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Evolution and origin of life.



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LOWEST FORMS OF LIFE.

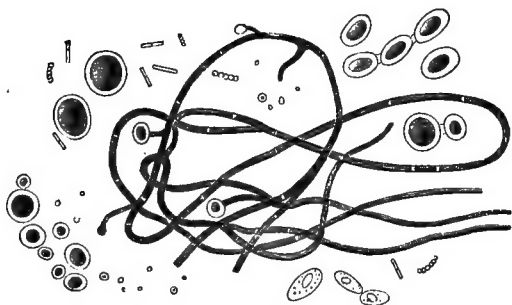


FIG. 1.

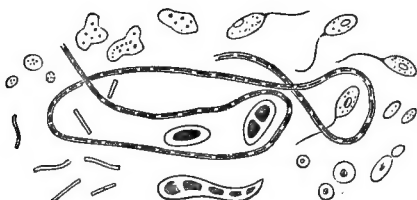


FIG. 2.

Organisms Produced within Closed Flasks which had been
Previously Heated to 270-275° F. for Twenty Minutes, and
to Temperatures over 230° F. for One Hour. (X 800).
See pp. 175-178.

EVOLUTION
AND THE
ORIGIN OF LIFE

BY

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London:
MACMILLAN AND CO.
1874.

“To experience we refer, as the only ground of all physical inquiry. But before experience itself can be used with advantage, there is one preliminary step to make, which depends wholly on ourselves: it is the absolute dismissal and clearing of the mind of all prejudice from whatever source arising, and the determination to stand or fall by the result of a direct appeal to facts in the first instance, and of strict logical deduction from them afterwards.”

SIR JOHN HERSCHEL: *Discourse on the Study of Natural Philosophy.*

“The fair question is, Does the newly proposed view remove more difficulties, require fewer assumptions, and present more consistency with observed facts than that which it seeks to supersede? if so the philosopher will adopt it, and the world will follow the philosopher—after many days.”

MR. JUSTICE GROVE: *Inaugural Address as President of the British Association, 1866.*

P R E F A C E.

SINCE the publication of my larger work, "The Beginnings of Life," in 1872, some long-overhanging clouds have been dispersed. Well-informed men of science no longer doubt that swarms of Bacteria can be made to appear within sealed glass vessels containing suitable fluids, after the vessels and their contents have been exposed to the temperature of boiling water. The thorough establishment of this fact has been of the greatest importance.

Statements as to the reality of so unexpected an appearance of Bacteria were previously received with actual disbelief or the profoundest scepticism. It was much easier to imagine that I had been mistaken or deceived than to suppose that living Bacteria could really appear within closed flasks which had been subjected to the conditions men-

tioned. This time-serving and popular verdict once announced, was received with all the more readiness, because others, not having exercised the necessary care, had for a time failed to confirm my results. Now, however, that the facts have been substantiated and are generally acknowledged, the subsequent course of events has made it useful to ask, Why the majority of persons competent to judge, previously received the statements referred to with so much incredulity?

To this question only one answer can be given. The results were discredited because it was a generally accepted belief amongst men of science that exposure to the temperature of boiling water would have killed all pre-existing Living Matter within the flasks. And this being the case, the appearance of swarms of Bacteria in the experimental fluids in the course of a few days could only be explained by the supposition that what has been called 'Spontaneous Generation' had occurred. To acknowledge this, however, was rank heresy, and was in opposition to some of the most cherished and sacred beliefs of many men of science.

In such an emergency what were scientific in-

investigators to do? Should they equivocate and, without definite trial as to the truth or untruth of their old belief in the destructive influence of boiling water, at once cast it aside? This would certainly seem a strange course to pursue, seeing that a strong belief in the lethal effects of boiling water had been the measure of their previous profound unbelief of facts now demonstrated to them! Yet this course was in some respects the easiest—it would expose those who followed it to less external friction, and it allowed them still to hold fast to a long-cherished conviction as to the truth of the dogma *omne vivum ex vivo*, which they perhaps found it impossible to throw aside. But whatever extenuating circumstances might be pleaded, it cannot be concealed that the course actually adopted was not the method by which men of science usually pursue their investigations.

The question of ‘Spontaneous Generation,’ therefore, at present stands in this position. If it be really true that all known forms of Living Matter are killed by a brief exposure in the moist state

to the temperature of 212° F., there is no longer room for doubt. The occurrence of 'Spontaneous Generation' must in this case be admitted as an established fact, or 'Law of Nature.'

But the evidence now in our possession concerning the death-point of heated Living Matter all tends to show that it is killed at a temperature below that of boiling water, as will be seen by the facts recorded in the last essay of this volume. The two previous papers, moreover, contain the records of experiments proving that Bacteria and their germs are killed at 140° F.—these being the very organisms that most frequently make their appearance in the fluids within closed experimental vessels which have been previously heated to 212° F.

In the present aspect of the question, therefore, these three papers contain all the evidence needed for the establishment of the occurrence of 'Spontaneous Generation.' In the two parts of the first essay, however, I have endeavoured to show that the acknowledgment of this natural origin of Living Matter carries with it no contradictions, and is indeed in accordance with the present state of scientific knowledge. I have further striven to give,

in brief compass, a synopsis of other evidence in favour of Archebiosis and Heterogenesis (the two processes now included under the phrase 'Spontaneous Generation'); to indicate some of the modifications in biological doctrine which a belief in the present occurrence of these processes will necessitate; and generally to show, to the best of my ability, the weakness and untenability of the old view.

In the execution of a task so complicated and difficult, there must necessarily be many imperfections. I am anxious, however, to promote what I consider to be the cause of truth, and have therefore collected the papers included in the present volume, in the hope that they may be of some little use and interest to those desirous of obtaining information on the present aspect of the great questions to which they relate.

QUEEN ANNE STREET, CAVENDISH SQUARE,

October 26th, 1874.

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I.

THE EVOLUTION HYPOTHESIS,
AND THE ORIGIN OF LIFE.

PART I.

YEAR by year the word 'Evolution' becomes diffused more widely through our literature, and the central idea which it implies grows familiar to an ever-increasing multitude of readers. We have witnessed within the last few years a marvellous awakening of interest in the minds of the public generally to questions of science, and it so happens that a discussion of the Doctrine of Evolution has been more or less directly involved in those departments of Science and Philosophy which have during this period received the largest share of popular attention.

Perhaps the greatest impetus was given to the spread of the doctrine about fourteen years ago, by the publication of Mr. Darwin's now celebrated "Origin of Species." This volume has been followed by quite a library of works and memoirs on the same subject—partly scientific and partly popular. From about the same date also, Mr. Herbert Spencer has been engaged in systematically elaborating the

principles of an all-comprehensive Evolution Philosophy, and the results of his genius and labour are now undoubtedly influencing the thoughts of a rapidly widening circle of readers. Both in this country and abroad the doctrine of Evolution is gradually but surely gaining ground amongst the most reflective, and although many other writers have been more or less influential in determining this result, it has been in the main brought about by the two above mentioned.

Evolution implies continuity and uniformity. It teaches us to look upon events of all kinds as the products of continuously operating causes—it recognises no sudden breaks or causeless stoppages in the sequence of natural phenomena. It equally implies that natural events do not vary spontaneously. It is a philosophy which deals with natural phenomena in their widest sense: it embraces both the present and the far-distant past. It seeks to assure us that the properties and tendencies now manifest in our surrounding world of things, are in all respects similar to those which have existed in the past. Without a basis of this kind, the Evolution Hypothesis would be a mere idle dream. Uniformity is for it an all-pervading necessity. Starting from facts of daily observation and from scientific experiments, the properties and

tendencies of things are noted and grouped ; whilst philosophers, using the knowledge thus gained, seek to trace back the progress of events and show how this complex world has gradually been derived from a world of more and more simple composition. We are taken back in imagination even much further. We are referred to a primal haze or nebula—as the gigantic germ of a future Universe. This was the conception of Kant and of Laplace.

But whether we follow the philosopher in his bold speculations concerning the past, or listen to the biologist making his predictions as to the future stages which the germ of a given animal will pass through in the progress of its evolution—in each case the ‘uniformity of nature’ is tacitly assumed. This assumption underlies almost all our thoughts and actions, even in every-day life. And without such a belief regarding the succession of events science would be impossible—the very idea of it, in fact, could never have arisen. In its absence we could neither fathom the past nor illumine the future. As Mr. Mill said,*—“Were we to suppose (what it is perfectly possible to imagine) that the present order of the universe were brought to an end, and that a chaos succeeded in which there was no fixed succession of events, and the past gave no assurance of the future; if a human

* Syst. of Logic, 6th edit. vol. ii. p. 98.

being were miraculously kept alive to witness this change, he surely would soon cease to believe in any uniformity, the uniformity itself no longer existing."

It is true that in earlier times no absolute belief in the uniformity of nature existed, even amongst the select few. The Greek philosophers, including Aristotle, recognised 'chance' and 'spontaneity' as finding a definite place in Nature, and to this extent they were not sure that the future would resemble the past. But as we have become more familiar with a wider range of natural phenomena and with their mutual relations or order of appearance, so has the conception of chance or spontaneity disappeared from the scientific horizon—driven out of the field by the steady advance of Law and Order. Those who embrace the Evolution Philosophy are foremost in this opinion—they believe that no effects of whatsoever kind can occur without adequate causes, and, the conditions being similar, that the same results will always follow the action of any given cause. Their whole creed is, in fact, pre-eminently based upon an assumed Uniformity of Nature.

The present is essentially a time of transition in matters of opinion. Men who have been educated in one system of scientific beliefs are gradually being converted to another, because the new system is

thought to be more harmonious with the observed order of natural phenomena. This has been the case even with our chief exponents of Evolution. They have themselves had to unlearn much which they had previously learned. The doctrine of Evolution has thus been developed only by the sacrifice of many previous early beliefs and modes of thought. But it often happens that an old belief will—unknown perhaps to the person himself—leave decided traces of its previous influence, and thus prevent for a time the full realization or adoption of all the logical consequences of new views. This vestige of the old state of opinion or habit of thought is more especially apt to remain in directions where unexplained facts still exist and strong prepossessions or prejudices bar the way. Some modes of this half-unconscious inconsistency may become obvious to one worker or thinker, and some to another, according to the particular direction which his investigations or thoughts may have taken; and if they are of an important nature such inconsistencies should be pointed out from time to time. With the view, therefore, of strengthening an hypothesis which I, in common with so many other workers in science, believe to be true, I now venture to allude to certain apparent anomalies in the declared opinions of the most prominent upholders of the doctrine of Evolution in this country. It seems

all the more desirable that this should be done, since the inconsistencies may be easily shown to be wholly uncalled for, and to involve sundry unscientific conceptions. Yet the modifications of opinion which appear to be demanded—on the ground of fact as well as on the ground of reason—will necessitate very considerable and almost revolutionary changes in the accepted code of biological doctrines.

An examination of the facts of science generally and of various every-day phenomena, teaches us, according to the Evolutionist, that Matter of different kinds, situated as it is and has been, gradually tends within certain limits to become more and more complex in its internal and external constitution. Coupling this conclusion with various astronomical data, with geological data, and with facts derived from the study of the past forms of Life upon our globe, the Evolutionist essays to penetrate through the long vista of bygone ages, till he may rest his speculative gaze upon a vast rotating nebular mass of gaseous matter of comparatively simple though unknown constitution, from which he supposes our Universe to have been slowly evolved. Without futile questionings as to the explanation or cause of the existence of the Nebula—without speculation as to what simpler or more complex matter may have immediately preceded it—it is obvious that we may

for our own convenience direct our attention especially to any particular stage of its hypothetical existence. At the same time we must be equally free to admit that, in concentrating our attention upon the nebular stage, or upon any other, we arbitrarily break into a mysterious cycle of existence whose Cause is to us unfathomable. It is needless for my purpose, however, to attempt to concentrate the reader's attention upon a period so remote in the history of our Universe. We are led to believe that the primordial nebula as it cooled and condensed acquired a more rapid axial rotation; that masses were gradually thrown off from its circumference; and that these in their turn condensed into rotating spheroids, which continued to circulate round the parent mass in elliptical orbits. Assuming, then, with the Evolutionist, that our own planet had a past history of this kind, we must also assume that it gradually changed from a gaseous to a fluid state before beginning to solidify by the formation of a superficial crust—a crust which gradually thickened as the fervent heat of it and of the fluid nucleus abated by heat radiations into space. Until this stage of the Earth's history had been far advanced, no Living Things could have existed upon its surface. "Hence," as Sir William Thomson said,* "when

* Inaugural Address at Meeting of British Association, *Nature*, Aug. 3, 1871, p. 269.

the Earth was first fit for life there were no living things on it. There were rocks, solid and disintegrated, water, air all round, warmed and illuminated by a brilliant sun, ready to become a garden." Living things must, however, have appeared upon its surface at some very remote epoch, since their remains are to be found far down in the rocks which at present constitute its crust. How, therefore, it may be asked, is the first appearance of 'Living matter' upon the earth to be accounted for?

We should not needlessly invoke an unknown act of Creative Power—we must not, even with Sir William Thomson, resort to the strange notion of an importation of living germs upon a "moss-grown fragment from the ruins of another world," unless more ordinary natural causes fail and it be found really necessary to invent some such hypothesis—and the necessity here could never be shown since Sir W. Thomson's hypothesis shirks the question of the Origin of Life so far as our earth is concerned, and merely hands it over as an unsolved problem to the denizens of another sphere. Now, the thoroughgoing Evolutionist repudiates the notion of Creation in its ordinary sense; he believes that the operation of natural causes, working in their accustomed manner, were alone quite adequate to bring into existence a kind of matter presenting a new order of complexity, and

displaying the phenomena which we have generalised under the word 'Life.' Living matter is thus supposed to have come into being by the further operation under new conditions of the same agencies as had previously led to the formation of the various inorganic constituents of the Earth's crust—such mineral and saline substances as we see around us at the present day. What we call 'Life,' then, is regarded as one of the natural results under actual conditions of the growing complexity of our primal nebula. So that, in accordance with this view, we have no more reason to postulate a miraculous interference or exercise of Creative Power to account for the evolution of 'living matter' in any suitable portion of the Universe (whether it be on this Earth or elsewhere), than to explain the appearance of any other kind of matter—the magnetic oxide of iron, for instance. So far, all th^orough Evolutionists are quite agreed. This is the view of Spencer, Lewes, Huxley, and others—possibly of Darwin. I say possibly of Darwin, because on this subject it so happens that the language of this most distinguished exponent of Evolution is quite unusually tinged with a previous point of view. Speaking of the probable commencement of Life upon our globe, Mr. Darwin says* :—"I believe that animals have

* Origin of Species, 6th edit. 1872, pp. 424 and 429.

descended from at most only four or five progenitors, and plants from an equal or lesser number. Analogy would lead me one step further, namely, to the belief that all animals and plants have descended from some one prototype. . . . There is grandeur in this view of life, with its several powers, having been originally breathed by the Creator into a few forms, or into one; and that whilst this planet has gone cycling on according to the fixed law of gravity, from so simple a beginning endless forms, most beautiful and most wonderful, have been and are being evolved." Taking into account the phraseology made use of in the above quotation, we have little difficulty in recognising the views of an Evolutionist, dwarfed and modified though they are by an ultimate appeal to a Creative act only a little less miraculous and singular than the mythical origin of our reputed ancestors—Adam and Eve. Some existing naturalists may perhaps contend that Mr. Darwin ought to have kept more closely to the Mosaic record—replacing his one primordial form by a dual birth of male and female, without whose mutual influence no "biological individuals" can in their opinion come into existence. Such a supposition, it is true, would be as antiquated and unnecessary from the Evolutionist's point of view as is the whole notion of life having been originally "breathed" into one

or more organic forms. Mr. Spencer's language is happily free from both these defects : he neither uses the phraseology of the Creative Hypothesis, nor does he adopt a definition of biological "individuality" at variance with the Evolution Philosophy. He distinctly teaches that living matter must have been at first formless, and that multiplication would have taken place, as amongst the lowest living units of the present day, exclusively by agamic methods—nay, more, he teaches that living matter must have been the gradual product or outcome of antecedent material combinations. "Construed in terms of evolution," he says,* "every kind of being is conceived as a product of modifications wrought by insensible gradations on a pre-existing kind of being, and this holds fully of the supposed 'commencements of organic life,' as of all subsequent developments of organic life."

But on the question whether the process of Archebiosis (life-evolution) is likely to have occurred once only, as Mr. Darwin seems to hint, or in multitudinous centres scattered over the earth's surface, Mr. Spencer makes no definite statement. The latter belief would, however, be entirely in accordance with his general doctrine ; and we seem all the more entitled to infer that Mr. Spencer inclines to the notion of a multiple

* Principles of Biology, vol. ii. Appendix, p. 482.

occurrence of Archebiosis, both in space and in time, since he does not reject the possibility of its occurrence in our own day. Granting "that the formation of organic matter and the evolution of life in its lowest forms may go on under existing cosmical conditions," he believes it "more likely that the formation of such matter and of such forms took place at a time when the heat of the earth's surface was falling through those ranges of temperature at which the higher organic compounds are unstable." But conclusions which we are only able to infer from the writings of Mr. Spencer have been distinctly enunciated by Mr. G. H. Lewes. In a criticism of the "Darwinian Hypotheses," he very forcibly pointed out that it is quite compatible with the hypothesis of evolution to admit a variety of starting points for the formation of living matter, and he consequently laid down in principle a very important extension of the Darwinian doctrine, in its application to higher organisms. He said: * "Although observation reveals that the bond of kinship does really unite many divergent forms, and the principle of Descent with Natural Selection will account for many of the resemblances and differences, there is at present no warrant for assuming that all resemblances and

* *Fortnightly Review*, 1868.

differences are due to this one cause, but, on the contrary, we are justified in assuming a deeper principle which may be thus formulated: All the complex organisms are evolved from organisms less complex, as these were evolved from simpler forms: the link which unites all organisms is not always the common bond of heritage, but the *uniformity of organic laws acting under uniform conditions*. . . . It is therefore consistent with the hypothesis of Evolution to admit a variety of origins or starting points." In this paper Mr. Lewes distinctly postulates the probability of a repetition of the process of Archebiosis, wherever the conditions were favourable, and though he says nothing against the continuance of such a process in the present day, neither does he dwell upon it as a probability.

Professor Huxley's* opinions on the subject of Archebiosis are very similar to those of Mr. Spencer, with the exception that he seems more strongly opposed to the notion of its occurrence at the present day, and it is to this aspect of the question that I would now direct the reader's attention. Why should men of such acknowledged eminence in matters of Philosophy and Science as Mr. Herbert Spencer and Professor Huxley promulgate a notion which seems

* Inaugural Address at Meeting of British Association, *Nature*, Sept. 15, 1870, p. 404.

to involve an arbitrary infringement of the Uniformity of Nature ?

They would both have us believe that living matter came into being by the operation of natural causes—that is, by the unhindered play of natural affinities operating in and upon matter which had already acquired a certain degree of molecular complexity. They believe that the simpler kinds of mineral and crystalline matter continue to come into being now as they have ever done; nay, more, they believe that the higher kind of matter, originally initiated by the operation of natural causes, continues to ‘grow’ both in animal and in vegetal forms, solely under similar influences, and yet they consider themselves justified in supposing that natural causes are now no longer able independently to initiate this living matter or protoplasm. Again, we find Professor Tyndall* also affirming, in the most unhesitating language, the ultimate similarity between crystalline and living matter: affirming that all the various structures by which the two kinds of matter may be represented are equally the “results of the free play of the forces of the atoms and molecules” entering into their composition. And yet he, too, would have us believe that whilst differences in degree of molecular complexity alone separate living from not-living

* *Fragments of Science*, 4th edit. (1872), pp. 85—87, and 113—119.

matter, the physical agencies which freely occasion the growth of living matter are now incapable of causing its origination.

Why, we may fairly ask, should a supposed difference be erected by Evolutionists between Origination and Growth in the case of living matter, whilst no one dreams of making any such distinction in reference to crystalline matter? Is it true that the process of growth differs from the process of origination, and, if so, in what respects? Philosophically speaking there is little difference. Take the case of the formation of the "silver tree" cited by Professor Tyndall. A weak galvanic current is passed through a solution of nitrate of silver, and simultaneously in a first increment of time a number of molecules of oxygen and of silver begin to aggregate independently into crystals of oxide of silver; in a second increment of time the operation of the same causes produces similar results, only now part of the new crystalline matter forms in connection with the existing recently-formed germs of crystals, though part of it may still aggregate independently. During a third, a fourth, and all succeeding increments of time in which the same causes operate amidst similar conditions, similar results must ensue. But, taking the process of origination that occurs in the first increment of time,

would Professor Tyndall have us believe that it is in any essential way different from that process of growth which may take place in a second, third, or fourth increment of time? Does not the very fact that origination and growth so often occur simultaneously in the case of crystalline matter, and under the influence of the same causes, show us that the two processes are intrinsically similar, and that conditions favourable for growth are also likely to be favourable for origination? And if this be true for crystalline matter, may we not infer that it would also be true for living matter? These are questions neither asked nor answered in any definite manner by those whose opinions I have already cited. They are, however, questions by no means unworthy of an attentive consideration.

Although, as a general rule, conditions favourable for the growth of any particular kind of crystalline matter are likely to be favourable for its origination, still it must be acknowledged that the presence of a crystal will occasionally lead to its growth in a medium in which similar crystalline matter had previously shown no tendency to form independently—even in cases where the introduction of a non-crystalline nucleus would not be able to determine a similar formation of crystalline matter. Notwithstanding the general law, therefore, that conditions

favourable for the growth are also favourable for the origination of crystalline matter, we are compelled to admit that growth may be determined under certain conditions where origination does not occur, and that the presence of pre-existing crystalline matter favours the process. And a distinction of the same kind undoubtedly obtains in the case of living matter. We know quite positively that although Bacteria will not originate in a previously-boiled ammoniac tartrate solution, or 'Pasteur's solution,' that the addition of a few of these organisms (all other conditions remaining the same) to either one of the solutions will soon occasion a very considerable growth of the living matter of which Bacteria are composed.* We are thus reduced to ask, whether the influence of the pre-existing nucleus is relatively more potent or more necessary in the case of living matter than it is in the case of crystalline matter? And this is a question which unfortunately we are unable definitely to answer: such minute quantitative and qualitative distinctions cannot be made. But so long as we have no positive knowledge on this subject, we surely have little right to infer that processes both of origination and of growth continue in the case of crystalline matter, and that the process of growth alone survives in the

* *The Beginnings of Life*, vol. i. p. 325.

case of living matter. There are no facts easily discoverable upon which such a fundamental assumption can be legitimately based — for it is one which the Evolutionist should not admit except upon evidence of the clearest and most unambiguous nature.

The probabilities would certainly seem to be altogether in favour of the continuance of a natural process like Archebiosis after it had been once initiated, more especially when this natural process is so closely allied to another (namely, the 'growth' of living matter) which manifests itself with the utmost readiness on all parts of the earth's surface. So that unless very cogent reasons can be adduced against the occurrence of Archebiosis at the present day, looked at from an *à priori* point of view, there would seem scarcely room for doubt upon the subject. The properties and chemical tendencies of material bodies appear to be quite constant through both time and space. Speaking upon this subject in a recent discourse on 'Molecules,' Professor Clerk Maxwell says,* "We can procure specimens of oxygen from very different sources, from the air, from water, from rocks of every geological epoch. The history of these specimens has been very different, and if, during thousands of years,

* *Nature*, Sep. 25, 1873, p. 440.

difference of circumstances could produce difference of properties, these specimens of oxygen would show it. . . . In like manner, we may procure hydrogen from water, from coal, or, as Graham did, from meteoric iron. Take two litres of any specimen of hydrogen, it will combine with exactly one litre of any specimen of oxygen, and will form exactly two litres of the vapour of water. . . . Now, if during the whole previous history of either specimen, whether imprisoned in the rocks flowing in the sea, or careering through unknown regions with the meteorites, any modification of the molecules had taken place, these relations would no longer be preserved. . . . But we have another, and an entirely different method of comparing the properties of molecules. The molecule, though indestructible, is not a hard rigid body, but is capable of internal movements, and when these are excited it emits rays, the wave-length of which is a measure of the time of vibration of the molecule. . . . By means of the spectroscope the wave-lengths of different kinds of light may be compared to within one ten-thousandth part. In this way it has been ascertained, not only that molecules taken from every specimen of hydrogen in our laboratories, have the same set of periods of vibration, but that light having the same set of

periods of vibration, is emitted from the sun and from the fixed stars. . . . We are thus assured that molecules of the same nature as those of our hydrogen exist in those distant regions, or at least did exist when the light by which we see them was emitted." With evidence such as this before us, which could be multiplied to an enormous extent, we should hesitate before needlessly postulating any infringement of the uniformity of natural phenomena: we ought in fact only to entertain such a supposition when it has been lightly forced upon us. Certainly we should not resort to it and then strain the interpretation of natural and experimental phenomena into a forced accordance.

What, then, are the reasons assigned for the non-occurrence at the present day of the process of Archebiosis? All that Mr. Spencer says upon the subject is, that such a process seems to him more likely to have occurred at "a time when the heat of the earth's surface was falling through those ranges of temperature at which the higher organic compounds are unstable," than at the present day. Why such conditions would be more favourable than those now existing Mr. Spencer does not say; and that such an alteration should suffice to put a stop to Archebiosis, although we see living matter still

growing freely all over the earth under the most diverse conditions as regards temperature, seems very difficult to believe. Yet no other suggestion is offered in explanation of an assumption which seems essentially unscientific. For the assumption that Archebiosis took place only in the remote past puts this process on a *quasi* miraculous level, and tends to assimilate it to an act of special creation, the very notion of which Mr. Spencer, in other cases, resolutely rejects.

Again, what reason does Professor Huxley give, in explanation of his supposition as to the present non-occurrence of Archebiosis? He says,* if it were given to him "to look beyond the abyss of geologically recorded time" to a still more remote period of the earth's history, he would expect "to be a witness to the evolution of living protoplasm from not-living matter." And the only reason distinctly implied why a similar process should not occur at the present day, is because the physical and chemical conditions of the earth's surface were different in the past from what they are now. And yet, concerning the exact nature of these differences, or the degree in which the different sets of conditions would respectively favour the occurrence or arrest of an evolution of living matter, Professor Huxley cannot possess even the vaguest

* *Nature*, Sep. 15, 1870, p. 404.

knowledge. He chooses to assume that the unknown conditions existing in the past were more favourable to Archebiosis than those now in operation. This, however, is a mere assumption which may be entirely opposed to the facts. It is useless of course to argue upon such a subject, but still it might fairly be said, in opposition to his view of the impotency of present telluric conditions, that the abundance of *dead organic matter* now existing in a state of solution would seem to afford a much more easy starting-point for life-evolution than could have existed in that remote past, when no living matter had previously been formed, and consequently when no dead organic matter thence derived could have been diffused over the earth's surface.*

Professor Huxley is, however, very inconsistent, since, in spite of his declared expectation of witnessing the evolution of living from lifeless matter, if it were given him "to look beyond the abyss of geologically recorded time," he had said scarcely five minutes before, in reference to experimental evidence bearing upon the present occurrence of a similar process, that "if, in the present state of Science, the alternative is offered us, either germs can stand a greater heat than

* This is a consideration of great importance; since those who believe that Archebiosis occurs in organic solutions at the present day, have not yet professed to show that it can occur in saline solutions free from all traces of organic matter.

has been supposed, or the molecules of dead matter, for no valid or intelligible reason that is assigned, are able to re-arrange themselves into living bodies, exactly such as can be demonstrated to be frequently produced in another way, I cannot understand how choice can be, even for a moment, doubtful." Having thus expressed himself, it was a little strange that Professor Huxley almost immediately afterwards forgot to inform his audience what "valid or intelligible reason" he was able to assign for the occurrence of that evolution of not-living matter into living protoplasm, in the remote past to which he alluded. A supernatural interposition of creative power would explain the presence of living things upon our earth, just as easily as a supernatural preservation of living matter from the destructive effects of heat would account for the presence of living organisms within certain experimental flasks. But Professor Huxley most inconsistently says that even in the face of scientific evidence concerning the destructive powers of heat upon living matter, he would rather explain the presence of organisms in certain flasks on the hypothesis of a (supernatural) preservation of germs, than believe in the otherwise proved occurrence of a present life-evolution similar to that which he assumes to have taken place in the past. He is willing to accept the supernatural in the present, though he declines to interpret the past by its aid. He

assumes this attitude because no " valid or intelligible reason " is assigned in explanation of life-evolution, a belief in which would render unnecessary any appeal to the supernatural in the present ; though he himself postulates the occurrence of the same unexplained process in the past solely in order to avoid having recourse to the supernatural. Professor Huxley's position in reference to this question is very puzzling, and one cannot help wondering through what monochromatic glass he had been taking his observations (from his watch-tower) in order to come to the conclusion that " the present state of science " gives any sanction to such vacillations, or entitles him to appeal to a supernatural preservation of germs instead of trusting to the known uniformity of natural phenomena.

Sir William Thomson was certainly much more consistent. He too seemed inclined to explain the experiments of our own day by resorting to the hypothesis of a supernatural preservation of germs, and similarly he seems not unwilling to explain the original advent of Life upon this globe, by another assumed process of " contagion." He has resort neither to a creative hypothesis, nor to the hypothesis of a natural becoming of living matter, but, shelving the question of " origin " altogether, he suggests that our Earth may have become peopled with organic forms owing

to the advent upon it, in the remote past, of a "moss-grown fragment from the ruins of another world." Sir William Thomson's hypothesis seems strangely improbable in itself, though it has, in comparison with the views of other distinguished authorities, the somewhat rare merit of being not inconsistent with his notions concerning the experiments of to-day. He does not reject the supernatural in the past, whilst resorting to it for the present—he resorts to it in the present and in the past alike, and curiously evades altogether the real problem as to the Origin of Life.

Since so little—or rather nothing—is said by Professor Huxley in support of his supposition that living matter does not originate in the present day, even though the process of origination is so closely akin to that of growth, and though the process of growth is taking place at every moment of our lives, in every region of the globe, and under the most varied conditions—amidst tropical heat and icy coldness, on mountain-tops and deep down in almost unfathomable ocean-beds,—it seems only reasonable to suppose that he must have been influenced by some strong prepossessions. And so far as one can gather from his Presidential Address before the British Association, from which I have already quoted, he does appear to have been powerfully biased by theoretical considerations. One of these we shall now consider.

Much stress is laid by certain writers upon the fact that "the doctrine of spontaneous or equivocal generation has been chased successively to lower and lower stations in the world of organized beings as our means of investigation have improved."* So that, as another very eminent writer says, "if some apparent exceptions still exist they are of the lowest and simplest forms."† And it is usually inferred from this fact that further knowledge and improved means of observation will prove these apparent exceptions to be no exceptions to the supposed general rule—*omne vivum ex vivo*. A consideration of this kind seems to have powerfully influenced Professor Huxley.

Much confusion exists in reference to this point, which needs to be removed. In the first place, it must be freely admitted that many ancient notions, dating from the time of Aristotle, on the subject of "Equivocal or Spontaneous Generation," as a mode of origin for large and complex organisms, were altogether crude and absurd. Secondly, it is necessary to distinguish (and Professor Huxley did so) between two meanings of the phrase which have often been confounded with one another—viz., be-

* Prof. Lister, Introductory Lecture (University of Edinburgh), 1869, p. 12.

† Mr. Justice Grove (Presidential Address), Rep. of Brit. Assoc. for the Advancement of Science, 1866, p. 71.

tween Heterogenesis, or the mere allotropic modification of already existing living matter, and Archebiosis, or the independent origination of living matter. Thirdly, it should be distinctly understood that those who strictly adhere to the Evolution Hypothesis could never believe in the origination of any but the "lowest and simplest" organic forms by a process of Archebiosis. So that the gradual driving of the question back as one possibly applicable to such organisms only, is just what the Evolutionist would have expected, and therefore the objection above indicated should have been quite pointless for Professor Huxley.

Molecular combinations giving rise to units of protoplasm far below the *minimum visibile* stage of our most powerful microscope, would represent those initial collocations by which alone living matter could come into being—though the invisible 'germs' thus initiated may afterwards appear as minutest visible specks which grow into Bacteria, Vibriones, or Torulæ. We may, therefore, be further permitted to remark that even if it were given to Professor Huxley to "look beyond the abyss of geologically recorded time," he would be extremely unlikely to witness an "evolution of living protoplasm from not-living matter." At the most, he might see (that is, if equipped with a powerful microscope) only what he may

equally well see now—viz., a gradual emergence into the sphere of the visible of minute specks of living protoplasm. But though *he* might, when looking back to this remote age, be inclined to consider such appearances as testifying to the evolution of living protoplasm from not-living matter, he would perchance find it just as difficult to convince others of the absence of invisible salamandrine germs (derived perhaps from the “moss-grown fragment of another world”) as he is himself difficult to be convinced by similar appearances at the present day. Professor Huxley appears, for the time, to have lost sight of a consideration justly deemed by Professor Tyndall to be of great importance in the interpretation of evolutionary phenomena—viz., the enormous difference in point of size between the first constituent molecules of protoplasm and the minutest visible organisms. As Professor Tyndall* puts it, compared with their constituent elements, “the smallest vibrions and bacteria of the microscopic field are as behemoth and leviathan”—even though the latter are often less than $\frac{1}{30000}$ of an inch in diameter. How then could Professor Huxley expect that he might be able to witness those initial combinations which may never be seen by mortal eye? All that he might have seen

* Fragments of Science, 4th edit. (1872), p. 151.

then he can see now—however much this may fall short of his declared expectation.

Thus it would appear that a consistent belief in the Evolution Hypothesis almost necessarily carries with it a belief in the continuance of the process of Archebiosis from the remote epoch when living matter first appeared upon this earth down to the present time. The Evolutionist teaches us that living matter is not in its essence different from other kinds of matter, and that it originally came into being, like the various forms of mineral and crystalline matter, by the operation of mere natural causes. As Professor Huxley says:—* “Carbon, hydrogen, oxygen, and nitrogen are all lifeless bodies. Of these carbon and oxygen unite in certain proportions and under certain conditions to give rise to carbonic acid; hydrogen and oxygen produce water; nitrogen and hydrogen give rise to ammonia. These new compounds, like the elementary bodies of which they are composed, are lifeless. But when they are brought together under certain conditions, they give rise to the still more complex body, protoplasm; and this protoplasm exhibits the phenomena of life.” Again we know that the properties and chemical tendencies of compounds and elementary substances are the same

* *Fortnightly Review*, Feb. 1869.

now as they have been in past ages. So that if living matter has once arisen naturally and independently, and if it still continues to "grow" freely under the most varied conditions upon and beneath the Earth's surface, the laws of uniformity alone, upon which all Science is based, should lead us to expect that it would continue to have a similar independent 'origin.' These conditions being fulfilled, we have, therefore, the best possible *à priori* warrant for the belief that living matter is continually coming into being by virtue of the operation of the same 'laws' or molecular properties as suffice to regulate its growth.

Let the Evolutionist attempt to deny it, and see what other difficulties and contradictions he plunges into, in addition to that lack of consistency which I have already pointed out.

If an evolution of living matter occurred only far back beyond the depths of geologically recorded time, and if, as Mr. Darwin* would have us believe, "all the living forms of life are the lineal descendants of those which lived long before the Cambrian epoch," how is the Evolutionist to explain the existence of the multitudinous myriads of lowest and almost structureless organisms which exist at the present day? He starts in his argument in favour of Evolution from the fact that the condition of homogeneity is one of neces-

* Origin of Species, 6th edit. p. 428.

sarily unstable equilibrium. All homogeneous matter inevitably tends to become more or less heterogeneous, and, of the different kinds of matter, none unites within itself the various qualities tending to favour this passage from the homogeneous to the heterogeneous in the same degree as living matter. These tendencies are daily exemplified to us by the phases of embryonic development passed through by the more or less homogeneous germs of multitudinous complex organisms. The embryonic development of one of the higher animals—of man himself for instance—is a kind of highly condensed epitome of animal evolution in general. And the varied forms of life of higher organization, both animal and vegetal, which have existed, and still exist, upon the surface of our earth, are all supposed by the Evolutionist to have arisen by dint of insensible modifications wrought through the long lapse of ages upon successive generations of organic forms. But if living matter, situated as it is and has been, contains within itself the potentiality of undergoing such mighty changes and of ever growing in complexity—if from originally structureless protoplasm (that is, structureless to our senses) all the varied forms of life have been derived, how is it that some of this very same matter should have remained through the long lapse of ages almost in its primitive structureless condition? Why should one portion of the living matter which came

into being in pre-Cambrian epochs have (in its successive developments) passed through such marvellous changes, whilst another portion has continued to grow—through all the inconceivably numerous generations that must have occurred between that time and the present—without undergoing change?*

What, then, is the meaning of the existence of Bacteria, *Torulæ*, *Amœbæ*, and such simplest organisms at the present day? Mr. Spencer saw the difficulty above indicated, but apparently did not fully realize its force. He attempted (very inconsistently, as it appears to me) to meet it by supposing that living matter may escape increasing organisation so long as it can avoid the influence of gross changes in 'external conditions'; and, just as inconsistently, he assumed that living matter could actually escape these changes in external conditions through that long lapse of ages which the lowest estimate regards as a period of no less than 100,000,000 of years. Speaking

* The multiplication of the lowest forms of life takes place so simply that, as Prof. Huxley has pointed out, it is nothing more than a process of 'discontinuous growth.' But that such similarity as is above indicated should exist between the structureless units of living matter of the present day and those of a remote past, if we have to do merely with direct descent or kinship, is regarded as highly improbable by Mr. Darwin, who says:—"Judging from the past, we may safely infer that not one living species will transmit its unaltered likeness to a distant futurity."—*Origin of Species*, (1872) 6th edit. p. 428.

of what he presumes to be ancient though almost structureless kinds of organisms, and endeavouring to account for their stationary condition as regards structure by supposing that they have succeeded through long ages in what I may perhaps be allowed to call 'dodging' all changes in their environment, Mr. Spencer says:—"New influences are escaped by the survival of species in the unchanged parts of their habitats, or by their spread into neighbouring habitats, which the change has rendered like their original habitats, or by both."

Now, in opposition to both these views of Mr. Spencer, many very cogent objections may be alleged. In the first place, in supposing that the organization of living matter would not increase even through ages of time unless it were subject to marked variations in external conditions, Mr. Spencer makes a supposition which seems notably at variance with his own doctrines of Evolution. Does he not for a time ignore those internal causes of change which must ever be in operation within living matter as within all other kinds of matter—and which, even in combination with approximately fixed external conditions, should suffice to produce a continually-increasing differentiation (organization) in living matter? Mr. Spencer himself says* :—"All finite forms of the homogeneous—all

* First Principles, 2nd edit. pp. 429 and 548.

forms of it which we can know or conceive—must inevitably lapse into heterogeneity. In three several ways does the persistence of force necessitate this. Setting external agencies aside, each unit of a homogeneous whole must be differently affected from any of the rest by the aggregate action of the rest upon it. The resultant forces exercised by the aggregate on each unit, being in no case alike both in amount and direction, cannot produce like effects on the units. And the various positions of the parts in relation to any incident force preventing them from receiving it in uniform amounts and directions, a further difference in the effect wrought on them is inevitably produced." Even this is not all: Mr. Spencer also points out that "every differentiated part is not simply a seat of further differentiations, but also a parent of further differentiations; since in growing unlike other parts, it becomes a centre of unlike reactions on incident forces, and by so adding to the diversity of forces at work adds to the diversity of effects produced. This multiplication of effects is proved to be similarly traceable throughout Nature." Now, if causes like these are inevitably at work upon and within the simplest forms of life, no change in external conditions would be needed in order to insure alteration or increasing complexity of structure, through months or years, to say nothing about

long ages.* But as a matter of fact (and now we turn to the second part of Mr. Spencer's argument), even if we were to grant that the liability of organisms to increase in complexity of structure, only "arises from the actions and reactions between organisms and their fluctuating environments," seeing that these changes in the environment are, as enumerated by Mr. Spencer, due to "astronomic, geologic, meteorologic, and organic agencies," organisms never could by any possibility shelter themselves through

* If an increasing complexity of structure does not gradually ensue, the specimens of living matter in question will, after a time, undergo some other molecular rearrangements, resulting in different manifestations of Heterogenesis—either of an ascending or of a retrograde nature. Heterogenetic transformations from vegetal to animal type are very frequent occurrences, whilst heterogenetic degradations (with resolution into colonies of lower forms) are still more frequent terminations of the lives of lower animal and vegetal organisms. The latter, especially, are often mere repetitions of similar, short-lived, and more or less independent, units. And if these original forms constantly reappear, so do the same crystalline forms always show themselves when given chemical compounds emerge from a state of solution. The different varieties of living matter (from the point of view of its molecular composition) are probably innumerable, and each minute difference in molecular constitution, at whatever period it may have been induced, causes the living unit either gradually or suddenly to unfold into a different organic form. Similarly recurring external influences acting upon similarly constituted units of living matter should constantly engender molecular variations resembling those which have previously existed, and these when produced must necessarily lead the living unit through evolutionary changes similar to those which others have undergone. To the "*uniformity of organic laws acting under uniform conditions*" (p. 15) we must, therefore, turn for the true explanation of the persistent recurrence of myriads of Living as well as of Crystalline Forms.

long ages of time from the influence of such external inciters of change. Mr. Spencer's explanation of the cause of the existence of multitudinous almost structureless organisms at the present day, therefore, as it appears to me, entirely falls to the ground. The lowest organisms cannot through long ages escape the incidence of new external conditions (such as we know, from actual observation, do powerfully modify them), neither, if they could, should the progress of organization thereby entirely cease—since the internal causes of change would still remain active and still continue to give rise to a 'multiplication of effects,' as Mr. Spencer has himself explained.

Thus the existence of such lowest and simplest organisms as the microscope everywhere reveals at the present day, is quite irreconcilable with the position that life-evolution has not occurred since an epoch inconceivably remote in Time. As I have elsewhere asked * :—"Would the Evolutionist really have us believe that such forms are direct continuations of an equally structureless matter which has existed for millions and millions of years without having undergone any differentiation? Would he have us believe that the simplest and most structureless *Amœba* of the present day can boast of a line of ancestors stretching back to such far remote periods that in comparison

* *The Beginnings of Life*, 1872, vol. i. p. xii.

with them the primæval men were but as things of yesterday?" On the other hand, let him admit the present occurrence of Archebiosis and Heterogenesis, and both the existence and protean variability of the lowest organisms are at once readily explained. We may suppose units of living matter continually seething into existence afresh, endowed with enormous plasticity, and developing either directly or indirectly into the various lower forms of life—thus new recruits are constantly appearing, ever ready to fill up the gaps occasioned by promotion and death amongst their predecessors.

I.

THE EVOLUTION HYPOTHESIS,
AND THE ORIGIN OF LIFE.

PART II.

HAVING endeavoured to show how very inconsistent is the view of certain leading Evolutionists, that Archebiosis was limited to the earliest stage or stages of the prodigious period during which living things have flourished upon the Earth's surface; and also how irreconcilable such a notion is with the fact of the existence of multitudes of almost structureless organisms at the present day, let us now turn to a brief consideration of the evidence which is considered by many to prove the present occurrence of Archebiosis. In other words, let us consider the nature of the evidence which may go to assure the Evolutionist that he need postulate no arbitrary infringement of the 'uniformity of nature,' and that living matter, like other kinds of matter, comes into being by virtue of the same laws or molecular properties as suffice to regulate its growth.

When Professor Huxley delivered his celebrated Inaugural Address before the British Association in 1870, by disregarding the consideration of adverse facts, and bringing to the front a long chain of evi-

dence on a subject which had only a very subordinate importance for the argument,* he did his best to convince the public that there was no evidence justifying a belief in the present occurrence of Archebiosis — and that, on the contrary, the doctrine “*omne vivum ex vivo*” was still, not only in the ascendant, but “victorious along the whole line.” He showed, by what doubtless appeared to the majority of his audience a brilliantly conclusive chain of evidence, that the sole cause of putrefaction in certain experimental fluids was, as maintained by Pasteur, their unperceived contamination with atmospheric germs. Professor Huxley, however, fell into the error which M. Pasteur had previously committed — he ascribed to the presence and influence of germs of Bacteria, phenomena which have now been shown to be producible, and actually produced in many instances, by the mere dead organic particles which the air contains in such abundance. Speaking of living Bacteria germs, Professor Huxley summed up by saying:—“Considering their lightness and the wide diffusion of the organisms which produce them, it is impossible to conceive that they should not be suspended in the atmosphere in myriads.” Had Professor Huxley himself made some careful and discriminating experiments on this part of the subject,

* See *Nature*, Sep. 22 and 29, 1870.

he might have found that the supposed impossibility of conception was entirely delusive. Again, it is well known that Professor Huxley did at the time discredit the now admitted fact that Bacteria will appear in sealed flasks (containing suitable fluids) whose air has been expelled by boiling.* He discredited this fact because he believed that Bacteria must have been destroyed by the process of boiling—and because he was unwilling to believe that they could be produced *de novo*. He went, as we have seen, so far as to say that he would rather discredit scientific evidence concerning the destructive influence of heat upon living matter, than believe in the present occurrence of a life-evolution similar to that which he postulates for the past—although he is quite unable to assign any valid reason for making such a distinction between present evolutionary potentialities and those assumed to exist in an unknown past.

What then has been the subsequent progress of events? In the first place it has been shown by Professor Burdon Sanderson, myself, and others, that living Bacteria germs are not diffused through the air to any appreciable extent,† and this is now a very widely accepted doctrine, in spite of its being, as Professor Huxley imagined, an impossible conception.

* *Nature*, Sep. 15, 1870, p. 403.

† *Beginnings of Life*, 1872, vol. ii. p. 6.

Secondly, the fact, which he discredited, of the appearance of Bacteria in closed flasks after the boiling of their contents, has been fully substantiated by Professor Sanderson, Professor Huizinga, and others. They have satisfied themselves as to the correctness of my statements, and have found that swarms of Bacteria will appear in the course of a few days within sealed experimental flasks whose fluids have been previously boiled. Meanwhile it has been shown, and is believed by the majority of biologists, that the briefest exposure to the influence of boiling water (212° F.) is destructive to all living matter. Whilst those who have attempted accurately to define the precise degree of heat which suffices to kill the lower infusorial organisms have invariably found that none of them could survive exposure to a temperature of 140° F. for five minutes.* Indeed all the simplest forms which can be individually watched are found to be killed when suddenly exposed to temperatures below 131° F., and Max Schultze † as well as Kühne has ascertained that many of them perish even at or below 122° F.

Thus it has been established that living protoplasm is certainly destroyed by sudden exposure to a temperature of 140° F. when in the moist state, irre-

* Proceedings of Royal Society, No. 145 (1873), pp. 325—331.

† *Das Protoplasma*, 1863, p. 63.

spective of the nature of the fluid in which it may be immersed; and it has been established that living matter *of the same kind* will constantly make its appearance in many suitable fluids—safely protected from contamination—within a few days after the fluids have been exposed to the very much more destructive temperature of 212° F.—to say nothing of still higher grades of heat. What conclusion, then, can be drawn by men of science, but that Archebiosis still occurs—that living matter, like crystalline matter, is still capable of arising *de novo*, or independently of pre-existing germs? The Evolutionist's belief in the uniformity of nature becomes still further justified by evidence.

Stripped from all unnecessary and unessential complications, such is our present state of knowledge on this problem as to the present occurrence or non-occurrence of Archebiosis. It is easy, of course, for any person who does not investigate a subject for himself, and who continues to disregard the investigations of others as long as their results are opposed to views which he may chance to have adopted, to continue unshaken by the course of events. But it is none the less rash and disingenuous for him to renew (without comment or rectification) the proclamation that his particular view is still "victorious along the whole line," after the inexorable logic of

facts has shown it to be otherwise.* Later in the day Professor Huxley may, perchance, learn more readily to distinguish the difference between 'marking time' and marching.

Again, many of those who do not fully believe in the Evolution Hypothesis, and who still cling, more or less fully, to a 'vitalistic' philosophy appear bewildered, if not terrified, by the conclusion which stubborn facts threaten to force upon them. They behave in the most inconsistent manner, and, in dealing with these facts imperilling their old prepossessions, they find it convenient to forget for the time and occasion the ordinary rules of scientific research. Although the turning-point of the whole question as to the present occurrence or non-occurrence of Archebiosis must obviously depend upon the precise temperature at which living matter ceases to live, none of those who are opposed to a belief in its present occurrence, and have taken part in the controversy, will fairly face this all-important part of the question. They are alert enough to realize their danger. For those who have admitted that Bacteria will appear in previously-boiled fluids would have no possible loophole for escape, if they also found the facts I have mentioned concerning the thermal death-point of Bacteria, and living matter generally, to be correct. Hence they studiously

* See Professor Huxley's "Critiques and Addresses," 1873, with his remarks on this subject at the conclusion of the Preface.

avoid investigating, or ever writing anything definite, upon this part of the subject. Taking advantage of the enormous weight of prejudice and prepossession against views which they are themselves for one reason or another unwilling to admit; taking advantage also of collateral complexities with which the subject is unavoidably beset, they succeed (either knowingly or unwittingly) in introducing confusion into their treatment of the question by dealing with side-issues as though they were essentials. The incoherence of their argument seems, in fact, to remain undiscovered by a large majority of their readers, and they thus contrive to escape detection—like cuttle-fish behind the clouds produced by their own ink.

Well may Sir John Herschel have said,* when speaking of the use and abuse of hypotheses, that “a bigoted adherence to them, or, indeed, to peculiar views of any kind, in opposition to facts as they arise, is the bane of all philosophy.” Well indeed will it be for Science generally, or the Cause of Truth, when her followers in all departments learn more fully to act in accordance with wise precepts such as these †:—“Experience once recognized as the fountain of all our knowledge of nature, it follows that in the study of nature and its laws, we ought at once

* Discourse on the Study of Natural Philosophy, p. 204.

† *Idem*, p. 79.

to make up our minds to dismiss as idle prejudices, or at least suspend as premature, any pre-conceived notion of what might or what ought to be the order of nature in any proposed case, and content ourselves with observing, as a plain matter of fact, what *is*. To experience we refer as the only ground of all physical inquiry. But before experience itself can be used with advantage, there is one preliminary step to make, which depends wholly on ourselves: it is the absolute dismissal and clearing the mind of all prejudice, from whatever source arising, and the determination to stand or fall by the result of a direct appeal to facts in the first instance, and of strict logical deduction from them afterwards."

Having said thus much concerning recent events and the nature of the experimental evidence by which the occurrence of Archebiosis is to be established, it will be well now finally to scrutinize the basis of the old and of the new beliefs in order to ascertain the relative cogency of the arguments upon which they rest. We may also test them generally in the way that hypotheses are usually tested—that is, we may strive to form an independent judgment upon the question, as to which of them gives the best explanation of the largest number of phenomena, and which best enables us to predict new facts.

Reduced to its simplest form, the fundamental fact whose interpretation is doubtful may be thus expressed:—*Certain of the most minutest living things are known to appear in some fluids independently of pre-existing visible germs.** In explanation of phenomena of this kind, observed by the aid of the microscope two hypotheses are offered:—

(1). The hypothesis of *Archebiosis* (carrying *Heterogenesis* with it as a necessary consequence), which supposes that these minutest living things have come into being, and into the region of the visible, by a process of chemical combination and growth, similar in kind to that by which crystalline germs originate in other fluids.

(2). The hypothesis of *Panspermism* (discrediting both *Archebiosis* and *Heterogenesis*), which supposes that the minutest living things above referred to have merely developed in the fluids, owing to the accidental presence therein of invisible 'germs' thrown off from pre-existing living organisms.

Thus we start with two possibilities which, in the eyes of the Evolutionist at least, have about an equal amount of probability. Each hypothesis is also supported by an analogical argument of considerable force.

The analogy tending to support the first hypothesis

* See "The Beginnings of Life," vol. i. pp. 292, *et seq.*

is drawn from the essential similarity existing between living matter and other forms of matter, and from the fact that Crystals, which originate independently in fluids, make their appearance in suitable media just as these lowest living Organisms are observed to do. Although this analogy has not been dwelt upon by those who do not share my views on this question, it is one which many of them would be bound to accept, since the relationship between crystals and organisms is generally admitted by Evolutionists. This relationship is distinctly affirmed by Mr. Herbert Spencer and also just as completely by Professor Tyndall—as I have already mentioned. Professor Huxley, moreover, speaks most distinctly on this subject. He says :*—“ It is not probable that there is any real difference in the nature of the molecular forces which compel the carbonate of lime to assume and retain the crystalline form, and those which cause the albuminoid matter to move and grow, select and form, and maintain its particles in a state of incessant motion. The property of crystallizing is to crystallizable matter what the vital property is to albuminoid matter (protoplasm). The crystalline form corresponds to the organic form, and its internal structure to tissue structure. Crystalline force being a property of matter, vital force is but a property of matter.”

* Essay on “The Physical Basis of Life,” *Fortnightly Review*, 1869.

The second hypothesis may be said to be based upon (rather than merely supported by) the analogy, derived from the observed universality of 'reproduction' amongst living things. It is argued that if all living things, as far as the process is visible, are 'reproduced' or derived from pre-existing parent living things, so, it is probable that cases in which the process is invisible would come under the same otherwise universal law—expressed by the phrase *omne vivum ex vivo*.

But this formula, which has now become a party watchword, bases its supposed universality and authority, partly (*a*) upon a simple and incomplete observation of phenomena, and partly (*b*) upon an erroneous assumption.

(*a*). On the side of observation the formula *omne vivum ex vivo* is supposed to derive its authority from the fact that the experience of mankind generally—both skilled and unskilled—testifies to its truth. Here, however, the authority of the formula is invalidated, on account of a grave misapprehension as to the real nature of the problem. It is almost unnecessary to say that observation is of no avail in regions where it becomes impossible, and consequently that observation cannot tell us whether previously invisible specks of living matter have arisen from invisible living germs or by an inde-

pendent elemental mode of origin. Thus, living matter may have been continually coming into being all over the surface of the Earth ever since the time of man's first appearance upon it, and yet the fact that no member of the human race has ever seen (or is ever likely to see) such birth, throws even no shade of doubt upon the probability of its occurrence.

What then becomes of the supposed validity of this much respected phrase, *omne vivum ex vivo*? No more requires to be said than that it is an instance of one of those rude and loose inductions, common amongst the uneducated, and in early days even amongst scientific men. As the late Mr. Mill* said:—"The unprompted tendency of the mind is to generalize its experience, provided this points all in one direction; providing no other experience of a conflicting character comes unsought. The notion of seeking it, of experimenting for it, of *interrogating* Nature (to use Bacon's expression) is of much later growth. The observation of nature by uncultivated intellects is purely passive; they accept the facts which present themselves, without taking the trouble of searching for more. . . . But though we have always a propensity to generalize from unvarying experience, we are not always warranted in doing so,

* System of Logic, 6th edit., vol. i., p. 349.

Before we can be at liberty to conclude that something is universally true because we have never known an instance to the contrary, we must have reason to believe that if there were in nature any instances to the contrary, we should have known of them." Now it was only by an utter inattention to this latter all-important requirement that the "past experience of mankind" could ever have appeared to warrant the induction *omne vivum ex vivo*.

As Mr. Mill pointed out,* the proposition, "all swans are white," must have appeared to Europeans, not many years ago, an "unequivocal instance of uniformity in the course of nature." Subsequent experience has shown that they were mistaken, although they and all their predecessors through many centuries had observed nothing to contradict this proposition. "The uniform experience therefore of the inhabitants of the known world, agreeing in a common result, without one known instance of deviation from that result, is not always sufficient to establish a general conclusion."

(b). The exposure of the untruth of certain old and crude doctrines concerning 'spontaneous generation,' many of which date from the earliest times; and the fact that the belief in this mode of generation has been successively driven, with increasing knowledge,

* *Loc. cit.* vol. i., pp. 348, 351.

from higher to lower forms of life, till at the last it is maintained as a mode of origin only for the very lowest and most minute of living things, has been regarded by many (as I have already pointed out) as one of the most weighty arguments against this kind of generation. But this objection, as before shown, is robbed of all its seeming strength when it is said that the modern Evolutionist would only expect to obtain evidence concerning the *de novo* origin of the minutest specks of Living Matter—gradually emerging into the region of the visible and subsequently developing into the most elementary Forms of Life.

Thus the formula, *omne vivum ex vivo*, has even no sufficient *à priori* warrant. It is an induction which has been formed after a partial scrutiny of the facts—one that has been arrived at, not in accordance with the modern methods of experimental inquiry, but by the ancient custom of mere passive observation and enumeration, against which the founder of the Inductive Philosophy so strongly raised his voice.*

So far, then, it would seem that at least as much is to be said in favour of the new as of the old hypothesis, even from a mere *prima facie* considera-

* Named by him "Inductio per enumerationem simplicem, ubi non reperitur instantia contradictoria."

tion of their merits, whilst a closer scrutiny of their respective foundations tells much more in favour of the new hypothesis of Archebiosis; in the first place, because no reason can be shown why the process of life-evolution should have been arrested; and secondly, because if it does occur at the present time, it never could come under the direct observation of anybody, and consequently the general experience of mankind concerning the 'reproduction' of living things, upon which the second hypothesis and the dictum *omne vivum ex vivo* have been founded, would in no way be questioned—the facts would lie altogether outside this experience.*

But a belief in Archebiosis, whether past or present, seems to me necessarily to carry with it a belief in Heterogenesis. So that if Archebiosis be continually taking place, Heterogenesis should be an equally common phenomenon. And even for those who believe that Archebiosis took place in the past though it has now ceased, Heterogenesis would remain as a very possible and even probable process from an

* As I have elsewhere said :—“ Living matter, like crystalline matter, is only *formable* by a synthesis of its elements. As crystals have not the power of self-multiplication, they have only one mode of origin. But because organisms have reproductive powers, the obviousness of these modes of increase has sufficed to cast doubts upon the reality of the independent origin of living units.”—The Beginnings of Life, vol. ii., p. 77.

à priori point of view. Such a process is, in fact, for living matter what Allotropism is for crystalline matter. And it so happens that the evidence in favour of the present occurrence of Heterogenesis is even stronger and much more varied than that in favour of the present occurrence of Archebiosis. Whilst the latter is a strongly warranted inference, the former is a matter of direct observation. So that concerning the present occurrence of Heterogenesis we may say, (1), that it follows as an almost necessary consequence from the physical doctrines of life; (2), that it is a process which admits of daily observation by skilled observers; and (3), that it explains many series of phenomena of the most varied nature, which would otherwise remain quite inexplicable.*

* It is worthy of note, moreover, that it is the recognition of the present occurrence of Heterogenesis which is the still-important necessity. A belief of this kind will carry with it all those important changes in biological doctrine and in medical science which seem to me both necessary and inevitable. The further belief as to the present occurrence of Archebiosis, is an extension of the 'Spontaneous Generation' doctrine, which, though it may be logically demanded and warranted, is one of altogether secondary importance in relation to the changes of doctrine that it would involve. This state of things is the more important, because a belief in Heterogenesis is open to biologists of all shades of opinion. Indeed those biologists who still believe in the existence of a special 'vital principle,' would in all probability only infer the occurrence of Heterogenesis from such experimental facts as would warrant, on the part of the Evolutionist, a belief in the present occurrence of Archebiosis. The Evolutionist, however, is bound to recognize a difference between living organic matter and dead organic matter, which the Vitalist, holding himself aloof from positive scientific

Our present position may perhaps be best illustrated by tabulating in parallel columns a statement of the principal reasons and facts which seem to support the hypothesis of the present occurrence of Archebiosis and Heterogenesis, but which are more or less inexplicable by the hypothesis of Panspermism and an exclusive Life-transmission doctrine. I am compelled to arrange the matter in this apparently one-sided form, because I know of no large classes of facts adverse to the hypothesis of the present continuance of Archebiosis and Heterogenesis.

In support of the present occurrence of Archebiosis.

1. Our belief in the Continuity of natural phenomena seems to require it.

2. The fact that crystalline matter still comes into being, or originates under the majority of those conditions in which its growth occurs.

3. The fact that the microscopical evidence in favour of origination is similar in the case of crystalline and living matter—both appear to arise *de novo*.

Against Panspermism and an exclusive Life-transmission doctrine.

1. The postulation of an infringement of the Continuity of natural phenomena without adequate cause.

2. The impossibility of explaining why living matter, which still grows under the most varied conditions, should have ceased to originate under many of them.

3. This apparent *de novo* origin of specks of living matter is only to be denied by the assumption of their derivation from invisible germs pre-existing in the fluid—and for this assumption there is no independent warrant.

evidence so far as these particular doctrines are concerned, might not feel called upon to admit.—See “The Beginnings of Life,” vol. i., pp. 244—249.

4. Living matter (in the form of the specks above mentioned) does appear quickly and abundantly in situations where the uniformity of natural phenomena entitles us to believe that no living matter (whether visible or invisible) could have pre-existed.

*In support of the present occurrence of Heterogenesis.**

5. Actual observation with the microscope of many phenomena of this kind. (Vol. II., chaps. xvii., xix.—xxii.)

6. The analogy between the phenomena of Heterogenesis and those of Allotropism. (Vol. II., pp. 49—85.)

7. The fact that the presence of certain of the lowest organisms within higher organisms is more or less determinable at will. (Vol. II., pp. 317—345.)

8. Our power of determining the presence of animal or plant-like organisms at will, in vessels containing certain organic infusions. (Vol. II., pp. 209—219, 231—235.)

4. The occurrence of Life-Evolution or Archebiosis in certain experimental flasks is only to be denied by making another assumption (as to the power of resisting heat displayed by living matter), which is not only without scientific warrant, but is in opposition to established facts.

Against Panspermism and an exclusive belief in Homogenesis.

5. The gratuitous assumption that many otherwise good observers should all be deceived—simply because their interpretations contradict the preconceived opinions of the majority of naturalists.

6. The absence of all reason why phenomena of Allotropism should not show themselves with different kinds of living matter, as with simpler molecular compounds.

7. The impossibility of explaining this, except by the disproved assumption that higher organisms are always permeated by innumerable invisible and generally latent germs of such lowest organisms.

8. Our power of watching all the stages by which these animal or plant-like forms are produced, from the "pellicle on hay infusions," and our inability otherwise to account for their first appear-

* As most of these facts have not been referred to in this communication, I subjoin, after each pair of paragraphs, references to the chapters or pages in which the subject has been discussed or referred to in my work, "The Beginnings of Life."

9. The Order observable in the succession of living things which appear in very many organic infusions:—1. Bacteria; 2. Monads and Amœba, or Fungus Germs; 3. Ciliated Infusoria; 4. Rotifers.

10. The wide diffusion and constant association of certain organisms (Rotifers, Sloths, and Nematoids) which multiply by very large and well-known germs.

11. Our power of determining the presence of some of the higher Fungi (Mushrooms), and of Nematoids, at will, in certain organic mixtures.

12. The wide diffusion and numerical abundance of Desmids and Diatoms. (Vol. II., pp. 420, 435—455.)

13. The inconstancy of particular species of lower organisms in the same habitats from year to year; and the sudden appearance of other organisms in enormous numbers in situations where they had not previously existed.

14. The uniform association of certain other organisms with their accustomed matrices, *e.g.*, of particular kinds of Mould with particular organic substances, and the almost invariable association of Euglenæ with Rotifers.

15. The observed relations between size of matrix and grade of

ance as bodies of comparatively large dimensions.

9. No reasonable explanation of this order in accordance with the hypothesis of Homogenesis, and with facts known concerning the distribution of the germs of such organisms. (Vol. II., pp. 297—306, 502—535.)

10. These facts, quite irreconcilable with the ascertained absence of such germs or organisms from the atmosphere, and an exclusive doctrine of Homogenesis. (Vol. II., pp. 535—538.)

11. These results seemingly inexplicable from the point of view of Panspermism and Homogenesis. (Vol. II., pp. 433, 537 *note.*)

12. Inexplicable by what we know concerning their absence from the atmosphere and their modes of multiplication.

13. Facts either incapable of being explained by, or directly opposed to, the hypothesis of Homogenesis and Panspermism. (Vol. II., pp. 535, 454.)

14. The uniformity of such associations quite incompatible with known facts concerning the prevalence of germs of Fungi, Euglenæ and Rotifers in the air. (Vol. II., pp. 302 *note*; 535, 508 *note.*)

15. The impossibility of explaining away all these observa-

organization attained by the allotropic product, where a vegetal matrix is transformed into an animal organism.

16. The interchangeability of those organic forms which are derivable from originally similar vegetal matrices.

17. The extreme variability of the simplest representatives of the Vegetal Kingdom. (Vol. II., pp. 150—165, and Appendix D.)

18. The present existence amongst such lower organisms of all the gradations by which a purely sexual process of generation is gradually evolved. (Vol. II., p. 552, and *Table*.)

19. The explanation of the mysterious phenomena of "Alternate Generation"—and especially of the anomalous exceptions in the case of *Medusæ* with large eggs.

20. The existence of multitudes of almost structureless organisms at the present day. (Vol. II., pp. 605—622.)

tions, by assuming them to be the faulty inferences of hasty or unskilled observers. (Vol. II., chaps. xx.—xxii.)

16. Difficulties similar to those last-mentioned. (Vol. II., pp. 491—499.)

17. The impossibility of denying the united but independent testimony of so many observers, although such variability seems irreconcilable with the notion of these organisms being direct continuations of an extremely ancient similar matter which has resisted change for ages.

18. The impossibility of finding any explanation of this fact which shall be consistent with an exclusive doctrine of Life-transmission and Homogenesis.

19. The absence of all explanation, not only of the phenomena but of the exceptions thereto. (Vol. II., pp. 560—571.)

20. The impossibility of believing that living matter could persist with so little change for 100,000,000 years or upwards, whilst other portions of the same matter have progressively developed into all the varied forms of Life that have appeared upon this globe.

Taken as a whole, the amount of evidence, both deductive and inductive, seems, to say the least,

very decidedly to preponderate in favour of the present occurrence of Archebiosis and Heterogenesis. Both fact and reason appear to be notably insufficient on the side of the counter hypothesis. So that a careful consideration of the respective merits of the two views—looked at merely as hypotheses—seems to show in a very unmistakable manner which is most worthy of our acceptance.

Whilst the hypothesis of Panspermism is based upon an illegitimate belief, is at variance with many uniformities of nature, and is wholly incapable of embracing the required facts, that of Archebiosis and Heterogenesis is legitimate in its foundation, is not at all at variance with natural uniformity, and is capable already of explaining a very wide circle of facts pertaining to the past and present history of our globe.

But these are the very tests by which we are accustomed to probe a new hypothesis, with the view of ascertaining its probable truth or falsity. What Mr. Justice Grove* said in 1866, when speaking of the Darwinian hypothesis, is now just as applicable concerning the hypothesis of the present occurrence of Archebiosis and Heterogenesis. He said:—
“The fair question is, Does the newly proposed view remove more difficulties, require fewer assumptions,

* Presidential Address, in Report of British Association, 1866, p. xxviii.

and present more consistency with observed facts than that which it seeks to supersede? if so, the philosopher will adopt it, and the world will follow the philosopher—after many days.”

But if, even when looked at merely as hypotheses, that of Archebiosis with Heterogenesis seems so likely to drive the old doctrine out of the field, we instinctively look around us for one of those “crucial instances” which may serve, as has been so often the case in the history of science, finally to decide between the contending claims of old and new views.

Now a crucial instance of this kind (or opportunity of employing the all-decisive experimental “Method of Difference”), does very fortunately lie within our reach, and has already been referred to. Its true value, however, may be now more clearly seized by the reader; so that for this reason, and because all past discussions on the question of the Origin of Life have shown that this is the part of the subject whose scientific basis is least understood or most persistently disregarded, I venture, even though with some slight reiteration to make a few concluding remarks on this crucial test—whose decision the opponents of Archebiosis attempt to set aside by a mixture of what appears to me to be, illogical arguments and groundless assumptions.

There are only two possible modes of accounting for the fact that "certain of the most minute living things are known to appear in some fluids independently of pre-existing visible germs." If therefore it can be shown that living though invisible germs did not pre-exist in certain fluids in which such minute living things subsequently make their appearance as usual, we thereby prove that in such instances they must have owed their appearance to the other process—viz., to Archebiosis. Nothing can be plainer than this; if a given event must be occasioned by one or other of two causes, and if in certain instances we can show that the event followed, notwithstanding the absence of one of these causes, then the event must have been occasioned by the other cause. An experiment of this nature is named a 'crucial instance' or *experimentum crucis*.

Let us look then into the nature of the crucial instance which lies at our disposal in this emergency.

If we wish to ascertain whether living matter exists, or rather if we wish to make sure that living matter does not exist in any given fluid, the only course open to us is to submit the fluid to the influence of agencies which we have previously ascertained to be capable of 'killing' such matter—that is to say, of putting an end to the combination of properties the existence of which formerly gave us

the right to call it 'living' matter. As scientific men, we distinguish one body or class of bodies from another by the fact of their possessing certain distinctive attributes or properties—and happily, in this respect, common usage does not differ from scientific usage. No one would think of calling any metal 'gold' unless it possessed the combination of properties peculiar to gold; no physicist would call a body a 'magnet' unless he could show that it possessed magnetic properties; no chemist would call a fluid 'alcohol' unless he could show that such fluid possessed the known properties of alcohol, and similarly no biologist would call a body 'living' unless it possessed those attributes or properties which we are accustomed to regard as fundamental or characteristic. Again, no man of science would dream of crediting living matter in an imaginary case with properties different from those which he has on all previous occasions found it display—if he had reason to believe that it existed in any given medium, he would of course look for it on the basis of its known properties, just as a chemist would search for gold in any solution thought to contain it, by having regard only to its accustomed or known properties. The man of science necessarily starts from and assumes the truth of the 'uniformity' of natural phenomena at every step—to do otherwise,

in fact, would be a mockery of science. Assumed invisible germs, therefore, if postulated at all by the man of science, must be postulated to exist with properties similar to those of known germs, since they would all alike be composed of protoplasm, or simplest living matter. And, again, if he has satisfied himself that, in all known instances where trial has been made, the sudden exposure of living matter to a moist heat of 140° F. has proved destructive to it—that is, has destroyed the combination of attributes which previously entitled it to be called ‘living’—he can only conclude that this is a general truth which he may take for granted in the future.

Being, from the Evolutionist’s point of view, altogether a question of physical or molecular property, the resistance of protoplasm or living matter to heat stands upon the same level as that of the degree of heat necessary to destroy or ‘kill’ one of the simpler chemical compounds; or the degree of heat necessary to cause ebullition in a given fluid. These are all cases in which, as Mr. Mill said,* “we reckon with the most unfailing confidence upon uniformity,” so that “when a chemist announces the existence of a newly-discovered substance, if we confide in his accuracy, we feel assured that the conclusions he has arrived at will hold universally, though the

* System of Logic, 6th edit., vol. i., p. 351.

induction be founded but on a single instance." Now here, far from being based upon a single instance, the fact that very many different kinds of living matter are killed by a temperature of 140° F., rests upon the repeatedly recorded observations of several independent investigators—upon the observations of Pouchet, Liebig, Cantoni, Hoppe-Seyler, Kühne, Max Schultze, myself, and others.

But as it is the fact that living matter is killed at 140° F., and as it is also true that certain fluids heated to much higher temperatures (to 212° F. and upwards) and subsequently exposed to certain conditions free from all possibility of contamination with living matter, will shortly swarm with the living things whose mode of origin we desire to learn, the man of science is compelled to conclude that such living Organisms must have originated independently of living germs, and, therefore, after the manner of Crystals. Here then is our 'crucial instance.'

Thus by this simple resort to the 'Method of Difference,' we are enabled to solve our problem, and finally decide between two rival hypotheses. The occurrence of Archebiosis is, therefore, established as a natural phenomenon, and like other natural phenomena, we are entitled to believe not only that it will recur whenever the conditions are similar or otherwise suitable, but that it has been in operation through

the whole of that long vista of bygone ages during which the surface of our Earth has been occupied by living things.

Having been compelled by experimental evidence and observation to believe in the present occurrence of Archebiosis and Heterogenesis, I will now strive to lay before the reader some of the principal and most interesting consequences resulting from such a recognition of natural uniformity. This I will endeavour to do in as short a space as is compatible with my object of giving the intelligent reader some notion of the general scope and nature of the biological doctrines which recent experiments and observations seem to force upon us. A detailed statement of the facts and reasons upon which the following views are based, may be found by those who desire it elsewhere.*

Living matter is constantly being formed *de novo* in obedience to the same laws and tendencies as those which determine all the more simple chemical combinations. The qualities which we summarize under the word 'life' are in all cases due to the combined molecular actions and properties of the aggregate that displays them, just as the properties

* The Beginnings of Life.

which we include under the word 'magnetism' are due to particular modes of collocation that have been assumed by the molecules of iron.

Living matter is especially characterized by the complexity of its molecules and their state of continual intestine movement. This peculiarity, as well as other related qualities, make the simplest aggregates of such matter especially prone to undergo those secondary structural re-arrangements which all plastic and homogeneous masses of matter are liable to exhibit. And although in the case of living matter these re-arrangements manifest themselves by producing what we call 'organization,' still the forms and structures which many of the lowest organisms tend to assume are entirely referrible to the polarity of their molecules—just as the forms of crystals are the results of like, though simpler, polarities.

And, speaking generally, the complexity of organization attainable by the lower animal forms gradually tends to increase as the masses of matter from which new forms are to develop increase in size: owing apparently to the multiplication of effects that may be induced by the production of several series of molecular re-arrangements within the larger aggregates. The changes progress, however, in each case till a condition of moving equilibrium is established

between the sum total of molecular actions taking place within the living aggregate and the forces of its environment.

The power of undergoing spontaneous division (fission or gemmation), which is manifested by living matter, and upon which all the phenomena of 'reproduction' depend, is apparently one of its most fundamental properties, though it is in itself a result of that molecular mobility and complexity to which we have previously referred.

And it is this same molecular mobility which makes an aggregate of living matter, in the form of a simple organism, very prone to undergo changes in its intimate constitution—either 'spontaneously,' or under the incidence of a known change in external forces. Some new conditions may not visibly affect it, others may cause its 'death,' whilst others again may affect it only to such an extent as to bring about some modification of its molecular constitution, which, by reason of an altered polarity, entails a more or less marked transformation of form and structure (Heterogenesis).

Thus the marvellous convertibility of lower organisms, their ability to undergo self-multiplication, and their tendency to become (under favourable conditions) more complexly organized, are all necessary consequences of those physical doctrines concerning

'life,' the truth of which is deemed to have been established by previously-recorded experiments.

These myriads of lowest forms of life, multiplying only by processes of fission and gemmation, constitute an inextricably-tangled plexus of more or less convertible animal or vegetal forms (corresponding pretty closely with the Protista of Professor Haeckel) which, though often reappearing, are for the most part evanescent and transitory states, either of comparatively new-born living matter, or of portions of matter which have become individualized by heterogenetic processes occurring in the substance of the higher forms of life. But, howsoever derived, such forms constitute a vast assemblage of 'Ephemeromorphs' amongst which Heterogenesis occurs almost as frequently as Homogenesis.*

Gradually, however, the first traces of those processes of 'conjugation' and of internal gemmation begin to manifest themselves, which subsequently become perfected into 'sexual' modes of reproduction.

And when animal and vegetal organisms manifesting that cyclical homogenesis known as 'alternate generation,' appear upon the scene, and with them those simpler allies (formed from large germs), which

* The song of the Ephemeromorphs might be, in the words of Ovid,
"Corpora vertuntur; nec quod fuimus, sumusve,
Cras erimus."

undergo a direct process of development, we first begin to obtain such regularly-recurring and definite assemblages of animal and vegetal forms as are usually grouped under the name of 'species.'

Until such assemblages of repeating individuals make their appearance—that is, until Homogenesis becomes the rule—the 'laws of Heredity' can scarcely be said to come into operation. Hence, the complexly-interrelated individuals constituting this vast underlying plexus of Infusorial and Cryptogamic life must remain wholly uninfluenced, so far as their form and structure are concerned, by what Mr. Darwin has termed 'Natural Selection.' Such vegetal and animal organisms, however, gradually tend to become more and more complex. An ascending development takes place; and as this occurs, the causes which originally sufficed to determine their form and structure, and which for a time continue to induce comparatively rapid and marked deviations, become less and less capable of bringing about structural modifications during the life of the individual. Changes which are now less rapidly accomplished have to be perfected in a succession of individuals. Thus is the operation initiated of those subtle and more slowly modifying agencies which have been so admirably illustrated by Mr. Darwin.

But amongst specific forms of slight complexity

the influence of Natural Selection as a modifying agent is probably much less important than it is amongst more active and complex animal forms : and in all cases its action in producing change may be assisted by 'spontaneous' internal changes in the molecular activity of certain parts of the organism, or by other internal changes more obviously induced by modifications in the sum total of 'external conditions' acting upon the organism.

Each cause of specific modification, however, whether acting alone or in concert with one of the other producers of internal change, can only come into play in subordination to the ever potent laws of 'organic polarity,' by which a multiplication of effects is apt to be induced.

In addition to these conclusions concerning the present order of events, we seem warranted in drawing the following speculative deductions concerning the past:—

An elemental origin of living matter similar to that which takes place at the present day, and, in addition, all the related heterogenetic phenomena, have probably been taking place on the surface of our globe since the far remote period when such matter was first engendered.

The countless myriads of living units which have been evolved in different ages of the world's history

must, in each period, have given rise to innumerable multitudes of what have been called 'trees of life,' branching out into animal and vegetal forms of almost inconceivable variety. Myriads of these 'trees,' including all their branches and innumerable ramifications, may have wholly died out during the many vicissitudes of the earth's surface and the long lapse of ever fruitful ages; though the descendants or ultimate ramifications of some of such trees—dating back to quite different and perhaps far-distant epochs—may still survive. How far, however, the roots of any of the 'trees' from which the existing higher forms of life are derived, may have extended back into the depths of geologic time, we are utterly unable to estimate.

Throughout all this life-evolving period of the history of our globe, the progress of 'organization' seems to have been essentially similar. And that this should be so, seems readily explicable by the consideration that living things, both as regards their origin and their subsequent differentiation or development, are the immediate products of ever-acting natural laws or material properties. These properties should act therefore now as they have ever done, and so continue to produce almost similar effects.

The lower the forms of life—that is, the nearer they are to their source—the greater seems to have

been the similarity amongst those which have been produced in different ages—just as the lowest forms are now similar in all regions of the earth. On the other hand, the longer any particular ‘tree of life’ has lived (of which there have been countless multitudes born in each age), the wider may be the divergence of form presented by the ultimate outgrowths of any two of them, or of outgrowths of similar rank produced from trees which have developed during different ages—especially when the assemblages of organisms constituting one of these ideal trees, have lived under the influence of any unusual set of telluric conditions.

The ‘vertebrate’ grade of organization *may* have been many times attained by ultimate branches of different ‘trees of life.’ But how long or when the particular ‘tree of life,’ from one of the branches of which Man was developed, appeared upon the earth, it is quite impossible to say.

II.

ON THE TEMPERATURE AT WHICH *BACTERIA*,
VIBRIONES, AND THEIR SUPPOSED GERMS
ARE KILLED WHEN IMMERSED IN FLUIDS
OR EXPOSED TO HEAT IN A MOIST STATE.

FOR more reasons than one we may, perhaps, now look back with advantage upon the friendly controversy carried on rather more than a century ago between the learned and generous Abbé Spallanzani and our no less distinguished countryman Turberville Needham. Writing concerning his own relation to Needham, the Abbé said*, "I wish to deserve his esteem whilst combating his opinion"; and in accordance with this sentiment, we find him treating his adversary's views with great respect, and at the same time repudiating much of the empty and idle criticism in which so many of Needham's contemporaries indulged with regard to his work. This criticism, Spallanzani says†, "Without looking into details, contented itself by throwing doubt upon some of the facts, and by explaining after its own fashion others whose possibility it was willing to admit." He moreover warmly reprobated the ignorant and disrespectful

* *Nouvelles Recherches sur les Découvertes Microscopiques et la Génération des Corps Organisés, &c.* London and Paris, 1769, vol. i., p. 69.

† *Loc. cit.*, p. 9.

statements made by an anonymous writer who had shown himself little worthy of being heard upon the subjects in dispute. Spallanzani on this occasion very wisely said* :—"When it is a question concerning observations and experiments, it is necessary to have repeated them with much circumspection before venturing to pronounce that they are doubtful or untrustworthy. He who will allow himself to speak of them with contempt, and who can only attempt to refute them with writings composed by the glimmer derived from a treacherous lamp, will not find himself in a condition to retain the esteem of learned men." The anonymous writer (in his 'Lettres à un Américain'), to whom Spallanzani referred, had gone so far as to doubt the statements of Needham as to the constant appearance of organisms in infusions which had been previously boiled, and also intimated that even if they were to be found, it was only because they had been enabled to resist the destructive influence of the boiling fluid. This latter assertion was emphatically denied by Spallanzani, his denial being based upon a most extensive series of experiments with eggs in great variety and with seeds of all degrees of hardness; these were all found to be killed by a very short contact with boiling water. Spallanzani had thoroughly satisfied

* *Loc. cit.*, p. 114.

himself that even very thick-coated seeds could not resist this destructive agent ; whilst he thought that the idea, entertained by some, of the eggs of the lowest infusoria being protected from the injurious influence of the boiling water by reason of their extreme minuteness, was a supposition so improbable as scarcely to deserve serious consideration. Such a notion was, he thought, wholly opposed to what was known concerning the transmission of heat. Whilst, therefore, the opinion of those who believe that eggs have the power of resisting the destructive influence of boiling water could be fully refuted, Spallanzani thought it by no means followed that the infusoria which always, after a very short time, appeared in boiling infusions had arisen independently of the existence of eggs. The infusions being freely exposed to the air, it was very possible that this air had introduced eggs into the fluids, which by their development had given birth to the infusoria.*

After the lapse of a century it has at last been clearly shown that this supposition of aërial con-

* A few pages further on this view is thus shortly expressed :—“ Il est évident que tous les tentatives faites avec le feu, peuvent bien servir à prouver que les animaux microscopiques ne naissent point des œufs que l'on supposait exister dans les infusions avant qu'on leur fit sentir le feu ; mais cela n'empêche pas qu'ils n'aient pu être formés de ceux qui auront été portés dans les vases après l'ébullition.”

tamination advanced by Spallanzani (warrantable and natural as it was at the time) is one which, in the great majority of cases, is devoid of all foundation in fact, so far as concerns the organisms essentially associated with processes of putrefaction, viz. *Bacteria* and *Vibriones*. The means of proving this statement based upon independent observations made by Professor Burdon Sanderson and myself, were recently submitted to the consideration of the Royal Society:† Before the reading of this communication I was under the impression that almost everyone of those who had taken part in the controversies which had been carried on both here and abroad concerning the Origin of Life were prepared to admit, as Spallanzani had done, that the eggs or germs of such organisms as appear in infusions were unable to survive when the infusions containing them were raised to the temperature at which water boils. This impression was produced in part by the explicit statements on this subject that had been made by very many biologists, and also in part by a comparatively recent and authoritative confirmation which this view as to the destructive effects of boiling infusions upon *Bacteria* had received. Little more than two years ago Professor Huxley, as President of the British

* See Proceedings of Royal Society, No. 141, 1873, p. 129.

Association for the Advancement of Science recorded experiments in his Inaugural Address which were obviously based upon this belief as a starting-point; and subsequently, in one of the Sectional Meetings, after referring to some of my experiments, and to the fact that all unmistakeably vital movements ceased after *Bacteria* had been boiled, Professor Huxley added* :—"I cannot be certain about other persons, but I am of opinion that observers who have supposed that they have found *Bacteria* surviving after boiling have made the mistake which I should have done at one time, and, in fact, have confused the Brownian movements with *true living* movements." Some eminent biologists do not now (in reference to the experiments cited in my last communication) suggest that the organisms found in the infusions were dead and had been there before the fluids were boiled: they express doubts concerning that which seems formerly to have been regarded as established, and now wish for evidence to show that the germs of *Bacteria* and *Vibriones* are killed in a boiling infusion of hay or turnip, as they have been proved to be in 'Pasteur's Solution' and in solutions containing ammoniac tartrate and sodic phosphate.

With the view of removing this last source of

* See Report in Quart. Journ. of Microscop. Science, Oct. 1870.

doubt more effectually, and also of refuting the unwarrantable conclusion* of M. Pasteur, to the effect that the germs of *Bacteria* and *Vibriones* are not killed in neutral or slightly alkaline fluids at a temperature of 212° F., I almost immediately after the reading of my last communication commenced a fresh series of experiments.

Nearly two years ago, in my 'Modes of Origin of Lowest Organisms,' I brought forward evidence to show that *Bacteria*, *Vibriones*, and their supposed germs are killed at a temperature of 140° F. (60° C.) in neutral or very faintly acid solutions containing ammoniac tartrate and sodic phosphate, and also evidence tending to show that these living units were killed in neutral infusions of hay and in acid infusions of turnip at the same temperature.

The crucial evidence adduced concerning the degree of heat destructive to *Bacteria*, *Vibriones*, and their germs, in the saline solution, was of this nature. The solution had been shown to be incapable of engendering *Bacteria* and *Vibriones* (under all ordinary conditions) after it had been boiled, although it still continued capable of supporting the life and

* Reasons for this opinion have been fully set forth in "The Beginnings of Life," vol. i., pp. 374 *et seq.*; or the discriminating reader may at once find my justification for this expression by reading pp. 58—66, of M. Pasteur's memoir in "Ann. de Chim. et de Physique," 1862.

encouraging the rapid multiplication of any of these organisms which were purposely added to it. Some of this boiled solution therefore was introduced into flasks previously washed with boiling water; and when the fluids had sufficiently cooled, that of each flask was inoculated with living *Bacteria* and *Vibriones*—in the proportion of one drop of a fluid quite turbid with these organisms to one fluid ounce of the clear saline solution.* These mixtures containing an abundance of living organisms were then heated to various temperatures, ranging from 122° F. (50° C.) to 167° F. (75° C.); and it was invariably found that those which had been heated to 122° or 131° F. became quite turbid in about two days, whilst those which had been raised to 140° F., or upwards, as invariably remained clear and unaltered. The turbidity in the first series having been ascertained to be due to the enormous multiplication of *Bacteria* and *Vibriones*, and it being a well-established fact that such organisms when undoubtedly living always rapidly multiply in these fluids, the conclusion seemed almost inevitable that the organisms and their germs must have been killed in the flasks which were briefly subjected to the temperature of 140° F. How else are we to

* Fuller details concerning these experiments may be found in the little work already mentioned at pp. 51—56, and also in "The Beginnings of Life," vol. i., pp. 325—332.

account for the fact that these fluids remained quite unaltered although living organisms were added to them in the same proportion as they had been to those less-heated fluids which had so rapidly become turbid? Even if there does remain the mere possibility that the organisms and their supposed germs had not actually been killed, they were certainly so far damaged as to be unable to manifest any vital characteristics. The heat had, at all events, deprived them of their powers of growth and multiplication; and these gone, so little of what we are accustomed to call 'life' could remain, that practically they might well be considered dead. And, as I shall subsequently show, the production of this potential death by the temperature of 140°F. enables us to draw just the same conclusions from other experiments, as if such a temperature had produced a demonstrably actual death.* Seeing also that these saline solutions were inoculated with a fluid in which *Bacteria* and *Vibriones* were multiplying rapidly, we had a right to infer that they were multiplying in their accustomed manner, "as much by the known method of fission, as by any unknown and assumed method of reproduction." So that, as I at the time said,† "These experiments seem to show, therefore, that even if *Bacteria* do multiply by means of invis-

* See p. 95.

† Modes of Origin of Lowest Organisms, 1871, p. 60.

ible gemmules, as well as by the known process of fission, such invisible particles possess no higher power of resisting the destructive influence of heat than the parent *Bacteria* themselves possess."

This is, in fact, by far the most satisfactory kind of evidence that can be produced concerning the powers of resisting heat enjoyed by *Bacteria* and *Vibriones*, because it also fully meets the hypothesis as to their possible multiplication by invisible gemmules possessed of a greater power of resisting heat, and because no mere inspection by the microscope of dead *Bacteria* can entitle us positively to affirm that they are dead, even though all characteristically vital or 'true living' movements may be absent.

Facts of a very similar nature were mentioned in the same work strongly tending to show that *Bacteria* and *Vibriones* are also killed at the same temperature in other fluids, such as infusions of hay or turnip. These facts were referred to in the following statement* :—"Thus, if on the same slip, though under different covering-glasses, specimens of a hay-infusion turbid with *Bacteria* are mounted, (*a*) without being heated, (*b*) after the fluid has been raised to 122° F. for ten minutes, and (*c*) after the fluid has been heated to 140° F. for ten minutes, it will be found that in the course of a few days the *Bacteria* under *a* and *b* have notably increased in quantity, whilst those under *c* do

* Modes of Origin of Lowest Organisms, 1871, p. 60.

not become more numerous, however long the slide is kept. Facts of the same kind are observable if a turnip-infusion containing living *Bacteria* is experimented with; and the phenomena are in no way different if a solution of ammoniac tartrate and sodic phosphate (containing *Bacteria*) be employed instead of one of these vegetable infusions. The multiplication of the *Bacteria* beneath the covering-glass, when it occurs, is soon rendered obvious, even to the naked eye, by the increasing cloudiness of the film."

The facts just cited concerning the behaviour of thin films of turbid infusions which had been heated to different temperatures gave me the clue as to the proper direction of future work. It would seem that when mounted in the manner described, such thin films of infusion continue capable of supporting and favouring the multiplication of any already existing *Bacteria* and *Vibriones*, although under such conditions no new birth of living particles appears to take place even in these fluids. The question then arose as to whether, by subjecting larger quantities of the same infusions to any particular sets of conditions, we could ensure that they also should continue to manifest the same properties; because if so, it would be almost as easy to determine the death-point of *Bacteria* and *Vibriones* when exposed to heat in these infusions, as it had been to determine it for the saline solutions already mentioned.

It was pointed out by Gruithuisen early in the present century, that many infusions, otherwise very productive, ceased to be so when they were poured into a glass vessel whilst boiling, and when this was filled so that the tightly-fitting stopper touched the fluid. Having myself proved the truth of this assertion for hay-infusion, it seemed likely that, by having recourse to a method of this kind, I should be able to lower the virtues of boiled hay- and turnip-infusions, to the level of those possessed by the boiled saline solution with which I had previously experimented—that is, to reduce them to a state in which, whilst they appear (under these conditions) quite unable of themselves to engender *Bacteria*, *Vibriones*, they continue well capable of favouring the rapid multiplication of such organisms.

This was found to be the case; and I have accordingly performed upwards of one hundred experiments with inoculated portions of these two infusions raised to different temperatures. The mode in which the experiments were conducted was as follows:—

Infusions of hay and turnip of slightly different strengths were employed. These infusions, having been first loosely strained through muslin, were boiled for about ten or fifteen minutes, and then whilst boiling strained through ordinary Swedish filtering-paper into a glass beaker which had pre-

viously been well rinsed with boiling water. A number of glass bottles or tubes were also prepared, which, together with their stoppers or corks, had been boiled in ordinary tap water for a few minutes.* They were taken out full of the boiling fluid; and the stoppers or corks being at once inserted, the vessels and their contents were set aside to cool. When the filtered infusion of hay or turnip had been rapidly cooled down to about 110° F. (by letting the beaker containing it stand in a large basin of cold water), it was inoculated with some of a turbid infusion of hay swarming with active *Bacteria* and *Vibriones*—in the proportion of one drop of the turbid fluid to each fluid ounce of the now clear filtered infusion.† The beaker was then placed upon a sand-bath, and its contained fluid (in which a thermometer was immersed) gradually raised to the required temperature. The fluid was maintained at the same temperature for five minutes by alternately raising the beaker from and replacing it upon the sand-bath. The bottles to be used were then one by one uncorked, emptied,

* The vessels employed have varied in capacity from two drachms to four ounces; some have been provided with glass stoppers and others with very tightly fitting corks; and the latter I find have answered quite as well as the former. On the whole I have found tightly corked one-ounce phials to be about the most convenient vessels to employ in these inoculation experiments.

† It was found desirable to filter the infusions after they had been boiled, because the boiling generally somewhat impaired their clearness.

and refilled to the brim with the heated inoculated fluid.* The corks or stoppers were at once very tightly pressed down so as to leave no air between them and the surface of the fluids. The beaker was then replaced upon the sand-bath and the gas turned on more fully, in order that the experimental fluid might be rapidly raised to a temperature 9° F. (5° C.) higher than it had been before. After five minutes' exposure to this temperature other bottles were filled in the same manner, and so on for the various temperatures the influence of which it was desired to test.

Thus prepared, the bottles and tubes have been exposed during the day to a temperature ranging from 65° F to 75° F. And generally one had not to wait long in order to ascertain what the results were to be. In some cases, if the contents of the vessels were to become turbid, this was more or less manifest after an interval of forty-eight hours; in other cases, however, the turbidity manifested itself three or more days later: the reason of this difference will be fully discussed in a subsequent communication.

For the sake of simplicity and brevity, the necessary particulars concerning the 102 experiments have been embodied in the following Table:—

* At this stage, of course, *very great care* is needed in order to avoid all chance of accidental contamination either with living organisms or with unheated fragments or particles of organic matter.

Inoculation Experiments made with the view of ascertaining the Temperatures at which *Bacteria*, *Vibriones*, and their supposed Germs are killed in Organic Infusions.

NEUTRAL HAY-INFUSION.

Temp. to which exposed.	Number of Experiments made.	Date of Turbidity, if any.	Results at Expiration of the 8th day.
122° F. (50° C.) 131° F.	1 7	24 hours. 48 hours.	Turbid. All turbid.
140° F. 149° F.	9 4	{ 1 in 48 hours. 6 in 60 hours. 1 in 3 days. 1 in 8 days. 2 in 5 days. 1 in 8 days }	All turbid. {Three turbid. One clear.
158° F. 167° F. 176° F. (80° C.)	15 4 12	All clear. All clear. All clear.

ACID TURNIP-INFUSION.

Temp. to which exposed.	Number of Experiments made.	Date of Turbidity, if any.	Results at Expiration of the 8th day.
122° F. 131° F.	... 7	{ 5 in 24 hours. 2 in 48 hours. }	... All turbid.
140° F. 149° F.	12 10	{ 6 in 40 hours. 4 in 3 days. 2 in 4 days. 1 in 3 days. 3 in 5 days. 1 in 7 days. 2 in 8 days. }	All turbid. {Seven turbid. Three clear.
158° F. 167° F. 176° F.	17 4	All clear. All clear. ...

The experimental results above tabulated seem naturally divisible into three groups. Thus, when heated only to 131° F., all the infusions became turbid within two days, just as the inoculated saline solutions had done.* Heated to 158° F. all the inoculated organic infusions remained clear, as had been the case with the saline solutions in my previous experiments when heated to 140° F. There remains, therefore, an intermediate heat zone (ranging from a little below 140° to a little below 158° F.) after an exposure to which the inoculated organic infusions are apt to become more slowly turbid, although inoculated saline solutions raised to the same temperatures invariably remain unaltered. The full explanation of these apparent anomalies I propose to make the subject of a future communication to the Royal Society; meanwhile we may quite safely conclude that *Bacteria*, *Vibriones*, and their supposed germs are either actually killed or else completely deprived of their powers of multiplication after a brief exposure to the temperature of 158° F. (70° C).

This evidence now in our possession as to the limits of 'vital resistance' to heat displayed by *Bacteria*, *Vibriones*, and their supposed germs in neutral saline solutions, and in neutral or acid organic infusions, is

* In the experiments already referred to, p. 85.

most pertinent and valuable when considered in relation to that supplied by other sets of experiments bearing upon the all-important problem of the Origin of Life. These latter experiments alone may possibly leave doubt in many minds; but the more thoroughly they are considered in relation to the evidence brought forward in this communication, the more fully, I venture to think, will every lingering doubt as to the proper conclusion to be arrived at be dispelled.

Thus we now know that boiled turnip or hay-infusions exposed to ordinary air, exposed to filtered air, to calcined air, or shut off altogether from contact with air, are more or less prone to swarm with *Bacteria* and *Vibriones* in the course of from two to six days; but, placed under slightly different conditions such as were employed in the inoculation experiments above quoted, although infusions of the same nature do not undergo 'spontaneous' putrefactive changes, yet when living *Bacteria* and *Vibriones* are added, and not subsequently heated, putrefaction *invariably* takes place and the fluids thus situated rapidly become turbid. There is therefore nothing in the conditions themselves tending to hinder the process of putrefaction, so long as living units are there to initiate it. Our experiments now show that as long as the added *Bacteria*, *Vibriones*, and their supposed germs are subjected to a heat not exceeding 131° F. (55° C.),

putrefaction invariably occurs within two days; whilst, on the contrary, whenever they are subjected to a temperature of 158° F. (70° C.) putrefaction does not occur. To what can this difference be due, except to the fact that the previously living organisms, which, when living, always excite putrefaction, have been killed by the temperature of 158° F.? It would be of no avail to suppose that the absence of putrefaction in these latter cases is due to the fact that a heat of 158° F., instead of killing the organisms and their germs, merely annuls their powers of reproduction, because in the other series of experiments (with which these have to be compared), where similar fluids are exposed to ordinary or purified air, or are shut off from the influence of air altogether, the most active putrefaction and multiplication of organisms takes place in two, three, or four days, in spite of the much more potent heat of 212° F. to which any pre-existing germs or organisms must have been subjected. The supposition, therefore, that the *Bacteria*, *Vibriones*, and their germs were not killed in our inoculation experiments at the temperature of 158° F., but were merely deprived of their powers of reproduction, would be no gain to those who desire to stave off the admission that *Bacteria* and *Vibriones* can be proved to arise *de novo* in certain cases. Let us assume this (which is indisputably proved by these inoculation experi-

ments), viz. that an exposure to a temperature of 158° (70° C.) for five minutes deprives *Bacteria*, *Vibriones*, and their germs of their usual powers of growth and reproduction—that is, that it reduces them to a state of potential, if not necessarily to one of actual death. What end would be served by such a reservation? The impending conclusion could not be staved off by means of it. The explanation of what occurs in the other set of experiments, where the much more potent heat of 212° F. is employed, still would not be possible without having recourse to the supposition of a *de novo* origination of living units, so long as those which may have preexisted in the flasks could be proved to have been reduced to such a state of potential death. It would be preposterous, and contrary to the whole order of Nature, to assume that the vastly increased destructive influence of a heat of 212° F. had restored vital properties which a lesser amount (158° F.) of the same influence had completely annulled.

The evidence supplied by these different series of experiments, in whichever way it is regarded, as it seems to me, absolutely compels the logical reasoner to conclude that the swarms of living organisms which so often make their appearance in boiled infusions treated in one or other of the various modes already proved to be either destructive or exclusive of preexisting living things are the products of a

new brood of 'living' particles, which, in the absence of any coexisting living organisms, must have taken origin in the fluid itself. For this mode of origin of living units, so long spoken of and repudiated as 'spontaneous generation,' I have proposed the new term Archebiosis.

III.

FURTHER OBSERVATIONS ON THE TEMPERATURE AT WHICH *BACTERIA VIBRIONES*, AND THEIR SUPPOSED GERMS ARE KILLED WHEN EXPOSED TO HEAT IN A MOIST STATE; AND ON THE CAUSES OF PUTREFACTION AND FERMENTATION.

WHILST a heat of 140° F. (60° C.) appears to be destructive to *Bacteria*, *Vibriones*, and their supposed germs in a neutral saline solution, a heat of 149° or of 158° F. is often necessary to prevent the occurrence of putrefaction in the inoculated fluids when specimens of organic infusions are employed. What is the reason of this difference? Is it owing to the fact that living organisms are enabled to withstand the destructive influence of heat better in such fluids than when immersed in neutral saline solutions? At first sight it might seem that this was the conclusion to be drawn. We must not, however, rest satisfied with mere superficial considerations.

The problem is an interesting one; yet it should be clearly understood that its solution, whatever it may be, cannot in the least affect the validity of the conclusion arrived at in my last paper, viz. that living matter is certainly capable of arising *de novo*. We were enabled to arrive at the con-

clusion above mentioned regarding Archebiosis by starting with the undoubted fact that a heat of 158° F. reduces to a state of potential death all the *Bacteria*, *Vibriones*, and their supposed germs which an organic infusion may contain. The inquiry upon which I now propose to enter, therefore, touching the degree of heat *below this point* which may suffice to kill such organisms and their supposed germs in an organic infusion, and touching the cause of the delayed putrefaction apt to take place in inoculated organic infusions which have been heated to temperatures above 140° and below 158° F., is one lying altogether outside the chain of fact and inference by which the occurrence of Archebiosis is proved.

It seems to me that the solution of the problems which form the subject of the present communication can only be safely attempted by keeping constantly before our minds two main considerations:—

Thus, in the experiments whose results it is now our object to endeavour to explain, the fluids have been inoculated with a compound consisting partly (*a*) of living units, and partly (*b*) of a drop of a solution of organic matter in a state of molecular change; so that in many cases where putrefaction has been initiated after the inoculating

compound has been heated to certain temperatures, there is the possibility that this process of putrefaction may have been induced (in spite of the death of the organisms and their germs) owing to the influence of *b*, the dissolved organic matter of the inoculating compound; that is to say, the heat to which the mixture has been exposed may have been adequate to kill all the living units entering into the inoculating compound, although it may not have been sufficient to prevent its not-living organic matter acting as a ferment upon the infusion.*

And there are, I think, the very best reasons for concluding that in all the cases in which turbidity has occurred after the organic mixtures have been subjected to a heat of 140° F. (60° C.) and upwards, this turbidity has been due, not to the survival of the living units, but rather to the fact that the mere dead organic matter of the inoculating compound has acted upon the more unstable organic infusions in a way which it was not able to do upon the boiled saline fluids.

In order more fully to explain the grounds upon which this conclusion is based, it will now be necessary to recast the results of the 102 inoculation experiments recorded in my last communica-

* See "The Beginnings of Life," vol. ii., p. 2.

tion.* They require to be thrown into a new tabular form, in order to show how the results differed amongst themselves when organic infusions of different strengths were employed. The consideration of this aspect of the question was purposely delayed, in order to avoid the introduction of unnecessary complications into my last communication, seeing that the conclusion which I then sought to establish was in no way affected by these facts.

NEUTRAL HAY-INFUSION.

Infusion of sp. gr. 1005.				Infusion of sp. gr. 1002.		
Temperature.	Number of Expts.	Date of Turbidity, if any.	Results at the Expiration of the 8th day.	Number of Expts.	Date of Turbidity, if any.	Results at the Expiration of the 8th day
122° F. (50° C.)	1	24 hours.	Turbid.
131° F.		48 hours.	All turbid	1	48 hours.	Turbid.
140° F. (60° C.)	7	1 in 48 hrs. 6 in 60 hrs.	All turbid.	2	1 in 3 days. 1 in 8 days.	All turbid.
149° F.						
158° F. (70° C.)	15	...	All clear.
167° F.	4	...	All clear.
176° F. (80° C.)	12	...	All clear.

* See *Proceedings of the Royal Society*, No. 143, p. 230.

Reference will be made to these Tables in the setting forth of my reasons for the conclusion that

ACID TURNIP-INFUSION.

Infusion of sp. gr. 1008.				Infusion of sp. gr. 1005.		
Temperature.	Number of Expts.	Date of Turbidity, if any.	Results at the Expiration of the 8th day.	Number of Expts.	Date of Turbidity, if any.	Results at the Expiration of the 8th day.
122° F. 131° F.
	5	24 hours.	All turbid.	2	48 hours.	All turbid.
140° F.	6	40 hours.	All turbid.	6	4 in 3 days. 2 in 4 days. 1 in 3 days. 1 in 7 days. 2 in 8 days.	} All turbid. } Four turbid. } Three clear.
149° F.	3	5 days.	All turbid.	7		
158° F. 167° F. 176° F.	17 4	All clear. All clear. ...

the more or less delayed putrefaction which takes place in inoculated organic infusions raised to the temperature of 140° F., and to other degrees of heat above this point, is due to the influence of the not-living ingredient (*b*) of the inoculating compound. These reasons are the following:—

1. Because the turbidity which has occurred in inoculated organic infusions that have been subjected to a temperature of 140° F. has always manifested

itself appreciably later, and advanced much more slowly than in similar mixtures which had not been heated above 131° F.; whilst it has commenced even later, and progressed still more slowly, when occurring in mixtures previously heated to 149° F. Such facts might be accounted for by the supposition that exposure in these organic fluids to the slightly higher temperature suffices to retard the rate of growth and multiplication of the living units of the inoculating compound, although the facts are equally explicable upon the supposition that the later and less energetic putrefactions are due to the sole influence of the mere organic matter of the inoculating compound.

2. So far as the evidence embodied in the Tables goes, it tends to show that the more unstable different specimens of similar infusions are (that is, the stronger they are), the more rapidly and frequently does late turbidity ensue, and the more this late turbidity approaches, both in time of onset and in rate of increase, to that which occurs when inoculated infusions are not heated to more than 131° F.—when both living and not-living elements of the inoculating compound act conjointly as ferments. Such facts show quite clearly that where the intrinsic or predisposing causes of change are strong, there less potent exciting agencies are more readily capable of coming into play; but they still do not enable us

to decide whether the exciting cause of this delayed turbidity is in part the living element whose vitality and rate of reproduction has been lowered by the heat or whether the effects are wholly attributable to the mere organic matter of the inoculating compound.

So far, therefore, we have concomitant variations which are equally compatible with either hypothesis. But it will be found that each of the three succeeding arguments speaks more and more plainly against the possible influence of the living element, and in favour of the action of the organic matter of the inoculating compound, as an efficient exciting cause of the delayed putrefactions occurring in the cases in question.

3. As stated in my last communication,* when single drops of slightly turbid infusions of hay or turnip previously heated to 140° F. are mounted and securely cemented as microscopical specimens, no increase of turbidity takes place, although drops of similar infusions heated only to 122° F. do notably increase in turbidity (owing to the multiplication of *Bacteria*) when mounted in a similar manner. Under such restrictive conditions as these, in fact, a drop of an inoculated and previously heated organic infusion behaves in precisely the same manner as a drop of a similarly treated ammoniac-tartrate solution. In

* *Loc. cit.* p. 228 [p. 87].

each case, when heated to 140° F., turbidity does not occur, apparently because there are no living units to multiply, and because in these mere thin films of fluid dead ferments are as incapable of operating upon the organic fluids as they are upon the ammoniac-tartrate solutions.

4. Because, in the case of the inoculation of fluids which are not easily amenable to the influence of dead ferments, such as a solution containing ammoniac tartrate and sodic phosphate, this delayed turbidity does not occur at all. Such inoculated fluids become rapidly turbid when heated to 131° F., though they remain clear after a brief exposure to a temperature of 140° F. When the living units in the inoculating compound are killed, there is nothing left to induce turbidity in such solutions. The mere fact that these fluids do not undergo change when exposed to the air proves conclusively that they are very slightly amenable to the influence of the ordinary dead organic particles and fragments with which the atmosphere abounds. The absence of delayed turbidity in these fluids serves, therefore, to throw much light upon the cause of its occurrence in the organic infusions.

5. And, lastly, I can adduce crucial evidence supplied by the 'Method of Difference,' speaking with its accustomed clearness. Two portions of the same

hay- or turnip-infusion can be inoculated in such a manner as to supply us with the information we require. In the one case we may employ a drop of a turbid ammoniac-tartrate solution previously heated to 140° F., in which, therefore, the living units would certainly be killed; whilst in the other we may add another unheated drop of the same turbid saline solution to the organic fluid, and then heat this mixture also to the temperature of 140° F. The comparative behaviour of these two inoculated fluids (placed, in the ordinary manner, in previously boiled corked phials) should be capable of showing us whether the living elements of the inoculating compound were able to survive when heated in the organic infusion. If they did survive, the fluids inoculated in this manner ought to undergo putrefaction earlier and more rapidly than those inoculated with the drop of turbid fluid, in which we know the *Bacteria*, *Vibriones*, and their supposed germs would have been reduced to a state of potential death. With the view of settling this question, therefore, the following experiments were made:—

Description of Experiments.	Results.	Inferences.
A. Boiled ammoniac-tartrate solution, inoculated with an unheated drop of a similar solution turbid with <i>Bacteria</i> , &c.	Turbid in 40 hours.	That boiled ammoniac-tartrate solution is a fluid inoculable by living <i>Bacteria</i> , &c., and favourable for their growth and rapid multiplication.
B. Boiled ammoniac-tartrate solution, inoculated with a drop of a turbid saline solution previously heated to 140° F.	Clear at the expiration of 8th day.	That <i>Bacteria</i> , <i>Vibriones</i> , and their supposed germs are either killed or deprived of all power of multiplication when heated to 140° F. in this fluid.
C. Boiled turnip-and-hay-infusions, inoculated with a drop of a turbid saline solution previously heated to 140° F.	Turnip-infusions turbid in 2½ days. Hay-infusions clear at expiration of 8th day.	The precisely similar behaviour of the turnip-and hay-infusions of series C and series D respectively shows that the <i>Bacteria</i> , <i>Vibriones</i> , and their supposed germs are as inoperative in series D as they are known to be in series C; whilst the behaviour of the hay-infusions shows that they are little amenable to the influence of the drop of the saline fluid when its living units are killed.
D. Boiled turnip-and hay-infusions, inoculated with a drop of an unheated turbid saline solution, the inoculated fluid being subsequently heated to 140° F.	Turnip-infusions turbid in 2½ days. Hay-infusions clear at expiration of 8th day.	
E. Boiled turnip-and hay-infusions, inoculated with a drop of an unheated saline solution, the inoculated fluid being subsequently heated to 140° F.	Turnip-infusions turbid in 28 hours. Hay-infusions turbid in 38 hours.	Shows that the heat of 131° F. is not sufficient to kill <i>Bacteria</i> , <i>Vibriones</i> , and their supposed germs in organic infusions, and, again, that turnip-infusions are more rapidly influenced by such an inoculating agent than some hay-infusions.*

No experiments could speak more decisively. Those of series B show that *Bacteria*, *Vibriones*, and their

* These experiments of series C, D, and E were many times repeated with specimens of the same turnip- and hay-infusions, the specific gravity of the former being about 1008 and that of the latter 1005. Different specimens [of the material to be infused, and] of hay especially, vary so much that it becomes absolutely essential to use portions of the same infusion for the comparative experiments of these different series.

supposed germs are either actually or potentially killed when heated to 140° F. in the neutral saline fluid, which the experiments of series A show to be eminently favourable for their growth and reproduction. Being certain, therefore, that the living units are killed in the drops with which the fluids of series C were inoculated (because they were drops of the same fluid as was employed in series B), we may be equally certain that the turbidity and putrefaction which did ensue in the turnip-solutions of series C were due to the influence of the mere dead constituents of these drops of the turbid saline fluid; whilst, seeing that the behaviour of the fluids of series D was precisely similar to those of series C, we have a perfect right to infer that this series of fluids (D) was as devoid of living units as those of C are known to be—that is, that *Bacteria*, *Vibriones*, and their supposed germs are killed by the temperature of 140° F. in organic fluids, just as they are in saline fluids, although, as shown by the experiments of series E, they do not succumb to a heat of 131° F. These experiments of series C and D further illustrate the different degrees of amenability of different organic fluids to the same dead ferments; whilst the comparison of the results with the hay-infusions of series C and D with those previously cited (in which the inoculating compound was a drop of an organic

infusion heated to the same temperature of 140° F.) will illustrate the different influence of dissimilar dead ferments upon infusions of the same kind.

The evidence now in our possession shows, therefore, that whilst the temperature at which living ferments cease to be operative varies within very narrow limits (131°—140° F.),* that which destroys the virtues of not-living ferments varies within much wider limits, and depends not only upon the amount of heat employed, but also upon the nature of the putrescible or fermentable liquid to which such ferment is added, in conjunction with the degree of heat and other conditions to which the mixture is subsequently exposed.† Here, therefore, we have evidence as to the existence of a most important difference between living and not-living ferments, which has always been either unrecognised or more or less deliberately

* Liebig has proved that a temperature of 140° F. kills *Torulæ*, and always suffices to arrest a process of fermentation taking place under their influence in a sugar solution. *Torulæ* heated in water to 140° F. also fail to initiate fermentation in a sugar solution. I have also found that an exposure to a temperature of 131° F. for five minutes always suffices to destroy the life of Desmids, Euglenæ, Amœbæ, Monads, Ciliated Infusoria, Rotifers, Nematoids, and other organisms contained in specimens of pond-water. All these lower organisms seem to be destroyed at about the same temperature, as might have been expected from the fundamental relationship which must exist between these several varieties of the one substance—living matter.

† See "The Beginnings of Life," vol. i., p. 437.

ignored by M. Pasteur and his followers.* This difference is, moreover, thoroughly in accordance with the broad physico-chemical theory of fermentation which has been so ably expounded by Baron Liebig and others, and the truth of which may now be regarded as definitely established. According to this theory 'living' matter, as a ferment, would take rank merely as a chemical compound having a tolerably definite constitution; and this, we might reasonably infer, would, like other chemical compounds, be endowed with definite properties—and amongst others that of being decomposed or radically altered by exposure to a certain amount of heat. Looked at also from this essentially chemical point of

* See, for instance, all M. Pasteur's celebrated experiments in which he had recourse to an "ensemencement des poussières qui existent en suspension dans l'air," as recorded in chaps. iv. and v. of his memoir in "Ann. de Chimie et de Physique," 1862. M. Pasteur was engaged in an investigation one of the avowed objects of which was to determine whether fermentation could or could not take place without the intervention of living organisms, which M. Pasteur held (in opposition to many other chemists) to be the only true ferments. In his inoculating compound (dust filtered from the atmosphere), there was, as M. Pasteur was fully aware, a large amount of what his scientific opponents considered not-living ferment, whilst *possibly* there existed a certain number of living ferments. In explaining the results of his experiments, however, M. Pasteur and others thought he was pursuing a logical and scientific method when he attributed these results to the action of the possibly existing element of the inoculating compound, whilst he ignored altogether the other element which was certainly present in comparatively large quantity, and the testing of whose efficacy was the ostensible object of his research.

view, it would be only reasonable to expect that the molecular movements of living ferments with a lowered vitality might not be more marked or energetic than those which many not-living organic substances are apt to undergo; and this being the case, we might expect that there would often be a great practical difficulty in ascertaining whether a ferment belonging to the arbitrary and artificial (though, in a sense, justifiable and natural) category of "living" things had or had not been in operation.

It has, moreover, been most unmistakably proved that the limits of vital resistance to heat which *Bacteria*, *Vibriones*, and their supposed germs are capable of displaying are essentially the same in the three type fluids which I have employed—that is, in a weak saline fluid, in a neutral organic infusion, and in an acid organic infusion. No evidence exists really tending to show that these organisms or their germs are capable of withstanding the effects of heat better in one of such fluids than in another. We may therefore safely affirm that M. Pasteur never had any valid evidence in support of his conclusion that the germs of *Bacteria* and *Vibriones* can resist heat better in neutral or slightly alkaline solutions than in slightly acid mixtures. The experimental results which led him to arrive at such a conclusion were not logically capable of receiving any such interpretation, whilst

they can be legitimately accounted for in accordance with the broader physico-chemical theory of fermentation, the truth of which has now been established.* We may also safely affirm that M. Pasteur's more specific statement, to the effect that the germs of some *Bacteria* and *Vibriones* are capable of resisting the influence of a heat of 212° F. when in the moist state, though they are killed by a temperature of 230° F., was a conclusion altogether unwarranted by the evidence which he adduced. Finding that certain fluids treated after the manner introduced by Schwann always remained quite devoid of living organisms, M. Pasteur very legitimately concluded that pre-existing organisms and germs had been killed during the boiling of the liquid; but finding that when a little powdered chalk was added to fluids of the same kind (which in all other respects were treated in a similar manner) living organisms were after a time invariably found to appear, although they as invariably failed to appear when the same fluids were heated to a temperature of 230° F. (110° C.), two equally legitimate provisional conclusions were open

* I attempted to show, nearly three years ago (see *Nature*, July 14, 1870, pp. 224-228), that the differences which M. Pasteur ascribed to differences of vital resistance of organisms in particular fluids were just as explicable in accordance with the physico-chemical theory of fermentation, by reference to the different degrees of fermentability of the several fluids.

to M. Pasteur in explanation of these facts. What did M. Pasteur do? Following the same method as he had formerly employed,* he again ignored one of the equally possible interpretations, and unsuccessfully attempted to prove, by a repetition of similar reasoning,† that the different results in the two series of experiments were due to the fact that the germs of *Bacteria* and *Vibriones* which had been killed by the temperature of 212° F. in the first series were not killed by this temperature in the second series (in which a slightly alkaline fluid had been employed), although they were destroyed by the higher temperature of 230° F. Thus results which were due to the action of not-living ferments were ascribed to living ferments, and the possible action of not-living ferments was ignored, although, as I have said before, the ostensible object of M. Pasteur's researches was to inquire into the relative importance of not-living and living ferments, or whether, in fact, 'dead' substances (in the ordinary acceptance of the word) could act as ferments.

When viewed from the stand-point of the physico-chemical theory of fermentation, the apparently contradictory results arrived at by the same experimenter at different times or by different experi-

* See note * on page 113.

† See "Ann. de Chim. et de Phys." 1862, pp. 60-65.

menters, in this line of research, cease to be the inexplicable puzzle which they must always appear to those who place implicit faith in the narrower and too exclusive 'vital' theory of fermentation advocated by M. Pasteur and his followers.

My investigations have convinced me that, with regard to degree of fermentability, the various fermentable fluids and mixtures are divisible into three distinct subclasses :—

I. There are what may be called self-fermentable fluids or mixtures—that is, fluids or mixtures which, after exposure to a temperature of 212° F. or higher, are still capable of undergoing fermentative changes without the addition of less-heated matter, either not-living or living. The changes occurring in these self-fermentable fluids (in which pre-existing living things have been killed), when strictly protected from contact with adventitious particles, vary in rapidity and in intensity from the highest to the very lowest degrees of fermentability. These gradations are dependent principally upon the nature of the fluids or mixtures employed, and upon the degree of heat to which they have been submitted, though partly also to the temperature, pressure, presence or absence of filtered air, and degree of light to which the mixtures are subsequently exposed. For the sake of convenience, these gradations may be ranged into several

distinct groups, though of course any such divisions as I am now about to sketch are purely artificial and are connected with one another in nature by innumerable transitions.

Nature of Fluids.

A. Turnip-infusion with cheese, turnip - infusion neutralized by liquor potassæ, ordinary turnip-infusion, strong hay-infusion, &c.

B. Turnip-infusion neutralized by liquor potassæ, ordinary turnip-infusion, ordinary hay-infusion, &c.

C. Beer-wort,* &c.

D. Weak hay-infusions, urine, solutions containing ammoniac carbonate and sodic phosphate with minute organic impurities, &c.

Nature of Results.

Within two to four days marked turbidity, owing to the appearance of swarms of *Bacteria* and *Vibriones*. Fluids more or less fœtid. (*Putrefaction.*)

No uniform turbidity, but growth of flocculi in a more or less clear liquid. After a time the flocculi (composed of aggregated *Bacteria* and *Vibriones*) gradually subside, and the activity of the process ceases. Fluids either fœtid or having a mere sour odour.

Fluids which become more or less uniformly and rapidly turbid, owing to the appearance of swarms of *Torula*.

Do not become visibly turbid or produce visible flocculi, although on microscopical examination they may be found to contain living *Bacteria* pretty uniformly distributed, but in comparatively small quantities. The odour is often not more appreciably altered than the clearness of such solutions.

* I have had no experience with such a fluid myself. M. Pouchet's observations were, however, most striking on this subject (see his "Nouvelles Expériences," Paris, 1864, p. 190).

Nature of Fluids.

E. Weak hay-infusions, urine, solutions containing ammonio-citrate of iron and minute organic impurities,* &c.

F. Solutions of ammoniac tartrate and sodic phosphate with minute organic impurities, &c.

G. Weak or strongly acid infusions, and also many saline solutions containing organic impurities.

Nature of Results.

Same as in the last group,† though after weeks or months a dirty-looking sedimentary matter slowly accumulates at the bottom of the flask, which on microscopical examination is found to be composed partly of *Bacteria* with *Vibriones* and *Leptothrix*, and partly of *Torula* or more thickly-walled fungus-germs.

Same as in the last group, only the dirty sedimentary matter which accumulates never contains either *Bacteria*, *Vibriones*, or *Leptothrix*. Living *Torula* and thick-walled fungus-germs in various stages of formation are frequently met with, and also, occasionally, a mycelium resulting from the development of some of these bodies.

May remain permanently barren, and never show any traces of organisms either dead or living.‡

* In solutions containing iron, green organisms may subsequently be found (see "Beginnings of Life," vol. ii. p. 157).

† This, in fact, is in many cases the kind of change which the fluids last described ultimately undergo.

‡ See many negative experiments recorded in "The Beginnings of Life," vol. i. ch. xi. Mr. W. N. Hartley has laboured very industriously to disprove something which I never asserted (see Proceedings of Royal Society, vol. xx. p. 140). In my early paper in *Nature*, I expressly stated that organic impurities were always present in the saline solutions which I employed; and, as may be seen by the note appended to the conclusions of that paper, I never claimed to have

II. To the second subclass belong fluids which, after exposure to a temperature of 212° F. or higher, may be kept clear or apparently unaltered so long as they are shut off from contact with unheated atmospheric or other organic particles, but which do undergo putrefaction, or more or less marked fermentation, soon after they are brought into contact even with mere not-living organic matter.

The experiments recorded in this communication have most conclusively proved the efficacy of not-living organic matter as a ferment or inciter of change in previously barren fluids. And combining the knowledge derived from these experiments with that which we now possess concerning the absence of living *Bacteria*, *Vibriones*, and their germs in the air, together with the known prevalence of minute organic particles and fragments of various kinds, the explanation of M. Pasteur's celebrated experiments in which

established that living organisms could appear in saline solutions free from traces of organic impurity. Mr. Hartley did attempt to work with approximately pure saline solutions, and in other respects also the conditions of his experiments differed so much from mine, that the results which he obtained could not possibly be considered to disprove what I had previously stated. Some of his flasks were heated to 180° C., a temperature about which I had said nothing; and whilst his organic infusions were too weak, some of his saline solutions were too concentrated, though the strengths of others were not given at all. Fluids were also employed (such as urine, heated to 130° C.) which I had not made use of, and which I should not have thought of experimenting with.

he had recourse to an "ensemencement des poussières qui existent en suspension dans l'air," becomes quite easy and legitimate without having recourse to the hypothesis of Panspermism.* Now, also, are we enabled to understand all the apparent inconsistencies of those experiments in which previously boiled fluids have been exposed to the ordinary air of different localities, and have then been resealed. If many specimens of these fluids remained unchanged, whilst others, after a few days, swarmed with *Bacteria* and *Vibriones*, we may now very safely attribute these previously puzzling results to the comparative absence or presence of organic fragments in the particular volumes of air which chanced to get into the flasks, and to the different nature of the fluids employed by different experimenters.†

Many of the fluids which habitually remain clear after a previous ebullition in flasks whose necks have been plugged with cotton-wool, many times bent, or hermetically sealed after the entry of calcined air, or when enclosed in vessels which are completely full (in Gruithuisen's fashion), belong to this subclass. In

* See the experiments before alluded to, which are recorded in chaps. iv. and v. of his Memoir.

† See M. Pasteur's Memoir, chap. vii., and also *Compt. Rend.*, Nov. 5, 1860. See also a record of other experiments made with the air of alpine regions by MM. Pouchet, Joly, and Musset, in *Compt. Rend.*, Sept. 21, 1863.

other cases, however (as in many of those instances where urine or hay- or turnip-infusions have been employed), those who do not content themselves with a mere naked-eye inspection of the apparently pure fluids would find on microscopical examination of the sediment that such fluids were to a low degree self-fermentable—that they correspond, in fact, with group E of the last subclass;* whilst, in addition, my researches have shown that many of such fluids are capable of being rendered self-fermentable to a marked degree, if, instead of subjecting them to contact with calcined air or variously filtered air, its reflux after ebullition is altogether prevented by hermetically sealing the neck of the flask during ebullition. Operating in this way, I have repeatedly found that fluids freed from the pressure of air and from its influence altogether, become to a marked extent self-fermentable, although the same fluids exposed to filtered or calcined air under ordinary atmospheric pressure remain unaltered and barren, or at most exhibit the very low degree of fermentability referred

* Other fluids richer in organic matter or otherwise more favourably endowed, instead of presenting this low degree of self-fermentability, are notably prone to undergo change when we attempt to preserve them in the manner described, although such modes of preparation do suffice for preserving so many fluids. This has been fully admitted by Schröder and Dusch, Schwann, Pasteur, and others.

to as characterizing group E.* But just as amongst the self-fermentable fluids we find there are some which only engender *Torulæ* or other allied fungus-germs, so now we find that some previously boiled fluids, even when fully exposed to the air, swarm only with *Torulæ*. Those exciting agents derived from the atmosphere which, with one set of fluids, initiate changes leading to the evolution of *Bacteria*, with another set lead only to the evolution of *Torulæ*. And whilst telling us that the *Bacteria* which appear in previously barren fluids after exposure to air are not due to their contamination with germs of *Bacteria*, some observers would have us conclude that the *Torulæ* which appear in other previously barren fluids after a similar exposure are the products of preexisting aërial germs of such organisms. This conclusion, however, cannot readily be accepted in the face of the evidence derived from the closed-flask experiments with self-fermentable fluids of the lowest degree.† Such experiments, in fact, render the hypothesis as to the widespread distribution of aërial germs of *Torulæ* wholly unnecessary, by showing that certain fluids, by reason

* In illustration of this statement see "The Beginnings of Life," Appendix C, Exps. viii., ix., xiv., xv., xviii., xx., xxvi., xxx., xxxiii., and xxxvi.

† The only evidence in favour of such a conclusion is not one jot more conclusive than that which was formerly adduced in favour of the universal prevalence of *Bacteria*-germs in the air.

of certain intrinsic peculiarities, when they undergo fermentation give rise to *Torulæ* only. We are thus led to conclude that whilst some fluids are capable of engendering both kinds of organisms, others tend only to produce one or other of them—whether the fluids are contained in closed flasks or in open vessels exposed to the incidence of atmospheric particles. I have more than once seen nothing but *Torulæ* appear in an infusion of turnip exposed to the air after it had been heated in a closed tube to a temperature of 293° F. for twenty minutes, and I have once seen the same thing occur in an unheated infusion of turnip exposed to the air, though on all other occasions such infusions have swarmed only with *Bacteria* and *Vibriones*. On the other hand, a boiled ammoniac-tartrate solution exposed to the air, though protected from an excess of atmospheric particles (for the advent of a large number of these might in some cases incite putrefaction), is never found to contain *Bacteria*; the fluid continues clear, though a sediment gradually accumulates at the bottom of the flask, amongst which *Torulæ* and other fungus-germs are constantly to be found—more numerous though otherwise very similar to those which are to be met with in flasks closed during ebullition, or in others to which only filtered air is admitted. Although *Torulæ* only appear in such fluids, they continue all the time to be eminently inoculable by

Bacteria; and, again, when the *Torulæ* begin to decay they are apt to incite a more or less manifest putrefaction, during which the fluid gradually becomes turbid with *Bacteria*. It is, in fact, a general rule that putrefaction is apt to supervene upon a fermentation of a more smouldering type.

III. In the third subclass I include fluids which, after exposure to 212° F. or higher temperatures, are unable, either alone or under the influence of ordinary atmospheric particles or fragments, to undergo putrefaction, although such a process can invariably be initiated by bringing the fluids into contact with living ferments. As example of such fluids, I may cite the neutral saline solution to which I have so often referred and that known as Pasteur's solution. Other fluids of the same kind have lately been referred to by Professor Huizinga.* The fact that certain fluids cannot be made to undergo putrefaction by the influence of dead organic particles, although they become at once amenable to the influence of living units, unmistakably shows the superior potency of living ferments; their action has, moreover, invariably proved to be certain and inevitable in all the cases in which they were known to be present. Even these least fermentable fluids of our third subclass invariably become turbid within three days after

* See *Nature*, March 20, 1873, p. 380.

inoculation with living units, if maintained at a temperature of about 70°F.; whilst when other more changeable fluids are inoculated, putrefaction ensues with equal certainty, though with much greater rapidity.

What we have learned, therefore, concerning the *invariable uniformity* of simple inoculation experiments should of itself teach us how difficult it would be to account for cases of delayed putrefaction, or for cases in which a mere smouldering fermentation is set up, by the old though now well-nigh exploded notion of contamination by preexisting germs. Where living ferments really exist, the course of events is definite and almost invariable in its rapidity; but where fermentation takes place as a result of chemical changes occurring in the fluid itself (either by its own unaided powers, or under the stimulating influence of a less-heated organic ferment) there is abundant room for all the irregularity and variation actually encountered. These cases of irregularity and variation have always, on other grounds, defied all legitimate attempts to bring them individually within the pale of a narrow and exclusively 'vital' theory of fermentation; and now a wider experience with living ferments equally tends to show the impossibility of legitimately explaining a great mass of irregular phenomena by means of agents whose action is shown to be constant and almost invariable.

Thus it can now be proved, by evidence of a most unmistakable nature, that the process of putrefaction which invariably occurs in previously boiled putrescible infusions contained in flasks with narrow but open necks is not commonly (is, perhaps, only very rarely) initiated by living germs or organisms derived from the atmosphere; it can also be proved that putrefaction and the appearance of swarms of living organisms may occur in some boiled fluids when they are simply exposed to air which has been filtered through a firm plug of cotton-wool or through the narrow and bent neck of a flask, to air whose particles have been destroyed by heat, or even in fluids hermetically sealed in flasks from which all air has been expelled. The evidence in our possession is therefore most complete on this part of the subject: it shows beyond all doubt, not only that putrefaction may and does very frequently occur under conditions in which the advent of atmospheric particles, whether living or dead, is no longer possible, but also that living particles derived from the atmosphere can only be very rare and altogether exceptional initiators of the putrefaction which invariably occurs in previously boiled infusions exposed to the air.

Again, the evidence which we now possess with reference to the influence of heat upon *Bacteria*, *Vibriones*, and their supposed germs is no less deci-

sive. It has been unmistakably proved that such organisms and their imaginary germs are either actually or potentially killed by a brief exposure to the temperature of 140° F. when in the moist state; and it had also been previously established that they are invariably killed by desiccation even at much lower temperatures.*

But if living germs do not come from the air to contaminate the previously boiled fluids, and if it is not possible for any of them to have escaped the destructive influence of heat in the boiling fluid or on the walls of the vessel in which the fluid is contained, what can be the mode of origin of the swarms of living things which so rapidly and invariably appear in such infusions when contained in open flasks, and which so frequently appear when the infusions are contained in flasks whose necks are closed against

* See the experiments and conclusions of Dr. Burdon Sanderson in Thirteenth Report of Med. Officer of Privy Council, p. 61. This fact of the inability of these organisms and their germs to resist desiccation shows the futility of some objections which have been from time to time raised by those who thought that *Bacteria*, *Vibriones*, and their germs might resist the destructive influence of heat (by adhesion to the glass above the level of the fluid, or even in the fluid itself), just as dried and very thick-coated seeds have been known to do. Dry heat would seem to be even more fatal to such organisms and their germs than a moist heat of the same degree, owing to their extreme inability to resist desiccation: if they become dry they are killed at a temperature of about 104° F., whilst if they remain moist they succumb, as we have seen, to a temperature of 140° F.

atmospheric particles of all kinds? They can only have arisen by the process which I have termed Archebiosis.

Conclusions.

If a previously boiled ammoniac-tartrate solution remains free from *Bacteria* and *Vibriones* when exposed to the air, it is because the air does not contain living organisms of this kind or their supposed 'germs,' and because mere dead organic particles are not capable of initiating Putrefaction in such a fluid.

And if ordinary organic infusions previously boiled and exposed to the air do rapidly putrefy though *some* of the same infusions when exposed only to filtered air remain pure, it is because such fluids are in the absence of living units, quite amenable to the influence of the dead organic particles which the air so abundantly contains, although they are not self-fermentable.

Whilst if other more changeable fluids, after previous boiling, when exposed to filtered air or cut off altogether from contact with air, do nevertheless undergo Putrefaction or Fermentation, it is because these fluids are self-fermentable, and need neither living units nor dead organic particles to initiate those putrefactive or fermentative changes which lead to the evolution of living organisms.

IV.

THE DESTRUCTIVE INFLUENCE OF
HEAT UPON LIVING MATTER.

WATER is boiling merrily over a brisk fire, when some luckless person upsets the vessel, so that the heated fluid exercises its scathing influence upon an uncovered portion of the body—hand, arm, or face. Those who have seen much of the effects produced upon the human skin by such accidents, will have acquired information not unworthy of influencing their opinion on some more general problems connected with the action of heat upon living matter. Here, at all events, there is no room for doubt. Boiling water unquestionably exercises a most pernicious and rapidly destructive action upon the living matter of which we are composed. There is no need to appeal to the sufferer's sensations for this information. This, indeed, is a point of view which we may for the present dismiss. For however agonizing these sensations may be, they could only supply us with information upon a collateral point with which we are not at present concerned. Apart from such subjective effects there are objective effects. That is, we are

casily able to see the changes produced by boiling water upon living matter—revealing themselves as they do by an immediately altered appearance of the skin, and by the terrible wound so quickly produced. Upon these distressing, though, unfortunately, only too familiar consequences of the action of heat upon living matter, it is not necessary for me further to dwell; I would merely have the reader so far bear them in mind that they may not be incapable of recall during the perusal of this article. The occasional revival of such impressions may perhaps prove a little instructive to those who chance to be at all dubious as to the destructive effects of boiling water upon lower organisms.

Probably, however, some of my readers may already be possessed by the notion that the disastrous effects just referred to are consequences following rather from the fact of the high organization of man's tissues than from any intrinsic incompatibility of nature between living matter and boiling water. The thought is natural enough and not unjustifiable. On the other hand, it will not do to attach much importance to it. Let us for a moment consider the effects produced upon an ordinary hen's egg by a brief immersion in boiling water. Here we have the 'white,' composed of albumen similar to that which enters so largely into the composition of

living tissues, turned from a clear fluid into an opaque solid ; and we have the 'yelk,' made up of a dense aggregation of the simplest living units, also more or less solidified. In spite of the investing calcareous shell, these very obvious and destructive effects can be produced upon this large egg or 'germ' by an exposure for three or four minutes to the influence of boiling water. Yet the living matter in this case is so simple that it possesses next to no organization—it is so little vitalized that it can only be considered to be half alive.

The conclusion would seem, therefore, to force itself upon us that there is something intrinsically deleterious in the action of boiling water upon living matter—whether this living matter be of high or of low organization.

This subject is one of great importance in many respects, so that it may repay us to look into the evidence bearing upon it with some degree of care. It is of great practical importance, for instance, in reference to the process of disinfection by heat, where we have to do with articles of furniture or wearing apparel used by a person suffering from a contagious disease. Because, in such a case, what we ought undoubtedly to know is whether the temperature of boiling water, or even some lower temperature,

suffices to kill any living particles which may act as so-called 'germs of disease.' This is a subject upon which there should be no room for doubt. Again, from a purely scientific point of view, the question is of equal cogency because of its bearing upon one of the most momentous problems in biological science—namely, that of the Origin of Life. It is on this latter account, more especially, that I now take up the inquiry as to the grade or degree of heat which proves destructive to different kinds of living matter.

A preliminary word of explanation, therefore, must be given concerning the bearings of this question upon the Origin of Life problem.

It is at present very generally admitted, upon the strength of well-known experiments, that living matter will appear and grow rapidly in hermetically sealed flasks containing certain fluids after the flasks and their contents have been thoroughly raised to the temperature of boiling water for ten minutes or more. These experiments we may mentally label as Series A. Other experiments, which we may similarly label Series B, had also shown that a brief exposure in the moist state to a temperature considerably below the boiling point of water, is destructive to all kinds of living matter submitted to its influence. The experiments of Series A, therefore, taken in conjunction with those of Series B, must (if the latter results are as

reliable as the former) be held to prove that living matter can originate independently, or *de novo*, through the mere productive properties of certain infusions or solutions.

If the facts are true, is it possible to stave off the conclusion? Whilst the candid reader is asking himself this question, I may further point out to him that as the previously discredited results belonging to Series A are no longer denied, doubt is now only possible upon a subject hitherto supposed to be settled—namely, as to whether living matter is really killed by exposure in the moist state to a temperature of 212° F. Obviously, at such a juncture, it rested more especially with those Panspermatists who chose still to be opponents of ‘spontaneous generation,’ to show that this belief concerning the destructive efficacy of boiling water, upon the truth of which they had previously relied, was erroneous—seeing that the advocates of spontaneous generation had demonstrated the truth of their position with reference to the experiments of Series A. Should the Panspermatists fail to produce this evidence as to the untruthfulness of their old view, they must not expect to hear that they have the best of the argument; and still less will they be able to hold their ground if, whilst abstaining themselves from all experiments belonging to Series B, their scientific opponents do

make careful investigations in this direction, and arrive at the conclusion, that not only was the old opinion right as to the destructive action of boiling water, but that living matter unaccustomed to the influence of heat is killed by a brief exposure even to the much lower temperature of 140° F.

Such being the present aspect of the problem, those most interested in it may remember that knowledge would not advance in the rapid way which it does, were it not for the fact that the difficulties of one generation of men often disappear before the clearer, because more unprejudiced, vision of the next. Growing gradually more familiarized with the facts, those who come after us will be more and more influenced by them, and at the same time less warped by theoretical considerations already out of harmony with our present state of knowledge. I may perhaps be allowed to use the words of Mr. Darwin and say with him: "I look with confidence to the future,—to young and rising naturalists, who will be able to view both sides of the question with impartiality." We are now in a state of transition. We are gradually learning to accept the doctrines of Evolution, as applicable to different departments of knowledge, though, as is so frequently the case when new doctrines are being adopted, the transition is effected by many in a partial manner—they, unconsciously, perhaps, en-

deavour to make a sort of compromise, trying to retain some of their most deeply-rooted convictions and mix them harmoniously with new views. Metallic mercury, however, will not mix with water, and it will be found that there is a similar incompatibility between the explanations of the Panspermatists and our present state of knowledge in regard to the question of the Origin of Life.

It remains for me now, therefore, to trace the different steps by which we have arrived at our present knowledge concerning the destructive effect of Heat upon Living Matter. And, to do this effectually, I must refer my readers to good work done in the latter third of the last century by the acute and learned Abbé Spallanzani, whilst he was engaged in promulgating Panspermatist doctrines in opposition to the views of our countryman Needham, who, in those days, steadfastly proclaimed the truth and reality of 'spontaneous generation'—though the philosophical doctrines by which he was influenced caused him to limit the acceptation of the phrase to what we now understand by the term Heterogenesis.

I refer first of all to the work of Spallanzani, partly because he alone, of all those who have adopted Panspermatist views and have taken part in this controversy, has fairly and fully faced the question

of the degree of heat which proves fatal to various living things, by making it the subject of direct investigation. Others who have since defended similar views (including Pasteur in France, and Huxley and Sanderson in this country) have not made the thermal death-point of living matter a special subject of investigation, and have more or less distinctly confounded the issues of this question with that of the cognate though really distinct problem, as to whether certain infusions could themselves prove mother liquids and give independent birth to living matter. Dire confusion has thus been produced. A problem of a very simple nature has been made to appear very complex, whilst those who are able clearly to understand the real nature of the question at issue are left to marvel why the followers of Spallanzani have never ventured frankly to deal with the question of the limits of 'vital resistance' to heat, as Pouchet termed it. Certainly they have displayed, to say the least, a strange sluggishness in reference to this exceedingly important problem.

But apart from the fact that no Panspermatist, or declared opponent of spontaneous generation, since the time of Spallanzani has fully and directly experimented upon the subject, I am all the more induced to call the reader's attention to the Abbé's treatment

of it because, with some few exceptions, his investigations were conducted in a manner which cannot be improved upon at the present day, and because his reasonings in reference to them are characterised by great sagacity and fairness—allowance being made for the actual state of knowledge in his time. The work of the learned Abbé to which I shall especially refer is entitled in the admirable French translation by Jean Senebier, "*Opusclues de Physique Animale et Végétale*," the translation itself having been published at Geneva in 1767.

Reflecting upon the import of experiments of his own that he had just recorded, in which living organisms were found in closed vessels containing infusions of certain vegetable seeds after these closed vessels had been immersed in boiling water for half, or, in some cases, nearly three-quarters of an hour, Spallanzani frankly avows * that if the first of the new organisms had not come into being by some such independent method as that suggested by Needham, they must have appeared either because certain 'germs' from which they had been derived had been able to resist the destructive influence of boiling water for nearly three-quarters of an hour, or because, after the cooling of the closed vessels, some of the organisms observed had passed from the air through certain

* *Loc. cit.* p. 48.

imaginary pores in the glass. At the first glance these seemed, as he says, "deux suppositions également impossibles, ou du moins très difficiles à concevoir." For very excellent reasons, not difficult for the reader to imagine, the Abbé then points out that the latter hypothesis, at all events, is entirely untenable. The question thus became one of the simplest description. If no good reason could be found in support of the seemingly improbable supposition that the experimental results referred to were to be accounted for by a survival of germs, then, as he confessed, he must admit the fact of an independent and germless origin of living things. Whilst, if on the other hand, it should appear probable that germs or reproductive particles of living things could survive the influence of such a prolonged immersion in boiling fluids, he would not feel at all bound (on the strength of his previous experiments) to believe in the independent origin of living matter. This simple issue was fully realized by Spallanzani, and acting in accordance with the most obvious of scientific principles, he carefully sought for fresh evidence by means of well-directed experiments, in order to guide him towards a conclusion as to whether germs of living things could or could not have resisted the action of boiling water for more than half an hour.

He approached the question in the following

manner :—"Can one," he says, "find any proof sufficient to banish, or, at all events, to diminish one's natural repugnance to admit that the germs of animalcules of the lowest order have the power of resisting the action of boiling water? In reasoning from the germs or eggs of animals with which we are acquainted, would it be difficult for us to imagine animalcules having this peculiarity? It is true that we are not acquainted with any eggs endowed with such properties. I have already considered this subject in the ninth chapter of my Dissertation. I there show how several kinds of eggs of insects—not to speak of eggs of birds—perish under a heat less than that of boiling water. I have shown also that the seeds of plants are destroyed when they are exposed to the heat of boiling water, and that even those whose outer coat is of the hardest description are not thereby spared." But he goes on to say, as he had only been able hitherto to make his observations on a limited number of eggs and seeds, there was the chance that more extended observations might reveal some capable of resisting this generally destructive influence. He says he had never lost his hope—with regard to seeds more especially—since he had seen a statement by Duhamel to the effect that some grains of wheat had germinated after having been heated in a stove to a temperature above

the boiling point of water.* And as there is a considerable resemblance between seeds and eggs, Spallanzani was led to hope that something of the same alleged extraordinary capacity for resisting heat might be possessed by the eggs or germs of such organisms as make their appearance in previously boiled fluids. He was therefore stimulated to undertake fresh observations upon eggs and seeds generally, with the view, on the one hand, of ascertaining the precise temperature which proved fatal to each kind, and, on the other, of finding out whether these eggs or seeds were capable of resisting a greater degree of heat than the several animals or plants to which they belonged.

This latter part of the inquiry was rightly deemed by Spallanzani to be of great importance and capable of affording him much guidance towards the proper interpretation of his other experiments. He had already determined that the lower Infusoria themselves are killed at a temperature of 34° Réaumur, or $108\frac{1}{2}^{\circ}$ F. ; and now having found that such organisms

* Heated in all probability in the dry state. But it is well known that seeds and desiccated animals can resist the influence of heat much better in the dried state than when they are thoroughly moistened and then heated, and it is as to the effects of heat upon living matter under the latter conditions that we are at present concerned. For this reason, therefore, I shall not dwell upon certain other experiments of Spallanzani, in which he heated seeds in the midst of dry sand—they lie outside the boundaries of our present inquiry.

would appear within closed vessels previously subjected to a temperature of 212° F. (owing, as he was inclined to think, to a survival of their germs), Spallanzani was anxious to ascertain whether the difference in the capacity of resisting heat, imagined to exist in this case between parents and germs, could be justified by the establishment of similar differences in heat-resisting capacity between other parent organisms and their germs.

In carrying out these inquiries, Spallanzani adopted the following method : * He placed the eggs, seeds or organisms, made use of in his experiments, in a vessel containing cold water, into the upper strata of which was immersed the bulb of a thermometer. The water was then heated slowly, and when the thermometer indicated that the temperature had been attained whose effect it was desired to test, the eggs, seeds, or organisms were at once withdrawn and placed, under suitable conditions, in a separate vessel where their subsequent fate could be watched. The effects of different grades of heat upon the objects experimented with were thus estimated, and the temperature in successive trials was mostly made to differ from that last employed by 5° R., or about 11° F. Operating in this way, and, in the case of eggs or seeds, subsequently taking great care to place

* *Loc. cit.*, p. 53.

those used in the different trials, under similar conditions, alike favourable for germination or development, Spallanzani obtained the following results:—

Of Frogs' eggs only an extremely small number developed after having been simply raised to the temperature of 131° F., whilst not one developed which had been heated to 145° or upwards. The effects of intermediate temperatures were not tried, and consequently the precise death-point was not ascertained. The chances are, however, more in favour of its being under than over 140° F. Tadpoles produced from similar eggs all perished at 111° , and the same temperature likewise proved fatal to the parent Frogs from which the eggs had been derived, as well as to aquatic Salamanders and to some Fish with which experiments were made.

Silk-worms' eggs, and the eggs of the Elm-moth (*Papillon de l'Orme*), developed less and less frequently when successive batches were heated to temperatures approaching $144\frac{1}{2}^{\circ}$. When they were actually submitted to this heat all perished, though the highest temperature followed by development is not recorded. Silk-worms themselves, as well as the caterpillars of the Elm-moth, were uniformly killed as soon as the water in which they were immersed attained $108\frac{1}{2}^{\circ}$.

Eggs of the common Blow-fly only developed in

very small numbers when raised to the temperature of 135° , whilst all perished at 140° . The larvæ reared from these eggs all died, as those of the silk-worm and Elm-moth had done, as soon as the temperature of the water rose to $108\frac{1}{2}^{\circ}$. Other adult larvæ of the same species with which experiment was subsequently made perished at the same heat.

In addition, Spallanzani experimented with certain aquatic organisms, though he was unable to discover their eggs, and consequently was unable to make experiments as to their power of resisting heat. Thus he found that Leeches perished at 111° , and the Nematoids known as "Vinegar Eels" at 113° . Other aquatic Worms were killed at 111° , whilst Water Fleas died at 107° .

So far, therefore, Spallanzani's results were most uniform: the different kinds of eggs were killed by mere momentary exposure to a temperature of about 140° F., whilst the animals to which they were related perished at or about 110° .

The Abbé next turned his attention to the power possessed by plants and their seeds of resisting the action of heated water. These observations were conducted in the same manner, though only the roots of the plants were immersed in the water whilst its heat was being raised. The plants with which experiment had been made were afterwards carefully

replaced in earth. Much care was also taken when the seeds were sown to keep the batches distinct from one another, and to place them as much as possible under the influence of similar conditions.

Spallanzani's first experiments were made with the seeds of the Chick-pea, Lentil, Wheat-grass, Flax, and Clover. The water was heated slowly, and the seeds were taken out as soon as the desired temperature was attained, so that there was only a momentary exposure to the temperatures about to be cited. Of those which had been exposed to 190° F. many did not germinate; still fewer of the seeds that had been exposed to 201° produced young plants, whilst of those heated to 212° not one germinated. After the young plants which had been developed from seeds heated to lower temperatures had grown for thirteen days, their capability of resisting heat was tested in the manner described, and with this result: Those whose roots had been momentarily exposed to 156° continued to live after they had been replanted, whilst others whose roots had been exposed to 167° and upwards speedily dried up and perished, though all alike had been replanted in carefully watered earth.

These were the only complete experiments made by Spallanzani with plants and their seeds; but many other kinds of seeds only—including those of

the broad-bean, barley, kidney-bean, maize, vetch, spinach, beetroot, turnip, and mallow—were exposed to the influence of heat whilst packed in dry sand. Although this method is less exact and trustworthy, and is one with which we are not now concerned, still it may be stated that only four of the numerous seeds with which experiment was made after this fashion survived their brief exposure in the dry state to the temperature of 212° : all the others failed to germinate.

The Abbé's researches, therefore, taught him three things: (1), that eggs can endure a decidedly higher degree of heat than that proving fatal to animals of the kind from which they have been derived; (2), that an analogous difference exists between seeds and plants in respect to their capacity of withstanding the action of heat; and (3), that seeds and plants can resist higher grades of heat than eggs and animals respectively.

After calling attention to these conclusions, Spallanzani said,* "*Je suis sans-doute bien éloigné de prétendre expliquer ces résultats; je sens la difficulté de l'entreprise, de sorte que j'hazarderai tout au plus quelques conjectures, en les donnant pour ce qu'elles valent, et en laissant à chacun la liberté de penser ce qu'il voudra.*" As his conjectures, however, cannot

* *Loc. cit.*, p. 64.

be much improved upon at the present day, it will be as well to call the reader's attention to them and briefly point out their nature.

At the first glance, the Abbé says, the superior power of resisting heat displayed by eggs and seeds as compared with animals and plants might be supposed to be due to the developed organisms feeling the effects of heat more rapidly, owing to their being free from those envelopes which enclose the egg or the seed. But the weight of this supposed reason soon disappears—in the case of eggs, at all events. Looking to the thinness of their investing membrane this supposition, as Spallanzani says, “*paroit tout-à-fait peu vraisemblable, quand on pense à la facilité et à la rapidité du feu pour pénétrer une portion de matière si mince.*” He quickly dismisses—as being even more improbable—the notion that the smallness of the germ or egg can act as its safeguard, by rendering it less amenable to the influence of heat. Having thus cleared the ground, Spallanzani states what seems to him to be the principal reason of the difference observed. We ought to reflect, he says, upon the difference between the life of an animal in its egg stage, and its subsequent life as a developed organism. For however deficient our knowledge may be upon this subject, we may feel assured that life shows less of the characters of life in the egg than in

the organism which is born from it. The life of the egg is "très foible"—"sa vie est moins vie." And then Spallanzani asks whether the fact of this life of the embryo within the egg being "so small and so feeble"—being "a life which deserves so little the name of life"—may not be the reason that eggs are able to bear the influence of heat better than the developed organisms whose existence is more active and complex? He believes this to be the principal reason of the increased power of resisting heat displayed by eggs, and in support of it calls attention to the fact that many animals (as well as plants), when the rate of their vital phenomena is lowered, during winter sleep, are much better able to withstand many injurious external influences than when they are displaying to the full all the manifestations which constitute their 'life.' Animals—such as frogs and salamanders, for instance—resist the effects of injuries better, when they have been incurred during the benumbing cold of winter than at periods of the year when organisms like these are full of life and activity.

A similar difference obtains between the degree and complexity of the life of seeds as compared with that of plants, and this difference may in part similarly explain the superior power of resistance to heat shown by seeds. Here, also, amongst plants, we find that ability to withstand hurtful influences generally

increases as their life becomes more sluggish. Thus Spallanzani says, "One may say that in winter plants live less fully than at other seasons, and during this period they are also much less liable to perish when they are plucked from the ground or unduly pruned, than if they had been treated in the same manner during summer."

Again, whilst a difference of the same kind may in part be cited as the cause of the less injurious effect of heat upon seeds and plants as compared with that which it exercises over eggs and animals respectively, Spallanzani believed that the greater tenacity of life shown by seeds is only in part due to the fact that the outer coats of most seeds are much harder than those of eggs. Thus, the envelopes of some seeds which are only killed at a temperature near 212° are not harder than the shell of an egg, which is nevertheless killed at the much lower temperature of 140° F. This difference is explicable, according to Spallanzani, rather by the fact that the fluids contained within the egg are so much more abundant than those within the seed. The greater moisture of the animal embryo causes it more rapidly to experience the full effect of the heat, so that with a short exposure to a given temperature the egg may be easily killed whilst the seed escapes.*

* Spallanzani's argument thus naturally suggests the notion that many

Now then for the application of the facts, towards the interpretation of Spallanzani's other experiments in which the lowest organisms appeared in closed flasks whose contents had been exposed to the temperature of boiling water for half an hour. Certainly the 'germs' of such animalcules could not be supposed to have survived an ordeal of this kind, if they are to be compared with the eggs of animals, seeing that the death of the latter has been brought about by momentary exposure to a temperature far short of the boiling point. The supposition would however seem more possible if, instead of comparing these germs with the eggs of animals, one regarded them as belonging to the same category as the seeds of plants. Spallanzani frankly admits that they would seem to be more allied to eggs than to seeds, though he attempts to bridge the gap by saying that certain eggs are known (to which these germs may be allied) in some respects

of the seeds with which he experimented required a high temperature to kill them, merely on account of their dryness. If the seeds had been well soaked in cold water beforehand, so as to have thoroughly moistened them, might they not have been killed at a much lower temperature—that is, only a little, if at all, above 140° F., or the temperature which proved destructive to the more moist animal germs? Facts to be subsequently mentioned (p 161), which have been ascertained by Max Schultze and Kühne, would seem to render this very probable, and compel us to regard Spallanzani's experiments with seeds as needing repetition with the modification above suggested. The plants also, like the animals, should have been wholly instead of partially immersed in the heated water.

resembling seeds. Such eggs,—“become dry, are preserved in this state, and then develop like seeds after they have been placed in some damp medium.” “Why then,” he adds,* “may not the germs of the lowest kind of animalcules be possessed of a similar nature?” He next adduced various considerations which led him to regard this view as more and more probable, though none of his reasons would be deemed very weighty or even relevant by physiologists of the present day. The space at my disposal will not permit of my following him into these discussions—the reader curious on the subject can, however, consult Spallanzani’s work for himself.

In respect therefore to the questions with which we are now more especially concerned, the controversy carried on between Spallanzani and Needham about a century ago led to the following important results. Not a single living thing, egg, or seed, had been shown to be able to resist, when in the moist state, an exposure to boiling water for a single moment. All naturally moist forms of living matter with which experiment had been made, had been shown to be killed by a much lower heat—that is, at a temperature of about 140° F. or less.

And in order to account for the appearance of the lowest animalcules in previously boiled fluids, other-

* *Loc. cit.*, pp. 69—73.

wise than by 'spontaneous generation,' Spallanzani was compelled to assume (*a*) that the unknown germs whose existence had been postulated, notwithstanding their animal origin, were of the nature of seeds rather than eggs, because they were capable of undergoing desiccation with impunity—such ability to survive desiccation conferring upon them the greater power of resisting heat which characterizes seeds. Nay, further (*b*), although no seeds could be shown to be able to resist the influence of boiling water, Spallanzani assumed that these unknown seed-like germs might be able to do so.* Thus alone was he able to continue in the Panspermatist faith—on the strength of these hazardous assumptions only, could he refuse assent to the probability of a germless origin of living matter, more or less after the fashion suggested by Needham and others.

We may, therefore, now consider how far the progress of science has tended to confirm or reverse the hypothesis by which Spallanzani sought to shelter

* He had only met with a few seeds which had resisted a momentary exposure in dry sand to a temperature of 212° F. But seeing that not one of the numerous seeds with which he had experimented had been able to survive a similar momentary exposure to boiling water, he had no real warrant for supposing that the germs in question would be able to do so. Spallanzani, in short, here committed the error of arguing that what had occurred in dry sand might occur in water—even though his own experience had not supplied him with a single instance of survival of egg or seed after it had been even momentarily scathed by boiling water.

himself, and ward off the conclusions of his opponent. He saw fully, and had frankly admitted,* that there was but one means of escaping from Needham's conclusions. But, were these means legitimate?

(a) Although it is doubtless true that the superior dryness of seeds does enable them to resist the influence of heat longer than moist eggs are able to do, and therefore also enables them apparently to resist for a brief period a temperature notably higher than would have proved fatal to them had they been in a moist state—it is altogether another question when we have to decide whether the naturally moist Bacteria or their germs are really endowed with this seed-like property of developing after desiccation. To maintain his Panspermism in the face of his own experiments, Spallanzani was compelled to assume that the germs of the lower Infusoria do possess this potentiality. Modern science, however, declares that they have no such property. We are told most unreservedly by Professor Burdon Sanderson,† not only that “the germinal particles of microzymes [Bacteria] are rendered inactive by thorough drying without the application of heat,” that is, by mere exposure to air for two or three days at a temperature of

* See p. 142.

† “Thirteenth Report of the Medical Officer of the Privy Council,” p. 61.

104° F., but also that, "fully-formed Bacteria are deprived of their power of further development by thorough desiccation." Thus is the most important assumption made by Spallanzani swept away, and with it all the strength that his position may have appeared to possess. His followers cannot hope to save their germs from the full action of heat, however much they may wish to do so (and there are strong signs that they are thus influenced) by assuming the pre-existence of a protective desiccation. Are they not told, on what is to them the unquestionable authority of Professor Sanderson, that such desiccation would be in itself destructive?

(b) We are left, therefore, face to face with only one other question. Has the progress of science, it may be asked, since the time of Spallanzani, in any way tended to strengthen the possibility that Bacteria germs, or any forms of living matter in the moist state, can resist the destructive action of boiling water, even for two or three minutes? To this question a negative answer may be unreservedly given. The progress of science has, on the contrary, shown that such a supposition becomes more and more improbable when judged by the light of all uncomplicated investigations bearing on the subject. To these results of modern research I must now call the reader's attention.

In the first place the specific question with which

we are more immediately concerned, as to the thermal death-point of Bacteria and their germs, has itself been answered by most decisive experiments. As the writer has elsewhere already shown,* all direct experimentation on this subject leads to the conclusion that Bacteria and their germs, whether visible or invisible, are killed by a brief exposure to a heat of 140° F. in the moist state. Thus Dr. Sanderson's experiments having proved that the germs of these organisms are, as regards their ability to withstand desiccation, related to eggs rather than to seeds, the writer's own experiments tend still further to strengthen this resemblance by showing that these Bacteria germs (like the eggs with which Spallanzani experimented) are invariably killed at a temperature of about 140° F.

Although, therefore, my experiments are not favourable to Spallanzani's assumptions, they are entirely in accordance with his experiments. The thermal death-point ascertained by him for the eggs of Insects and of Batrachia agrees almost exactly with that which I have established for Bacteria germs—although at the time my own experiments were made I was unaware of these particular results obtained by Spallanzani.†

* In the two papers which precede this.

† Up to that time I had read his earlier work entitled, "Nouvelles

Is there, then, anything in this fact concerning Bacteria and their germs at all at variance with what we might have been led to expect, judging from our knowledge of the capacity for resisting heat shown by other kinds of living matter? Here again a negative answer may be unreservedly given. The grounds for this opinion must, however, be set forth; and in dealing with this important question I will range what I have to say under the following heads:—(1) The results obtained by many other investigators working quite independently of one another (and in many cases also without distinct reference to the Origin of Life question) all go to show that different kinds of living matter are killed, when heated in the moist state, at or below the temperature of 140° F. (2) The only known exceptions to this rule, furnished by organisms in hot springs, are cases of a special kind differing altogether from those with which we are at present concerned: though even here it is found that all such organisms perish at temperatures short of the boiling point. (3) Our knowledge as to the thermal death-point of Living Organisms and of units of Living Matter is remarkably harmonious, and is in accordance there-

Recherches sur les Découvertes Microscopiques, et la Génération des Corps Organisés, &c., and not the one from which I have just been quoting.

fore with what we know concerning the unity of living matter generally. (4) The assumptions entertained by some modern Panspermatists in support of their notion that living matter unaccustomed to the influence of heat is able to resist the destructive action of boiling water, are of the most frivolous nature—alike unsupported by experiment and contradicted by all ordinary experience.

(1.) Liebig proved that sugar-yeast (*Torula cerevisiæ*) entirely lost its power of growth and germination at 140° F.* It was ascertained by Tarnowski, after numerous experiments conducted, as Sachs says,† “with all possible precautions,” that spores of *Penicillium* and other common fungi, also most closely related by nature to Bacteria, “entirely lost their power of germinating when heated in their own nourishing fluids” to a temperature of 131° F. Again, it has been ascertained by Dr. Timothy

* M. Pasteur’s very important mode of effecting the preservation of wines is based upon a similar fact. He says (*Compt. Rend.* t. lxxv. 1872, p. 304):—“Toutes les maladies habituelles des vins sont dues à des champignons microscopiques.” Raising the wines in bottle to a temperature of about 60° C. (140° F.) suffices, he adds, to kill these fungi and their germs. In short an experience of six or seven years has shown that “après une exposition rapide à une température comprise entre 55 et 65 degrés” the wines no longer become sick, but even improve in quality.

† “Lehrbuch der Botanik,” 3rd ed., p. 626.

Lewis* that the germs of tape-worms are invariably killed at the temperature of 140° , whilst Professor Mantegazza has shown that the male reproductive particles of frogs are killed by exposure to a heat of 131° . So far, therefore, concerning germs, in addition to what I have already mentioned about Spallanzani's observations upon the eggs of Insects and Batrachia.

Turning our attention next to adult organisms of different kinds or to their elemental parts, the following facts may be cited. Pouchet† found that all kinds of Ciliated Infusoria were certainly killed at 131° F., and whilst confirming this observation the writer found that a brief exposure to the same temperature always sufficed to kill Amœbæ, Monads, Euglenæ, Desmids, Rotifers, Nematoids, and other minute aquatic organisms. The writer did not try to ascertain what was the lowest temperature which would prove fatal to these organisms, though this has been done by other observers. Spallanzani, for instance, ascertained that Ciliated Infusoria, Waterfleas, Leeches, Nematoids, and other worm-like creatures, all perished at $107-113^{\circ}$ F.; whilst Max

* "Eighth Annual Report of the Sanitary Commissioners with the Government of India," 1871, p. 139. Prof. Pellizzari has also of late arrived at similar results (see "The London Medical Record," Oct. 14, 1874, p. 641).

† "Nouvelles Expériences," &c., 1864, p. 33.

Schultze* and Kühne† (in part working over the same ground) have quite recently fixed the heat limits fatal to such organisms at temperatures varying between 104° and 113° F. At these temperatures, indeed, the protoplasm entering into their formation, as well as that of the tissue elements of higher animals, was not only killed, it became coagulated and assumed the condition named by Kühne 'heat-stiffening.' Both Max Schultze and Kühne also found that the protoplasm of plant-cells with which they experimented (belonging to the genera *Urtica*, *Tradescantia*, and *Vallisneria*) was similarly killed and altered by a very brief exposure to a temperature of 118½° F. as a maximum. All accurate new observations, therefore, go to prove that different kinds of living matter, whether in the form of germ or of developed organism, are killed by a brief unaccustomed exposure in the moist state to a temperature at or below 140° F.

(2.) So far I have been referring to the influence of heat upon living matter when it is suddenly applied to an altogether unaccustomed extent. This is the mode of operation with which we are especially concerned, as with the view to the interpretation of

* "Das Protoplasma," Leipzig, 1863, pp. 33 and 46.

† "Untersuch ueber das Protoplasma und die Contractilität," Leipzig, 1864, pp. 46 and 103.

experiments on the Origin of Life question we wish to know the effects of great heat upon organisms accustomed to ordinary atmospheric and aquatic temperatures. On the other hand, it should be pointed out that organisms have been found living in hot springs at temperatures very considerably above those I have just been quoting ; although the very highest of the temperatures, under the influence of which living things have been reported as existing in thermal springs, is still a few degrees below the boiling point of water. The various observations made upon this subject have been collected and criticised with much care by Professor Jeffries Wyman,* to whose paper I would refer the reader. The most remarkable instances—that is, the highest temperatures cited which are at all trustworthy—in which *Confervæ*, or allied organisms, have been met with, are thus summarized by Professor Wyman. “The statements we have quoted,” he says, “give satisfactory proof that different kinds of plants may live in water of various temperatures, as high as 168° F. as observed by Dr. Hooker in Sorujkund ; 174° as observed by Captain Strachey in Thibet ; 185° as observed by Humboldt in La Trinchéra ; 199° as observed by Dr. Brewer in California ; and 208° as observed by Descloizeaux in Iceland.”

* “American Journal of Science and Arts,” vol. xlv., Sep. 1867.

Having no grounds for criticising these observations, we are bound to look upon them, provisionally at least, as correct and taken with all due care—though it is only fair to add that both Max Schultze and Cohn appear to be not altogether satisfied with some statements of the same kind.* Such instances, if thoroughly accurate, may perhaps be taken as examples of the highest temperature that it is possible for living matter to endure, even where it has been inured to the influence of heat in the most gradual manner. And the real point of view from which such facts should be regarded is pointed out by Professor Wyman when he says: “Having become adapted through a long series of years to their surroundings, such organisms may be supposed to live under circumstances the most favourable possible for sustaining life at a high temperature. It is a well-known physiological fact, that living beings may be slowly transferred to new and widely different conditions without injury; but if the same change is suddenly made they perish. In the experiments made in our laboratories, the change of conditions is relatively violent, and therefore liable to destroy life by its suddenness.” Hence it is that a considerably lower temperature than that of the springs above mentioned suffices to kill all

* See Max Schultze, “Das Protoplasma,” Leipzig, 1863, p. 67.

living matter not previously inured to the influence of heat.

(3.) Omitting, therefore, the facts concerning the existence of living organisms in thermal springs as being altogether peculiar, and lying outside the boundaries of our present inquiry, all that we know about the unaccustomed influence of high temperatures upon living things can easily be shown to be even more harmonious than it may at the first glance appear. We have only to bear in mind two or three general principles in order to be able to harmonise the several experimental results arrived at with the now very generally admitted doctrine as to the oneness or generic resemblance existing between all forms of living matter. We must bear in mind, first of all, the consideration enforced by Spallanzani, that there are different grades of vitality, or, in other words, different kinds of living matter exhibiting more or less of the phenomena known as vital; and that of these kinds those which exhibit the most active life are those which would be most easily killed by heat. Thus we should expect the latent 'life' of the germ, egg, or seed to be less easily extinguished than the more subtle and, at the same time, more active life of the fully developed tissue element or organism; and we should also expect that the vegetal element or organism

would, as a rule, be less readily killed than the more highly vitalised animal element or organism. These principles, based upon the consideration of relative complexity of life, are, however, subject to the influence of a disturbing cause. Thus, we must also take into account, in the case of animals, whether we have to do with the elements of a warm-blooded or of a cold-blooded organism, since, contrary to what might otherwise have been the case, custom or habitual conditions may tend to render the more active tissue elements of warm-blooded animals better able to withstand the influence of heat than similar elements of less highly vitalised cold-blooded animals. Keeping these considerations in view, therefore, we may see by the following figures, how harmonious are the facts already ascertained!

Temperatures at which Death occurs.

Simple aquatic organisms	are killed at 104°-113° F.
<i>(Spallanzani, Max Schultze and Kühne.)</i>	
Tissue elements of cold-blooded animal—Frog	104°
<i>(Kühne.)</i>	

	are killed at
Tissue elements of warm-blooded animal—Man	111°
<i>(Stricker and Kühne.)</i>	
Tissue elements of Plants—Urtica, Tradescantia and Vallisneria	116½°–118½°
<i>(Max Schultze and Kühne.)</i>	
Eggs, Fungus-spores, and Bacteria germs*	122°–140°
<i>(Spallanzani, Liebig, Tarnowski and others.)</i>	

So far as can be ascertained by really scientific methods, free from all obvious possibilities of misrepresentation, these are the temperatures which undoubtedly kill the different varieties of that common life-stuff known as Protoplasm — the “physical basis of life,” as it has been termed by Professor Huxley. That it should present this comparative unity in its behaviour towards heat as well as to other physical agencies, is surely not in antagonism with the generally-approved biological doctrines, of which Professor Huxley has made himself the most celebrated exponent in this country. In his own forcible language he tells us as follows:*

—“Beast and fowl, reptile and fish, mollusc, worm,

* “Lay Sermons,” pp. 126–129.

and polype, are all composed of structural units of the same character, namely, masses of protoplasm with a nucleus What has been said of the animal world is no less true of plants. . . . Protoplasm simple or nucleated is the formal basis of all life. . . . Thus it becomes clear that all living powers are cognate, and all living forms are fundamentally of one character."

Before the breath of controversy had arisen Professor Huxley was, moreover, inclined to believe that this protoplasm or primitive life-stuff would display a comparatively uniform behaviour under the influence of heat. Thus in the essay from which I have just quoted, he says, it cannot "be affirmed with perfect confidence that all forms of protoplasm are liable to undergo that peculiar coagulation at a temperature of 40°—50° Centigrade which has been called 'heat-stiffening,' though Kühne's beautiful researches have proved this occurrence to take place in so many and such diverse living beings, that it is hardly rash to expect that the law holds good for all."

(4.) I now turn to say a very few words concerning the general attitude and specific statements made by those who, still wishing not to give in their adherence to the fact of the occurrence of 'spontaneous generation,' affect to believe that Bacteria germs or other

kinds of living matter can resist the influence of boiling water.

In the first place it should be said that not one of these modern Panspermatists has striven to justify his position by scientific evidence bearing directly upon the death-point of Bacteria and their germs, whilst several of them have openly attempted to make good their position in the most unscientific manner — that is, by adducing, when facts seem adverse, experiments admitting of two interpretations, as though they only admitted of one, and then of these two possible interpretations selecting that which the experiments were neither calculated to warrant nor originally destined to illustrate. This shuffling with conclusions becomes all the more reprehensible when the interpretation selected is known to be directly contradicted by other less equivocal evidence, as to the very existence of which, however, those who adopt this course take care to say nothing.* This is a kind of treason to Science, of which one can only hope that the future may prove less prolific than the past has been.

And, if we turn now to the specific statements made by those who profess to believe that Bacteria and their germs are able to resist the influence of

* See p. 136. Also "Nature," 1873, Nos. 190-193, 206, 207, and 209.

boiling water—we discover in the first place that all who advance such suppositions find it convenient to pass unnoticed the several series of experiments by which it has been proved, that Bacteria and their germs are uniformly killed by an exposure to 140° F. for five minutes. My opponents find it most convenient to take no notice of these experiments, though no one has as yet attempted to dispute their cogency. They prefer to talk vaguely, as though such experiments had never been made, and to adduce various theoretical reasons whose validity they do not attempt to test experimentally. To do this, indeed—as they must be more than half aware—would be a vain attempt, since the suppositions which they advance are opposed to generally-accredited facts and scientific doctrines, even if they have not already been specifically refuted.

The suppositions principally dwelt upon may be ranged under three categories.

(a.) It is assumed by some that the mere minuteness of the germs of Bacteria may serve to protect them from that destructive influence which heat exercises upon living matter generally.* This is

* Some of those who rely upon this supposed reason have resorted to direct attempts to ascertain the death-point of the germs of other organisms, although their results have been, in part, vitiated by the evaporation of the drop of fluid employed—so that the organisms were subsequently exposed to the higher degrees of heat in a *dry* state.

an old objection entirely unsupported by facts, and those who dwell upon it may be reminded that it was unhesitatingly rejected by the former chief of their school, Spallanzani, who said, "un raisonnement de cette sorte est absolument contraire à toutes les notions que nous avons du feu." They may be further reminded that the writer's own experiments completely meet this objection, since they refer to the death-point of invisible germs of Bacteria just as much as to the death-point of those which are visible.*

(b.) Others, without definitely committing themselves to the belief that Bacteria germs can resist the destructive influence of boiling water when they are immersed in it, affect to believe that some germs may have escaped its influence by being 'spurted' out of the fluid on to the sides of the glass when the process of boiling commenced. How any such germs could escape the moistening and destructive influence of the hot steam with which they would still be in contact these reasoners do not say, though some of them are cautious about openly suggesting an antecedent and protective state of extreme desiccation in the face of Dr. Sanderson's experiments proving that this would be in itself destructive. The futility of this objection, so far as the general question is

* See p. 86.

concerned, has, moreover, been completely demonstrated by the fact that organisms will occur just as freely under conditions where no such objection can be alleged, that is, when the vessel and its contents are heated by submergence in boiling water, after it has been hermetically sealed—a mode of heating which those who adduced the objection above mentioned ought to have known had been occasionally adopted by different experimenters since the time of Spallanzani.

(c.) The third objection raised is no less remarkable, owing to its being similarly brought forward as an unsupported supposition in the face of much other evidence testifying to its nullity. When the writer's earlier experiments were first recorded, the public was authoritatively told by Professor Huxley that the results were quite unworthy of credence. The fact that tons of meats and vegetables were annually preserved from putrefaction by a very similar process was supposed to be the strongest evidence that he had in some manner deceived himself. It was never suggested or thought of, therefore, at this time that such moist meats and vegetables were incapable of being heated through, even when pounds of them were aggregated together. It was, in short, implicitly said that they could be so heated, and the fact of the preservation of the meats and vegetables

was itself deemed to be the best evidence that all 'germs' which might have been contained in their interior had been killed. Now that the writer has demonstrated to unbelievers, and when others have ascertained for themselves, that organisms are to be met with and that putrefaction will occur within almost airless and hermetically sealed flasks whose contents have been previously boiled, the tactics of these unbelievers are entirely changed. Forgetting altogether the previous objection upon which they relied so long as they doubted the writer's facts, they now advance an interpretation of his results, which must carry with it its own stultification to the minds of those who are not similarly oblivious of their previous position. The writer's methods are declared to be faulty because he did not think it needful to free his infusions from all particles, however minute and however soft. Impetuous critics now shake their heads, and talk with apparent learning about "the protective influence of lumps." Whilst heat was previously supposed to be capable of operating as a germ-killer through pots of meats and vegetables, and whilst it has been proved to act in the same way through the thick and dry envelopes of seeds, now a pea or a minute particle of cheese, even though smaller than a pin's head, is thought to exercise a "protective influence" over imaginary germs!

Such puerilities might safely be left to die a natural death, though it may be as well to remind those who profess to trust to them, that although they do not put their notions to the test of direct experiment, others have, for certain practical reasons, had occasion to do so. Dr. Timothy Lewis, who has been for some time in Calcutta carrying on, in concert with Dr. D. Cunningham, important sanitary investigations, has, amongst other things, directed his attention to the vitality of tape-worm germs in cooked meat. He proved, first, that tape-worm germs are undoubtedly killed by exposure for five minutes to a temperature of 140° F.; and then, with the view of ascertaining how far they would be likely to experience such a temperature in the ordinary process of meat-cooking, he made other important observations having considerable interest for us. Dr. Lewis found that when legs of mutton had been put into the boiler almost as soon as the water, their central temperature averaged 140° F. by the time the water around them had reached the boiling point; and that after the water had boiled for five minutes, the internal temperature of other legs of mutton which had remained in the boiler had on an average reached 170° . This is a practical method of dealing with the question which those sceptical dreamers who talk of the "protective influence of lumps" would do well to imitate.

After this I may perhaps be deemed fully justified in quoting two very typical experiments for the further consideration of those who stave off their belief in the occurrence of 'spontaneous generation'—either by relying upon insufficient reasons for doubting the influence of boiling water, or because of their following Pasteur, Cohn, and others, in supposing that certain peculiar Bacteria germs are not killed except by a brief exposure to a heat of 227° or 230° F. For even if we could grant them these limits, of what avail would the concession be towards averting the dreaded admission of the occurrence of 'spontaneous generation,' in the face of the following experiments, and others of a similar nature?

Experiment I.—A strong infusion of turnip was rendered faintly alkaline by liquor potassæ, and to this a few separate muscular fibres of a cod-fish were added. Some of this mixture was introduced into a flask of nearly two ounces capacity. Its neck was drawn out and afterwards hermetically sealed by the blow-pipe flame, whilst the fluid within was boiling. When thus closed the flask was about half full of fluid. It was then introduced into a digester which was gradually heated, and afterwards kept at a temperature of 270 – 275° F., for twenty minutes, though it seems also well to point out that if we

include the time taken for the water of the digester (in which the closed flask was immersed) to attain this heat, and also again to cool down to 230° F., this flask was exposed to temperatures above 230° F. for ONE HOUR, as I myself carefully noted at the time. When withdrawn from the digester the closed flask was kept at a temperature of 70-80° F. for eight weeks, and during part of this time it was exposed to the influence of direct sunlight. After it had been ascertained that the flask was free from all crack or fault, its neck was broken, in order that its contents might be examined. The reaction of the fluid was found to have become decidedly *acid*, and it had a sour though not fœtid odour, as though a fermentative process had been taking place in the solution. The fluid was very slightly turbid, and there was a well-marked sediment consisting of reddish-brown fragments, and of a light flocculent deposit. On microscopical examination the fragments were found to be portions of altered muscular fibre, whilst the flocculent deposit was composed for the most part of granular aggregations of Bacteria. In the portions of fluid and of deposit which were examined, there were thousands of Bacteria of most diverse shapes and sizes, either separate or aggregated into flakes. There were also a large number of monilated chains, of various lengths, of a kind very frequently

met with in abscesses and other situations, where pyæmia or low typhoid states of the system exist, in the human subject. There were, in addition, a large number of *Torula* corpuscles, as well as of brownish nucleated spore-like bodies, gradually increasing in size from mere specks, about $\frac{1}{30000}$ th up to $\frac{1}{2500}$ th of an inch in diameter. Lastly, there was a small quantity of a mycelial *Fungus* filament, bearing short lateral branches, most of which were capped by a single spore-like body. (See Fig. 1.)

Experiment II. A strong infusion of common cress (*Lepidium sativum*), to which a few of the leaves and stalks of the plant were added, was inclosed in an hermetically-sealed flask in the same way, heated in the digester at the same time (and therefore to the same temperature), and was subsequently exposed to the influence of the same conditions as I have already mentioned in connection with the last experiment. This flask was, however, opened one week later—that is at the close of the ninth week after it had been heated in the digester to 270–275° F. Before breaking the neck of the flask the inbending of the glass under the blowpipe flame showed that it was still hermetically sealed. The reaction of the fluid was found to be distinctly acid, though there was no notable odour. The fluid itself was tolerably clear and free from scum, but there was a dirty-looking

flocculent sediment at the bottom of the flask, amongst the *débris* of the cress. On microscopical examination (with a $\frac{1}{2}$ th "immersion" objective) much altered chlorophyll existed, either dispersed or aggregated amongst the other granular matter of the sediment, and amongst some of this three minute and delicate *Protamæbæ* were seen, varying in form, and creeping with moderately rapid slug-like movements. They contained no nucleus, and presented only a few granules in their interior. In the same drop of fluid, and also in others subsequently examined, more than a dozen very active *Monads* ($\frac{1}{4000}$ th of an inch in diameter) were seen, each provided with a long, rapidly-moving lash by which neighbouring granules were freely knocked about. There were many smaller motionless and tailless spherules of different sizes, whose body-substance presented a similar appearance to that of the *Monads*—and of which they were, in all probability, earlier developmental forms. There were also several unjointed *Bacteria*, presenting most rapid progressive movements accompanied by quick axial rotations. Many *Torula* corpuscles and other *Fungus* "spores" also existed, as well as portions of a mycelial filament containing equal segments of colourless protoplasm within its thin investing membrane. (See Fig. 2.)

A drop of the fluid containing several of these active *Monads* was placed for about five minutes on a glass-slip in a water oven maintained at a temperature of 140° F. All the movements of the *Monads* ceased from this time, and they never afterwards showed any signs of life.

These experiments are merely two of the most remarkable selected from several others in which even higher temperatures were originally had recourse to in order to free the fluids and flasks generally from anything like a trace of living matter. Nothing that has yet been alleged by way of objection to the admission of 'spontaneous generation' as an everyday fact, at all affects such experiments as these. The shortest way out of the difficulty would therefore be to doubt the facts. I can assure the reader, however, that they are as true and quite as reliable as those other results obtained when working with lower temperatures, which, though strongly disbelieved in at first, are now generally recognized as trustworthy. And although 'these now accredited results abundantly suffice, in face of our present knowledge concerning the limits of vital resistance to heat, to establish the strongest probability of the occurrence of 'spontaneous generation,' yet such experiments as those which I have now recorded even still further confirm this view, since it becomes in-

credible that whilst all known forms of living matter with which accurate experiment has been made invariably perish at or below 140° F., the particular examples of some of the same forms which appear within our sealed flasks have been able to survive a much longer exposure to 270–275° F. If this were true, then indeed would the cultivation of Science be a vain pursuit—‘uniformity,’ in fact, must be postulated and granted, or Science with humbled and sorrowful crest must retire from the field.

A word or two may be said in conclusion with reference to the interpretation which should be attached to such experiments as those just recorded. And this subject cannot be better introduced than by means of the following extract from the already-quoted and valuable paper by Professor Jeffries Wyman.* He says :—“ There can therefore be no certainty of the existence of spontaneous generation in a given solution, until it can be shown that this has been freed of all living organisms which it contained at the beginning of the experiment and kept free of all such from without during the progress of it. On the other hand, this kind of generation

* Whilst these pages have been passing through the press the sad news has reached us of the premature death of Professor Wyman. In him Science has lost one of her most faithful followers.

becomes probable, whenever it is made certain that Infusoria are generated in solutions in which the conditions just mentioned have been complied with. We say probable, because their appearance under such circumstances would not amount to a proof. The absolute proof of spontaneous generation must come from the formation of living organisms out of *inorganic* matter. If Infusoria are generated in solutions of organic matter, independently of spores or germs, the question may be fairly raised whether we do not begin the experiment with materials in which life already exists, even though this material is not in the form of distinct organisms." Now, these last few lines as they at present stand, tend, as it appears to me, to convey to the reader very erroneous impressions—and yet I am aware that views of the same kind are very commonly expressed, and seem to exist in an inchoate or half-realised form in the minds of many distinguished persons. It is for this reason, and on account of the authority attaching to Professor Wyman's statements that I am induced to take notice of this particular passage in order to attempt its rectification.

In the first place then, under the old term, 'spontaneous generation' are included two processes quite distinct from one another—namely, Hetero-

genesis and Archebiosis. With regard to Heterogenesis, this is merely the opposite of Homogenesis; and the latter is the name for that mode of generation or reproduction amongst living things which is looked upon with most respect and which is most generally known. It is the process by which "like produces like"—that is, where the offspring grow into beings similar to their parents. In Heterogenesis, on the other hand, we have the birth of dissimilar products, the beginning of a new branch from a "life-tree," in which the offspring have no tendency to assume the parental type. This occurs, for instance, where the protoplasmic matter of an animal or of a vegetal cell becomes modified and resolved into Bacteria. Here we have to do with the mere transformation of living matter. It is therefore a truism to say that it can only take place where living matter pre-exists. And seeing that many investigators, amongst whom I may especially mention, Needham,* Pouchet, and Trécul, have, both now and formerly, understood by the phrase 'spontaneous generation,' merely such a process of metamorphosis of living matter as is implied by the term Heterogenesis, it is, to say the least, very misleading to assert without qualification that, "the absolute proof of spontaneous generation must come

* See "The Beginnings of Life," vol. i., pp. 246-252, and vol. ii., p. 181.

from the formation of living organisms out of inorganic matter.”

It seems obvious, however, that when Professor Wyman wrote this passage, he, forgetting for the time the more common acceptance of the phrase ‘spontaneous generation,’ must have used it only in the sense in which I now employ the term Archebiosis—in the sense, that is, of life-origination. But, even taking the phrase ‘spontaneous generation’ in this one sense only, how far, we may ask, was Professor Wyman justified in saying that its proof “must come from the formation of living organisms out of inorganic matter?”

The statement is, in my opinion, one which cannot be logically entertained by a believer in the ordinary physical doctrines of life; and consequently, if I am correct in this view, it should be professed by no consistent believer in Evolution. Those who do not assent to these physical doctrines of life would probably never be able to believe in Archebiosis at all—to the ‘vitalist,’ life is an immaterial principle specially created, and therefore our flask experiments terminating in the birth of new organisms, could at the most be regarded by him as proving the occurrence of Heterogenesis. Life was there, he might say, as an indestructible principle, so that the new organisms which appear are simply new embodiments of this

'principle'—a kind of transformation has taken place. This, in short, is the view to which a vitalist would be driven, if he had become convinced that no germs of Bacteria, or of such other organisms as are found in our flasks, could have survived the preliminary process of heating. Such a vague sort of position is not open, however, to evolutionists or to those who believe in the now generally accepted physical doctrines of life. They are bound to recognize the undoubted distinction which exists between mere dead organic matter, and that organic matter which displays the phenomena of life. They should no more think of calling a body 'living' which could not be made to display the characteristics of life, than they would call a body 'magnetic,' when it would show none of the properties pertaining to magnetism. If they had learned, therefore, that living matter when exposed to heat of a certain intensity became lifeless matter (that is, that it could no longer be made to display the phenomena of life), the process by which new living protoplasm comes into existence amongst this dead organic material, would be, for them, as much an instance of its new independent origin as if the process had occurred in the midst of mere inorganic elements. The term Archebiosis is therefore applicable to the process that must take place in our ordinary flask

experiments where we deal with dead organic matter, just as it is also applicable to those more primordial combinations which first gave birth to living protoplasm on the surface of our Earth.

Should there be persons still reluctant to accept this conclusion, simply because they cannot bring themselves to believe in a process of 'origin,' concerning the actual steps of which they know nothing, one may ask whether it ever occurred to them to doubt the fact of the increase in size of an elm-tree simply because they were similarly unable to know the intimate nature of those nutritive processes, by which dead inorganic elements are fashioned into its living substance—because in short they do not know the secret steps of the process by which its 'growth' is accomplished!

And in reply to those who speak scornfully, because of the complexity of some of the organic mixtures which have been employed, I would say that my object throughout has been to establish, in the first place, the mere fact of the occurrence of Archebiosis. And whilst thinking that this could be done most easily, if at all, by making use of organic mixtures, I also felt sure (for the reasons already stated) that it could be done just as decisively with them as with simpler solutions of more definite composition.

Having established the fact of the present elemental 'origin' of living matter, those who choose may afterwards seek to discover and explain the steps by which it occurs. This appears to me to be at present an almost hopeless line of research. And certainly before venturing to attempt to unravel a problem so exceedingly difficult and complex, I should strive to solve the riddle of organic 'growth.' What, for instance, are the steps of the process by which protoplasm is produced from the elements of a simple solution of ammoniacal tartrate, when Bacteria are rapidly growing and multiplying therein? Here is the problem of growth and nutrition in its simplest form! Yet who can solve it? Till this is done we shall stand little chance of discovering the chemical stages of the process by which Living Matter originates.

Meanwhile let none forget, that, from the point of view of our ignorance of the intimate nature of both processes, we may as reasonably doubt the fact that Living Matter grows as we may doubt the fact that it can arise independently—and, further, let none forget that Origin and Growth are in essence merely stages of one and the same process.

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