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ON SO-CALLED SPONTANEOUS GENERATION.

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When I announced a month ago that I would read you a paper on " Spontaneous Generation," I had no idea that one of the greatest living naturalists was going to give a most able *résumé* on the subject, or perhaps I should have hesitated in coming before you. Nevertheless I feel it is a matter for congratulation that I did so, as many unanswered questions have arisen since Professor Huxley delivered his address at Liverpool.

Two hundred and two years ago Francesco Redi successfully combated the then prevalent doctrine of spontaneous generation by the most simple, nay, almost childlike experiments, such as putting meat under fine gauze, and so showing that maggots are not spontaneously generated. Since that day the tendency of experiments has certainly been in favour of Redi's aphorism, " Omne vivum e vivo."

The question, however, all turns upon that little word *omne*, all; whether all living things originate from germs, or whether some may originate spontaneously from not living matter.

Now, there can be no doubt but that there was a first cell and a first organism which had no progenitor. Professor Huxley said last week, that although he could not believe anything in the absence of evidence upon the subject, that " expectation is permissible where belief is not ;" and that if it were given him " to look beyond the abyss of geologically recorded time to the still more remote period, when the earth was passing through physical and chemical conditions, which it can no more see again than a man can recall his infancy," he " should expect to be a witness of the evolution of living protoplasm from not living matter."

To show you that I am not biassed in this matter, and that I am no partisan, I tell you I go farther in my expectation than Professor Huxley, and I think that if we could produce the conditions we might see amœbiform protoplasm originating even yet from

inorganic matter. Perhaps, as Dr. Bastian suggests, colloid may be intermediate between inorganic and organic living material, but I tell you, gentlemen, this is all expectation, and should not be belief, as we have not at present a tittle of evidence in its favour. No doubt, with Mr. Charles Darwin's hypothesis, the origin of living organic from inorganic matter would supply a gap in the evolution of the animal kingdom : but we must not on that account found a scientific belief.

Now, sir, I shall very carefully sift the supposed evidence in favour of spontaneous generation ; I shall divide this evidence into that which is purely microscopical and that which is dependent on experiment.

First, with regard to the microscopical evidence. This consists in the assertion, that some observers have seen organic living cells and fungus spores built up by the aggregation of minute granules. Now, there is very strong evidence that this does not happen ; the organisms described as fungus spores are in some cases not fungus spores at all, and in other cases they have been observed with a hilum or point at which they were attached to a parent. Surely we cannot believe this point of attachment was the character of a spore formed *de novo*.

On the other hand, I should be sorry to deny, with my present knowledge, that it is possible organisms of a simpler kind, such as unicellular organisms, may be built up in this way. If such a mode of evolution does take place, I still believe it is from pre-existing germs ; such gemmules, for instance, as Mr. Darwin believes in, in his beautiful provisional hypothesis of pangenesis. I believe, if it can be proved that organisms can be produced by aggregation, it will be found that this only takes place when pre-existing cells have given up their contents in the fluid experimented on.

In order that you may have a clear conception of Mr. Darwin's theory, I will read to you, to my mind, far the most lucid abstract of that theory that has ever been published. It is a portion of Dr. Hooker's address to the British Association at Norwich, in 1868.

Dr. Hooker said—" You are aware that every plant or animal commences its more or less independent life as a single cell, from which is developed an organism more or less closely similar to its parent. One of the most striking examples I can think of is afforded by a species of Begonia, the stalks, leaves and other parts of which are superficially studded with loosely attached cellular

bodies. Any one of those bodies, if placed under favourable conditions, will produce a perfect plant, similar to its parent. You may say that these bodies have inherited the potentiality to do so, but this is not all, for every plant thus produced, in like manner developes on its stalks and leaves myriads of similar bodies, endowed with the same property of becoming new plants; and so on, apparently interminably. Therefore the original cell that left the grand parent, not only carried with it this so called potentiality, but multiplied it and distributed it with undiminished power through the other cells of the plant produced by itself; and so on, for countless generations. What is this potentiality, and how is this power to reproduce thus propagated, so that an organism can, by single cells, multiply itself so rapidly, and within very narrow limits, so surely and so interminably? Mr. Darwin suggests an explanation, by assuming that each cell or fragment of a plant (or animal) contains myriads of atoms or gemmules, each of which gemmules he supposes to have been thrown off from the separate cells of the mother-plant, the gemmules having the power of multiplication, and of circulating throughout the plant: their future development he supposes to depend on their affinity for other partially developed cells in due order of succession. Gemmules which do not become developed, may, according to his hypothesis, be transmitted through many succeeding generations, thus enabling us to understand many remarkable cases of reversion or atavism. Hence, the normal organs of the body have not only the representative elements of which they consist diffused through all the other parts of the body, but the morbid states of these, as hereditary diseases, malformations, &c., all actually circulate in the body as morbid gemmules.

"As with other hypotheses based on the assumed existence of structures and elements that escape our senses, by reason of their minuteness or subtlety, this of Pangenesis will approve itself to some minds and not to others. To some these inconceivably minute circulating gemmules will be as apparent to the mind's eye as the stars of which the milky way is composed: others will prefer embodying the idea in such a term as potentiality, a term which conveys no definite impression whatever, and they will like it none the less on this account.

"Whatever be the scientific value of these gemmules, there is no question but that to Mr. Darwin's enunciation of the doctrine of

Pangenesis we owe it, that we have the clearest and most systematic *résumé* of the many wonderful phenomena of reproduction and inheritance that has yet appeared; and against the guarded entertainment of the hypothesis, or speculation if you will, as a means of correlating these phenomena, nothing can be urged in the present state of science. The President of the Linnean Society, a proverbially cautious naturalist, thus well expresses his own ideas of Pangenesis—'If,' he says, 'we take into consideration how familiar mathematical signs and symbols make us with numbers and combinations, the actual realization of which is beyond all human capacity; how inconceivably minute must be those emanations which most powerfully affect our sense of smell and our constitutions; and if, discarding all preventions, we follow Mr. Darwin, step by step, in applying his suppositions to the facts set before us, we must, I think, admit that they may explain some, and are not incompatible with others; and it appears to me that Pangenesis will be admitted by many as a provisional hypothesis, to be further tested, and to be discarded only when a more plausible one shall be brought forward.'"

I have brought the subject of Pangenesis before you to-night because I believe I have observed certain very remarkable changes in the tissues of the larva of the fly prior to the formation of the perfect insect, which have prepared me to believe it is possible that organs or organisms are sometimes developed by aggregation of excessively minute gemmules, such as those which Mr. Darwin's hypothesis demands.

From observation which I made upon this subject, I found that the semi-fluid cellular matter, from which the fly is developed, is derived partly from the disintegrated tissues of the larva, and partly from the fat bodies or omenta.

After the larva ceases to feed, the tissues begin to degenerate. The muscles may be observed at this time in a state of continuous activity, rythmic contractions commencing at one extremity of each set of fibres, and passing regularly with a wave-like motion to the opposite extremity. At the same time, large bright nuclei, 1-1000th of an inch in diameter, appear in rows in the centre of the muscular fibres. These are ultimately set free by the degeneration and waste of the muscles, and exhibit a granular appearance, but are readily distinguished by their great transparency and low refractive power.

At the same time a series of remarkable changes take place in

the fat bodies, which consist in the adult feeding larva of flattened hexagonal cells filled with very opaque, highly refractive white granular matter. These cells now begin to exhibit clear spaces in their centre, which presently become converted into nuclei exactly like those formed in the muscles. The granular matter of the omental cell then becomes condensed about the nucleus, leaving a clear space around the circumference of the cell; the cells separate from each other, and the cell wall undergoes disintegration.

The free nuclei developed from the muscular fibres of the larva now begin to collect around them aggregations of molecular matter, derived from the degeneration of the muscles and other larval tissues, so that all the nuclei are soon surrounded by similar molecular aggregations, each about 1-150th of an inch in diameter.

The precise nature of the changes which take place immediately afterwards are more difficult to observe, but after the second day of the pupa state, numerous delicate nebulous-looking cells, about 1-1000th of an inch in diameter, replace some of these aggregations, and bright nuclei, 1-3000th to 1-5000th of an inch in diameter, make their appearance amongst them. The majority of the aggregations remain, however, and become more dense toward their circumference. The growth of the imaginal tissues* evidently proceeds at the expense of some of these aggregations, whilst those which remain, undergo marked changes; they increase in size, lose their original nuclei, and become invested by a delicate membrane. When the imago emerges from the pupa, a large number of these corpuscular aggregations remain in all parts of the insect; they disappear during the development of the imago, and when it is mature, not one can be detected.

If these observations are correct, there is certainly something in the process very like the development of organisms by aggregation; we find nuclei aggregating around them; molecules, which ultimately become invested in a membrane, and these molecules in turn are capable of reproducing muscles, nerves, and other tissues similar to those from which they originated. The development of the perfect fly from the larva seems, to my mind, a striking proof of the correctness of Mr. Darwin's theory of pangenesis, and also to point to the fact, that organisms may originate in a hitherto unknown manner. Even admitting that this method of origination is possible, we must not conclude that such organs or organisms

* Tissues of the Imago or perfect fly.

arise *de novo*, but rather by the aggregation, and after development of existing germs or gemmules.

With regard to the experimental evidence, it has been arrived at from two classes of experiments.

The first aims at the production of known organic forms from solutions of animal or vegetable matter. The second aims at the production of new and unknown forms, under new conditions in saline solutions.

I shall consider these two sets of experiments separately.

In the first, or simplest set of experiments, the most contradictory evidence has been arrived at by different observers. The whole, to my mind, may, however, be summed up in the following.

If we receive the usually accepted belief that the boiling temperature destroys germs, we must accept spontaneous generation as a fact. If, on the other hand, we believe that germs are not killed in this manner, these experiments only show that if the greatest possible care is used, germs may not be admitted and a negative result may be arrived at, and yet that germs may find their way into the flasks of the most careful experimenter, and may afterwards germinate.

Now, sir, I have instituted a series of the most careful experiments, which have shown conclusively to my mind that germs are not destroyed by the boiling temperature.

I took a neutral solution of acetate of ammonia and put into it a number of spores of the little mould known as *Penicillium glaucum*, and boiled them well. I then enclosed some of the boiled fluid and germs in capillary glass tubes, like those used for preserving vaccine lymph. I then carefully examined the tubes by scrutinizing them with the microscope for an hour each, and not a spore had germinated, not a mycelial filament existed in the tubes. I then put the tubes into a warm place by the stove, and in twenty-four hours numerous mycelial filaments of considerable length had protruded from many of the spores. Now, gentlemen, I should think the most hardy advocate of spontaneous generation would hardly assert that these spores had originated *de novo*, and germinated in a single night and day.

To make the experiment more complete, I enclosed in another tube some spores which had not been boiled, and I found about the same number had germinated in this tube, as in those containing the boiled spores.

I have tried another set of experiments of a similar kind. I

boiled a vegetable infusion containing a quantity of the bead-like growing mycelium of some fungus, probably a state of *Penicillium*, and mounted a few portions in a cell for the microscope. I then carefully examined and drew these portions, and watched them from hour to hour, and saw new cells formed and new buds put out. I have done this again and again with the same result.

I have further found that this process is arrested in sealed tubes after a few hours; I cannot tell why, but I strongly suspect from the absence of dissolved air in the fluids: Mr. Cooke has suggested it may possibly be from the absence of dissolved nitrogen. I strongly suspect it is from this fact that we are able to preserve meats, &c., *in vacuo*.

Of this at least there can be no doubt, both the growing mycelium and the spores of the common blue mould, *Penicillium glaucum*, will grow after boiling, and it is nevertheless possible to preserve meat, &c., on a large scale, by enclosing it *in vacuo* after boiling it.

I may here remark that Dr. Bastian's eighth experiment,* in which he found that an infusion of turnip decomposed more rapidly when enclosed *in vacuo* than a similar solution enclosed in a flask containing air, is simply incomprehensible, and is a contradiction to the well-known process of preserving meats, vegetables, fish, &c.

I think, sir, very few will believe we are justified, *without evidence*, in believing a temperature somewhat higher will kill these spores if boiling does not. I therefore look upon it that no evidence is afforded by such experiments,—as those I have included under this first division,—in favour of generation *de novo*, if my observations are confirmed.

The second series of experiments, which aim at the production of new and unknown organisms, afford a wider field for speculation. I must confess, however, that in every case which I have seen, these so-called new organisms have appeared to me undoubtedly foreign bodies, which have accidentally gained access to the solutions.

The most recent experiments of this kind were carried out by Dr. Bastian, and their results have been published in "Nature." In these experiments a solution of sodic-phosphate and ammoniac carbonate was enclosed *in vacuo* whilst boiling, and certain spiral fibres and portions of a fungus, like *Penicillium* in fruit, were found after a time in the solutions.

With a view to discover whether the spore-bearing portions of *Penicillium* would remain unaltered after boiling, I tried the unripe

* "Nature." Pt. xxxvi., p. 194.

spore-bearing filaments, and found that they were not altered in their appearance by such treatment. The ripe spores are, however, immediately scattered by contact with fluid. Now I can readily understand why no fungi were discovered until after a long lapse of time, in Dr. Bastian's solutions; although some might have been present from the first. I find solutions of sodic phosphate throw down a flocculent precipitate after a time, and in those specimens which Dr. Bastian was courteous enough to show me, I observed that the object was surrounded by just such a precipitate, which he called correctly enough granular matter. I suspect the collection of such a flocculus around the fungus drew his attention to the spot where the minute mass of fungus was.

Another reason for not believing that the fruit bearing stems of *Penicillium*, which Dr. Bastian figures, were formed in the solutions, is that these fungi never fructify in fluid. My friend Mr. M. C. Cooke tells me that he never heard of any fungi, except such as are parasites on insects, fructifying in fluid, or so long as a plentiful supply of fluid is present. As he very forcibly put it, take the vinegar plant as an example; so long as there is plenty of fluid, it never produces fruit; but take it out of the fluid, and its surface will soon be covered with blue mould. With regard to the so-called spiral fibre organisms of Dr. Bastian, they have puzzled me very much. I never, however, believe but that they were some very common accidental material which had found its way into his solutions. I observed that he only found these "organisms" in solutions containing sodic phosphate. I have tested and had tested for me three samples of crystals of this salt, and in all free soda was present. I have since tried the action of very dilute solutions of caustic alkali on various kinds of organic fibre, and have found wool fibres, minute particles of feathers, and some kinds of spiders' thread twist into spirals under its influence. Now, the spirals produced from spider's silk correspond most closely with Dr. Bastian's spiral fibre. In my own mind I have no doubt the specimen he kindly showed me was spider's silk.

At any rate I do not think, in the face of this, we ought to conclude that we have discovered spontaneous evolution from the appearance of spirals in an alkaline solution.

I apprehend then, sir, from what I have said, if my experiments are confirmed, which can easily be done, that at present, let our "philosophic faith be what it may," we have no evidence whatever of spontaneous evolution.