

The Project Gutenberg EBook of The Method By Which The Causes Of The Present And Past Conditions Of Organic Nature Are To Be Discovered.--The Origination Of Living Beings, by Thomas H. Huxley

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THE METHOD BY WHICH THE CAUSES OF THE PRESENT AND PAST CONDITIONS OF ORGANIC NATURE ARE TO BE DISCOVERED.

THE ORIGINATION OF LIVING BEINGS

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

By Thomas H. Huxley

In the two preceding lectures I have endeavoured to indicate to you the extent of the subject-matter of the inquiry upon which we are engaged; and now, having thus acquired some conception of the Past and Present phenomena of Organic Nature, I must now turn to that which constitutes the great problem which we have set before ourselves;—I mean, the question of what knowledge we have of the causes of these phenomena of organic nature, and how such knowledge is obtainable.

Here, on the threshold of the inquiry, an objection meets us. There are in the world a number of extremely worthy, well-meaning persons, whose judgments and opinions are entitled to the utmost respect on account of their sincerity, who are of opinion that Vital Phenomena, and especially all questions relating to the origin of vital phenomena, are questions quite apart from the ordinary run of inquiry, and are, by their very nature, placed out of our reach. They say that all these phenomena originated miraculously, or in some way totally different from the ordinary course of nature, and that therefore they conceive it to be futile, not to say presumptuous, to attempt to inquire into them.

To such sincere and earnest persons, I would only say, that a question of this kind is not to be shelved upon theoretical or speculative grounds. You may remember the story of the Sophist who demonstrated to Diogenes in the most complete and satisfactory manner that he could not walk;

that, in fact, all motion was an impossibility; and that Diogenes refuted him by simply getting up and walking round his tub. So, in the same way, the man of science replies to objections of this kind, by simply getting up and walking onward, and showing what science has done and is doing—by pointing to that immense mass of facts which have been ascertained and systematized under the forms of the great doctrines of Morphology, of Development, of Distribution, and the like. He sees an enormous mass of facts and laws relating to organic beings, which stand on the same good sound foundation as every other natural law; and therefore, with this mass of facts and laws before us, therefore, seeing that, as far as organic matters have hitherto been accessible and studied, they have shown themselves capable of yielding to scientific investigation, we may accept this as proof that order and law reign there as well as in the rest of nature; and the man of science says nothing to objectors of this sort, but supposes that we can and shall walk to a knowledge of the origin of organic nature, in the same way that we have walked to a knowledge of the laws and principles of the inorganic world.

But there are objectors who say the same from ignorance and ill-will. To such I would reply that the objection comes ill from them, and that the real presumption, I may almost say the real blasphemy, in this matter, is in the attempt to limit that inquiry into the causes of phenomena which is the source of all human blessings, and from which has sprung all human prosperity and progress; for, after all, we can accomplish comparatively little; the limited range of our own faculties bounds us on every side,—the field of our powers of observation is small enough, and he who endeavours to narrow the sphere of our inquiries is only pursuing a course that is likely to produce the greatest harm to his fellow-men.

But now, assuming, as we all do, I hope, that these phenomena are properly accessible to inquiry, and setting out upon our search into the causes of the phenomena of organic nature, or, at any rate, setting out to discover how much we at present know upon these abstruse matters, the question arises as to what is to be our course of proceeding, and what method we must lay down for our guidance. I reply to that question, that our method must be exactly the same as that which is pursued in any other scientific inquiry, the method of scientific investigation being the same for all orders of facts and phenomena whatsoever.

I must dwell a little on this point, for I wish you to leave this room with a very clear conviction that scientific investigation is not, as many people seem to suppose, some kind of modern black art. I say that you might easily gather this impression from the manner in which many persons speak of scientific inquiry, or talk about inductive and deductive philosophy, or the principles of the "Baconian philosophy." I do protest that, of the vast number of cant in this world, there are none, to my mind, so contemptible as the pseudoscientific cant which is talked about the "Baconian philosophy."

To hear people talk about the great Chancellor—and a very great man he certainly was,—you would think that it was he who had invented science, and that there was no such thing as sound reasoning before the time of Queen Elizabeth. Of course you say, that cannot possibly be true; you perceive, on a moment's reflection, that such an idea is absurdly wrong, and yet, so firmly rooted is this sort of impression,—I cannot call it an idea, or conception,—the thing is too absurd to be entertained,—but so completely does it exist at the bottom of most men's minds, that this has been a matter of observation with me for many years past. There are many men who, though knowing absolutely nothing of the subject with which they may be dealing, wish, nevertheless, to damage the author of some view with which they think fit to disagree. What they do, then, is not to go and learn something about the subject, which one would naturally think the best way of fairly dealing with it; but they abuse the originator of the view they question, in a general manner, and wind up by saying that, "After all, you know, the principles and method of this author are totally opposed to the canons of the Baconian philosophy." Then everybody applauds, as a matter of course, and agrees that it must be so. But if you were to stop them all in the middle of their applause, you would probably find that neither the speaker nor his applauders could tell you how or in what way it was so; neither the one nor the other having the slightest idea of what they mean when they speak of the "Baconian philosophy."

You will understand, I hope, that I have not the slightest desire to join in the outcry against either the morals, the intellect, or the great genius of Lord Chancellor Bacon. He was undoubtedly a very great man, let people say what they will of him; but notwithstanding all that he did for philosophy, it would be entirely wrong to suppose that the methods of modern scientific inquiry originated with him, or with his age; they originated with the first man, whoever he was; and indeed existed long before him, for many of the essential processes of reasoning are exerted by the higher order of brutes as completely and effectively as by ourselves. We see in many of the brute creation the exercise of one, at least, of the same powers of reasoning as that which we ourselves employ.

The method of scientific investigation is nothing but the expression of the necessary mode of working of the human mind. It is simply the mode at which all phenomena are reasoned about, rendered precise and exact. There is no more difference, but there is just the same kind of difference, between the mental operations of a man of science and those of an ordinary person, as there is between the operations and methods of a baker or of a butcher weighing out his goods in common scales, and the operations of a chemist in performing a difficult and complex analysis by means of his balance and finely-graduated weights. It is not that the action of the scales in the one case, and the balance in the other, differ in the principles of their construction or manner of working; but the beam of one is set on an infinitely finer axis than the other, and of course turns by the addition of a much smaller weight.

You will understand this better, perhaps, if I give you some familiar example. You have all heard it repeated, I dare say, that men of science work by means of Induction and Deduction, and that by the help of these operations, they, in a sort of sense, wring from Nature certain other things, which are called Natural Laws, and Causes, and that out of these, by some cunning skill of their own, they build up Hypotheses and Theories. And it is imagined by many, that the operations of the common mind can be by no means compared with these processes, and that they have to be acquired by a sort of special apprenticeship to the craft. To hear all these large words, you would think that the mind of a man of science must be constituted differently from that of his fellow men; but if you will not be frightened by terms, you will discover that you are quite wrong, and that all these terrible apparatus are being used by yourselves every day and every hour of your lives.

There is a well-known incident in one of Moliere's plays, where the author makes the hero express unbounded delight on being told that he had been talking prose during the whole of his life. In the same way, I trust, that you will take comfort, and be delighted with yourselves, on the discovery that you have been acting on the principles of inductive and deductive philosophy during the same period. Probably there is not one here who has not in the course of the day had occasion to set in motion a complex train of reasoning, of the very same kind, though differing of course in degree, as that which a scientific man goes through in tracing the causes of natural phenomena.

A very trivial circumstance will serve to exemplify this. Suppose you go into a fruiterer's shop, wanting an apple,—you take up one, and, on biting it, you find it is sour; you look at it, and see that it is hard and green. You take up another one, and that too is hard, green, and sour. The shopman offers you a third; but, before biting it, you examine it, and find that it is hard and green, and you immediately say that you will not have it, as it must be sour, like those that you have already tried.

Nothing can be more simple than that, you think; but if you will take the trouble to analyze and trace out into its logical elements what has been done by the mind, you will be greatly surprised. In the first place, you have performed the operation of Induction. You found that, in two experiences, hardness and greenness in apples go together with sourness. It was so in the first case, and it was confirmed by the second. True, it is a very small basis, but still it is enough to make an induction from; you generalize the facts, and you expect to find sourness in apples where you get hardness and greenness. You found upon that a general law, that all hard and green apples are sour; and that, so far as it goes, is a perfect induction. Well, having got your natural law in this way, when you are offered another apple which you find is hard and green, you say, "All hard and green apples are sour; this apple is hard and green, therefore this apple is sour." That train of reasoning is what logicians call a syllogism, and has all its various parts and terms,—its major premiss, its minor premiss, and its conclusion. And, by the help of further reasoning, which, if drawn out, would have to be exhibited in two or three other syllogisms, you arrive at your final determination, "I will not have that apple." So that,

you see, you have, in the first place, established a law by Induction, and upon that you have founded a Deduction, and reasoned out the special conclusion of the particular case. Well now, suppose, having got your law, that at some time afterwards, you are discussing the qualities of apples with a friend: you will say to him, "It is a very curious thing,—but I find that all hard and green apples are sour!" Your friend says to you, "But how do you know that?" You at once reply, "Oh, because I have tried it over and over again, and have always found them to be so." Well, if we were talking science instead of common sense, we should call that an Experimental Verification. And, if still opposed, you go further, and say, "I have heard from the people in Somersetshire and Devonshire, where a large number of apples are grown, that they have observed the same thing. It is also found to be the case in Normandy, and in North America. In short, I find it to be the universal experience of mankind wherever attention has been directed to the subject." Whereupon, your friend, unless he is a very unreasonable man, agrees with you, and is convinced that you are quite right in the conclusion you have drawn. He believes, although perhaps he does not know he believes it, that the more extensive Verifications are,—that the more frequently experiments have been made, and results of the same kind arrived at,—that the more varied the conditions under which the same results have been attained, the more certain is the ultimate conclusion, and he disputes the question no further. He sees that the experiment has been tried under all sorts of conditions, as to time, place, and people, with the same result; and he says with you, therefore, that the law you have laid down must be a good one, and he must believe it.

In science we do the same thing;—the philosopher exercises precisely the same faculties, though in a much more delicate manner. In scientific inquiry it becomes a matter of duty to expose a supposed law to every possible kind of verification, and to take care, moreover, that this is done intentionally, and not left to a mere accident, as in the case of the apples. And in science, as in common life, our confidence in a law is in exact proportion to the absence of variation in the result of our experimental verifications. For instance, if you let go your grasp of an article you may have in your hand, it will immediately fall to the ground. That is a very common verification of one of the best established laws of nature—that of gravitation. The method by which men of science establish the existence of that law is exactly the same as that by which we have established the trivial proposition about the sourness of hard and green apples. But we believe it in such an extensive, thorough, and unhesitating manner because the universal experience of mankind verifies it, and we can verify it ourselves at any time; and that is the strongest possible foundation on which any natural law can rest.

So much by way of proof that the method of establishing laws in science is exactly the same as that pursued in common life. Let us now turn to another matter (though really it is but another phase of the same question), and that is, the method by which, from the relations of certain phenomena, we prove that some stand in the position of causes towards the others.

I want to put the case clearly before you, and I will therefore show you what I mean by another familiar example. I will suppose that one of you, on coming down in the morning to the parlour of your house, finds that a tea-pot and some spoons which had been left in the room on the previous evening are gone,—the window is open, and you observe the mark of a dirty hand on the window-frame, and perhaps, in addition to that, you notice the impress of a hob-nailed shoe on the gravel outside. All these phenomena have struck your attention instantly, and before two minutes have passed you say, "Oh, somebody has broken open the window, entered the room, and run off with the spoons and the tea-pot!" That speech is out of your mouth in a moment. And you will probably add, "I know there has; I am quite sure of it!" You mean to say exactly what you know; but in reality what you have said has been the expression of what is, in all essential particulars, an Hypothesis. You do not 'know' it at all; it is nothing but an hypothesis rapidly framed in your own mind! And it is an hypothesis founded on a long train of inductions and deductions.

What are those inductions and deductions, and how have you got at this hypothesis? You have observed, in the first place, that the window is open; but by a train of reasoning involving many Inductions and Deductions, you have probably arrived long before at the General Law—and a very good one it is—that windows do not open of themselves; and you therefore conclude that something has opened the window. A second general law that you have arrived at in the same way is, that tea-pots and spoons do not go out of a window spontaneously, and you are satisfied that, as they are not now where you left them, they have been removed. In the third place, you look at the marks on the window-sill, and the shoemarks outside, and you say that in all previous experience the former kind of mark has never been produced by anything else but the hand of a human being; and the same experience shows that no other animal but man at present wears shoes with hob-nails on them such as would produce the marks in the gravel. I do not know, even if we could discover any of those "missing links" that are talked about, that they would help us to any other conclusion! At any rate the law which states our present experience is strong enough for my present purpose.—You next reach the conclusion, that as these kinds of marks have not been left by any other animals than men, or are liable to be formed in any other way than by a man's hand and shoe, the marks in question have been formed by a man in that way. You have, further, a general law, founded on observation and experience, and that, too, is, I am sorry to say, a very universal and unimpeachable one,—that some men are thieves; and you assume at once from all these premisses—and that is what constitutes your hypothesis—that the man who made the marks outside and on the window-sill, opened the window, got into the room, and stole your tea-pot and spoons. You have now arrived at a 'Vera Causa';—you have assumed a Cause which it is plain is competent to produce all the phenomena you have observed. You can explain all these phenomena only by the hypothesis of a thief. But that is a hypothetical conclusion, of the justice of which you have no absolute proof at all; it is only rendered highly probable by a series of inductive and deductive reasonings.

I suppose your first action, assuming that you are a man of ordinary common sense, and that you have established this hypothesis to your own satisfaction, will very likely be to go off for the police, and set them on the track of the burglar, with the view to the recovery of your property. But just as you are starting with this object, some person comes in, and on learning what you are about, says, "My good friend, you are going on a great deal too fast. How do you know that the man who really made the marks took the spoons? It might have been a monkey that took them, and the man may have merely looked in afterwards." You would probably reply, "Well, that is all very well, but you see it is contrary to all experience of the way tea-pots and spoons are abstracted; so that, at any rate, your hypothesis is less probable than mine." While you are talking the thing over in this way, another friend arrives, one of that good kind of people that I was talking of a little while ago. And he might say, "Oh, my dear sir, you are certainly going on a great deal too fast. You are most presumptuous. You admit that all these occurrences took place when you were fast asleep, at a time when you could not possibly have known anything about what was taking place. How do you know that the laws of Nature are not suspended during the night? It may be that there has been some kind of supernatural interference in this case." In point of fact, he declares that your hypothesis is one of which you cannot at all demonstrate the truth, and that you are by no means sure that the laws of Nature are the same when you are asleep as when you are awake.

Well, now, you cannot at the moment answer that kind of reasoning. You feel that your worthy friend has you somewhat at a disadvantage. You will feel perfectly convinced in your own mind, however, that you are quite right, and you say to him, "My good friend, I can only be guided by the natural probabilities of the case, and if you will be kind enough to stand aside and permit me to pass, I will go and fetch the police." Well, we will suppose that your journey is successful, and that by good luck you meet with a policeman; that eventually the burglar is found with your property on his person, and the marks correspond to his hand and to his boots. Probably any jury would consider those facts a very good experimental verification of your hypothesis, touching the cause of the abnormal phenomena observed in your parlour, and would act accordingly.

Now, in this suppositious case, I have taken phenomena of a very common kind, in order that you might see what are the different steps in an ordinary process of reasoning, if you will only take the trouble to analyse it carefully. All the operations I have described, you will see, are involved in the mind of any man of sense in leading him to a conclusion as to the course he should take in order to make good a robbery and punish the offender. I say that you are led, in that case, to your conclusion by exactly the same train of reasoning as that which a man of science pursues when he is endeavouring to discover the origin and laws of the most occult phenomena. The process is, and always must be, the same; and precisely the same mode of reasoning was employed by Newton and Laplace in their endeavours to discover and define the causes of the movements of the heavenly bodies, as you, with your own common sense, would employ to detect a burglar. The only difference is, that the nature

of the inquiry being more abstruse, every step has to be most carefully watched, so that there may not be a single crack or flaw in your hypothesis. A flaw or crack in many of the hypotheses of daily life may be of little or no moment as affecting the general correctness of the conclusions at which we may arrive; but, in a scientific inquiry, a fallacy, great or small, is always of importance, and is sure to be constantly productive of mischievous, if not fatal results.

Do not allow yourselves to be misled by the common notion that an hypothesis is untrustworthy simply because it is an hypothesis. It is often urged, in respect to some scientific conclusion, that, after all, it is only an hypothesis. But what more have we to guide us in nine-tenths of the most important affairs of daily life than hypotheses, and often very ill-based ones? So that in science, where the evidence of an hypothesis is subjected to the most rigid examination, we may rightly pursue the same course. You may have hypotheses and hypotheses. A man may say, if he likes, that the moon is made of green cheese: that is an hypothesis. But another man, who has devoted a great deal of time and attention to the subject, and availed himself of the most powerful telescopes and the results of the observations of others, declares that in his opinion it is probably composed of materials very similar to those of which our own earth is made up: and that is also only an hypothesis. But I need not tell you that there is an enormous difference in the value of the two hypotheses. That one which is based on sound scientific knowledge is sure to have a corresponding value; and that which is a mere hasty random guess is likely to have but little value. Every great step in our progress in discovering causes has been made in exactly the same way as that which I have detailed to you. A person observing the occurrence of certain facts and phenomena asks, naturally enough, what process, what kind of operation known to occur in nature applied to the particular case, will unravel and explain the mystery? Hence you have the scientific hypothesis; and its value will be proportionate to the care and completeness with which its basis had been tested and verified. It is in these matters as in the commonest affairs of practical life: the guess of the fool will be folly, while the guess of the wise man will contain wisdom. In all cases, you see that the value of the result depends on the patience and faithfulness with which the investigator applies to his hypothesis every possible kind of verification.

I dare say I may have to return to this point by-and-by; but having dealt thus far with our logical methods, I must now turn to something which, perhaps, you may consider more interesting, or, at any rate, more tangible. But in reality there are but few things that can be more important for you to understand than the mental processes and the means by which we obtain scientific conclusions and theories. Having granted that the inquiry is a proper one, and having determined on the nature of the methods we are to pursue and which only can lead to success, I must now turn to the consideration of our knowledge of the nature of the processes which have resulted in the present condition of organic nature.

Here, let me say at once, lest some of you misunderstand me, that I have extremely little to report. The question of how the present condition of organic nature came about, resolves itself into two questions. The first is: How has organic or living matter commenced its existence? And the second is: How has it been perpetuated? On the second question I shall have more to say hereafter. But on the first one, what I now have to say will be for the most part a negative character.

If you consider what kind of evidence we can have upon this matter, it will resolve itself into two kinds. We may have historical evidence and we may have experimental evidence. It is, for example, conceivable, that inasmuch as the hardened mud which forms a considerable portion of the thickness of the earth's crust contains faithful records of the past forms of life, and inasmuch as these differ more and more as we go further down,—it is possible and conceivable that we might come to some particular bed or stratum which should contain the remains of those creatures with which organic life began upon the earth. And if we did so, and if such forms of organic life were preservable, we should have what I would call historical evidence of the mode in which organic life began upon this planet. Many persons will tell you, and indeed you will find it stated in many works on geology, that this has been done, and that we really possess such a record; there are some who imagine that the earliest forms of life of which we have as yet discovered any record, are in truth the forms in which animal life began upon the globe. The grounds on which they base that supposition are these:—That if you go through the enormous thickness of the earth's crust and get down to the older rocks, the higher vertebrate animals—the quadrupeds, birds, and fishes—cease to be found; beneath them you find only the invertebrate animals; and in the deepest and lowest rocks those remains become scantier and scantier, not in any very gradual progression, however, until, at length, in what are supposed to be the oldest rocks, the animal remains which are found are almost always confined to four forms—'Oldhamia', whose precise nature is not known, whether plant or animal; 'Lingula', a kind of mollusc; 'Trilobites', a crustacean animal, having the same essential plan of construction, though differing in many details from a lobster or crab; and Hymenocaris, which is also a crustacean. So that you have all the 'Fauna' reduced, at this period, to four forms: one a kind of animal or plant that we know nothing about, and three undoubted animals—two crustaceans and one mollusc.

I think, considering the organization of these mollusca and crustacea, and looking at their very complex nature, that it does indeed require a very strong imagination to conceive that these were the first created of all living things. And you must take into consideration the fact that we have not the slightest proof that these which we call the oldest beds are really so: I repeat, we have not the slightest proof of it. When you find in some places that in an enormous thickness of rocks there are but very scanty traces of life, or absolutely none at all; and that in other parts of the world rocks of the very same formation are crowded with the records of living forms, I think it is impossible to place any reliance on the supposition, or to feel oneself justified in supposing that these are the forms in which life first commenced. I have not time here to enter upon the technical grounds upon which I am led to this conclusion,—that could hardly be done properly in half a dozen lectures on that part alone;—I must content myself with saying that I do not at all believe that these are the oldest forms of life.

I turn to the experimental side to see what evidence we have there. To enable us to say that we know anything about the experimental origination of organization and life, the investigator ought to be able to take inorganic matters, such as carbonic acid, ammonia, water, and salines, in any sort of inorganic combination, and be able to build them up into Protein matter, and that that Protein matter ought to begin to live in an organic form. That, nobody has done as yet, and I suspect it will be a long while before anybody does do it. But the thing is by no means so impossible as it looks; for the researches of modern chemistry have shown us—I won't say the road towards it, but, if I may so say, they have shown the finger-post pointing to the road that may lead to it.

It is not many years ago—and you must recollect that Organic Chemistry is a young science, not above a couple of generations old,—you must not expect too much of it; it is not many years ago since it was said to be perfectly impossible to fabricate any organic compound; that is to say, any non-mineral compound which is to be found in an organized being. It remained so for a very long period; but it is now a considerable number of years since a distinguished foreign chemist contrived to fabricate Urea, a substance of a very complex character, which forms one of the waste products of animal structures. And of late years a number of other compounds, such as Butyric Acid, and others, have been added to the list. I need not tell you that chemistry is an enormous distance from the goal I indicate; all I wish to point out to you is, that it is by no means safe to say that that goal may not be reached one day. It may be that it is impossible for us to produce the conditions requisite to the origination of life; but we must speak modestly about the matter, and recollect that Science has put her foot upon the bottom round of the ladder. Truly he would be a bold man who would venture to predict where she will be fifty years hence.

There is another inquiry which bears indirectly upon this question, and upon which I must say a few words. You are all of you aware of the phenomena of what is called spontaneous generation. Our forefathers, down to the seventeenth century, or thereabouts, all imagined, in perfectly good faith, that certain vegetable and animal forms gave birth, in the process of their decomposition, to insect life. Thus, if you put a piece of meat in the sun, and allowed it to putrefy, they conceived that the grubs which soon began to appear were the result of the action of a power of spontaneous generation which the meat contained. And they could give you receipts for making various animal and vegetable preparations which would produce particular kinds of animals. A very distinguished Italian naturalist, named Redi, took up the question, at a time when everybody believed in it; among others our own great Harvey, the discoverer of the circulation of the blood. You will constantly find his name quoted,

however, as an opponent of the doctrine of spontaneous generation; but the fact is, and you will see it if you will take the trouble to look into his works, Harvey believed it as profoundly as any man of his time; but he happened to enunciate a very curious proposition—that every living thing came from an 'egg'; he did not mean to use the word in the sense in which we now employ it, he only meant to say that every living thing originated in a little rounded particle of organized substance; and it is from this circumstance, probably, that the notion of Harvey having opposed the doctrine originated. Then came Redi, and he proceeded to upset the doctrine in a very simple manner. He merely covered the piece of meat with some very fine gauze, and then he exposed it to the same conditions. The result of this was that no grubs or insects were produced; he proved that the grubs originated from the insects who came and deposited their eggs in the meat, and that they were hatched by the heat of the sun. By this kind of inquiry he thoroughly upset the doctrine of spontaneous generation, for his time at least.

Then came the discovery and application of the microscope to scientific inquiries, which showed to naturalists that besides the organisms which they already knew as living beings and plants, there were an immense number of minute things which could be obtained apparently almost at will from decaying vegetable and animal forms. Thus, if you took some ordinary black pepper or some hay, and steeped it in water, you would find in the course of a few days that the water had become impregnated with an immense number of animalcules swimming about in all directions. From facts of this kind naturalists were led to revive the theory of spontaneous generation. They were headed here by an English naturalist,—Needham,—and afterwards in France by the learned Buffon. They said that these things were absolutely begotten in the water of the decaying substances out of which the infusion was made. It did not matter whether you took animal or vegetable matter, you had only to steep it in water and expose it, and you would soon have plenty of animalcules. They made an hypothesis about this which was a very fair one. They said, this matter of the animal world, or of the higher plants, appears to be dead, but in reality it has a sort of dim life about it, which, if it is placed under fair conditions, will cause it to break up into the forms of these little animalcules, and they will go through their lives in the same way as the animal or plant of which they once formed a part.

The question now became very hotly debated. Spallanzani, an Italian naturalist, took up opposite views to those of Needham and Buffon, and by means of certain experiments he showed that it was quite possible to stop the process by boiling the water, and closing the vessel in which it was contained. "Oh!" said his opponents; "but what do you know you may be doing when you heat the air over the water in this way? You may be destroying some property of the air requisite for the spontaneous generation of the animalcules."

However, Spallanzani's views were supposed to be upon the right side, and those of the others fell into discredit; although the fact was that Spallanzani had not made good his views. Well, then, the subject continued to be revived from time to time, and experiments were made by several persons; but these experiments were not altogether satisfactory. It was found that if you put an infusion in which animalcules would appear if it were exposed to the air into a vessel and boiled it, and then sealed up the mouth of the vessel, so that no air, save such as had been heated to 212 degrees, could reach its contents, that then no animalcules would be found; but if you took the same vessel and exposed the infusion to the air, then you would get animalcules. Furthermore, it was found that if you connected the mouth of the vessel with a red-hot tube in such a way that the air would have to pass through the tube before reaching the infusion, that then you would get no animalcules. Yet another thing was noticed: if you took two flasks containing the same kind of infusion, and left one entirely exposed to the air, and in the mouth of the other placed a ball of cotton wool, so that the air would have to filter itself through it before reaching the infusion, that then, although you might have plenty of animalcules in the first flask, you would certainly obtain none from the second.

These experiments, you see, all tended towards one conclusion—that the infusoria were developed from little minute spores or eggs which were constantly floating in the atmosphere, which lose their power of germination if subjected to heat. But one observer now made another experiment which seemed to go entirely the other way, and puzzled him altogether. He took some of this boiled infusion that I have been speaking of, and by the use of a mercurial bath—a kind of trough used in laboratories—he deftly inverted a vessel containing the infusion into the mercury, so that the latter reached a little beyond the level of the mouth of the 'inverted' vessel. You see that he thus had a quantity of the infusion shut off from any possible communication with the outer air by being inverted upon a bed of mercury.

He then prepared some pure oxygen and nitrogen gases, and passed them by means of a tube going from the outside of the vessel, up through the mercury into the infusion; so that he thus had it exposed to a perfectly pure atmosphere of the same constituents as the external air. Of course, he expected he would get no infusorial animalcules at all in that infusion; but, to his great dismay and discomfiture, he found he almost always did get them.

Furthermore, it has been found that experiments made in the manner described above answer well with most infusions; but that if you fill the vessel with boiled milk, and then stop the neck with cotton-wool, you 'will' have infusoria. So that you see there were two experiments that brought you to one kind of conclusion, and three to another; which was a most unsatisfactory state of things to arrive at in a scientific inquiry.

Some few years after this, the question began to be very hotly discussed in France. There was M. Pouchet, a professor at Rouen, a very learned man, but certainly not a very rigid experimentalist. He published a number of experiments of his own, some of which were very ingenious, to show that if you went to work in a proper way, there was a truth in the doctrine of spontaneous generation. Well, it was one of the most fortunate things in the world that M. Pouchet took up this question, because it induced a distinguished French chemist, M. Pasteur, to take up the question on the other side; and he has certainly worked it out in the most perfect manner. I am glad to say, too, that he has published his researches in time to enable me to give you an account of them. He verified all the experiments which I have just mentioned to you—and then finding those extraordinary anomalies, as in the case of the mercury bath and the milk, he set himself to work to discover their nature. In the case of milk he found it to be a question of temperature. Milk in a fresh state is slightly alkaline; and it is a very curious circumstance, but this very slight degree of alkalinity seems to have the effect of preserving the organisms which fall into it from the air from being destroyed at a temperature of 212 degrees, which is the boiling point. But if you raise the temperature 10 degrees when you boil it, the milk behaves like everything else; and if the air with which it comes in contact, after being boiled at this temperature, is passed through a red-hot tube, you will not get a trace of organisms.

He then turned his attention to the mercury bath, and found on examination that the surface of the mercury was almost always covered with a very fine dust. He found that even the mercury itself was positively full of organic matters; that from being constantly exposed to the air, it had collected an immense number of these infusorial organisms from the air. Well, under these circumstances he felt that the case was quite clear, and that the mercury was not what it had appeared to M. Schwann to be,—a bar to the admission of these organisms; but that, in reality, it acted as a reservoir from which the infusion was immediately supplied with the large quantity that had so puzzled him.

But not content with explaining the experiments of others, M. Pasteur went to work to satisfy himself completely. He said to himself: "If my view is right, and if, in point of fact, all these appearances of spontaneous generation are altogether due to the falling of minute germs suspended in the atmosphere,—why, I ought not only to be able to show the germs, but I ought to be able to catch and sow them, and produce the resulting organisms." He, accordingly, constructed a very ingenious apparatus to enable him to accomplish this trapping of this "germ dust" in the air. He fixed in the window of his room a glass tube, in the centre of which he had placed a ball of gun-cotton, which, as you all know, is ordinary cotton-wool, which, from having been steeped in strong acid, is converted into a substance of great explosive power. It is also soluble in alcohol and ether. One end of the glass tube was, of course, open to the external air; and at the other end of it he placed an aspirator, a contrivance for causing a current of the external air to pass through the tube. He kept this apparatus going for four-and-twenty hours, and then removed the 'dusted' gun-cotton, and dissolved it in alcohol and ether. He then allowed this to stand for a few hours, and the result was, that a very fine dust was gradually deposited at the bottom of it. That dust, on being transferred to the stage of a microscope, was found to contain an enormous number of starch grains. You know that the materials of our food and the greater portion of plants are composed of starch, and we are constantly making use of it in a variety of ways, so that there is always a quantity of it suspended in the air. It is these starch grains which form many of those bright specks that

we see dancing in a ray of light sometimes. But besides these, M. Pasteur found also an immense number of other organic substances such as spores of fungi, which had been floating about in the air and had got caged in this way.

He went farther, and said to himself, "If these really are the things that give rise to the appearance of spontaneous generation, I ought to be able to take a ball of this 'dusted' gun-cotton and put it into one of my vessels, containing that boiled infusion which has been kept away from the air, and in which no infusoria are at present developed, and then, if I am right, the introduction of this gun-cotton will give rise to organisms."

Accordingly, he took one of these vessels of infusion, which had been kept eighteen months, without the least appearance of life, and by a most ingenious contrivance, he managed to break it open and introduce such a ball of gun-cotton, without allowing the infusion or the cotton ball to come into contact with any air but that which had been subjected to a red heat, and in twenty-four hours he had the satisfaction of finding all the indications of what had been hitherto called spontaneous generation. He had succeeded in catching the germs and developing organisms in the way he had anticipated.

It now struck him that the truth of his conclusions might be demonstrated without all the apparatus he had employed. To do this, he took some decaying animal or vegetable substance, such as urine, which is an extremely decomposable substance, or the juice of yeast, or perhaps some other artificial preparation, and filled a vessel having a long tubular neck with it. He then boiled the liquid and bent that long neck into an S shape or zig-zag, leaving it open at the end. The infusion then gave no trace of any appearance of spontaneous generation, however long it might be left, as all the germs in the air were deposited in the beginning of the bent neck. He then cut the tube close to the vessel, and allowed the ordinary air to have free and direct access; and the result of that was the appearance of organisms in it, as soon as the infusion had been allowed to stand long enough to allow of the growth of those it received from the air, which was about forty-eight hours. The result of M. Pasteur's experiments proved, therefore, in the most conclusive manner, that all the appearances of spontaneous generation arose from nothing more than the deposition of the germs of organisms which were constantly floating in the air.

To this conclusion, however, the objection was made, that if that were the cause, then the air would contain such an enormous number of these germs, that it would be a continual fog. But M. Pasteur replied that they are not there in anything like the number we might suppose, and that an exaggerated view has been held on that subject; he showed that the chances of animal or vegetable life appearing in infusions, depend entirely on the conditions under which they are exposed. If they are exposed to the ordinary atmosphere around us, why, of course, you may have organisms appearing early. But, on the other hand, if they are exposed to air from a great height, or from some very quiet cellar, you will often not find a single trace of life.

So that M. Pasteur arrived at last at the clear and definite result, that all these appearances are like the case of the worms in the piece of meat, which was refuted by Redi, simply germs carried by the air and deposited in the liquids in which they afterwards appear. For my own part, I conceive that, with the particulars of M. Pasteur's experiments before us, we cannot fail to arrive at his conclusions; and that the doctrine of spontaneous generation has received a final 'coup de grace'.

You, of course, understand that all this in no way interferes with the 'possibility' of the fabrication of organic matters by the direct method to which I have referred, remote as that possibility may be.

1 ([return](#))

[Those who wish to study fully the doctrines of which I have endeavoured to give some rough and ready illustrations, must read Mr. John Stuart Mill's 'System of Logic'.]

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