all others, which differences

* "Origin of Species," 4th edit., p. 577.

are at the present time constant and inherent. He has begun with a single race, and out of that single race he has produced many, differing alike from their common progenitor, and from each other. These new races breed true, *i. e.*, steadily transmit their peculiarities by reproduction; they are independent of local and temporary circumstances; in fact, they are neither more nor less than species, and would unhesitatingly be recognized as such by naturalists *if* their origin were only unknown. It is unnecessary to instance particular examples, the facts are patent to every one, whether in respect to cattle, horses, dogs, fowls, vegetables, or flowers. How then has man done this? He has done it simply by availing himself of observed natural variations, which he has trusted to inheritance to perpetuate. Directly to produce variation is entirely beyond his power, he knows nothing of its causes, and can in no way influence it. He simply selects, and so controls. The variations in character in individuals of any species do not as a general rule tend to effect any specific change, if intercrossing be freely permitted, because they are perpetually neutralizing one another. But man, perceiving some variation useful to himself, isolates and preserves it by preventing the intercrossing which is calculated to destroy it. The process is repeated generation after generation, with the like precaution, until at last the variation is fixed, it has become specific. While, therefore, these facts concerning breeds prove conclusively that variation and inheritance can produce species, they show, further, that to do this a certain selection is necessary to prevent the counteracting influence of intercrosses. In the case of domestic breeds this is done by man's arbitrary isolation. Is there anything in Nature corresponding to this, and capable of producing the like effect? Undoubtedly there is. In some cases there is the very same thing at work,—isolation; a few individuals of a species are often separated locally from all others, and exposed, therefore, to but little intercrossing. If variations occurred here, there would manifestly be far greater chance, so far, of their being perpetuated and becoming specific, than in a locality where a large number were to be found together. More important, however, than this, as more generally applicable, and really more potent, is the principle which Mr. Darwin has denominated Natural Selection, and which forms the key to his whole hypothesis. All living beings reproduce themselves in a geometrical ratio of increase, which must inevitably lead to an overcrowding, a jostling, a struggle, both for position and subsistence. The fact that it is so is indisputable. What follows, then? Clearly there must be a selection perpetually

going on. Not every seed that ripens can possibly germinate, not all that germinate can grow up, not every one that grows up can come to maturity and reproduce itself; and so in like manner with animals. There is a perpetual struggle for existence going on, both among rival races and rival individuals; this struggle must lead to selection. But selection on what principle? A mere indiscriminate selection would have as little tendency to bring about specific differences in nature as an indiscriminate isolation of individuals would have to produce an improvement in breeds. The selection to be effective must be one which lays hold of particular variations, and tends to perpetuate them, to the exclusion of others. Is this the case here? Again we may say, from the very nature of things it must be so. The selection being mainly of the nature of a competition, it follows that just those races, those individuals, will be successful which are most perfectly adapted to the conditions under which the struggle is carried on. But the variations occurring in individuals cannot but be in many cases of considerable moment to such adaptation, either beneficially or otherwise. If the former, those individuals will be precisely such as natural selection will inevitably tend to preserve; if the latter, they will be such as natural selection will inevitably reject. The same will take place with the descendants of the favoured few, and so by a continual sifting out of those which lack the advantageous variation, or possess it in a smaller measure, the predominance of the altered form becomes yearly greater and greater, the counteracting influence of intercrossing as a consequence less and less, the variation is strengthened and rendered constant, and a specific difference is the result. Granted that species vary, that their variations frequently have a bearing on their adaptation to the circumstances of their life, that they have a tendency to transmit variations by inheritance, that there is a continual process of selection among individuals going on, which of necessity favours those possessed of advantageous variations to the exclusion of others, and there is no alternative left but to conclude that the Darwinian hypothesis is *possible*. The elements contained in it are real elements, their action and reaction exactly that which is asserted; the result is inevitable. The causes assigned by Mr. Darwin for the existence of specific differences, are not only real causes, such as *may* account for phenomena similar to those sought to be explained; but are, further, causes actually at work in the domain where these phenomena occur. Not only, therefore, is the hypothesis possible, but it is also established as to some extent true. Few, if any, probably will deny that there are *some* races of living beings whose

specific differences have been occasioned by such causes as those alleged by Mr. Darwin. This, however, is very far from satisfying the hypothesis, which is, not that some races have thus originated, but that *all* have. Mr. Darwin believes "that animals have descended from at most only four or five progenitors, and plants from an equal or lesser number."* Analogy, indeed, would lead him "one step farther," namely, to the belief that "all animals and plants have descended from some one prototype." But this he regards as "immaterial whether or not it be accepted,"† inasmuch as no distinct evidence can here be alleged. Confining ourselves, then, to the hypothesis proper, the descent of all living beings from some eight or ten progenitors, we proceed to our second inquiry.

2. Its adequacy. Are the causes alleged *sufficient* to account for all the specific differences known to exist? Here it is not pretended that more than an approximate answer can be given. It is not always possible to give even a probable account of *how* certain differences might thus have been occasioned. All that Mr. Darwin asserts is that his hypothesis can be shown to be adequate in so many, even of the apparently most difficult cases, that there is no valid reason on this ground for rejecting it, but rather much reason for regarding it as probably true. To estimate the validity of this position, it will be necessary to examine somewhat more in detail the extent and power of the two great elements in the hypothesis above defined—inherited variation and natural selection. The necessarily limited space of the present paper will render it, however, imperative in doing this to confine ourselves to illustrations of the *kind* of differences capable of being thus produced, instead of fully discussing any one or more crucial cases.

That differences in size, in colour, in detailed form, and the relative development of different parts, occur in species, and are liable to be inherited, no one probably will deny. I restrict myself therefore to variations going beyond these.

And firstly, be it noticed such variations include many striking structural changes. Thus we have such cases on record as of a woman being born with two or three toes of the right foot completely joined together with skin, partially webbed in fact; her children being free from the peculiarity, its reappearance however in some of her grandchildren, now in the foot, and now in the hand, but always on the right side; its perpetuation to her great-grandchildren in like

* P. 570. † P. 571.

fashion; and so on for yet another generation. Or, again, of the absence of nails, accompanied with perfect baldness, carried down through four generations; or of hare-lip, carried down through five generations. Or, again, of deaf-dumbness, transmitted through four generations; of albinoism and other alterations in the eye, similarly hereditary. Especially do such instances prove the wonderful power of the principle of inheritance. At every successive reproduction, the influence of the original variant diminished by one half, so that by the fourth generation it amounted only to one thirty-second, by the fifth generation only to one sixty-fourth part of the total influence. Yet so strong is the tendency to reproduce variations, even when, as in these examples, of a highly disadvantageous or even abortive character, that, notwithstanding, the peculiarity still made its appearance. In a similar way the hereditary character of structural diseases, as consumption, mania, &c., is acknowledged by all. These, then, are cases where we may say everything was against the inheritance of the variation, and yet it was inherited. Had the variations been beneficial, and so themselves have tended to preservation—had, for example, the palmation of the toes occurred in a bird living partly in the water, or the baldness in another to whom head-feathers were inconvenient (and the like phenomenon has been observed to be hereditary in doves); or, again, had similar changes taken place, only in an opposite direction, —say the strengthening of the lungs instead of their weakening, or the addition of pigment to eyes formerly devoid of it, instead of its withdrawal from eyes formerly possessed of it; had especially, owing to the favourable influence of such variations, and the consequent multiplication of their possession, some of the successive generations been born of parents both of whom varied in the same manner;—had this been so, we cannot doubt but that races of living beings would have come into existence differing most markedly in structure from their progenitors, and forming species which the anti-Darwinian naturalist would ridicule the idea of ever having sprung from the source they did.

Then, in the next place, it must be observed that such variations extend also to notable differences connected with habit and manner of growth. Thus no one will dispute the marked physiological distinction between a tree that sheds its leaves in the autumn and regains them in the spring, and another that retains its leaves all the year round. The internal system of such trees is manifestly widely different. Yet we have an example of a tree, the plane-tree, occasionally varying by becoming evergreen. One such in the island of Crete was famous

in Pliny's days and for long afterwards; then it disappeared; within the last few years it has reappeared, fresh shoots out of an old trunk cut down (which does not seem when standing to have been evergreen), again showing the old characteristic. This variety, we are told, could not be propagated elsewhere, the seedlings withering everywhere but in their native spot, no doubt from lack of some peculiarity in the soil or situation. We can, however, readily believe that, had the appropriate soil and situation been plentiful, this variety might have turned out its progenitor, have become confirmed in its difference, and been ranked as a remarkably distinct species.

But further, variation extends also to instincts and habits of life; acquired instincts are hereditary quite as much as natural ones. The case of the *pointer* is an excellent instance, the instinct of pointing being one known sometimes to occur as a variation, and being also one certainly transmissible by inheritance. It is highly probable that it was indeed originally nothing but an individual variation, become now by selection and inheritance the permanent characteristic of a race. Variations of habits in domestic animals, such as different degrees of docility, preference for particular kinds of food, fondness for various pursuits, &c., are too familiar to every one to need especially insisting on. Nor is there any doubt that such variations are to some extent hereditary. To take but a single instance, this time from creatures in a state of nature:—the dread of man, undoubtedly hereditary in many wild species, is shown, by the experience obtained in newly-peopled islands, to be an acquired, not an original instinct.

In the same way as instincts and habits may thus be occasioned by change of circumstances, as well as by natural variability, so may differences in structure and development be brought about by altered conditions of life. Every one knows how largely use and disuse tend to modify powers; few, however, probably realize the extent to which this involves also modification of structure. The superior use of the right hand and arm in man renders it the strongest and most adaptive; it also lengthens and enlarges the bones composing them. Continual practice in running will conduce to greater fleetness, which again depends in part upon the relative size of certain bones. Not only are persons born with short sight and long sight, but these can also be acquired by use. The sailor, habitually accustomed to descry distant objects, lengthens his sight, can see farther than others. The student, used to poring over his book, shortens his sight, can see nearer than others, but at the same time loses his power of seeing far off. Now what does this involve? There is in the eye a wonderful power

of altering the focus of the lenses so as to suit divers distances. This power is, however, not only naturally variable, but so little constant as to be considerably altered by use in a particular direction, even through a comparatively short space of time. It can be increased in either direction; but this increase brings with it a corresponding limitation in the other. Similar alterations in structure by use in respect to the ear might easily be instanced. So, again, there is no doubt that the size and proportional development of the stomach and other internal organs are directly influenced by the nature of the food habitually eaten; the size of the lungs by the temperature of the air breathed, and the occupation of their possessor.

These are the kind of elements with which natural selection has to deal: differences in structure, physiological character, instinct, and habit; differences, some of them directly occasioned by the conditions of life, the use and disuse of particular organs, some of them by causes existent before birth, and of which we know nothing; differences all certainly transmissible by inheritance. To appreciate the wide extent of the ground covered by such known individual variations would require a detailed survey of facts infinitely fuller than the bare outline here afforded, which, as already remarked, merely professes to give illustrations of the different *kinds* of variations observable. We may, however, even from this meagre view, unhesitatingly conclude thus much:—that there is no *class* of specific differences which facts do not fully warrant us in regarding as possibly caused by inherited variations. The *amount* of such differences will come under consideration further on.

But now, in the next place, of the power and extent of natural selection as a process for preserving and confirming such variations. Here we need carefully to bear in mind the exceedingly complex relations in which all living beings stand. There are first their relations to inanimate nature, to soil, climate, and situation. Then there are their direct relations to one another, the presence of one being necessary to the well-being or existence of another, or acting as a check upon its development; so that the increase or decrease of one will entail at once the increase or decrease of the other. Then, thirdly, there are their indirect relations, caused by that competition of races and individuals before dwelt upon; those which do not directly affect each other's well-being, yet struggling together for existence, by reason of a greater number of germs being constantly produced than can possibly attain to maturity. These various relations affect species in every part of their

being, their structure, their physiology, and their habits; affect them also at different periods of their life in a different way, and in regard to different elements in their constitution. Every species being thus subject throughout its whole life to an immense variety of stringent tests by which its relative predominance is determined, the result of this must plainly be, on the average, the maintaining each species at its highest pitch of perfection in respect to the particular conditions to which it is exposed. With this, however, will always be also a distinct tendency to preserve variations, even of the slightest kind. In any given area the largest number of individuals, whether of plants or animals, will be found capable of co-existing, when the differences between them are at their greatest. It is well known, for instance, that a heavier crop of hay is obtained from a field sown with mixed seed than from one sown with only a single kind, simply because in the former instance more individual plants are capable of growing together than in the latter. The tendency of the struggle for existence being, of course, to preserve in every case the largest number of individuals possible, there will thus be an intrinsic advantage in every variation, apart from any positive bearing it may have on the well-being of the species. The severity of the struggle with individuals of the same species will at all events be diminished, and so a greater chance of preservation be afforded. Thus, even supposing no change to take place in the conditions of life, it is quite credible that natural selection should so seize hold of and confirm even indifferent variations as to make them permanent. How much more if they are of a kind directly profitable.

But the conditions of life do not remain unchanged. The development of a new variety or species in the manner just noticed, the diminution or extinction of another by deterioration (for species certainly vary in both directions), the immigration or chance introduction of some foreigner previously unknown there, would at once alter the relations of each species to the other, and so affect the kind of test by which their predominance was determined. Irregularity in the seasons might give especial advantage to some individuals and races, especial disadvantages to others, and thus tend to extinguish certain variations and preserve others, besides leading to internal alterations of relation. Changes in physical geography brought about by geological forces would be still more potent, as producing differences in the conditions of life more permanent and extensive. A greater or less elevation, an altered flow of rivers, a different course of ocean currents, the connection or disconnection of land with land,—all would

bring with them most important changes in the conditions of life, and so a new set of tests by which natural selection should work, leading to the extinction of existing races and the development out of the surviving ones of new. Which changes, moreover, as Captain Maury especially has well shown, would by no means be confined to the particular places where the geological forces were actually at work, but, from the extremely complex relations in which all parts of Nature stand to one another, would extend their influence more or less over the whole earth.

Taking all these circumstances into account, then, the conclusion seems plain, that there are in the diverse and exceedingly intricate conditions of life to which all species are exposed, in different places and at different times, amply sufficient points of contact between natural selection and inheritable variation to account for variations of every kind being taken hold of and preserved in such a way as eventually to lead to their appearance as specific differences. This being so, the inference would at once arise (bearing in mind the former conclusion as to the kinds of variation actually observable), that the Darwinian hypothesis of the origin of species is capable of accounting for every kind of specific difference known to exist; that is, that it is *adequate*.

Before, however, fully endorsing this conclusion, it will be necessary to consider that further point alluded to above, the *amount* of the differences. Now, taking the hypothesis as it stands, that all past and existing species of living beings inhabiting the earth have sprung from at most some eight or ten original forms, the amount of difference does seem overwhelmingly enormous. To suppose, for example, that all vertebrate animals, or all exogenous plants, have descended from the same progenitor, is an immense exertion for the imagination. But how does Mr. Darwin's hypothesis suppose this transformation and development to have taken place? By single strides? No; but by an exceedingly long series of exceedingly small steps. A traveller standing at the foot of Mont Blanc, viewing through his telescope another who had reached the top, and then scanning the marvellous obstacles of mountain peaks, precipices, and glaciers that lay between, might be disposed to say that it was impossible for any one to climb from where he stood to that lofty summit. To the imagination, merely taking into account the enormous height, the apparently insuperable hindrances, it might well seem so; and yet step by step, through long and often circuitous paths, round obstacles, if not over them, it could be done. Just so with Darwinism. Not by a sudden transition from class to

class or order to order, but by the change of species into varieties, varieties into species, each transition involving no greater alteration than is known in such cases often actually to take place, the transformation of the one into the many *could* have taken place. Granted, then, that there was time enough for such slow development to have produced the effects we see; granted that the conditions of life have varied in different places and times to a sufficient extent to cause natural selection to have been carried on in exceedingly diverse directions; and there is nothing in the *amount* of the differences, as distinguished from their kind, which presents any valid obstacle to the adequacy of Darwinism. That the conditions of life have thus been perpetually varying, the testimony of geology assures us in the plainest terms. That the time has been enormously long, is, according to most geologists, equally certain; while those who dispute the assertion do so, not by producing positive evidence that it was actually short, but by rebutting their opponents' arguments, by showing merely that it *need* not have been long. Still, therefore, even if the position of these be admitted as well established, it remains an open question whether, after all, the time *may* not have been amply long enough for all to have occurred which Mr. Darwin's hypothesis requires.

One further remark only is necessary before leaving this part of the subject. It is by no means to be imagined that every difference now distinguishing species from species was seized hold of by that natural selection which led to their separation. The principle of correlation of growth, on which the whole science of comparative anatomy and palæontology depends, tells us that a difference in any one member involves also differences in other and related members, so that from a tooth only the whole structure of an animal may be inferred. The particular point of variation on which natural selection seized, might thus be but a single element in the total of differences that ultimately characterized the species, the remainder being the result of correlation. This should ever be borne in mind when inquiring into the possible way in which particular characteristics could have been exposed to the influence of natural selection. They may never have been exposed to it at all, but be the correlated results of other and far less apparent differences, which were so exposed.*

3. We pass, then, thirdly, to the *consistency* of the hypo-

* From here to the end of the paper was delivered extempore, being written out afterwards. No attempt has been made to preserve the original phraseology in thus reproducing it; the matter and arrangement have, however, been strictly adhered to.

thesis. Are all the phenomena observable in Nature, which should be if Darwinism be true? The extent and pretension of the hypothesis expose it to the severest possible tests in this respect. If all living beings have indeed descended from a very few original progenitors, then there must flow from this certain well-marked characteristic in respect (1) to their present nature and relations, (2) to their distribution in space, and (3) to their distribution in time. Under these three heads, then, I propose to consider the most important of the tests of consistency to which Darwinism is fairly subject, confining myself as before to a general outline, without entering fully into details.

First, then, of the present characteristics of living beings, their nature and relations to one another. The principle of Darwinism being the development of a vast number of forms by gradual divergence from a single original, it follows at once that if the hypothesis be true, all such forms should be capable of being arranged in groups of continually increasing diversity, retaining, however, even to the last, some characteristics in common. Not only should we expect to find in every species resemblances to the first progenitor of the whole, but also resemblances to the various intermediate members of the series, these resemblances increasing as the line approached its termination; the whole sum of characteristics possessed by each species being the result of successive additions upon those common characteristics belonging to the whole class. And precisely so we find it. The whole natural system of classification is based upon this principle of group within group; first species, then genera, then sub-orders, orders, sub-classes, classes; the amount in common constantly diminishing as we ascend, yet something, and often a good deal, in common being found even to the last. This latter is an important point. Look for example at two of the great classes of plants, exogens and endogens;—what a group of characteristics does each present. Here are the exogens, growing by the formation of additional layers outside the old, possessed of two cotyledons in their seed, having leaves with reticulated veins, and flowers with the parts most commonly in multiples of four or five. Here are the endogens, growing by addition to the inside, possessed of but one cotyledon, having leaves with parallel veins, and flowers with the parts in multiples of three. And these characteristics are common to all the myriads of orders, genera, and species which each class contains. All exactly as was to be expected if the Darwinian hypothesis were true.

Then, further, it is to be noted that the distinction between

these successive groups is purely arbitrary. The division of classes into orders, and these into sub-orders and genera, is highly convenient, and as already noticed, on a general view, not without strong warrant of facts in its favour; but in its precise limitations it is arbitrary. Naturalists are perpetually divided not only as to which are species and which varieties, but as to where genera begin and end, how far orders and sub-orders are to be distinguished, and especially under what head particular species or genera are to be ranked. The constantly increasing divergence that appears as we ascend the scale almost necessitates such intermediate groups being introduced, and yet the gradations are in many cases so fine, the connecting links so numerous, as to render it a difficult if not a hopeless task to define and arrange these groups in a perfectly natural manner. Again, precisely what might have been expected if all these successive groups were the irregularly divergent but yet related descendants of a single progenitor.

Once more, it is to be noted that the differences which distinguish these various grades of groups from one another vary exceedingly as to the organs and characters which they concern. Now it is the most important which are found to differ, now the least; nor does this variation accord in any way with the importance of the classificatory distinction. Thus we have some orders of plants (as Cruciferæ) where the number and position of the stamens, the arrangement of the petals, &c., are alike throughout; the generic and specific characters being obtained for the most part from organs of less importance. And again, we have other orders (as Connaraceæ), where the most radical characters are found to vary between genus and genus; or in some cases even between species and species. Had the contrary been the case, and the most fundamental organs afforded the characteristics of the larger groups, the less fundamental those of the subordinate ones, and so on in regular gradation,—had this been so, the arrangement and relations of living beings would have presented a symmetry and manifest method strongly suggestive of especial design and arbitrary plan. The opposite to this, however, — irregularity, ununiformity, apparent lawlessness,—was naturally to be expected, if all these groups were really the diversified offspring of a common parent, since such diversification would be certain to proceed irregularly in different directions. And exactly thus we find it.

We come now to another test. If the Darwinian hypothesis be true, then not only have large groups of species descended from single progenitors, but the mode of descent has been by

an enormous number of intermediate forms. Are such intermediate forms to be found? Here we must, in the first place, inquire how far, supposing the hypothesis true, it were to be expected that they should be found. The mode of production alleged is a seizing hold by natural selection of profitable variations in individuals tending to the preservation of such to the exclusion of others. The same power that determines the greater predominance of the variant determines also the less predominance of the non-variant; so that if the variation be important, its preservation and confirmation carries with it, of necessity, the ultimate extinction both of the original, and also of the successive steps by which the full extent of variation was attained. It is thus a necessary consequence of Darwinism that at no one time should a large number of intermediate forms be found co-existing. Only in the case of indifferent variations not much affected by natural selection, or of other variations in particular stages of their progress, was it to be expected that such forms would be found. Their presence would be the exception, their absence the rule. And just so is it found to be in fact. Here and there are cases (*e.g.* the brambles) where intermediate varieties are so numerous and so finely transitional as to make it almost impossible to determine which are species and which not. In the majority of cases there is no such difficulty, but the specific differences are clearly marked. Again, precisely what the Darwinian hypothesis would have led us to expect.

Yet another test. If all existing species are the descendants of other and different species, it is natural to expect to find in them various marks of this descent over and above those common characteristics of classes, orders, and genera before alluded to; these marks varying in character according to the remoteness of the ancestor whom they concern. Thus it is well known that in artificial breeds there is an occasional tendency to revert to the peculiarities of the original stock, and this especially when several distinct breeds are intercrossed, and the variations of each thus neutralized by intermixture. The instance of the pigeons given by Mr. Darwin* will occur to every one who has read his book. The like reversion might naturally, then, be expected to take place among species in nature. And the facts accumulated by Mr. Darwin touching the occasional appearance of stripes and bars on various species of the horse genus, and especially on hybrids between any two of them,† show unmistakeably that the same kind of phenomena does, in fact, occur here also.

* Pp. 26-7. † Pp. 191-5.

Reversion is of course only to be expected where the character reverted to belongs to a comparatively recent ancestor. Another mark of descent, reaching further back, is the presence of organs in a disused or rudimentary condition which formerly were of importance. When any organ becomes, from changes in the conditions of life, unused, the most probable result would be that it should gradually become less and less perfectly developed; at the same time it is quite conceivable that it should be retained for some time fully developed, though no longer of use. Both cases are found in nature, the latter occasionally, as in the geese with webbed feet who never go into the water, and the woodpecker who never climbs a tree; the former frequently, as in the rudimentary teeth of whales, the rudimentary tail in tailless animals, the rudimentary wings of the apteryx or ostrich, the rudimentary stamens in female flowers, &c. Both manifestly present great difficulties on the ordinary theory of special creations, but fit in naturally with the Darwinian hypothesis of irregularly diverging common descent.

Then to go a step farther back yet. Not only have we disused and rudimentary organs, but also organs differing enormously in development and use, yet radically identical, or even capable of transformation into one another. Thus the wing of a bird, the arm of a man, the paw of a lion, the flipper of a seal, are all strictly homologous structures, made up of similarly related and connected bones, though externally so exceedingly different. Thus, again, in plants the different parts of the flower are seen occasionally to turn into mere leaves, showing the morphological identity of these so diverse organisms; while in some cases, as the white water-lily, the transition from sepals to petals, and from petals to stamens, may be seen in all its fine gradations even in a single flower. All this is of course just what was to have been expected, if the Darwinian hypothesis of the common origin of species having homologous structures, and the enormous capability of variation possessed by every part, be accepted as true. On any other theory such phenomena are simply curious but inexplicable facts.

Lastly, as the deepest-seated and farthest-reaching of all these marks of descent, we have the phenomena of embryology. It was to be expected that if whole groups of living beings have really descended from a common progenitor by subsequent variation, the differences thus resulting should be developed in each individual somewhat later in time than those fundamental characters which all inherit in common; in other words, that in the first stages of growth there should

be more resemblance between such related species than subsequently. The investigations into the gradual growth of embryos before birth show us that facts fully answer to this expectation. The differences between the members of the same class are slowly built up by the diverse development of forms at first utterly undistinguishable; and the more nearly allied the members are, the later do the differences between them appear.

The remaining test belonging to the head of present characteristics is one of an entirely different kind, which affords a natural transition to the next division concerning distribution in space. If the effect of natural selection upon species exposed to it be to preserve and perpetuate their most improved forms, it follows at once that in those places where natural selection is carried on most vigorously, there should the species be most improved. The severity of the selection depends mainly upon the amount of competition to which each living being is exposed; clearly, then, in wide-spread areas, where there are a large number both of races and individuals struggling together, it was to be expected that both improvement and extinction should go on most rapidly; in confined and isolated areas, where the races and individuals are fewer, it was to be expected that both these processes would go on much more slowly. And precisely so we find it. Isolated localities—as islands, fresh-water lakes, caves, &c.—are ever found to present the greatest number of peculiar forms, often so resembling bygone types as to receive the name of "living fossils." While, if the comparative improved condition of the species generally be inquired after, it needs but to put the flora and fauna of an isolated and extended area into actual competition, the result speaks for itself. The species from the latter, if introduced into the former, speedily supplant and extinguish the greater part of them, while those from the former are altogether unable to retaliate if transferred to the latter.

We come now to the second division of tests of consistency, those, namely, which concern *distribution in space*,—tests perhaps the severest of any to which the hypothesis is subject. Darwinism supposes that every species of a genus has descended from an original single species; that every such representative species in each order has descended in like manner from one original, and so on. But these species and genera are scattered in all directions over the face of the globe. It is incumbent on the upholders of Darwinism to show, then, (1) how the original representative species could have become so distributed as that their varied descendants should appear in the places they now do; and (2) that the systematic

affinities of the flora and fauna of different places accord with the mode of origination thus assumed.

First, then of the means of dispersal. To enter into this at all at length would require the whole evening; it must suffice, therefore, to allude to a couple of instances of a very different but equally important character, by way of illustration. The close affinity between the Arctic flora on high mountains in all parts of the world, however remote, appears a case of peculiar difficulty. How can the supposed common progenitors of these nearly allied or even identical species, so different from those existing in the adjoining temperate or tropical countries, have become distributed into their several places? The answer is found in the prevalence at a comparatively recent period of great cold over large portions of the earth's surface, accompanied with glaciers and other Arctic phenomena. Such increased cold would naturally drive the Arctic flora of the north pole southwards in all directions over districts now utterly uncongenial to it. On the diminution of the cold, this flora would plainly retire not only northwards, but also up the mountains in all parts, the congenial portions of which, now so completely isolated, would thus be clothed everywhere with species drawn from a common source, exactly as we should surmise to have been the case from their intimate systematic relations. This instance is one where great apparent difficulty is turned into confirmation. The second is one which on the face of it remarkably confirms the hypothesis of common descent. Oceanic islands, if not peopled by special creation, can only conceivably have been peopled by birds, insects, seeds, &c., having been either blown or washed thither. Only some species, plainly, could thus be conveyed—*e.g.* of land mammals, only those which could fly, namely *bats*. It is a remarkable fact that the only mammals that are found on such islands (*i.e.* those very far removed from the mainland) are precisely bats, just as this theory of distribution would require. But further, these bats are in many cases of peculiar species, found nowhere but in their several islands, exactly as might have been expected if they were the descendants of isolated individuals long ago blown thither. That they should be thus peculiar, and the only mammals found there, though others are fully as capable of living there, are facts alike inexplicable on the theory of special creations.

But, secondly, of the relation between geographical connection and the affinities of flora and fauna. This appears in many ways. Thus the species existing in different islands of a group, though often very distinct, are always more nearly related to one another than to those on the mainland. The flora and

fauna of islands resemble most closely those of the nearest continent, with a few exceptions, where ocean currents or prevalent winds afford especial means of transit from other sources. The flora and fauna of whole continents, as America, present similar internal affinities, though spread over areas most diverse in situation and climate. The same truth appears also in the marvellous effect of great natural boundaries, as impassable isthmuses, however narrow, and deep sea-beds, in regulating the affinities of marine fauna. Lastly, the same is seen also when we turn to the records of geology, the fossil remains of the old and new worlds for instance, presenting similar, though somewhat less marked, differences with those observable in the living species. It is, not, of course, pretended that these facts afford any direct argument in favour of Darwinism; they are merely so many tests which it must answer satisfactorily in order to be established as consistent. The fact that it does answer them is of value as an argument only by reason of the number and severity of tests, it being improbable that an untrue hypothesis should not somewhere be caught tripping.

The last item considered—the analogy between the fossil remains and existing species of the same areas—leads naturally to the last division of these tests of consistency, those, namely, which concern distribution in time. Unwarrantable as it was shown to be to expect a large number of intermediate forms to be found co-existing at any one time, it is clear that if Darwinism be true, such intermediate forms in innumerable hosts must have existed, now here and now there, in days gone by. Surely, then, we ought to find the proof that they did so exist in the remains preserved to us in the rocks. Now that these remains prove that, for the most part, different species formerly lived upon the earth from those now inhabiting it, and that this difference steadily increases as the strata examined are more and more remote,—this geology proves incontestably. Still we have no such enormous number of strictly intermediate forms as might, *à priori*, have been expected. How is this? In the first place, it may be asked, how far is it really reasonable to expect that such intermediate forms should be preserved? Geological formations are undoubtedly going on at the present time; changes in species, at all events in domestic breeds, are also going on; how far, then, would these changes be perceptible in the formations? But rarely, and as it were by chance, do any remains of these animals or plants become entombed at all. Now and then a skeleton or some stray bones may be carried away by a river, or become embedded in sand or mud, not however without enormous risks of total disintegration; now and then a fallen

tree may sink into a muddy lake or bog and be preserved, if speedily enough excluded from the air; but how utterly inadequate would such occasional entombments be to afford an index of the whole existing fauna and flora,—how absurdly insufficient for pronouncing as to whether any changes in these or other species had been going on during the time of such formations. We may safely assert that the geological formations now being produced could only most exceptionably give any indication of the truth of Mr. Darwin's hypothesis, supposing that hypothesis to be true. On what principle and by what right, then, are we entitled to expect that past formations shall do so? and why should we regard their not doing so as an objection to the truth of the hypothesis? We know, again, that at the present time geological formations are purely local, and probably temporary, so that only a few parts of the whole earth's surface would have the remotest chance of having their inhabitants preserved. Is it not probable that the formations we now have in the rocks were equally local and equally temporary? Once more, we know that notable changes in the flora and fauna of places are often produced by the immigration of species from elsewhere, who supplant and extinguish the old ones. Is it fair, then, to ascribe similar sudden changes in the fossil remains of successive layers of deposits invariably to new creations? In a word, taking known facts touching present geologic changes as our guide, not one of the fancied objections to Darwinism drawn from the geologic records of the past can be allowed the slightest weight. It is most unreasonable to expect that there should be preserved in the rocks the innumerable intermediate forms which the Darwinian hypothesis requires, because of the extremely small proportion of formerly existing living beings possibly entombed there, and the probably local and temporary character of the deposits; while the difficulty which the sudden appearance of new species and groups of species is thought to present, falls to the ground at once when the known results of immigration are further borne in mind. And here we may fairly turn the question the other way, and ask what geological evidence would satisfy an anti-Darwinian? Suppose a series of intermediate varieties were shown linking together two successive species, what would he say to it? Why, that they were not distinct species at all, but merely varieties; or if the grades were a little less fine, that every one of the intermediate forms was itself a specially created and immutable species. The foregone conclusion would colour everything.

But, secondly, what evidence bearing upon the subject does geology really afford? It shows us an immense number of

additional species, all referable to the same great classes as those now existing, and mostly referable to the same orders also. It shows us that these species were most like in those periods of the earth's past existence nearest to each other in point of time, most unlike in those most remote. It gives us the clearest proof of gradual alteration in the predominant species from period to period, or even within the same period, each strata and each layer of strata being on the whole intermediate in character between those immediately above and below. It gives us especially a number of most valuable additional links in the chain of being, which tend to bring genus and genus, order and order, class and class, ever nearer and nearer to each other. In a word, its whole evidence is, considering its imperfect character, precisely what the Darwinian hypothesis would have led us to expect.

Thus on every hand, and in every possible way, the consistency of the theory is tried, and still it stands the test. In many respects, no doubt, the evidence at our disposal is insufficient to warrant definite conclusions; in others the consistency is rather hypothetically possible than demonstrably certain; but in no respect does there seem the slightest reason to pronounce it certainly inconsistent.

4. It remains only now to apply the last inquiry concerning the hypothesis;—is it *harmonious?* It is of course conceivable for an hypothesis to be both possible, adequate, and sufficient, so far as our evidence goes, and yet not be true. It is asked, then, is there any ground of analogy to render it probable that Darwinism, if it eventually answers these three main requirements, is the true explanation of the phenomena in question? In other words, is the method in which it asserts species to have originated one which there is reason to regard as in accordance with the ordinary and known workings of God? Here, then, we come to the Theology of Darwinism. Its relations to Scripture I purposely pass by, for I do not believe that Scripture was ever meant to teach us science, and hence that the less they are brought into comparison, the better for each. But as regards its Theology, I make two remarks. In the first place, it assumes no cause, force, or influence other than those known to be at work at the present day. By growth and reproduction, all living beings now propagate themselves, by inheritance they communicate their characteristics to their descendants, by natural selection the predominance of race and individual are determined; by these, co-working with variation, *some* changes at all events, be they few or many, be they great or small, are unquestionably produced. All that Darwinism requires of us is to be-

lieve that thus it has always been from the time when God first created living beings on the earth, and that to these causes are to be ascribed all the changes in such beings subsequently introduced. Looking at it in this light, I confess myself utterly at a loss to understand how any objection can possibly be taken to Darwinism theologically. We believe that all living things we now see about us were made by God, by means and under the influence of these causes involved in Darwinism; we feel no difficulty in so believing; why, then, should we feel difficulty in so believing as to all living things in the past? Nay, the analogy goes further yet. For if it be once established that the causes involved in Darwinism are adequate to produce the effects ascribed to them, then, being causes undoubtedly employed by God as instruments at the present time, there is at once the strongest possible presumption that they were the causes employed by Him in time past.

The second ground of analogy to which I would refer concerns the corresponding alterations in inanimate nature. The soil, the climate, the relations of sea and land, have differed as widely in bygone times from what they now are, as the species conditioned by them; they have changed, often contemporaneously as these have changed. In what manner do we conceive that these changes were brought about?—by miracles? No; but by the working of the same laws and forces as are at work at the present day. Darwinism, then, simply asks us to regard God's method of effecting changes in living beings as the same with His method of effecting changes in inanimate nature,—transition, extinction, development,—not fresh creation.

In conclusion, I would say that, as every one acquainted with Mr. Darwin's book will have seen long ago, the present paper makes no claim to originality. It is simply an attempt to exhibit in a concise form the logical value of the most important arguments adduced by Mr. Darwin, and the inference to be deduced from them. What that inference is, cannot, I think, be mistaken by any one who has followed the line of reasoning pursued. It is that Darwinism, though very far from being established as a true hypothesis, owing to lack of evidence in many important particulars, is yet supported by so strong an array of testimony of all kinds as to be certainly *credible*, and so a good working hypothesis for investigators to keep in mind. Mr. Darwin's own book is professedly but a meagre abstract of the evidence on behalf of the hypothesis he has in store. The full statement has long been promised, and, in respect to one important part of the subject, is announced as now "preparing for publication." It were

rashness in the extreme to jump to any definite conclusion until this fuller statement has been seen and weighed. And even then much further investigation into facts will probably be needed before a final decision can be made. Meanwhile, I submit that Darwinism is certainly to be maintained as *credible*.

THE CHAIRMAN.—I think you will all agree with me in passing a vote of thanks to Mr. Warington for the very admirable and distinct manner in which he has stated the arguments of Darwin. I think, whether we agree or disagree with Mr. Warington, we must be very much indebted to him for the lucid manner in which he has done this; and I will go so far as to say that I think he has done more justice to Darwinism than the book of Darwin himself. We have thought it expedient in a matter of this kind, particularly as the paper is not quite finished, and was not laid before the Council before it was read, that all discussion upon the subject should be deferred till another meeting. I may say that the paper is worthy of fair discussion, and I do not think it would be fair to discuss it without full preparation. Mr. Warington has stated the thing so clearly and systematically that as an anti-Darwinian I am much obliged to him; for it has only proved to me, if I may venture to express my humble opinion, that Darwinism is not a bit more credible than I thought it was before. But that is a matter on which persons have a right to form their own opinion; and Mr. Warington has put the matter in such a plain, logical, and dispassionate manner, as fairly to open up the question for future discussion, and in doing so I think he has done good service to the Victoria Institute.

MR. REDDIE.—I beg leave to announce that I have in my possession the first part of Mr. Warington's paper, which has been already written out, and that it will be in the printer's hands to-morrow morning. I may also venture to say—since Mr. Warington has promised me the remainder of the paper in a day or two—that copies of the whole paper will be in print and ready for distribution, to members who may wish to join in the discussion, by Saturday morning next. Sir, I cannot sit down without expressing how cordially I concur in your commendation of the clearness of Mr. Warington's paper, and in the vote of thanks to him for it. I especially wish to say this, because, as an anti-Darwinian, I must add, that I have not been in the least convinced by anything that Mr. Warington has advanced. On the contrary, after hearing his arguments, I feel if possible only the more persuaded that the theory of Mr. Darwin is *inharmonious*, *inadequate*, *inconsistent*, and utterly *incredible*. (Hear, hear.)

The Meeting was then adjourned.