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Title: A Critical Examination of the Position of Mr. Darwin's Work, "On the Origin of Species," in Relation to the Complete Theory of the Causes of the Phenomena of Organic Nature

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Release date: November 1, 2001 [EBook #2926]  
Most recently updated: January 22, 2013

Language: English

Credits: Produced by Amy E. Zelmer, and David Widger

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**A CRITICAL EXAMINATION  
OF THE POSITION  
OF MR. DARWIN'S WORK,  
"ON THE ORIGIN OF SPECIES,"  
IN RELATION TO THE COMPLETE THEORY  
OF THE CAUSES OF THE PHENOMENA OF  
ORGANIC NATURE**

**Lecture VI. (of VI.), "Lectures To Working Men", at the  
Museum of Practical Geology, 1863, On Darwin's work:  
"Origin of Species".**

**By Thomas H. Huxley**

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IN the preceding five lectures I have endeavoured to give you an account of those facts, and of those reasonings from facts, which form the data upon which all theories regarding the causes of the phenomena of organic nature must be based. And, although I have had frequent occasion to quote Mr. Darwin—as all persons hereafter, in speaking upon these subjects, will have occasion to quote his famous book on the "Origin of Species,"—you must yet remember that, wherever I have quoted him, it has not been upon theoretical points, or for statements in any way connected with his particular speculations, but on matters of fact, brought forward by himself, or collected by himself, and which appear incidentally in his book. If a man 'will' make a book, professing to discuss a single question, an encyclopaedia, I cannot help it.

Now, having had an opportunity of considering in this sort of way the different statements bearing upon all theories whatsoever, I have to lay before you, as fairly as I can, what is Mr. Darwin's view of the matter and what position his theories hold, when judged by the principles which I have previously laid down, as deciding our judgments upon all theories and hypotheses.

I have already stated to you that the inquiry respecting the causes of the phenomena of organic nature resolves itself into two problems—the first being the question of the origination of living or organic beings; and the second being the totally distinct problem of the modification and perpetuation of organic beings when they have already come into existence. The first question Mr. Darwin does not touch; he does not deal with it at all; but he says—given the origin of organic matter—supposing its creation to have already taken place, my object is to show in consequence of what laws and what demonstrable properties of organic matter, and of its environments, such states of organic nature as those with which we are acquainted must have come about. This, you will observe, is a perfectly legitimate proposition; every person has a right to define the limits of the inquiry which he sets before himself; and yet it is a most singular thing that in all the multifarious, and, not unfrequently, ignorant attacks which have been made upon the 'Origin of Species', there is nothing which has been more speciously criticised than this particular limitation. If people have nothing else to urge against the book, they say—"Well, after all, you see, Mr. Darwin's explanation of the 'Origin of Species' is not good for much, because, in the long run, he admits that he does not know how organic matter began to exist. But if you admit any special creation for the first particle of organic matter you may just as well admit it for all the rest; five hundred or five thousand distinct creations are just as intelligible, and just as little difficult to understand, as one." The answer to these cavils is two-fold. In the first place, all human inquiry must stop somewhere; all our knowledge and all our investigation cannot take us beyond the limits set by the finite and restricted character of our faculties, or destroy the endless unknown, which accompanies, like its shadow, the endless procession of phenomena. So far as I can venture to offer an opinion on such a matter, the purpose of our being in existence, the highest object that human beings can set before themselves, is not the pursuit of any such chimera as the annihilation of the unknown; but it is simply the unwearied endeavour to remove its boundaries a little further from our little sphere of action.

I wonder if any historian would for a moment admit the objection, that it is preposterous to trouble ourselves about the history of the Roman Empire, because we do not know anything positive about the origin and first building of the city of Rome! Would it be a fair objection to urge, respecting the sublime discoveries of a Newton, or a Kepler, those great philosophers, whose discoveries have been of the profoundest benefit and service to all men,—to say to them—"After all that you have told us as to how the planets revolve, and how they are maintained in their orbits, you cannot tell us what is the cause of the origin of the sun, moon, and stars. So what is the use of what you have done?" Yet these objections would not be one whit more preposterous than the objections which have been made to the 'Origin of Species.' Mr. Darwin, then, had a perfect right to limit his inquiry as he pleased, and the only question for us—the inquiry being so limited—is to ascertain whether the method of his inquiry is sound or unsound; whether he has obeyed the canons which must guide and govern all investigation, or whether he has broken them; and it was because our inquiry this evening is essentially limited to that question, that I spent a good deal of time in a former lecture (which, perhaps, some of you thought might have been better employed), in endeavouring to illustrate the method and nature of scientific inquiry in general. We shall now have to put in practice the principles that I then laid down.

I stated to you in substance, if not in words, that wherever there are complex masses of phenomena to be inquired into, whether they be phenomena of the affairs of daily life, or whether they belong to the more abstruse and difficult problems laid before the philosopher, our course of proceeding in unravelling that complex chain of phenomena with a view to get at its cause, is always the same; in all cases we must invent an hypothesis; we must place before ourselves some more or less likely supposition respecting that cause; and then, having assumed an hypothesis, having supposed cause for the phenomena in question, we must endeavour, on the one hand, to demonstrate our hypothesis, or, on the other, to upset and reject it altogether, by testing it in three ways. We must, in the first place, be prepared to prove that the supposed causes of the phenomena exist in nature; that they are what the logicians call 'vera causae'—true causes;—in the next place, we should be prepared to show that the assumed causes of the phenomena are competent to produce such phenomena as those which we wish to explain by them; and in the last place, we ought to be able to show that no other known causes are competent to produce those phenomena. If we can succeed in satisfying these three conditions we shall have demonstrated our hypothesis; or rather I ought to say we shall have proved it as far as certainty is possible for us; for, after all, there is no one of our surest convictions which may not be upset, or at any rate modified by a further accession of knowledge. It was because it satisfied these conditions that we accepted the hypothesis as to the disappearance of the tea-pot and spoons in the case I supposed in a previous lecture; we found that our hypothesis on that subject was tenable and valid, because the supposed cause existed in nature, because it was competent to account for the phenomena, and because no other known cause was competent to account for them; and it is upon similar grounds that any hypothesis you choose to name is accepted in science as tenable and valid.

What is Mr. Darwin's hypothesis? As I apprehend it—for I have put it into a shape more convenient for common purposes than I could find 'verbatim' in his book—as I apprehend it, I say, it is, that all the phenomena of organic nature, past and present, result from, or are caused by, the inter-action of those properties of organic matter, which we have called ATAVISM and VARIABILITY, with the CONDITIONS OF EXISTENCE; or, in other words,—given the existence of organic matter, its tendency to transmit its properties, and its tendency occasionally to vary; and, lastly, given the conditions of existence by which organic matter is surrounded—that these put together are the causes of the Present and of the Past conditions of ORGANIC NATURE.

Such is the hypothesis as I understand it. Now let us see how it will stand the various tests which I laid down just now. In the first place, do these supposed causes of the phenomena exist in nature? Is it the fact that in nature these properties of organic matter—atavism and variability—and those phenomena which we have called the conditions of existence,—is it true that they exist? Well, of course, if they do not exist, all that I have told you in the last three or four lectures must be incorrect, because I have been attempting to prove

that they do exist, and I take it that there is abundant evidence that they do exist; so far, therefore, the hypothesis does not break down.

But in the next place comes a much more difficult inquiry:—Are the causes indicated competent to give rise to the phenomena of organic nature? I suspect that this is indubitable to a certain extent. It is demonstrable, I think, as I have endeavoured to show you, that they are perfectly competent to give rise to all the phenomena which are exhibited by RACES in nature. Furthermore, I believe that they are quite competent to account for all that we may call purely structural phenomena which are exhibited by SPECIES in nature. On that point also I have already enlarged somewhat. Again, I think that the causes assumed are competent to account for most of the physiological characteristics of species, and I not only think that they are competent to account for them, but I think that they account for many things which otherwise remain wholly unaccountable and inexplicable, and I may say incomprehensible. For a full exposition of the grounds on which this conviction is based, I must refer you to Mr. Darwin's work; all that I can do now is to illustrate what I have said by two or three cases taken almost at random.

I drew your attention, on a previous evening, to the facts which are embodied in our systems of Classification, which are the results of the examination and comparison of the different members of the animal kingdom one with another. I mentioned that the whole of the animal kingdom is divisible into five sub-kingdoms; that each of these sub-kingdoms is again divisible into provinces; that each province may be divided into classes, and the classes into the successively smaller groups, orders, families, genera, and species.

Now, in each of these groups, the resemblance in structure among the members of the group is closer in proportion as the group is smaller. Thus, a man and a worm are members of the animal kingdom in virtue of certain apparently slight though really fundamental resemblances which they present. But a man and a fish are members of the same sub-kingdom 'Vertebrata', because they are much more like one another than either of them is to a worm, or a snail, or any member of the other sub-kingdoms. For similar reasons men and horses are arranged as members of the same Class, 'Mammalia'; men and apes as members of the same Order, 'Primates'; and if there were any animals more like men than they were like any of the apes, and yet different from men in important and constant particulars of their organization, we should rank them as members of the same Family, or of the same Genus, but as of distinct Species.

That it is possible to arrange all the varied forms of animals into groups, having this sort of singular subordination one to the other, is a very remarkable circumstance; but, as Mr. Darwin remarks, this is a result which is quite to be expected, if the principles which he lays down be correct. Take the case of the races which are known to be produced by the operation of atavism and variability, and the conditions of existence which check and modify these tendencies. Take the case of the pigeons that I brought before you; there it was shown that they might be all classed as belonging to some one of five principal divisions, and that within these divisions other subordinate groups might be formed. The members of these groups are related to one another in just the same way as the genera of a family, and the groups themselves as the families of an order, or the orders of a class; while all have the same sort of structural relations with the wild rock-pigeon, as the members of any great natural group have with a real or imaginary typical form. Now, we know that all varieties of pigeons of every kind have arisen by a process of selective breeding from a common stock, the rock-pigeon; hence, you see, that if all species of animals have proceeded from some common stock, the general character of their structural relations, and of our systems of classification, which express those relations, would be just what we find them to be. In other words, the hypothetical cause is, so far, competent to produce effects similar to those of the real cause.

Take, again, another set of very remarkable facts,—the existence of what are called rudimentary organs, organs for which we can find no obvious use, in the particular animal economy in which they are found, and yet which are there.

Such are the splint-like bones in the leg of the horse, which I here show you, and which correspond with bones which belong to certain toes and fingers in the human hand and foot. In the horse you see they are quite rudimentary, and bear neither toes nor fingers; so that the horse has only one "finger" in his fore-foot and one "toe" in his hind foot. But it is a very curious thing that the animals closely allied to the horse show more toes than he; as the rhinoceros, for instance: he has these extra toes well formed, and anatomical facts show very clearly that he is very closely related to the horse indeed. So we may say that animals, in an anatomical sense nearly related to the horse, have those parts which are rudimentary in him, fully developed.

Again, the sheep and the cow have no cutting-teeth, but only a hard pad in the upper jaw. That is the common characteristic of ruminants in general. But the calf has in its upper jaw some rudiments of teeth which never are developed, and never play the part of teeth at all. Well, if you go back in time, you find some of the older, now extinct, allies of the ruminants have well-developed teeth in their upper jaws; and at the present day the pig (which is in structure closely connected with ruminants) has well-developed teeth in its upper jaw; so that here is another instance of organs well-developed and very useful, in one animal, represented by rudimentary organs, for which we can discover no purpose whatsoever, in another closely allied animal. The whalebone whale, again, has horny "whalebone" plates in its mouth, and no teeth; but the young foetal whale, before it is born, has teeth in its jaws; they, however, are never used, and they never come to anything. But other members of the group to which the whale belongs have well-developed teeth in both jaws.

Upon any hypothesis of special creation, facts of this kind appear to me to be entirely unaccountable and inexplicable, but they cease to be so if you accept Mr. Darwin's hypothesis, and see reason for believing that the whalebone whale and the whale with teeth in its mouth both sprang from a whale that had teeth, and that the teeth of the foetal whale are merely remnants—recollections, if we may so say—of the extinct whale. So in the case of the horse and the rhinoceros: suppose that both have descended by modification from some earlier form which had the normal number of toes, and the persistence of the rudimentary bones which no longer support toes in the horse becomes comprehensible.

In the language that we speak in England, and in the language of the Greeks, there are identical verbal roots, or elements entering into the composition of words. That fact remains unintelligible so long as we

suppose English and Greek to be independently created tongues; but when it is shown that both languages are descended from one original, the Sanscrit, we give an explanation of that resemblance. In the same way the existence of identical structural roots, if I may so term them, entering into the composition of widely different animals, is striking evidence in favour of the descent of those animals from a common original.

To turn to another kind of illustration:—If you regard the whole series of stratified rocks—that enormous thickness of sixty or seventy thousand feet that I have mentioned before, constituting the only record we have of a most prodigious lapse of time, that time being, in all probability, but a fraction of that of which we have no record;—if you observe in these successive strata of rocks successive groups of animals arising and dying out, a constant succession, giving you the same kind of impression, as you travel from one group of strata to another, as you would have in travelling from one country to another;—when you find this constant succession of forms, their traces obliterated except to the man of science,—when you look at this wonderful history, and ask what it means, it is only a paltering with words if you are offered the reply,—'They were so created.'

But if, on the other hand, you look on all forms of organized beings as the results of the gradual modification of a primitive type, the facts receive a meaning, and you see that these older conditions are the necessary predecessors of the present. Viewed in this light the facts of palaeontology receive a meaning—upon any other hypothesis, I am unable to see, in the slightest degree, what knowledge or signification we are to draw out of them. Again, note as bearing upon the same point, the singular likeness which obtains between the successive Faunae and Florae, whose remains are preserved on the rocks: you never find any great and enormous difference between the immediately successive Faunae and Florae, unless you have reason to believe there has also been a great lapse of time or a great change of conditions. The animals, for instance, of the newest tertiary rocks, in any part of the world, are always, and without exception, found to be closely allied with those which now live in that part of the world. For example, in Europe, Asia, and Africa, the large mammals are at present rhinoceroses, hippopotamuses, elephants, lions, tigers, oxen, horses, etc.; and if you examine the newest tertiary deposits, which contain the animals and plants which immediately preceded those which now exist in the same country, you do not find gigantic specimens of ant-eaters and kangaroos, but you find rhinoceroses, elephants, lions, tigers, etc.,—of different species to those now living,—but still their close allies. If you turn to South America, where, at the present day, we have great sloths and armadilloes and creatures of that kind, what do you find in the newest tertiaries? You find the great sloth-like creature, the 'Megatherium', and the great armadillo, the 'Glyptodon', and so on. And if you go to Australia you find the same law holds good, namely, that that condition of organic nature which has preceded the one which now exists, presents differences perhaps of species, and of genera, but that the great types of organic structure are the same as those which now flourish.

What meaning has this fact upon any other hypothesis or supposition than one of successive modification? But if the population of the world, in any age, is the result of the gradual modification of the forms which peopled it in the preceding age,—if that has been the case, it is intelligible enough; because we may expect that the creature that results from the modification of an elephantine mammal shall be something like an elephant, and the creature which is produced by the modification of an armadillo-like mammal shall be like an armadillo. Upon that supposition, I say, the facts are intelligible; upon any other, that I am aware of, they are not.

So far, the facts of palaeontology are consistent with almost any form of the doctrine of progressive modification; they would not be absolutely inconsistent with the wild speculations of De Maillet, or with the less objectionable hypothesis of Lamarck. But Mr. Darwin's views have one peculiar merit; and that is, that they are perfectly consistent with an array of facts which are utterly inconsistent with and fatal to, any other hypothesis of progressive modification which has yet been advanced. It is one remarkable peculiarity of Mr. Darwin's hypothesis that it involves no necessary progression or incessant modification, and that it is perfectly consistent with the persistence for any length of time of a given primitive stock, contemporaneously with its modifications. To return to the case of the domestic breeds of pigeons, for example; you have the Dove-cot pigeon, which closely resembles the Rock pigeon, from which they all started, existing at the same time with the others. And if species are developed in the same way in nature, a primitive stock and its modifications may, occasionally, all find the conditions fitted for their existence; and though they come into competition, to a certain extent, with one another, the derivative species may not necessarily extirpate the primitive one, or 'vice versa'.

Now palaeontology shows us many facts which are perfectly harmonious with these observed effects of the process by which Mr. Darwin supposes species to have originated, but which appear to me to be totally inconsistent with any other hypothesis which has been proposed. There are some groups of animals and plants, in the fossil world, which have been said to belong to "persistent types," because they have persisted, with very little change indeed, through a very great range of time, while everything about them has changed largely. There are families of fishes whose type of construction has persisted all the way from the carboniferous rock right up to the cretaceous; and others which have lasted through almost the whole range of the secondary rocks, and from the lias to the older tertiaries. It is something stupendous this—to consider a genus lasting without essential modifications through all this enormous lapse of time while almost everything else was changed and modified.

Thus I have no doubt that Mr. Darwin's hypothesis will be found competent to explain the majority of the phenomena exhibited by species in nature; but in an earlier lecture I spoke cautiously with respect to its power of explaining all the physiological peculiarities of species.

There is, in fact, one set of these peculiarities which the theory of selective modification, as it stands at present, is not wholly competent to explain, and that is the group of phenomena which I mentioned to you under the name of Hybridism, and which I explained to consist in the sterility of the offspring of certain species when crossed one with another. It matters not one whit whether this sterility is universal, or whether it exists only in a single case. Every hypothesis is bound to explain, or, at any rate, not be inconsistent with, the whole of the facts which it professes to account for; and if there is a single one of these facts which can be shown to be inconsistent with (I do not merely mean inexplicable by, but contrary to) the hypothesis, the

hypothesis falls to the ground,—it is worth nothing. One fact with which it is positively inconsistent is worth as much, and as powerful in negating the hypothesis, as five hundred. If I am right in thus defining the obligations of an hypothesis, Mr. Darwin, in order to place his views beyond the reach of all possible assault, ought to be able to demonstrate the possibility of developing from a particular stock by selective breeding, two forms, which should either be unable to cross one with another, or whose cross-bred offspring should be infertile with one another.

For, you see, if you have not done that you have not strictly fulfilled all the conditions of the problem; you have not shown that you can produce, by the cause assumed, all the phenomena which you have in nature. Here are the phenomena of Hybridism staring you in the face, and you cannot say, 'I can, by selective modification, produce these same results.' Now, it is admitted on all hands that, at present, so far as experiments have gone, it has not been found possible to produce this complete physiological divergence by selective breeding. I stated this very clearly before, and I now refer to the point, because, if it could be proved, not only that this 'has' not been done, but that it 'cannot' be done; if it could be demonstrated that it is impossible to breed selectively, from any stock, a form which shall not breed with another, produced from the same stock; and if we were shown that this must be the necessary and inevitable results of all experiments, I hold that Mr. Darwin's hypothesis would be utterly shattered.

But has this been done? or what is really the state of the case? It is simply that, so far as we have gone yet with our breeding, we have not produced from a common stock two breeds which are not more or less fertile with one another.

I do not know that there is a single fact which would justify any one in saying that any degree of sterility has been observed between breeds absolutely known to have been produced by selective breeding from a common stock. On the other hand, I do not know that there is a single fact which can justify any one in asserting that such sterility cannot be produced by proper experimentation. For my own part, I see every reason to believe that it may, and will be so produced. For, as Mr. Darwin has very properly urged, when we consider the phenomena of sterility, we find they are most capricious; we do not know what it is that the sterility depends on. There are some animals which will not breed in captivity; whether it arises from the simple fact of their being shut up and deprived of their liberty, or not, we do not know, but they certainly will not breed. What an astounding thing this is, to find one of the most important of all functions annihilated by mere imprisonment!

So, again, there are cases known of animals which have been thought by naturalists to be undoubted species, which have yielded perfectly fertile hybrids; while there are other species which present what everybody believes to be varieties <sup>1</sup> which are more or less infertile with one another. There are other cases which are truly extraordinary; there is one, for example, which has been carefully examined,—of two kinds of sea-weed, of which the male element of the one, which we may call A, fertilizes the female element of the other, B; while the male element of B will not fertilize the female element of A; so that, while the former experiment seems to show us that they are 'varieties', the latter leads to the conviction that they are 'species'.

When we see how capricious and uncertain this sterility is, how unknown the conditions on which it depends, I say that we have no right to affirm that those conditions will not be better understood by and by, and we have no ground for supposing that we may not be able to experiment so as to obtain that crucial result which I mentioned just now. So that though Mr. Darwin's hypothesis does not completely extricate us from this difficulty at present, we have not the least right to say it will not do so.

There is a wide gulf between the thing you cannot explain and the thing that upsets you altogether. There is hardly any hypothesis in this world which has not some fact in connection with it which has not been explained, but that is a very different affair to a fact that entirely opposes your hypothesis; in this case all you can say is, that your hypothesis is in the same position as a good many others.

Now, as to the third test, that there are no other causes competent to explain the phenomena, I explained to you that one should be able to say of an hypothesis, that no other known causes than those supposed by it are competent to give rise to the phenomena. Here, I think, Mr. Darwin's view is pretty strong. I really believe that the alternative is either Darwinism or nothing, for I do not know of any rational conception or theory of the organic universe which has any scientific position at all beside Mr. Darwin's. I do not know of any proposition that has been put before us with the intention of explaining the phenomena of organic nature, which has in its favour a thousandth part of the evidence which may be adduced in favour of Mr. Darwin's views. Whatever may be the objections to his views, certainly all others are absolutely out of court.

Take the Lamarckian hypothesis, for example. Lamarck was a great naturalist, and to a certain extent went the right way to work; he argued from what was undoubtedly a true cause of some of the phenomena of organic nature. He said it is a matter of experience that an animal may be modified more or less in consequence of its desires and consequent actions. Thus, if a man exercise himself as a blacksmith, his arms will become strong and muscular; such organic modification is a result of this particular action and exercise. Lamarck thought that by a very simple supposition based on this truth he could explain the origin of the various animal species: he said, for example, that the short-legged birds which live on fish had been converted into the long-legged waders by desiring to get the fish without wetting their bodies, and so stretching their legs more and more through successive generations. If Lamarck could have shown experimentally, that even races of animals could be produced in this way, there might have been some ground for his speculations. But he could show nothing of the kind, and his hypothesis has pretty well dropped into oblivion, as it deserved to do. I said in an earlier lecture that there are hypotheses and hypotheses, and when people tell you that Mr. Darwin's strongly-based hypothesis is nothing but a mere modification of Lamarck's, you will know what to think of their capacity for forming a judgment on this subject.

But you must recollect that when I say I think it is either Mr. Darwin's hypothesis or nothing; that either we must take his view, or look upon the whole of organic nature as an enigma, the meaning of which is wholly hidden from us; you must understand that I mean that I accept it provisionally, in exactly the same way as I accept any other hypothesis. Men of science do not pledge themselves to creeds; they are bound by articles of no sort; there is not a single belief that it is not a bounden duty with them to hold with a light hand and to

part with it cheerfully, the moment it is really proved to be contrary to any fact, great or small. And if, in course of time I see good reasons for such a proceeding, I shall have no hesitation in coming before you, and pointing out any change in my opinion without finding the slightest occasion to blush for so doing. So I say that we accept this view as we accept any other, so long as it will help us, and we feel bound to retain it only so long as it will serve our great purpose—the improvement of Man's estate and the widening of his knowledge. The moment this, or any other conception, ceases to be useful for these purposes, away with it to the four winds; we care not what becomes of it!

But to say truth, although it has been my business to attend closely to the controversies roused by the publication of Mr. Darwin's book, I think that not one of the enormous mass of objections and obstacles which have been raised is of any very great value, except that sterility case which I brought before you just now. All the rest are misunderstandings of some sort, arising either from prejudice, or want of knowledge, or still more from want of patience and care in reading the work.

For you must recollect that it is not a book to be read with as much ease as its pleasant style may lead you to imagine. You spin through it as if it were a novel the first time you read it, and think you know all about it; the second time you read it you think you know rather less about it; and the third time, you are amazed to find how little you have really apprehended its vast scope and objects. I can positively say that I never take it up without finding in it some new view, or light, or suggestion that I have not noticed before. That is the best characteristic of a thorough and profound book; and I believe this feature of the 'Origin of Species' explains why so many persons have ventured to pass judgment and criticisms upon it which are by no means worth the paper they are written on.

Before concluding these lectures there is one point to which I must advert,—though, as Mr. Darwin has said nothing about man in his book, it concerns myself rather than him;—for I have strongly maintained on sundry occasions that if Mr. Darwin's views are sound, they apply as much to man as to the lower mammals, seeing that it is perfectly demonstrable that the structural differences which separate man from the apes are not greater than those which separate some apes from others. There cannot be the slightest doubt in the world that the argument which applies to the improvement of the horse from an earlier stock, or of ape from ape, applies to the improvement of man from some simpler and lower stock than man. There is not a single faculty—functional or structural, moral, intellectual, or instinctive,—there is no faculty whatever that is not capable of improvement; there is no faculty whatsoever which does not depend upon structure, and as structure tends to vary, it is capable of being improved.

Well, I have taken a good deal of pains at various times to prove this, and I have endeavoured to meet the objections of those who maintain, that the structural differences between man and the lower animals are of so vast a character and enormous extent, that even if Mr. Darwin's views are correct, you cannot imagine this particular modification to take place. It is, in fact, easy matter to prove that, so far as structure is concerned, man differs to no greater extent from the animals which are immediately below him than these do from other members of the same order. Upon the other hand, there is no one who estimates more highly than I do the dignity of human nature, and the width of the gulf in intellectual and moral matters, which lies between man and the whole of the lower creation.

But I find this very argument brought forward vehemently by some. "You say that man has proceeded from a modification of some lower animal, and you take pains to prove that the structural differences which are said to exist in his brain do not exist at all, and you teach that all functions, intellectual, moral, and others, are the expression or the result, in the long run, of structures, and of the molecular forces which they exert." It is quite true that I do so.

"Well, but," I am told at once, somewhat triumphantly, "you say in the same breath that there is a great moral and intellectual chasm between man and the lower animals. How is this possible when you declare that moral and intellectual characteristics depend on structure, and yet tell us that there is no such gulf between the structure of man and that of the lower animals?"

I think that objection is based upon a misconception of the real relations which exist between structure and function, between mechanism and work. Function is the expression of molecular forces and arrangements no doubt; but, does it follow from this, that variation in function so depends upon variation in structure that the former is always exactly proportioned to the latter? If there is no such relation, if the variation in function which follows on a variation in structure, may be enormously greater than the variation of the structure, then, you see, the objection falls to the ground.

Take a couple of watches—made by the same maker, and as completely alike as possible; set them upon the table, and the function of each—which is its rate of going—will be performed in the same manner, and you shall be able to distinguish no difference between them; but let me take a pair of pincers, and if my hand is steady enough to do it, let me just lightly crush together the bearings of the balance-wheel, or force to a slightly different angle the teeth of the escapement of one of them, and of course you know the immediate result will be that the watch, so treated, from that moment will cease to go. But what proportion is there between the structural alteration and the functional result? Is it not perfectly obvious that the alteration is of the minutest kind, yet that slight as it is, it has produced an infinite difference in the performance of the functions of these two instruments?

Well, now, apply that to the present question. What is it that constitutes and makes man what he is? What is it but his power of language—that language giving him the means of recording his experience—making every generation somewhat wiser than its predecessor,—more in accordance with the established order of the universe?

What is it but this power of speech, of recording experience, which enables men to be men—looking before and after and, in some dim sense, understanding the working of this wondrous universe—and which distinguishes man from the whole of the brute world? I say that this functional difference is vast, unfathomable, and truly infinite in its consequences; and I say at the same time, that it may depend upon structural differences which shall be absolutely inappreciable to us with our present means of investigation. What is this very speech that we are talking about? I am speaking to you at this moment, but if you were to

alter, in the minutest degree, the proportion of the nervous forces now active in the two nerves which supply the muscles of my glottis, I should become suddenly dumb. The voice is produced only so long as the vocal chords are parallel; and these are parallel only so long as certain muscles contract with exact equality; and that again depends on the equality of action of those two nerves I spoke of. So that a change of the minutest kind in the structure of one of these nerves, or in the structure of the part in which it originates, or of the supply of blood to that part, or of one of the muscles to which it is distributed, might render all of us dumb. But a race of dumb men, deprived of all communication with those who could speak, would be little indeed removed from the brutes. And the moral and intellectual difference between them and ourselves would be practically infinite, though the naturalist should not be able to find a single shadow of even specific structural difference.

But let me dismiss this question now, and, in conclusion, let me say that you may go away with it as my mature conviction, that Mr. Darwin's work is the greatest contribution which has been made to biological science since the publication of the 'Regne Animal' of Cuvier, and since that of the 'History of Development' of Von Baer. I believe that if you strip it of its theoretical part it still remains one of the greatest encyclopaedias of biological doctrine that any one man ever brought forth; and I believe that, if you take it as the embodiment of an hypothesis, it is destined to be the guide of biological and psychological speculation for the next three or four generations.

1 ([return](#))

[ And as I conceive with very good reason; but if any objector urges that we cannot prove that they have been produced by artificial or natural selection, the objection must be admitted— ultrasceptical as it is. But in science, scepticism is a duty.]

\*\*\* END OF THE PROJECT GUTENBERG EBOOK A CRITICAL EXAMINATION OF THE POSITION OF MR. DARWIN'S WORK, "ON THE ORIGIN OF SPECIES," IN RELATION TO THE COMPLETE THEORY OF THE CAUSES OF THE PHENOMENA OF ORGANIC NATURE \*\*\*

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