

When the films of decomposed glass are circular spherical segments and colourless, the black cross and its accompanying tints are finely displayed, as in the system of rings seen along the axis of uniaxal crystals. When the films have the colour of thin plates, and are deeply spherical segments, the tints of the rings which accompany the black cross are singularly modified.

2. On Mr Darwin's Theory of the Origin of Species.

By Andrew Murray.

The position taken by Mr Darwin is, that all species have arisen by the natural process of ordinary generation. That the differences which we now see in them have arisen from slight variations in individuals having from time to time occurred, which have been perpetuated by inheritance, by successive stages and slow degrees, through unlimited spaces of time. Some of these slight variations he considers to originate in causes beyond our power of explanation, and which, although not the work of chance, we may call chance, for want of a better appellation—others to arise from habit, or from the excessive use or disuse of certain organs; but that when such a variation has once appeared, it is preserved by hereditary descent through a principle which he calls "natural selection," and which he deduces as a corollary from the struggle for existence which we see constantly going on around us. "As many more individuals," says he, "of each species are born than can possibly survive; and as, consequently, there is a frequently recurring struggle for existence, it follows that any being, if it vary, however slightly in any manner profitable to itself, under the complex and sometimes varying conditions of life, will have a better chance of surviving, and thus be *naturally selected*. From the strong principle of inheritance any selected variety will tend to propagate its new and modified form" (p. 5). Mr Darwin by no means shuns pushing his theory to its legitimate conclusion. In arguing as to the acquisition of new habits by some of his supposed transitional animals, he says, "In North America the black bear was seen by Hearne swimming for hours with widely-open mouth, thus catching, like a whale, insects in the water. Even in so extreme a case as this, if the supply of insects were constant, and if better adapted competitors did not already exist in the country, I can see no difficulty in a race of bears

being rendered by natural selection more and more aquatic in their structure and habits, with large and larger mouths, till a creature was produced as monstrous as a whale" (p. 184).* And the final conclusion to which he has arrived is summed up as follows:—
 "Analogy would lead me one step farther, namely, to the belief that all animals and plants have descended from some one prototype. But analogy may be a deceitful guide. Nevertheless, all living things have much in common in their chemical composition, their germinal vesicles, their cellular structure, and their laws of growth and reproduction. Therefore, I should infer from analogy, that probably all the organic beings which have ever lived on this earth have descended from some one primordial form into which life was first breathed" (p. 484).

Such is a general statement of the position taken by Mr Darwin; and in support of it, as might be anticipated from so accomplished a naturalist, we have in his work not only the chief arguments on which it rests ably stated, but numerous phenomena and facts in natural history applied to it, so as to test its probability by its consistency or inconsistency with them. These illustrations form, however, only a very small portion of the facts which he has accumulated, and which, he informs us, will be afterwards published in a larger and more elaborate treatise, and are now to be looked upon as no more than mere indications of the nature of the evidence he possesses, and proposes hereafter to adduce. A few of the most important of these I shall briefly notice, but I think their value may be perhaps better appreciated if I first state what I consider to be the essential qualities requisite for the existence and preservation of a species. I conceive that all species bear implanted within them two essential laws, without which they could not exist. The one, a power of accommodating themselves to a certain extent to circumstances; in other words, a power of modification or variation, as Darwin calls it. Without this the individuals composing the

* In quoting this, I do not at all mean to give it as a fair illustration of Mr Darwin's views. I only refer to it as indicating the extent to which he is prepared to go. The example here given I look upon (as I have reason to know Mr Darwin does himself) merely as an extreme and somewhat extravagant illustration, imagined expressly to show in a forcible way how "natural selection" would operate in making a mouth bigger and bigger, because more advantageous.

species would, under any change of circumstances, die, and, of course, the species would die with them. Now, it is not difficult to prove that this power of modification is possessed by plants and animals. I may instance the change which takes place in the wool of sheep, according as the animal is transferred from one climate to another—the change in the size of the chest and lungs which is said to take place in the second generation of animals transported from ordinary elevations to the intensely rarified air of lofty mountains, or the alteration that is found in shells, whether fresh-water or marine, when transferred into brackish water. But for evidence of this I need not go beyond the examples given by Mr Darwin himself. I think that all the instances of variation mentioned by him may be referred to this principle of modification. To this principle, and as designed for a similar purpose, do I refer the phenomena of hybridization. Putting aside a few exceptional cases, which may be explained on special grounds, I conceive that the well-known and undeniable general fact, that two distinct species may produce hybrid offspring, which hybrid offspring will be sterile either in the first or second generation, is strictly an instance of modification, allowed and intended for the preservation of the species. Conceive, for instance, a herd of deer, or any other animal, of which all the males have died off—conceive it to be the last herd of that species on the face of the earth. Except for this power of hybridization, the species is extinct, although it yet lives. Its propagation is at an end. No young can replenish its numbers, and the species endures only until the last individual has died off. But with the power of having fertile intercourse with a distinct species, another chance is given for its preservation. A hybrid is born; and if a male, it can have fertile offspring from the females of the original herd, and in a few generations all trace of the foreign blood will have been washed out. Such, I conceive, to be the uses of the principle of modification in its various phases, viz., the preservation of the species by the preservation and propagation of the individual. But the species may be lost in another way than by the death of its component individuals. It might, were there no check upon this power of modification, be lost by hybrids and modified individuals taking the place of species; in fact, were the power of variation unlimited and uncontrolled, all species would be confounded, and there would be nothing but an indiscriminate mass of creatures running all into each other, as

should be the case under Mr Darwin's theory were it true in fact.* Centaurs and mermaids, nay, even dryads, would cease to be impossible fables, and the beauty of creation would be lost in one undistinguishable chaos. To guard against this, and to preserve species from extinction by confusion, as well from extinction by death, nature has furnished species with another attribute as a counterpoise to the facility of modification and variation, and that is the tendency to reversion to type. This is seen working in two ways; the one in the reappearance of typical forms or peculiarities after having been absent for one or more generations. We see it well in our own race, where a parent's face and talents, lost in the child, reappear in the grandchild—where even hereditary diseases show themselves after the intermission of a generation or two. This phase of reversion to type is slightly alluded to, and slightly admitted as an element by Darwin. But the second, and, as it appears to me, by much the most important phase of reversion to type (and which is practically, if not altogether ignored by Mr Darwin), is the instinctive inclination which induces individuals of the same species by preference to intercross with those possessing the qualities which they themselves want, so as to preserve the purity or equilibrium of the breed. I again refer to our own race for an apt example. It is trite to a proverb, that tall men marry little women, tall women little men; a man of genius marries a fool, a great beauty the ugliest man she can find; and we are told that this is the result of the charm of contrast, or of qualities admired in others because we ourselves do not possess them. I do not so explain it. I imagine it is the effort of nature to preserve the typical medium of the race. Did a different feeling prevail, we should have our species broken up into giants and dwarfs, Newtons and idiots, Venuses or Apollos and satyrs, Sampsons and weaklings; or, if we should adopt Darwin's notions, the dwarfs, weaklings, and idiots, would all be extirpated by the predominancy of the stronger varieties. Now we know that this is not the case;

* One of Mr Darwin's explanations of the absence of intermediate forms may be taken as his answer to this objection—viz., that these forms are, in point of fact, numerically weaker than the forms on each side which they link together, and thus are liable to be exterminated sooner than them. But, admitting the fact to be that they are less numerous, why should they be so under Mr Darwin's theory? With unlimited powers of modification, why should the intermediate forms always be *originally* fewer.

and we may guess how strong the instinctive inclination for reversion to typical form is, when we look abroad among our acquaintances and see, notwithstanding the manifold inducements to disregard the promptings of nature consequent upon the artificial state in which we live, how few have refused compliance to this mysterious law. The control of parents, the desire for easily acquired wealth, the promptings of ambition, the cravings of vanity, and the accidents of opportunity, all suggesting other matrimonial connections, and, backed with what may be looked on as of more importance than either, the strong control over one's own feelings and desires acquired by the habits of civilised life, generally give way before this imperious constraint for reversion to type. It is less easy to give similar evidence of this phase of the revertive principle in other animals. In the wild, we only see its result in the uniformity of all individuals; in the domestic, man interferes, and by his breeding compels departure from the type, and increases it. But I believe it requires man's greatest care and watchfulness to prevent reversion, and that a breed neglected retrogrades in a very short time; and what is called the prepotent influence of pollen from the typical plant over that of neighbouring varieties is an instance which will be admitted by most hybridizers; and an analogous influence may be equally exercised in the case of hermaphrodites and fixed animals. This is my belief: but it is not that of all; the *possibility* of the new variety made by breeders and gardeners reverting to their parent forms is doubted by many, and denied by some. Mr Darwin of course disputes it, or at least does not admit it, and desiderates the evidence on which the statement has been so often made, that our domestic varieties, when run wild, gradually but certainly revert in character to their aboriginal stocks. Such a demand for proof may not be capable of immediate satisfaction. But where a fact is very generally accepted* as true, it will usually

* The point is one well worthy the attention of those who may have the opportunity of testing it. I have no doubt that many unscientific breeders could give at once instances which would bear upon it; but it will be observed that the question of whether they do bear upon it is one not unattended with difficulty: for instance, in our breeds of cattle how are we to know when a race or variety is reverting to its parent type—what was the parent form of our domestic cattle?—*Quien sabe*. But that they naturally retrograde or go away from the something which has been the aim set up in breeding to something else, certainly cannot be denied.

be found to be based on some foundation. *Vow populi, vow Dei*, is true in more sciences than politics. Passing this, however, I would next notice that the phenomena of hybridization do not stop with the law allowing the hybrid to have fertile offspring from the parent stock; there is another law which prevents it having such offspring from other hybrids or other species, and this is quite in accordance with my view of the precautions adopted by nature for the preservation of species. In the first place, *fertility*, to preserve it from extinction by extirpation of individuals; in the second place, *sterility*, to preserve it from extinction by confusion of races. Such are my views of the purpose and working of the compensating qualities implanted in species. And my first objection to the principles on which Mr Darwin's theory rests is, that it is founded on exaggerated and undue estimate of the one—the power of modification; and if not a negation, at least an inadequate concession of the other, viz. the principle of reversion to type.

Seeing, then, that the power of modification or variation is the principle on which his whole superstructure rests, Mr Darwin wisely takes care to fortify it by adducing striking instances illustrative of the extent to which this may take place. As the power of modification is to be seen in its most developed form in domestic animals, it is from them chiefly—indeed, so far as support to his theory goes, I may say it is from them entirely—that his illustrations are drawn. Now, it is usually said that domestic animals are not fair examples from which to reason in inquiries into species and their origin; and it is thought that the artificial circumstances under which they live alter their system so much as to render any argument drawn from them not worthy of reliance. I have no doubt that such artificial life and great change of habits has an important effect upon these animals, and more especially upon their reproductive system, different conditions of which (as Mr Darwin has well shown) have much effect in inducing subsequent variations in their descendants; but, as already said, I imagine a still more potent cause of the greater variability of domesticated animals to lie in their being deprived, through the agency of man, of the opportunity of allowing the revertive principle to come into operation by intercrossing. But it is no part of my argument to dispute the power of variation within certain limits; and as, for the above

reasons, I admit them to be greatest in domesticated species, I need not dally over the instances given by Mr Darwin, however interesting and suggestive they may be. I shall merely observe, with regard to those facts which he adduces as instances of natural variation, in contradistinction to artificial or domesticated variation, that they are limited, and consist (as it appears to me) entirely of such minor modifications as I have already alluded to, as permitted more or less in all species for the preservation of the individual. Seizing upon the variations (of which there is no want) which have given rise to disputes among naturalists as to whether particular forms are mere varieties or true species, he thence infers that these are species in the course of transmutation. That some naturalists, with too quick a perception of differences, should attempt to make species or sub-species out of varieties, should not prejudice the question; the blunders of the few are frail ground on which to rest a theory; and if the concurrent opinion of the majority be taken, the number of forms as to which doubt may fairly be entertained is comparatively few. And this Mr Darwin, with his usual fairness, frankly admits. "It may be asked," says he, "how is it that varieties which I have called incipient species become ultimately converted into good and distinct species, *which in most cases obviously differ from each other more than do the varieties of the same species;*" and he sets himself to account for this, but does not dispute the fact. He gives no instance of any wild plant or animal, subject to no restriction as to intermixture, having within the knowledge of man deviated into a well-established constant form, which would be admitted as a species by naturalists. He refers to Mr Buckman's experiments, as showing the extent of variation capable of being assumed by plants; but, on the one hand, these experiments may be classed as instances of artificial selection; and on the other, I hear (I have no personal knowledge on the subject) that there is considerable difference of opinion among botanists as to their trustworthiness. And even although they were trustworthy, their result is merely to show how various the modifications are which take place under altered circumstances, a fact which I do not deny. A writer in the "National Review" offers to supplement such instances by quoting from M. Roulin two naturally modified breeds of cattle descended from the cattle of the Pampas, and now found in the hottest parts of South America; one of

them called *Pelones*, and the other *Calougas*; the former possessing a very scanty fine fur, and the latter without any hair at all, and each peculiar to the district it inhabits, and either not transferable, or with difficulty transferable, into any colder region. He thinks that these would be admitted as species by naturalists. Far from it. It is merely a simple case of modification to suit altered condition of life. It is exactly the same case in oxen as we see in the Merino and Australian sheep; but such a variation is not what we desiderate. Show us an animal between the ox and the sheep, or rather a series of animals exhibiting the transitions between them. But Mr Darwin, in reply, tells us, that we cannot expect to trace these new species in their actual transit. While commencing their variation, we call them varieties; when they are farther removed, we dispute which they are; when they are complete, we call them species. He with some justice (but not entire justice) remarks, that we are here, as compared with the great spaces of time which he requires for the development of his new species, merely at a single point of view, and at no one point can you expect to see a passage taking place, because the assumption is that every passage is gradual. We see the present species; but we do not know that we either see its parent or its descendant. I admit that, under such premises, we cannot see the passage; but surely over the whole surface of the earth, and out of all the living creatures swarming upon it, we ought to detect some species whose parents have not yet perished, and whose descendants have already appeared. Mr Darwin would like to escape from this position—but he cannot. He says “It should always be borne in mind what sort of intermediate forms must on my theory have formerly existed. I have found it difficult, when looking at any two species, to avoid picturing to myself forms *directly* intermediate between them. But this is a wholly false view; we should always look for forms—*intermediate* between each species, and a common but unknown progenitor” (p. 280). Now this is merely confusing the thing; the process being gradual, there must be some exactly and directly true half-way intermediate form between the parent species and the descendant species, and it matters not to us that we know only one of these, nor does it matter that we know neither. What concerns us is, that there ought to be half-way steps between every form and something else which is either now living, or which has lived, on the face of the

earth, and if such do exist, we ought to find,—I do not say all,—but certainly many, or at least some of these. So far as regards the present age, Mr Darwin apologises for the absence of such intermediate forms, by supposing migrations to have taken place over large continuous areas, and the links to have been lost in the intermediate districts from unsuitableness of condition, or from geological changes having submerged certain districts, when, of course, the links existing here would be lost, and concludes a very specious and plausible argument on this head thus:—“Lastly, looking not to any one time, but to all time, if my theory be true, numberless intermediate varieties, linking most closely all the species of the same group together, must assuredly have existed; but the very process of natural selection constantly tends to exterminate the parent forms and the intermediate links, consequently evidence of their former existence could be found only amongst fossil remains” (pp. 177–9). Now, so far as these explanations are merely an answer to the question, Why we do not find such intermediate passages in any one particular portion of the globe? they might be accepted as an apology for their absence; but when applied to the whole of it, and to such myriads of creatures as inhabit it, it seems beyond all reasonable application of the doctrine of chances to accept it as a sufficient or even probable explanation. The very essence of the new theory is gradual passage, and slow descent by natural generation and inheritance—the parent species and the incipient species both subsisting at the same time, and the process of substitution being gradual and protracted. Mr Darwin’s own map of divergence, and the whole of his reasoning go to show how parent forms, and descendant and collateral forms, may all subsist and be going on in different localities and climates at the same time. It will not therefore do to say that the new varieties developed by natural selection “continually take the place of, and exterminate their parent forms,” and so prevent the occurrence of innumerable intermediate links everywhere throughout nature. But supposing that, for the sake of argument, we allow this apology for the moment, at least it can only apply to the present age of nature, or to some one definite period—it cannot also apply to past ages, or to any two or more consecutive ages; and Mr Darwin, as we have just seen, admits that “evidence of their former existence should be found amongst fossil remains” (p. 280). Are fossil remains of these then found? Is there any evidence in support of this to be drawn from

fossil remains? Mr Darwin himself shall answer the question. "Why, then," says he, "is not every geological formation and every stratum full of such intermediate links? Geology *assuredly 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" (p. 280). And a very grave objection it certainly is, that in the only two quarters where actual proof of facts (which must exist if the theory be true) can be sought for, and where, *à priori*, they might reasonably have been expected to be found, namely, the present and the past, they should be absent, or at least undiscoverable.

Those who are new to the subject may naturally be puzzled to guess how he escapes from such an embarrassing dilemma. The solution is abundantly simple. "The explanation," says he, "lies, as I believe, in the extreme imperfection of the geological record." Now I believe no one will dispute as an abstract proposition the extreme imperfection of the geological record. But I cannot admit that its imperfection is of that character or degree that will entitle Mr Darwin to plead it in his favour. He dwells on the poorness of our palæontological collections—the great spaces of time wholly, or nearly wholly, unrepresented in them—the extreme rarity of terrestrial animals in the deposits—the destruction of the soft parts of most animals, and the crushed state of many others. I shall not follow him into his details on these points. All that he says on the subject may be very true—is very true—but will avail him nought if, in any portion of the geological records, we can find any one succession of strata of moderate depth which may be fairly held to have been deposited unintermittently, and in which we find a liberal representation of the animals of any one class. And such records many of the enormous deposits of limestone rocks beyond doubt are—their whole phenomena indicating an uninterrupted period of tranquil deposition, extending over ages beyond our numbering, and the strata themselves bearing in their bosoms an excellent report on the molluscous animals of the period.* I have quite sufficient to test Mr Darwin's

* Mr Darwin himself remarks, that "two palæontologists, whose opinion is worthy of much deference, namely, Bronn and Woodward, have concluded that the average duration of each formation is twice or thrice as long as the average duration of specific forms" (p. 293). This opinion may be well-founded or not—I imagine it is; but it is difficult of application to the point at issue, on account of the real or possible intermissions which may have taken place in these formations.

apology, if I have, first, a lengthened and uninterrupted period of deposit, and next, the marine fossils in any one class of that period well represented in it. If I am told that such representation in fossils is not only imperfect as regards terrestrial and soft animals, but also as regards molluscous shell-bearing animals, I shall go to issue upon the point, and, I think, prove that we ought to have, and do have, as good a knowledge of what were the species of shell-bearing molluscs which lived in the seas which produced many of our fossil deposits, as we have of those living at the bottom of our own seas at the present day; and no one will say that that knowledge is very imperfect. A moment's consideration of the respective means we have of knowing each will show the probability of this. The only means we have of knowing the species in our present seas is by dredging, or by the still more imperfect system of picking up those shells which may be cast ashore. Now, dredging is a mere scraping of a little morsel of the bottom of the sea here and there; and yet, by adding up the accumulated observations made in various quarters, we have arrived at a most accurate knowledge of the inhabitants of those seas which have been examined. Some shells remain rare, others unique, but this does not prevent us believing in the accuracy of our knowledge. Compare this scraping here and there in the dark, with the deliberate open-day examinations which we can make of most geological strata; miles upon miles of coast cliffs—transverse sections in ravines—and piece by piece manipulation in quarries and mines—and I think it must be admitted, that so far as that class of animals which *can* be preserved in deposits goes, it cannot be said that our knowledge of them in continuous strata is imperfect; and as, therefore, we should there find the intervening links between older and younger species if they existed, and yet do not find them, the inevitable inference is that they do not exist.

Untenable as they appear to be, however, these arguments or apologies have satisfied Mr Darwin, and his system of natural *variation* being once admitted or held as proved, the remaining steps to natural *selection* are easy. The most essential, and one as to which I do not suppose there can be any difference of opinion, is founded on what he calls the struggle for existence. That such a struggle is constantly going on is familiar to us all; but, as I neither dispute its existence nor its bearing (always supposing his other premises to be correct), I shall not make any remarks upon it, or on some of

the other minor branches of his argument, such as sexual selection, divergence of character, the effects of use and disuse, acclimatization, laws of variation and geographical distribution (on all of which, had I space and time, I see much which I feel disposed to modify or controvert). These must either stand or fall with the other parts of his theory already noticed, or do not come into direct collision with the more vital objections which I have to it. There is, however, one topic still remaining which I should not wish so to pass over—viz., the effect of physical condition in influencing form originally, and affecting it afterwards. On this point Mr Darwin and I are widely at issue. He believes that it has had little or no influence upon them. “Neither the similarity nor the dissimilarity of the inhabitants of various regions,” says he, “can be accounted for by their climatal and other physical conditions.” To me, again, it appears that the effect of physical condition is one of the most powerful agents in determining the form of organic creatures; and I must be pardoned if I devote a few sentences to this part of the subject, because I look upon it as of the greatest importance, and ranking in the same category and scarcely less powerful as a proof of design on the part of the Creator than that drawn from the anatomical structure of the animal frame. Some of the instances bearing on it, given by Mr Darwin, are certainly difficult of explanation; but then, how little do we know of what the real essence of physical condition is! Look at North America, which in temperature and many other respects has a physical condition not greatly differing from our own. See how the inappreciable difference in physical condition is telling upon the white race there; the women in youth retaining the normal beauty of their race, but becoming prematurely old; the men becoming thin and sallow; the teeth decaying more rapidly; the average duration of life diminishing,—besides various other tokens of unsuitableness of climate. We all know very well that Bangalore, Darjeeling, and other hill stations in India, are mere expedients, better than nothing, but that the only real remedy for the sick Englishman is home,—home to the native physical condition. Seeing, then, that the essential part of physical condition is something of so subtle and undetectable a nature, is it a fair estimate of its effect to say, as Mr Darwin does, that certain large tracts in South Africa and South America are placed under like physical conditions; therefore, if there is any value in them, show us like product?

What! South America with its cordillera, and weeping Southern Archipelago, under similar physical conditions with arid South Africa? Even the unassisted eye can see that this is not so. But Mr Darwin chooses a test by which I am willing to abide (and which I had indeed selected for the same purpose in a paper I read on the Disguises of Nature at the meeting of the British Association at Aberdeen). It is the blind cave animals found in the limestone caverns both in Europe and America. Mr Darwin says, "It is difficult to imagine conditions of life more similar than deep limestone caverns under a nearly similar climate; so that on the common view of the blind animals having been separately created for the American and European caverns, close similarity in the organisation and affinities might have been expected; but as Schiodte and others have remarked, this is not the case, and the cave insects* of the two continents are not more closely allied than might have been anticipated from the general resemblance of the other inhabitants of North America and Europe." Now Mr Darwin, in this passage, has quite mistaken the gist of Schiodte's remark, and consequently misapplied it. It is quite correct for him to say that we should expect close similarity in the caves in question, but it is incorrect to say that "this is not the case;" for the similarity in some is marvellously close; and it is also incorrect to say that Schiodte and others have remarked that "this is not the case." As to the "others," indeed, I cannot speak, for I do not know to whom he refers, and I do not know any other author than Schiodte except Müller, who has written, from original observation, otherwise than incidentally upon the subject; but neither he nor Schiodte make any such remark. I presume the others alluded to by Mr Darwin are those who have followed Schiodte, and adopted or quoted his remark. The remark which he makes, and Mr Darwin has misapplied, is, "that the cave insects of the two continents are not more closely allied than might have been anticipated from the general resemblance of the other inhabitants of North America and Europe;"—a loose general remark, which, like an ancient oracle,

* Although Mr Darwin here uses the observations of Schiodte upon blind insects as an illustration, his remarks (as he himself has had the kindness to inform me) are not meant to be confined to them, but also to be applied to the whole of the animals found in caves. But as his theory, if true, should meet every case, a clear flaw in even one would be fatal to the whole, and I would have tested it with these insects, whether they had been referred to by Mr Darwin himself or not.

may be read either way. Darwin (a disbeliever in the effects of physical condition), we see, reads it that the resemblance is slight; Schiodte, on the other hand, who appears to be a thorough believer in its effects, sees nothing more in the marvellous resemblance than might have been anticipated. I say that he seems to be a thorough believer, because he goes so far as to separate those insects which are found in stalactite caves from those in other caverns, maintaining (and I believe with justice) that the two kinds were respectively confined to these classes of caves. But let us see how the fact actually stands as to resemblance. I shall take the eyeless *Anophthalmi*. It is not the only one which would suit me, but it is the most striking. Although belonging to the family of *Trechidæ*, it possesses very marked and distinctive characters, besides the want of eyes. Nothing comes very close to it. It stands out and apart, and can be distinguished in a moment. It is found nowhere but deep in limestone caverns; but this generic form is repeated by *different species* in almost every cave which has been examined. In the caves of Adelsberg in Carniola, the two species *Anophthalmus Schmidtii* and *Anophthalmus Bilimekii* are found. *Anophthalmus hirtus* and *Anophthalmus Hacquetii*, in the Grotto of Krimberg in Oberiggdorf; *Anophthalmus Scopoli*, in the Grotto of Setz in Corinthia; *Anophthalmus Doriæ* in the Grotto des Ours in Eastern Liguria. *Anophthalmus Ghiliani* has been taken in a cavern at Monte Viso, near the French frontiers. *Anophthalmus Gallicus* and *Anophthalmus Pandellei* in the Grotto of Betharram in the low Pyrenees; *Anophthalmus Crypticola* and *Anophthalmus Orcinus*, in the Grotto of Gargas, high Pyrenees; *Anophthalmus Raymondi* in two caves near Marseilles; and *Anophthalmus Tellkampfi* in the Mammoth Caves of Kentucky, all confined to their own caves, or districts of caves, and found nowhere else. Now, how is this? When I first became acquainted with Mr Darwin's theory, it was from the perusal of the short notice of its main elements, published about eighteen months ago in the Linnean Society's Proceedings; and the imperfect account of it there given induced me to suppose that he held that every species was descended from the one nearest to it, and hence, to infer that he would hold that all the *Anophthalmi* were connected one with the other by direct descent, and I imagined that the fact of closely allied species being found in the caves of Kentucky and the caves of Carniola, without any means of communication with each other, must be fatal

to his theory; but now that I understand it more correctly, I see that this difficulty can be got over by referring the two (as diverging descendants) to some common ancestor, not eyeless, who may have lived where the Atlantic Ocean now rolls, at some distant period when America and Europe were united. Therefore, the fact has not the significance I supposed. A *Trechus* may have wandered into each of these caves, and by process of natural selection, after frequent variation into all manner of other forms, which being unsuitable have not been preserved, have at last hit upon the form of an *Anophthalmus*. But if he thus saves his theory, what becomes of his disbelief in the effects of physical condition? If it has no effect, why have they all turned into *Anophthalmi*? The only explication which I can imagine for him is, that in every cave *Trechi* entered, and in each and all threw off descendants of all different kinds, as well as *Anophthalmi*, none of which were suited to the physical condition except the *Anophthalmi*, and therefore the latter alone survived. Whether this is a more philosophical explanation of their presence than the view that their production was influenced by the physical condition of the place, I leave to the reader to determine.

I shall only follow Mr Darwin for a few lines farther in his remarks on this subject. He says, "On my view, we must suppose that American animals having ordinary power of vision slowly migrated by successive generations from the outer world into the deeper and deeper recesses of the Kentucky Caves, as did European animals into the caves of Europe. We have had some evidence of this gradation of habit; for, as Schiodte remarks, 'animals not far remote from ordinary forms prepare the transition from light to darkness. Next follow those that are constructed for twilight; and last of all, those destined for total darkness.'" If Darwin reads this as meaning that there is a gradation in form and affinity between the animals which are found at the entrance, and those found in total darkness, he is in error—there is none. It is the gradation in adaptation to darkness that Schiodte is speaking of. Those at the entrance, with small eyes, belong to the *Pristonychi*, large black beetles found in cellars and such places. The *Anophthalmi* belong to the small *Trechidæ*, of which there are none specially found at the entrance.

I might take other exception to the facts adduced by Mr Darwin, or to his application of them. For instance, he says of the

woodpecker, "Can a more striking instance of adaptation be given than that of a woodpecker for climbing trees, and for seizing insects in the chinks of the bark? Yet in North America there are woodpeckers which feed largely on fruit, and others with elongated wings which chase insects on the wing; and on the plains of La Plata, where not a tree grows, there is a woodpecker, *which in every essential part of its organisation, even in its colouring, in the harsh tone of its voice, and undulatory flight, told me plainly of its close blood relationship to our common species; yet it is a woodpecker which never climbs a tree.*" (P. 184.) I have selected this instance both as a statement bearing upon the effect of physical condition which appears to me to require correction, and also as an illustration of the necessity, in such an investigation as this, of testing every fact before admitting it. This is a statement made upon Mr Darwin's own personal observation, confirming that of Azara. I do not believe there is a more upright and truthful man in Britain than Mr Darwin, and yet we look at things from such an opposite point of view, that I not only do not see what he avers in the above instance, but see quite the reverse. The woodpeckers he refers to are *Colaptes* (the La Plata species is, I believe, the *Colaptes campestris*); and so far from appearing to me to possess every essential point of the organisation of a woodpecker, they are one of the very instances which I have been in use to give as showing the alteration of structure in a type consequent upon different physical conditions of life. The *Colaptes*, although allied to the woodpeckers, differ from them in mode of life, inasmuch as they feed upon ants; and, in structure, inasmuch as not requiring that most essential part of the organisation of a woodpecker (its peculiar hammering bill and strong tail) they do not possess them, while they retain the peculiar tongue and accessory muscles still necessary for securing their insect food.

The strongest points in favour of the general results come to by Mr Darwin, are a class of facts which can scarcely be said to bear distinctively on his theory more than upon various other theories already promulgated, and more or less adopted. One of these is the fact, that all animals, and all plants, throughout all time and space, should be related to each other in group subordinate to group. Another not less formidable fact is the existence of the same homological parts in different animals, sometimes aborted, and sometimes largely developed.

These are two of the great difficulties attendant on the view of the independent creation of each individual species. But although they *were* fatal to that view, it does not fall to Mr Darwin as sole Œdipus to solve them. The doctrine of progressive development (to which Mr Darwin's view has many points of affinity), or any doctrine in which development of species *ex ovo* plays a part, will explain these facts equally well. The germ must bear some trace of its origin; and hence we should, under such a theory, see not only the relationships and homologies referred to, but also certain appearances which bear indications of reversion to type, such as the appearance of the stripes of the tiger in the young of the lion, &c. These, I own, are difficult to be explained (I do not say unexplainable) under the theory of independent creation, but natural, and to be expected, under any theory of development *ex ovo*,—not more under Mr Darwin's than under any other. The distinctive character of Mr Darwin's theory is not development *ex ovo*; that is the theory of Oken, of Agassiz, of the author of the "Vestiges of Creation;" nay, I may go farther back. It is the theory of Bonnet and of Priestley, who, however *involved* their ideas might be, still held "that all the germs of future plants, organical bodies of all kinds, and the reproducible parts of them, were really contained in the first germ." Darwin's, on the other hand, is gradual transition by slow and scarcely perceptible degrees; and, *so far as that specialty is concerned*, it has no more bearing than Oken's upon the classes of facts above referred to; and the distinction between them is not confined merely to the *modus operandi* of the process of development; it is much more material than that; it embraces the question of final causes, and bears on the very existence of design in the organic creation. The views of Agassiz and Oken do not challenge the fact of design existing in the wonderful adaptations of structure to purpose which we see everywhere displayed in living organisms. *Their* theory allowed us to retain our belief in the great argument on which the whole of natural theology is based; nay, even to place it on higher grounds, as the intelligence which performs its work by the intervention of a law or machinery designed by itself, and operating on a great scale, is superior to the intelligence which executes each individual detail directly and without such intervention. If it furnished no explanation of the causes of adaptation of structure to habit, at least it

did not prevent us from holding, if we chose, that, by some unexplained means, the germ of life was supplied with such a principle of growth as, under certain physical conditions, developed itself into these adaptations. We could hold design still to be there, although its direct means of operation was shrouded from our view in the laboratory of Nature. But Mr Darwin's theory is not only opposed, but absolutely inconsistent with any such idea. The talons of the eagle have not been framed as they are by design, to seize and hold its prey. The wonderfully constructed hand of the mole was not a designed gift from the Creator, but merely some variety of the hedgehog, which had broadest paws, and, being most adapted to digging, adopted the mode of life of the mole. The implement was not made for the animal, but the animal for the implement. The assumption is, that it is not alone beneficial variations which Nature makes. She makes them in any and every way; some being profitable, others the reverse; and the reason why we find all that have ever been seen on the face of the earth beneficially endowed (that is, provided with structures which, to the unilluminated eye, indicate design) is, that only those variations which happen to have been so endowed have been preserved,—the blots which Nature made having become extinct through the preponderance of the beneficially endowed. To use Mr Darwin's words, "Natural selection is daily and hourly scrutinizing throughout the world every variation, even the slightest; rejecting that which is bad, preserving and adding up all that is good, silently and insensibly working whenever and wherever opportunity offers at the improvement of each organic being, in relation to its organic and inorganic conditions of life." (P. 84.) Now, I cannot believe in such doctrine. When I look at the anatomy of any part of the body, and see exactly the same mechanism and contrivances had recourse to which a mechanician would have used to secure similar results, I cannot bring myself to believe that it is fortuitous, or other than evidence of the presence of direct design. A belief in such design I should be most loath to surrender, and I am therefore glad that, on other grounds, viz. the legitimate result of the argument already discussed, I have come to be of opinion that Mr Darwin's theory is unsound, and that I am to be spared any collision between my inclinations and my convictions.