FACTS AND REASONINGS CONCERNING THE HETEROGENOUS EVOLUTION OF LIVING THINGS*

IN all ages it has been believed by many that Living things of various kinds could come into being *de novo*, and without ordinary parentage. Much difference of opinion has, however, always prevailed as to the kinds of organisms which might so arise. And, although received as an article of faith by many biologists—perhaps by most—in the earlier ages, this doctrine or belief has, in more recent times, been rejected by a very large section of them. Definitely to prove or disprove the doctrine in some of its aspects is a matter of the utmost difficulty, and there are reasons enough to account for the wave of scepticism on this subject, which has been so powerful in its influence during the last century. The notions of the ancients were altogether crude, and founded upon insufficient proofs. It was not in their power to settle such a question; and when the inadequacy of the evidence on which they had relied became known, then much doubt was thrown also on the truth of the conclusion at which they had arrived. All this was natural enough. When, therefore, about a century ago, the rude microscopes of the time began to reveal a multitude of minute organisms whose existence had been hitherto unsuspected; when more facts became known concerning the various modes of reproduction amongst living things; and, above all, when the philosophical creeds of the day were supposed to be irreconcilable with such a doctrine, then a growing scepticism in the minds of many gradually developed into an utter disbelief in the possibility of the occurrence of what was called "spontaneous generation."

This was the state of things anterior to and during the time of the celebrated controversy between the Abbé Spallanzani and John Needham. Then it was that the former of these two champions, with the view of accounting for phenomena which would otherwise have necessitated his admission of the doctrine which he rejected, recklessly launched upon the world the *hypothesis* that multitudinous, minute, and almost metaphysical "germs" existed everywhere—ready to burst into active Life and development whenever they came under the influence of suitable conditions. Armed with this all-powerful *Panspermic* hypothesis, Spallanzani argued against the conclusions of Needham. His views on this subject were supported by the still more extravagant theories of Bonnet. The doctrine of "*L'Emboîtement des germes*" was the production of an unbridled fancy, and might, perhaps, never have been elaborated, had not the Leibnitzian doctrine concerning "Monads," as centres of force and activity, been already in existence, and at the time all-powerful in the philosophical world.

The controversy which was initiated by these two pioneers in microscopical research they were unable to terminate—the enigma which they sought to solve has, since their time, still pressed for solution, and still the tendency has been to solve it after one or other of the modes by which they attempted to account for the occurrence of the phenomena in question. It is and has been contended, on the one hand, that Living things can originate *de novo*, and without ordinary parentage; it is contended, on the other, that this is impossible—that every Living thing is the product or off-cast of a pre-existing Living thing, and that those which appear to arise *de novo* have, in reality, been produced by the development of some of the myriads of visible or invisible "germs" which pervade the atmosphere.

Now it is obvious, that of these two opposing doctrines, the one must be true and the other false: either Living beings can originate *de novo*, or they cannot. So long as any doubt remains upon this subject, we have to confess our ignorance concerning one of the very first principles of Biology. In the whole domain of Science, moreover, it is scarcely possible to propose a question which is more replete with interest than that which asks whether Living things can be evolved *de novo*. If settled in the affirmative, what light will be thrown upon the past and present history of our globe! How must our notions concerning Life, health, and disease be influenced in one way or the other by its solution!

Without entering into the history of the long controversy which has taken place upon this subject—details of which may be found in the works of Pouchet* and Pennetier,† and in the writings of Pasteur‡—I shall, before describing my own experiments and their results, merely relate as briefly as possible what conclusions have been come to concerning the degree of heat to which inferior organisms may be subjected with impunity, and what temperature, on the other hand, has been invariably found to be fatal to them. Fortunately there is at present much unanimity of opinion on this subject. As a result of numerous investigations which have been communicated to the French Academy, and to the Société de Biologie during the last ten years, we find that both the advocates and the opponents of heterogeny are, within certain limits, pretty well agreed on this most important aspect of the question. The many disbelievers and opponents of heterogeny who took part in these investigations, naturally desired that the power possessed by inferior organisms, both animal and vegetable, of withstanding the destructive influence of high temperatures, should be shown to be as high as possible. We may, therefore, with much safety, assume that the limits of vital resistance could not then be shown to be higher than that which these experimenters were compelled, after frequently repeated investigations, to ascribe to such inferior organisms.

In *dry air* or in a vacuum, organisms are capable of withstanding a notably higher temperature than when they are immersed in fluid. According to the direct observations of M. Pasteur, the spores of certain fungi belonging to the family *Mucedineæ*, seem to possess this tenacity of life to a very great extent; but even these, he says, though they still remain capable of germinating after having been raised, for a few minutes in dry air or *in vacuo*, to a temperature of from 120° to 125° C., lose this power absolutely and entirely after an exposure for half an hour, under similar conditions, to a temperature varying from 127° to 130°C. And the labours of the commission (consisting of the following members—MM. Balbiani, Berthelot, Broca, Brown-Séquard, Dareste, Guillemin, and Ch. Robin) appointed in 1869 by the Société de Biologie, to inquire into the subject, led them to the conclusion that the lower animals which were the most tenacious of life§—the rotifers, the "sloths," and the anguillules of tufts of moss or lichen—succumbed at even a much lower temperature than this. In dry air or *in vacuo*, therefore, we may look upon the temperature of 130°C. for thirty minutes, as marking the extreme limit of vital endurance under such conditions, so far at it has been hitherto possible to fix such a limit. There is, at present, no evidence forthcoming to upset this conclusion. When immersed in *fluids*, however, the power possessed by the inferior organisms of resisting the destructive influence of heat is not nearly so great. Comparatively few, whether animal or vegetable, have been found capable of resisting a temperature of 75°C.; and with regard to that of 100°C., it has been admitted, by MM. Claude Bernard and Milne-Edwards, by M. Pasteur, and by all the other most influential opponents of the doctrines of heterogeny, that such a temperature, even for one minute, has always proved destructive to all the lower organisms met with in infusions‖—so far as these had been made the subjects of special and direct experimentation. And, amongst all the diversity of form presented by the lowest Living things, there is so much of uniformity in property—living matter, as we know it, agrees in so many of its fundamental characters—that biologists and chemists alike may feel a reasonable assurance as to the probable universality of any such rule which has been proved to hold good for a very large number of organisms, more especially when, amongst this large number of cases, no exceptions have been encountered.

Practically, however, it will be found that, in order to appreciate the bearings of the experiments which I shall have to relate, it will be necessary for us more especially to know what are the limits of vital resistance to high temperatures, possessed by *spores* of Fungi on the one hand, and by *bacteria* and *vibrios* on the other.

I am not aware of any experiments tending to show that *spores* of Fungi can survive after exposure for even a few seconds in fluids raised to a boiling temperature (100° C.); whilst, on the

* This paper was originally intended for presentation to the Royal Society, but it was finally not presented, when I understood that, owing to the accumulation of many papers and other causes, no evening could be allotted on which it might be read and discussed. Its appearance *in extenso*, and at once, was thought preferable to the reading of its mere title before the Royal Society, with the probability of a very considerable delay in its publication. —H. C. B.

* Hétérogenie, Paris, 1859. † L'Origine de la Vie, Paris, 1868.

‡ Annales de Chimie et de Physique, 1862.

§ This extreme tenacity of life is perhaps due in part to the chitinous integument with which such animals are provided.

‖ It is quite fair to make this limitation, since we are only concerned with the origin of such organisms. Seeds of higher plants, provided with a hard coat, may—especially after prolonged periods of desiccation—germinate even after they have been boiled for a long time in water.

other hand, there is the concurrent testimony of many observers to the fact that, after such exposure, germination would never take place, because the spores were no longer living. This was the result obtained in many experiments made by Bulliard, and related in his "Histoire des Champignons." Mere contact with boiling water was found sufficient to prevent germination; and H. Hoffmann* similarly ascertained that an exposure for from four to ten seconds to the influence of boiling water sufficed to prevent the germination of all the fungoid spores with which he experimented. The experience of other observers has been similar to that above quoted, and amongst these we may cite M. Pasteur himself. Speaking of his experiments with boiled milk in Schwann's apparatus, M Pasteur says:—"Je n'ai jamais vu se former, dans le lait ainsi traité autre chose que des Vibrions et des Bacteriums, *aucune Mucédinée*, *aucune Torulacée*, *aucun ferment végétal.* Il n'y a pas de doute que cela tient à ce que les germes de ces dernières productions ne peuvent résister à 100° au sein de l'eau, ce que j'ai d'ailleurs constaté par des expériences directes."†

The evidence which we at present possess concerning the tenacity of life displayed by *bacteria* and *vibrios* in fluids whose temperature has been raised, is just as decisive as that concerning the spores of fungi. M. Pouchet's observations have lead him to believe that vibrios, in common with all the kinds of ciliated Infusoria, are killed by raising the temperature of the fluid which contains them to 55° C. M. Victor Meunier, also, never found any of these organisms alive after they had been similarly subjected to a temperature of 60° C. I have myself invariably found that vibrios were not only killed, but were broken up and more or less disintegrated, after the fluid had been boiled for even one minute. There is every reason also to believe that an exposure to similar conditions kills their less developed representatives—the primordial monads and bacteria. With reference to these organisms, however, one caution is necessary to be borne in mind by the experimenter. The movements of monads and bacteria may be and frequently are of two kinds. The one variety does not differ in the least from the mere molecular or Brownian movement, which may be witnessed in similarly minute non-living particles immersed in fluids; whilst the other seems to be purely vital—dependent, that is, upon their properties as living things. These vital movements are altogether different from the mere dancing oscillations which non-living particles display, as may be seen when the monad or bacterium darts about over comparatively large areas, so as frequently to disappear from the field of the microscope. After an infusion has been exposed for a second or two to the boiling temperature, these vital movements no longer occur, though almost all the monads and bacteria may be seen to display the Brownian movement in a well-marked degree. They seem to be reduced by the shortest exposure to a temperature of 100°C., to the condition of mere non-living particles, and then they become subjected to the unimpaired influence of the physical conditions which occasion these molecular movements.

Such is the evidence existing as to the power of resisting the destructive influence of heat, manifested by the organisms about which we are at present most interested. It is certainly harmonious enough with our ordinary experience, and is, therefore, not difficult for us to believe. Eggs of higher animals containing an embryo may fairly enough be compared with the lower organisms of which we have been speaking, so far as the matter of which they are composed is concerned; and knowing the profoundly modifying influence of water at a temperature of 100° C. upon the comparatively undifferentiated matter of the embryo and of the egg—and also, we may add, even upon the differentiated tissues of the parent fowl—need we wonder much that the same temperature should have been found hitherto to be destructive to the simple and naked living matter entering into the composition of fungus-spores, and of bacteria and vibrios? If any other result had been ascertained, would there not have been much more reason for surprise?

We must therefore be very cautious how we attempt to set aside the conclusions which have been arrived at on this subject, based as they have been upon direct evidence of a most positive character, on account of other evidence which is indirect and more or less ambiguous. Concerning the legitimacy of such an attempt which has been made by M. Pasteur, I shall have more to say hereafter.

Passing on, then, to the more immediate consideration of our subject, it should be distinctly understood that in all the discussions which have hitherto taken place on the possibility of the evolution of Living things, pre-existing *orgain: matter* has always been supposed to furnish the materials entering into the composition of the new organisms. New combinations and re-arrangements have been supposed to take place amongst the molecules of this pre-existing organic matter, under the agency of some mysterious force or forces—which new combinations of previously uncombined or differently combined molecules have been supposed to result in the production of such primordial living specks as monads and bacteria. The observations of preceding inquirers have also been conducted for the most part on infusions containing organic matter *in solution;* and since the molecules of such matter are then invisible, observers have, of course, been quite unable to follow, by any magnifying power at present attainable, variations in the modes of collocation of such invisible molecules. The minutest specks of living matter—the germs of monads and bacteria, and of the spores of fungi, less than $\frac{1}{50000}$" in diameter—may be seen gradually appearing under the microscope in previously homogeneous solutions containing none of them.* But although microscopical investigation enables us to adduce evidence of just the same kind in elucidation of the mode of origin of certain low organisms, as we possess in explanation of the mode of origin of crystals,† this evidence is not deemed adequate in the case of organisms. A living thing has been supposed to be a something altogether different, incapable of arising out of a mere collocation of matter and of motion; and, therefore, under the influence of this theoretical assumption, whilst chemists and physicists have thought that they could in a measure account for the genesis of crystals by reference to the affinities and atomic polarities of the ultimate constituents of such crystals, they have, for the most part, declined to adopt a similar mode of reasoning in order to account for the appearance of the minutest living specks in solutions containing organic matter. The same reservation is likewise made by the major part of the biologists of the present day. Whilst it is not an article of faith—whilst such a surmise scarcely crosses our minds—that crystals always proceed from pre-existing germs, in the case of Living things, on the contrary, the doctrine *omne vivum ex vivo* has become almost one of the "forms of thought." Principally owing, therefore, to certain theoretical views concerning Life, and in order to account for facts which would otherwise be adverse to these, biologists and others have been accustomed to make the most extensive postulations concerning the supposed universal distribution of "germs" of all the lower kinds of living things; whilst they have recourse to no parallel hypothesis to account for the appearance of crystals, although we know no more—can drive our knowledge back no further into the phenomena attendant upon the birth of crystals than we can into the phenomena which usher in the appearance of organisms. In each case, under suitable conditions, they appear at first as minutest visible specks, in solutions which were previously homogeneous. In the one

* Etudes mycologiques sur les fermentations.

† Annales de Chimie et de Physique, 1862, p. 60.

* A more complete account of this part of the subject will be given in a work on *The Beginnings of Life*, shortly to be published.

† Or, better still, concerning the mode of origin of those modified crystals which appear on mixing solutions of gum and carbonate of potash, as described by Mr. Rainey (*On the Mode of Formation of the Shells of Animals*, &c., 1858). The malate of lime contained in the gum is decomposed, but owing to the slow mixing of the solutions in the presence of gum, the insoluble carbonate of lime does not appear in its usual crystalline condition, but in globular modifications, resembling calculi. When portions of the two solutions are mixed under the microscope, Mr. Rainey thus describes what takes place:—"The appearance which is first visible is a faint nebulosity at the line of union of the two solutions, showing that the particles of carbonate of lime, when they first come into existence, are too minute to admit of being distinguished individually by the highest powers of the microscope. In a few hours exquisitely minute spherules, too small to allow of accurate measurement, can be seen in the nebulous part, a portion of which has disappeared, and is replaced by these spherical particles. Examined at a later period, dumb-bell-like bodies will have made their appearance, and with them elliptical particles of different degrees of excentricity" (p. 9). Mr. Rainey made use of one of Ross's $\frac{1}{8}$" object glasses. These modified crystals are produced with no more rapidity than the lowest living things seem to be in other solutions, during hot weather; and the shapes of the products in the two cases are remarkably similar, judging from Mr. Rainey's figures. The protraction of the process, brought about by the presence of gum, serves to bring out more clearly the real relationship existing between the formation of crystals and that of the lowest organisms, in homogeneous solutions.

That there are very strong reasons accounting for this belief I do not attempt to deny. There is, however, much evidence to show that the very same organisms which do propagate their kind after this acknowledged method, may themselves originate *de novo.* Whilst allowing, therefore, the widest generality to any given rule, we may well hesitate before, on this account, we reject certain other alleged facts which are complementary rather than contradictory.

case we have to do with *crystallisable* matter, in solution, and in the other with those big-atomed, unstable compounds which constitute the so-called *colloidal* states of matter. And it is well to call attention to the fact that, concerning these latter states, the late Professor Graham, one of the most cautious and philosophical of chemists, wrote:*—"Another and eminently characteristic quality of Colloids is their mutability. Their existence is a continual metastasis. . . . . . The Colloidal is, in fact, a dynamical state of matter, the crystalloid being the statical condition. The colloid possesses ENERGIA. *It may be looked upon as the probable primary source of the force appearing in the phenomena of vitality.* To the gradual manner in which colloidal changes take place (for they always demand time as an element) may the characteristic protraction of chemico-organic changes also be referred."

Granting, then, that microscopical evidence alone may not be quite satisfactory for settling the mode of origin of such primordial living things as monads and bacteria, it becomes obvious that we must endeavour to throw light upon this evidence by other methods of investigation. Still it should also be kept steadily in mind that microscopical evidence is equally powerless to throw light upon those primordial collocations which initiate the formation of crystals. The problem concerning the primordial formation of crystals and living things is essentially similar in kind. Any difference in degree between our present knowledge on these two subjects must not blind us as to their essential similarity. Monads and bacteria are produced as constantly in solutions of colloidal matter as crystals are produced in solutions containing crystallisable matter. Crystallisable substances are definite in composition, and give rise to definite statical aggregations; whilst colloidal substances, much more complex and unstable, give rise on the contrary to dynamical aggregations. These dynamical aggregations, though they at first make their appearance in the form of monads and bacteria, are, by virtue of the properties of their constituent molecules, endowed with the potentiality of undergoing the most various changes, in accordance with the different sets of influences to which they are submitted. They are dynamical aggregates, in fact, in a condition of unstable equilibrium, and are capable of being diverted into new modes of current and reciprocal molecular activity in response to changes in their medium or environment. These differences between the products met with in solutions containing crystallisable and colloidal matter respectively, may, however, be due simply to the original difference in nature between such kinds of matter. Respecting the origin of the first visible forms which appear in either kind of solution, the evidence which we possess is precisely similar in nature. If such microscopical evidence does not enable us to get rid of the doubt that the smallest visible specks of living matter may have originated from *invisible* "germs" of such organisms, neither does it any more enable us to dispense with the supposition that the smallest visible crystals may have originated from pre-existing *invisible* "germs" of crystals. There is, in fact, so far as actual scientific evidence goes, almost as good reason for a belief in the universal distribution of invisible "germs" of crystals, as there is for our belief in the universal distribution of invisible "germs" of monads and bacteria. The very existence of the one set of invisible "germs" is, in fact, just as hypothetical as the existence of the other. Monads and bacteria we do know; but concerning the existence of invisible "germs" of monads and bacteria we know just as little as we do concerning the existence of invisible "germs" of crystals.

And yet almost all the difficulties in finally settling the question of the truth or falsity of the doctrines of "spontaneous generation" are centered in this question as to the mode of origin of monads, bacteria, and such fungus-spores as similarly originate in homogeneous solutions in the form of the most minute specks of living matter.† Given the existence of such primordial living particles, and we can easily watch changes taking place in aggregations of them, which lead to the production of much larger and altogether different organisms. We can then trace out with the microscope various kinds of evolution—processes of so-called "spontaneous generation," in fact—the establishment of the reality of which is just as much in opposition to generally received biological notions, as is the supposition that the primordial units themselves are able to come into being *de novo* after particular modes of collocation of colloidal molecules hitherto invisible. The amount of difference between such invisible organic molecule and the speck-like organism less than $\frac{1}{50000}''$ in diameter, which appears in the previously homogeneous solution, may be no more real or striking than is the difference between some of the visible monads or bacteria and the much larger and higher kinds of living things, whose mode of origin I am about to describe, and which may be seen to arise after particular sets of changes have taken place in aggregations of such monads and bacteria. Of two things previously deemed alike improbable, the one which can come within the range of our vision may be shown to take place—the other being, unfortunately, beyond our ken, admits of no such proof. The unmistakeable upsetting of our preconceptions on the one subject should, however, make us cautious how, on theoretical grounds, we pronounce that to be impossible in the case of organisms which we, nevertheless, believe to be possible and actual in the case of crystals: especially when, in these two sets of cases, the amount of actual evidence which we possess is almost equal and similar.

Waiving, then, for the present the consideration of additional evidence as to the mode of origin of the primordial living particles, the monads and bacteria, and of the apparently similarly originating fungus-spores, I will describe some of the evolutional changes by which higher organisms may be seen to arise in a pellicle formed by an aggregation of the simpler kinds of living particles.

I.

The Mode of Origin of Unicellular Organisms and of Spores of Fungi in the "proligerous pellicle" of organic solutions.

What Burdach named the *proligerous pellicle* of organic solutions is made up of an aggregation of monads and bacteria in a transparent jelly-like stratum, on the surface of the fluid. It constitutes at first a thin scum-like layer, and although the monads and bacteria entering into its composition are motionless, M. Pouchet and others were not warranted in assuming from this fact alone that they were dead. There is, indeed, good reason for believing to the contrary, since, as pointed out by Cohn, when any of these particles are set free from the broken edge of a pellicle they always resume their movements. Motion, therefore, may simply be prevented by the presence of the transparent jelly-like material in which they are imbedded, although the particles may be undoubtedly living.

My observations on this subject have been principally carried on throughout the winter months; and this is a time not favourable for the appearance of ciliated Infusoria in organic infusions. Hence it is, perhaps, that I have not been able to witness any of those changes in the pellicle which have been described by Pouchet, as resulting in the evolution of *Paramecia*, *Kolpodæ*, and other ciliated Infusoria. The changes which I have observed, however, have been so indubitable in nature, have been seen so frequently, and have had such a close general resemblance to those which have been described as leading to the evolution of *Paramecia*, that I am quite disposed to believe in the correctness of the observations that have been made by Pouchet and others on this part of the subject.

My own observations have been conducted principally on the pellicle of hay infusions, and one of the commonest processes of what may be termed *secondary organisation* takes place in the following manner. In a pellicle which previously presented a uniform appearance, certain areas, altogether irregular in size and shape—though always presenting outlines bounded by curved lines—gradually make their appearance. These are, at first, distinguishable from the general ground-work of the pellicle only by their somewhat lighter aspect. On careful microscopical examination with high powers, it may be seen that the boundary of such an area—measuring it may be as much, or more than $\frac{1}{300}''$ in diameter—is pretty sharply defined from the surrounding unaltered granular stratum. The immediately contiguous granules of this are occasionally somewhat more tightly packed, though at other times no such change is observable. In either case the unaltered portion of the pellicle is quite different from the included lighter area, because in this an increase has, apparently, taken place in the amount of jelly-like material between the granules, and, as well, there is a certain alteration in the refractive index, and occasionally in the size of the granules (monads and bacteria) themselves. The next change observable is, that the included area shows lines crossing it here and there, which at first tend to map it out into certain larger divisions. These intersecting lines gradually increase in number, till at last

* Philosophical Transactions, 1862. Capitals and italics are employed as we give them in the memoir itself.

† Such, for instance, is one of the modes of origin of *Torula* cells.

the mass becomes divided into an aggregation of rounded or ovoid bodies each about $\frac{1}{6000}''$ in diameter. As these subdivisions are taking place, the mass as a whole separates from the unaltered pellicle by which it is surrounded. Occasionally there is

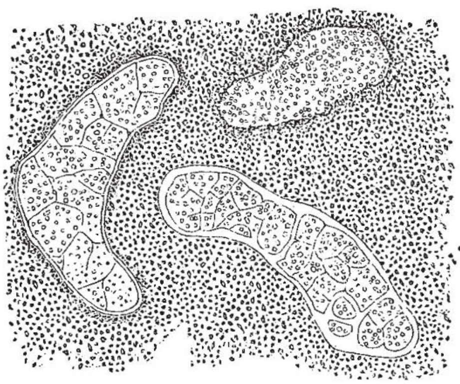

FIG. 1.—Development of Unicellular Organisms: three areas of differentiation showing different stages.

the most distinct interval, at a certain stage, between the parent pellicle and this differentiating mass, whose subdivisions also gradually separate from one another. These subdivisions now appear as independent unicellular organisms, bounded by a delicate membrane, and containing, perhaps, from four to eight of the altered monads and bacteria in their interior.

Throughout the winter months, such areas of differentiation and such resulting unicellular organisms were frequently met with. The unicellular organisms seem during such weather to persist for a very long time in this condition, merely, perhaps, increasing somewhat in size, and most of them ultimately become disintegrated without undergoing further development. They were always seen in a completely motionless condition, and presented no trace of a cilium, so that they were altogether different from the creature known as *Monas lens*. In one solution of hay in which such organisms had been present for some time, after a few days of warmer weather, several of them were found to have become spherical, and to have undergone a considerable increase in size. Some of these were as much as $\frac{1}{2000}''$ in diameter, and on one occasion a stage in the actual transition of one of these unicellular organisms into an *Amœba* was seen with the most perfect distinctness. One

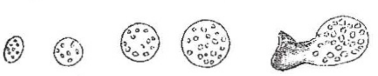

FIG. 2.—Representing gradual enlargement of Unicellular Organisms, and conversion of one of them into an *Amœba*.

half of the organism was distinctly amœboid in character, whilst the other half was almost unchanged, containing large granules like those in the unaltered cells. As slow alteration in shape, of a slug-like character, took place in the anterior diaphanous protoplasmic portion, slow rolling movements occurred amongst the granules in the posterior cell-like portion, whose matrix seemed to have been rendered more fluid. I watched this organism for about half an hour, and then, wishing to examine other portions of the specimen of pellicle in which it had been contained, I moved the glass and was afterwards unable to find this particular specimen again. Unfortunately, I could discover no other Amœbæ or transition states.*

In other cases the areas of differentiation, commencing in a somewhat similar way, terminate in the production of spores of fungi, and I will now describe the mode of evolution of such spores as I observed it taking place in portions of a pellicle having a brownish colour, from an old infusion of hay. The development of this brownish tinge in the earlier stages made it more easy to unravel the nature of the earlier changes. The areas which began to differentiate were generally not very large. They were at first quite colourless, and the granules were separated from one another by a notable amount of transparent jelly-like material. The granules themselves were mostly shaped like the figure 8, and each half was about $\frac{1}{30000}''$ in diameter. A later stage was seen, apparently, in other areas which had assumed a very faint brownish tinge, and which presented evidences that

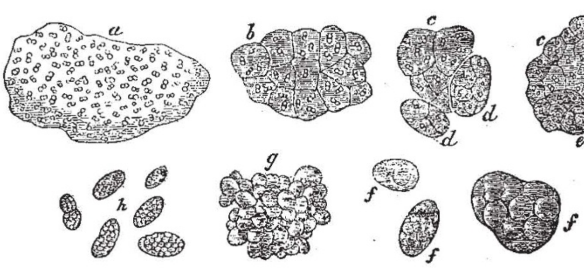

FIG. 3.—Mode of origin of Spores of Fungi out of differentiating portions of a Pellicle formed on an infusion of hay.

subdivision was taking place. As the process of subdivision progressed, so the brown tinge became gradually deeper. Ovoid masses were frequently seen about $\frac{1}{2000}''$ or $\frac{1}{1000}''$ in diameter, of a decidedly brown colour, with from 8 to 12 or more ovoid subdivisions within the common envelope. As multiplication advanced, the individual products lost all trace of their original granular condition. They became quite homogeneous and highly refractive masses of a brown colour, looking almost like large brown fat globules. At last, multiplication still proceeding, the distended, and always thin, cyst-like, general envelope becomes ruptured and disappears, leaving only an irregular mass of spherical or ovoidal bodies of various sizes. The individual segments, as soon as this process of multiplication has ceased, increase in size, and then gradually become less refractive and lighter in colour. A slight differentiation of their contents also again takes place, marked by the appearance of faint lines within, as they assume the appearance of ovoid bodies about $\frac{1}{4000}''$ in diameter.* Even when they have attained this stage of development, they may again undergo a process of division; though generally, after a time, they give origin to ordinary mycelial filaments.

Similar changes in the refractive index have been frequently noticed in other cases when a protoplasmic mass is at the same time differentiating and undergoing a process of multiplication,† whilst the mode and frequency of the sub-division is exactly comparable with what so frequently happens to the gonidia of lichens.

The changes which I have described represent, I think, only two extreme types of a mode of metamorphosis which is apt to take place in portions of the pellicle. In the one case a certain area of the pellicle, after undergoing some changes, resolves itself into a number of ovoid bodies, which collectively are about equal in bulk to the altered area itself; whilst, in the other case, at different stages, the segments of the altered area undergo a process of growth and sub-division, so that ultimately the mass of spores which results far exceeds in bulk that of the original area when it began to undergo change.

At other times intermediate processes are met with, and then fungus-spores are produced after a fashion more closely resembling that which leads to the production of the unicellular organisms above described. The areas of change are then larger than those last described, and colourless throughout, whilst the processes of growth and multiplication are less marked at the different stages. Where fungus-spores result after this fashion, the changes in the refractive index, and the homogeneous appearance previously alluded to, still generally manifest themselves at the ultimate stage of division, though nothing of this kind shows itself in the more simple process leading to the production of the unicellular organisms.

Now, however mysterious the nature of these changes may be, which take place, as it seems to us, "spontaneously" in the pellicle on the surface of a solution of organic matter, they are exactly comparable with other changes occurring within the terminal dissepiments of a kind of submerged mucor, named *Achlya prolifera*, similar to those which occur within the thecæ of certain other fungi and of certain lichens, and altogether analogous to that which, as Prof. Haeckel says, takes place in

* Prof. Hartig has, however, described a similar mode of origin of Amœbæ from unicellular organisms, in his observations on the phytozoa of *Marchantiæ*. See *Journal of the Microscopical Society*, 1855, p. 51.

* The markings of these spores are more obscure and less regular than they are represented (*h*) in the woodcut.

† See Nicolet, in Thompson's *Arcana Naturæ*, 1859.

some of the simplest *Amœbæ*, after they have encysted themselves. In all these cases, formless and apparently homogeneous or merely granular living matter, resolves itself more or less rapidly into a number of individualised segments, which are

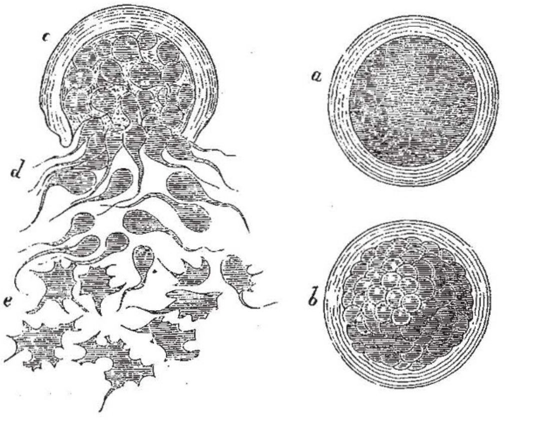

FIG. 4.—Representing subdivision of formless living matter within encysted *Protomyxa*, and exit of products from cyst as active tailed Zoospores, ch subsequently become converted into reptant Amœbæ (Haeckel).

capable of existing as independent living things.* These changes occur in the formless matter of definite organisms, and the products of subdivision tend to reproduce organisms of a similar kind; but the changes which take place in portions of the pellicle are changes occurring in fortuitously aggregated living matter, and the resulting products are, as might have been expected, more variable in kind. There is every reason to believe that the changes which take place in the homogeneous living matter of the encysted *Protomyxa* occur by reason of the molecular properties of this living matter, and are not occasioned by any occult influence exercised by the mere inert cyst-wall, which is but a product of the living matter that it

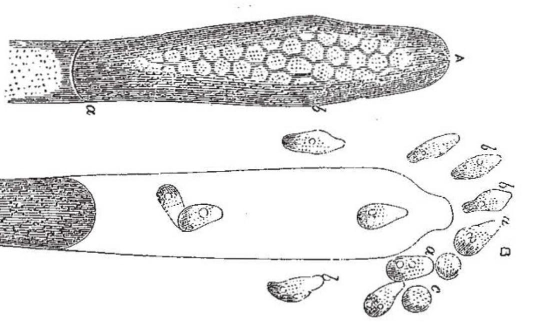

FIG. 5.—Showing the mode of origin of motile Zoospores within the terminal dissepiment of *Achlya* (Tulasne).

encloses. And so we have good reason for supposing that the changes which take place in the mere granular mucilage of the rapidly-formed terminal segment of an *Achlya*, by which this in the space of less than two hours resolves itself into free-swimming zoospores, is to be ascribed to the molecular properties of the mucilage itself which undergoes the change. In the pellicle, on the other hand, we have an aggregation of granular living matter also, and the observations which I have adduced simply go to show that those molecular properties of living matter which lead to its differentiation and further organisation, are not limited to the living matter that is contained within organisms of a definite type. Just as the changes which take place in the structureless living matter of these organisms seem to be due to the forces acting upon, and to the reactions amongst, the several molecules of which it is composed, so do the changes which occur in given areas of the pellicle seem referrible to the influence of physical forces upon the living molecules of which it is composed, and upon the mutual inter-action of these upon one another when under the influence of such incidence. In both cases the changes take place in living matter; in both cases they are the results of molecular activity: in the one set of cases they take place in a fortuitous aggregation of living matter, and the products are accordingly very variable in nature, whilst, in the other set of cases, just as they take place in living matter which constitutes part of a definite organism, so are the products more definite in kind.

But this process, which most certainly occurs in the living matter of a pellicle, is of a kind not hitherto generally recognised as one of the modes by which unicellular organisms, or spores of fungi, may originate. These are, in fact, instances of what has been called "spontaneous generation," or what we may better term heterogenous evolution. The majority of biologists would be as much inclined to believe that these processes did not take place as they are inclined to disbelieve that a monad or a bacterium may be born *de novo* in a solution of organic matter. The occurrence of the one process has been thought to be about as improbable as that of the other. Yet the one can be seen undoubtedly taking place with the aid of the microscope alone. Unfortunately, however, the organic molecules, which are supposed to coalesce in the solution of organic matter, in order to form the smallest visible Living particle, are themselves invisible. We cannot, therefore, trace the genesis of one of these particles with the aid of the microscope, any more than we are able to trace the genesis of a crystal beyond its *minimum visible* stage. In the one case physicists and biologists willingly assume that such ultimately visible particles are the products of a "spontaneous" coalescence of molecules, which are themselves invisible, whilst in the case of organisms they will grant no such assumption. They require us to prove, in fact, that such organisms have not been produced from pre-existing though perhaps invisible "germs," before they will grant for organisms that probability which they at once concede in the case of crystals. This difference which is made between the two cases seems due, in great part, to some theoretical views which are held concerning the nature of Life. And yet it would not be difficult to show that the metaphysical or vitalistic theories in question, to which they commit themselves, are directly opposed to some of the most accredited scientific doctrines of the day.* The doctrine of the Conservation of Energy and of the Correlation existing between the Vital and the Physical forces do, if pushed to their ultimate issues, inevitably bring us to the conclusion that the forces acting within all Living bodies are molecular forces, and that such forces are derived from the physical forces of the outside world just as surely as the matter of the organism formerly existed outside itself. The most careful interpretation of scientific evidence, moreover, would lead us to the conclusion that what is called the Life—or in other words the aggregate set of phenomena displayed by one of the simplest bodies which we call a living thing—is as much the essential and inseparable attribute of the particular molecular collocation which displays it, as the properties of the crystal are essential to the kinds and modes of aggregation of the molecules which enter into its composition. It may be maintained, therefore, that all *à priori* presumptions, based upon the best scientific evidence, would lead us to disbelieve the "vitalistic" theories which are still held by many at the present day. It is the vitalist, however, who alone has any logical reason for insisting that what may be a good and valid mode of accounting for the origin of crystals cannot be considered to hold good in the case of organisms. Those who believe that the forces acting in Living things manifest themselves in the individual molecules of which these are composed, and that such forces are convertible with the ordinary physical forces, have, on the other hand, strong *à priori* reasons for believing that Life will manifest itself wherever particular collocations of complex organic molecules occur. It rests, then, in reality, with the vitalist, who assumes the truth of a mere theory, in favour of which he can adduce no scientific evidence, to show why a different rule should be presumed to

* All this part of the subject will be much more fully treated in the work on *The Beginnings of Life*. The possession or not of the property of motility seems to be an altogether unessential characteristic. The products of subdivision of such an encysted *Amœba* as *Protomyxa aurantiaca* are motile zoospores, and so are those of the fungoid *Achlya prolifera*; but the reproductive products arising from the subdivision of the formless matter within the spore-cases of *Pezizæ* and other fungi are motionless, and this is the case also with the gonidia of the *Saprolegniæ*, which are fungoid organisms, otherwise almost undistinguishable from *Achlya*.

* This I shall attempt to show fully elsewhere.

hold good in the birth of crystals and of organisms respectively.* Those who hold opposite opinions need only suppose that molecules of "organic" matter, or some such complex molecules, may aggregate and arrange themselves after certain modes to produce a Living thing—just as a crystal, endowed with its particular properties, is producible by other modes of aggregation —because with them Life is considered to be a product of molecular collocation and of molecular change. And if the "vitalist" wishes to establish the existence of a more fundamental difference between crystals and organisms than we are prepared to grant, seeing that the scientific evidence seems to be against him, it remains for him at least to endeavour to show good grounds for the establishment of such difference.

It should be remembered, then, that in the present state of science all theoretical considerations seem favourable to the views of the evolutionists, and that the only thing which can be opposed to them is the *assumption* that those processes of reproduction which take place amongst all known varieties of living things are the *only* processes by which such living things can arise. But now, already, by means of microscopical evidence alone, it has been shown that Living things may arise by a process of heterogenesis—not as products of a pre-existing organism like themselves, but by a process altogether different from those which have been hitherto supposed to be general. And it is worth remembering, as we have before pointed out, that the supposed coalescence of invisible molecules and the changes which lead to the production of the minutest living monad in organic solutions, if they could be shown to be true, would not be one whit more startling than those very changes which, before disbelieved in, can now be easily shown to take place in the "proligerous pellicle." Have we not seen that out of a mere fortuitous aggregation of living particles, and the subsequent metamorphoses taking place therein, organisms appear which are much larger, and of a much higher type than those which preceded them, although such a mode of origin was formerly regarded almost as "impossible"?

* I will here make two quotations in order to show that the opinions of two of our leading scientific men (many others might have been quoted) are not at all opposed to the comparisons I have been instituting. They both, in fact, declare emphatically that the phenomena of Life are phenomena of molecular physics.

In his address to the Mathematical and Physical Section of the British Association in 1868, Prof. Tyndall, as president, speaking of a grain of corn, said :—"But what has built together the molecules of the corn? I have already said, concerning crystalline architecture, that you may, if you please, consider the atoms and molecules to be placed in position by a power external to themselves. The same hypothesis is open to you now. But if in the case of crystals you have rejected this notion of an external architect, I think you are bound to reject it now, and to conclude that the molecules of the corn are self-posited by the forces with which they act upon each other. It would be poor philosophy to invoke an external agent in the one case and to reject it in the other. . . . . But I must go still further, and affirm that in the eye of science the animal body is just as much the product of molecular force as the stalk and ear of corn, or as the crystal of salt or of sugar. . . Every particle that enters into the composition of a muscle, a nerve, or a bone, has been placed in its position by molecular force. And unless the existence of law in these matters be denied, and the element of caprice introduced, we must conclude that, given the relation of any molecule of the body to its environment, its position in the body might be predicted. Our difficulty is not with the *quality* of the problem, but with its *complexity*." (Pp. 4 and 5.)

Prof. Huxley, again, in his article on "Protoplasm," in the *Fortnightly Review* for February 1869, 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. I see no break in this series of steps in molecular complication, and I am unable to understand why the language which is applicable to any one term of the series may not be used to any of the others. We think fit to call different kinds of matter carbon, oxygen, hydrogen, and nitrogen, and to speak of the various powers and activities of these substances as the properties of the matter of which they are composed. . . . Is the case in any way changed when carbonic acid, water, and ammonia disappear, and in their place, *under the influence of pre-existing protoplasm*, an equivalent weight of the matter of life makes its appearance? . . . What justification is there, then, for the assumption of the existence in the living matter of a something which has no representative or correlative in the not living matter which gave rise to it?" (The passage I have marked by italics indicates the extent to which Prof. Huxley stops short of the views I have been endeavouring to support.)

Now I maintain that the logical outcome of such doctrines as these is that Life *may* manifest itself whenever certain particular collocations of complex molecules occur, just as surely as crystalline properties will be manifested by chloride of sodium whenever the molecules of this substance combine to form crystals. The *à priori* presumptions being rather in favour of (certainly not opposed to) the occurrence of the new evolution of Living things, we have to show, as well as we are able, that there *is* a tendency to the occurrence of such clusterings as will lead to the formation of bacteria or of fungus-spores, just as we feel compelled to believe that there is a tendency to the occurrence of those particular clusterings of molecules which result in the formation of crystals.

II.

On the probable Evolution of Living Things in Organic and Saline Solutions, which have been previously exposed to high Temperatures, in airless and hermetically sealed vessels.

We must now come to the consideration of all the experimental evidence which can be adduced in support of what the microscope teaches us as to the mode of origin of the lower kinds of Living things.

The method of experimentation which has been principally relied upon, has, since 1837, always been that introduced by Schwann. Sometimes the correspondence has been exact, and sometimes his experiments have been repeated with some slight modification. In this method, the solution of organic matter is first boiled in a flask, the neck of which is securely connected with a tube closely packed with portions of red-hot pumice-stone, or other incombustible substance: after the solution has been boiled for some time, and all the air of the flask has been expelled, the flask itself is allowed to cool whilst the tube containing the closely-packed red-hot materials is still maintained at the same temperature, in order that whatever air enters into the flask may be subjected to a calcining heat as it passes through the tube. When the flask has become cool it will then contain only the previously boiled solution in contact with air at ordinary atmospheric pressure, which has been calcined. Since it has been hitherto settled that the lower kinds of organisms which may be contained in the solutions, are destroyed when these fluids are raised to a temperature of 100° C., and that no organisms have been known to survive after having remained for thirty minutes in air raised to a temperature of 130° C., the boiling of the fluid for a time and the calcination of the air has generally been supposed to be a sufficient precaution to ensure the destruction of all organisms in the experimental media. Experiments conducted in this way have been said to yield negative results by some, whilst others have maintained that in spite of all such precautions, destined to destroy pre-existing Living things, they do, nevertheless, obtain low kinds of organisms, after two or three months, if not before, in their experimental fluids.

Negative results in these experiments can of course prove little or nothing ; they may be explained equally well by either party : either no organisms have been found, because they or all the germs which could give rise to them have been killed ; or it is just as fair for the evolutionists to explain the absence of organisms on the supposition that the particular fluids employed have not yielded them because of the severity of the destructive conditions to which the particular organic matter in the previously boiled fluids had been subjected. When organisms *are* found, however, in fluids which have been legitimately subjected to the conditions involved in Schwann's experiments, then one of two things is proven : either the amount of heat which hitherto was deemed adequate to destroy all pre-existing organisms is in reality not sufficient, or else the organisms found must have been evolved *de novo* as the evolutionists suppose. Unless, therefore, the standard of vital resistance to heat can be shown to be higher than it was formerly supposed to be, any single positive result when Schwann's experiment has been legitimately performed, is of far more importance towards the settlement of the question in dispute than five hundred negative results. It would tend to show that in the particular fluid employed organisms might be evolved *de novo*. And yet positive results in the performance of these experiments have been obtained again and again by Schwann himself, by Ingenhousz, by Mantegazza, Pouchet, MM. Joly, and Musset, Jeffries Wyman, Dr. Child, and, not to mention any others, even by M. Pasteur himself.*

But even this is not all ; organisms have been found in fluids which had been contained in closed vessels, after exposure to conditions still more severe. Prof. Jeffries Wyman, of Cambridge, U.S., published an account (which I am sorry to say I have been unable to obtain from any of our libraries) in 1862 of experiments in which he had boiled fluids containing organic matter for a period of two hours under a pressure of two atmospheres, that

* We have the testimony of M. Pasteur to the fact that organisms may almost always be met with when milk or some other alkaline fluid is made use of in Schwann's experiment. He says he has always met with negative results, however, if such fluids have been raised to a temperature of 110° C. rather than 100° C. Concerning his inferences from these experiments I shall have more to say hereafter.

is to say, at a temperature of 120·6° C. To the fluids so treated no air was allowed access, except what had passed through the capillary bores of white-hot iron tubes. And yet, when the flasks were broken, after a certain time, organisms were found in the fluids which had been submitted to these conditions. Prof. Mantegazza has obtained organisms from the fluids of hermetically closed flasks, after these, containing the putrescible fluids and common air at ordinary atmospheric pressure, had been subjected for some time to a temperature of 140° C.; and Prof. Cantoni, of Pavia, has obtained bacteria and vibrios in the fluids of similarly prepared closed flasks, after these had been exposed in a Papin's digester to a temperature of 142° C. for four hours. And still, though no positive evidence has been forthcoming to show that the standard of vital resistance can be raised—though nobody has shown that any Living thing which has been made the subject of experimentation has been found alive after an exposure for a minute or two in a fluid raised to 100° C., or after an exposure for thirty minutes to a temperature of 130° C. in dry air or *in vacuo*, many scientific men are as much disinclined as ever to admit that the organisms found by the above-mentioned observers could have been evolved *de novo*.

For all those, however, who form their opinions on such matters in accordance with scientific evidence, rather than in obedience to theoretical preconceptions, it must be admitted that the balance of evidence is at present altogether in favour of the supposition that organisms can arise *de novo*. And it only remains for those who are opposed to the notion from an *à priori* point of view to bring forward positive evidence tending to show that the standard of vital resistance, for the organisms in question, is much higher than it has been hitherto shown to be.

Some of the additional evidence I have now to bring forward, therefore, only tends to strengthen the validity of the conclusion which was already deducible from the experiments of Pouchet, Wymann, Mantegazza, Cantoni, and others.

Hitherto, in speaking of the experiments of Schwann, I have only incidentally referred to the destructive influence of heat upon the organic matter contained in the solutions, though very strong evidence could be adduced to show that such organic matter is notably altered after these solutions have been raised to the temperature of 100° C. The disruptive agency of heat is fairly enough supposed by the evolutionists to destroy some of the more mobile combinations in each solution—to break up, more or less completely, in fact, those very complex organic products, whose molecular instability is looked upon as one of the conditions essential to the evolutional changes which are supposed to take place. I shall postpone for the present the consideration of the question as to how far this destructive agency of heat is affected by the alkalinity, neutrality, or acidity of the fluids, though I shall towards the close of this paper bring forward evidence tending to show that organic matter in acid solutions is more damaged by the temperature of 100° C. than is that which is contained in neutral or slightly akaline solutions, when these are heated to the same extent.

To any one looking boldly at the problem, the question which now seems to suggest itself is, whether any other substances can be employed in place of the organic matter, such as would not be injured by a temperature of 100° C., and which, whilst containing the necessary elements for the formation of an organism, might also permit the occurrence of those peculiar molecular re-arrangements which result in the formation of Living things? Postponing, however, the consideration of this question for the present, I will first refer to the influence of another of the detrimental conditions involved in Schwann's experiments.

In those instances where the results were positive, and in which calcined air and previously heated organic solutions were shut up in hermetically-sealed vessels, nothing but the lowest forms of living things ever appeared—mere monads, bacteria, and vibrios—and these generally not till after the expiration of two, three, or more months. There seems reason to believe that the delay, and the very low forms of the organisms met with, are attributable, in great part, to the increased tension which almost invariably occurs within the closed vessels. In some cases the tension becomes so great that it ultimately bursts the flasks. This took place several times in the course of Dr. Child's experiments. After reflection upon these facts, it seemed to me that there was not much room for surprise, looking at it from the evolutionist's point of view, that the results should have been so unsatisfactory. The small amount of space above the level of the fluid, is already occupied, at the time that the flask is hermetically sealed, with air under ordinary atmospheric pressure. But, when putrefactive or evolutional changes begin to take place in the fluids containing organic matter, such changes are almost sure to be attended by the liberation of gases either simple or compound. And in direct proportion to the extent of the liberation, so does the tension and consequent pressure upon the fluid increase within the flask. This pressure upon the solutions might and probably would tend to prevent, in proportion to its extent, those life-giving re-arrangements which are presumed to take place amongst the molecules of the organic matter contained therein. Having come to this conclusion, and as there also seemed to be good reason for the belief that atmospheric air was not needed for the development of bacteria and vibrios, it occurred to me that it would be possible so to modify the experiment of Schwann that it might be repeated under conditions more satisfactory to the evolutionists, and at the same time in a way which would be not less in accordance with the views of the panspermatists. The withdrawal of all air from the flasks in which the boiled solutions were contained, rather than the admission of calcined air, seemed to be the kind of modification which was desirable.* Then the contamination of the boiled fluids with possible atmospheric germs would be as effectually provided against as if air had been only allowed to enter after it had been calcined, and the seemingly obvious advantage would be attained that there would be even greater freedom than usual for the commencement of evolutional changes, on account of the diminished pressure upon the fluids contained *in vacuo*. It was presumed, also, that changes might go on for a certain extent before the evolution of gases had been sufficient to exercise such a repressive influence as to prevent their continuance.

The results of experiments conducted upon these principles have been most uniform, and have been of such a nature as to tend to support the truth of the reasonings which dictated them.

The flasks employed have generally been of small size, capable of holding about two ounces of fluid. These have proved to be quite large enough, and their small size made it easy for me to manage the whole process with a very slight amount of assistance. The method adopted was as follows:—After each flask had been thoroughly cleaned with boiling water, three-fourths of it was filled with the fluid which was to be made the subject of experiment. With the aid of a small hand blow-pipe and the spirit-lamp flame, the neck of the flask, about three inches from its bulb, was then drawn out till it was less than a line in diameter. Having been cut across in this situation, the fluid within the flask was boiled continuously for a period of from ten to twenty minutes. At first ebullition was allowed to take place rapidly (till some of the fluid itself frothed over) so as to procure the more thorough expulsion of the air; then the boiling was maintained for a time at medium violence over the flame of my reading-lamp, whilst the greatly attenuated neck of the flask was heated in the flame of a spirit-lamp placed at a corresponding level. The steam for a time poured out violently into the flame of the spirit-lamp; and whilst my assistant (my wife) turned down the flame of the reading-lamp so as to diminish still further the violence of the ebullition, I directed the blow-pipe flame upon the narrow orifice of the neck of the flask, and so sealed it hermetically. Immediately that the orifice was closed, the heat was withdrawn from the body of the flask.

After a little practice I soon became able to procure in this way an almost perfect vacuum. Even though the vessels were

* I was actually led to adopt this important modification, perhaps, by a mere chance. In the spring of last year Mr. Temple Orme, of University College, had kindly undertaken to perform some experiments with me bearing upon the subject of "Spontaneous Generation." We at first proposed to repeat, with some very desirable variations which he suggested, Schultze's experiments. One day, however, he told me he had boiled an infusion of hay for four hours, and had then hermetically sealed the neck of the flask whilst ebullition continued. In this way a more or less perfect vacuum was procured. This he did as a sort of tentative experiment, and it was then, on thinking over the subject, that I resolved to give the plan a thorough trial, as it appeared to me that by so doing I should be working under conditions which were most in accordance with the theory of evolution. I performed four experiments at that time in concert with Mr. Temple Orme, with hay infusions which had been boiled for four hours and had then been sealed up *in vacuo*. In each of these fluids organisms were found after a comparatively short time. These were the first experiments performed under such conditions. In my subsequent work I have not had the benefit of Mr. Orme's personal assistance, although I have frequently profited by suggestions which he has made.

so small, momentary ebullition could generally be renewed again and again for the space of five minutes after they had been hermetically sealed, by the mere application of one of my fingers, which had been dipped into cold water, to a portion of the glass above the level of the fluid. The water-hammer effect was also very obvious in several which were tested in this fashion.

I believe that an almost perfect vacuum can be produced in this way; in the first violent ebullition the air is driven out of the flask by the fluid, and as ebullition is continuously kept up after this till the flask is hermetically sealed, there is always an outpouring of heated vapour, and no opportunity for a re-ingress of air. But, even if in any given case the vacuum should not prove to be absolute, it does not seem to me that there would be any material abatement from the severity of the conditions which the panspermatists have a right to demand. If, on the one hand, absolutely the whole of the air had not been expelled from the flasks during the process of ebullition, what remained would necessarily be mixed up with a very much larger quantity of continually renewed aqueous vapour, and the effect would probably be that any living things would be just as effectually and destructively heated as if they were lodged in the boiling solution itself; whilst if, on the other hand, the boiling had been arrested for one or two seconds before the complete closure of the almost capillary orifice at the mouth of the flask, even if any air entered, it would have had first to pass through the blow-pipe flame, and then through the white-hot capillary orifice—it would in fact have been calcined as in Schwann's experiment. The conditions of the experiment would then have been no less severe, and the only effect would be that the vacuum with which I prefer to work would have been rendered by so much the less complete. Although I make these remarks with the view of meeting criticisms, I am inclined to think that the vacuum in my experiments has been complete; and it should be remembered that M. Pasteur always adopted this method when he wished to preserve solutions for a time *in vacuo*. Whenever he desired to make comparative trials with the air of different localities, the solutions which had been prepared in this way were assumed by him to be contained *in vacuo*, so that the flasks could then be taken to the localities, with the air of which he wished to experiment. There the necks of the flasks were broken, in order that they might become filled with the air of the respective localities. After this had been done the flasks were resealed, and kept for future observation of their contained fluids. M. Pasteur, M. Pouchet, and others who adopted this method, carried away their experimental fluids *in vacuo*, during a two or three days' journey to the Alps or to the Pyrenees, and it never seemed to have occurred to either of them that evolutional changes might be taking place during their journey. M. Pasteur, in fact, habitually shut his eyes to all such possibilities, they did not come within the range of what he considered possible; such thoughts might, however, have suggested themselves to M. Pouchet and others, although this does not seem to have been the case.

After the flasks had been prepared in the way above mentioned, they were suspended beneath the mantelpiece in my study. During the day there was always a fire in the room, and at night I put my reading lamp underneath them with the flame properly turned down. So far as I have been able to ascertain, the temperature to which they have been subjected has mostly ranged between 23°—29° C. (75°—86° F.) Sometimes they have been exposed to the lower temperature and sometimes to the higher, and I suspect that a variation of this kind may perhaps be more favourable for the production of evolutional changes than maintenance at a constant temperature.

In detailing the results of the following experiments, I shall not enter into any minute description of the organisms found. My main object throughout has been to obtain evidence on the subject as to whether a *de novo* evolution of Living things could or could not take place. The demands upon my time have been so serious in the carrying on of these investigations, that occasionally it has only been small portions of the experimental fluids which have been examined. If, for instance, what I found in the first few drops of the fluid left no doubt in my mind as to the nature and abundance of some Living things contained therein, the remaining portions of the fluid were frequently not examined. Other bodies, therefore, may have been contained in the solutions, which were never seen at all.

H. CHARLTON BASTIAN

(*To be continued.*)

SCIENTIFIC SERIALS

THE *American Naturalist* for June contains several excellent articles. The first is by Prof. J. S. Newberry, "On the Surface Geology of the Basin of the great Lakes and the Valley of the Mississippi." In the northern half of this area down to the parallel of 38° to 40° N. lat., are found, not everywhere, but in most localities where the nature of the underlying rocks is such as to retain inscriptions made upon them, the unmistakeable indications of glacial action. Some of the valleys and channels which bear the marks of glacial action, evidently formed or modified by ice, and dating from the ice period of an earlier epoch, are excavated far below the present lakes and water-courses which occupy them. These valleys form a connected system of drainage at a lower level than the present river system, and lower than could be produced without a continental elevation of several hundred feet. Upon the glacial surface are found a series of unconsolidated materials, generally stratified, called the drift deposits. These consist in the lowest stratum of the Erie clays of Sir William Logan, above which are sands containing beds of gravel; and near the surface elephants' teeth have been found, water-worn and rounded. Upon these stratified clays, sands, and gravel of the drift, are scattered boulders and blocks of all sizes, of granite, greenstone, siliceous and mica slates, and various other metamorphic and eruptive rocks, generally traceable to some locality in the Eozoic area of the lakes. Among these boulders many balls of native copper have been found, which could have come from nowhere else than the copper district of Lake Superior. Above all these drift deposits, and more recent than any of them, are the "lake ridges," corresponding to our raised sea-beaches, embankments of sand, gravel, sticks, leaves, &c., which run imperfectly parallel with the present outlines of the lake margins, where highlands lie in the rear of such margins. The general conclusions drawn are the existence of a glacial epoch over the northern half of the continent of North America, probably contemporaneous with that of Europe, and with a climate comparable to that of Greenland; that the courses of these ancient glaciers correspond in a general way with the present channels of drainage; and that at this period the continent must have been several hundred feet higher than now.—"A Winter's Day in the Yukon Territory," by W. H. Dall, refutes the prevalent idea, perpetuated even by "official" reports, that the island of St. Paul is surrounded in winter by immense masses of ice, on which the polar bears and arctic foxes sail down from the north and engage in pitched battles with the wretched inhabitants. The fact is that there is no solid and very little floating ice near St. Paul in winter; the arctic foxes found there as well as on most of the islands were purposely introduced by the Russians for propagation, a certain number of skins being taken annually; and there is no authentic evidence that the polar bear has ever been found south of Behring's Straits. The country of Alaska comprises two climatic regions, which differ as widely as Labrador and South Carolina in their winter temperature. One contains the mainland north of the peninsula of Alaska and the islands north of the St. Matthew group; the other includes the coast and islands south and east of Kadiak, while the Aleutian Islands, with the group of St. Paul and St. George, are somewhat intermediate. A day's excursion during the winter season in the northern and more inhospitable of these two regions yielded a considerable number of interesting animals.—Articles of a popular character are "Our native Trees and Shrubs," by Rev. J. W. Chickering, Jun.; and "A Few Words about Moths," by A. S. Packard, Jun. A review of Principal Dawson's article in the *Canadian Naturalist* on "Modern Ideas of Derivation," criticises, favourably on the whole, that writer's strictures on the Darwinian theory of Natural Selection.—The Natural History Miscellany contains many interesting notes, either original or culled from English scientific journals.

The fourth part of the *Jenaische Zeitschrift für Medicin und Naturwissenschaft*, June 1870, contains the following important articles. 1. Gegenbauer on the skeleton of the limbs of Vertebrata, and of the Selachia and Chimæra in particular. 2. Abbe on a spectrum apparatus for the microscope. 3. Dr. Dohrn: Further researches on the structure and development of the Arthropoda, especially bearing on the Zoea stage of Crustacea; and lastly, a long and interesting paper by Ernst Haeckel on the "Plastiden-theorie," in which he treats fully of the deep-sea life brought to light by the dredgings of Drs. Wallich, Carpenter, Wyville Thomson, Huxley, and others, describing the Bathybius, Coccoliths, Globigerina, &c. He confesses himself unable to