plant, *Buttonia natalensis*, discovered by Mr. E. Button, and of a new date-palm, detected by Mr. M'Ken, curator of the Natal Botanic Gardens. The colony may be congratulated on possessing so energetic a society.

THE Portuguese Consul-General at Bangkok, a hale man and an excellent swimmer, while bathing in the River Menam, suddenly sank from having come into collision with an electric eel, and was drowned. The Siamese say such deaths are not uncommon.

WE have received from Mr. M. J. Barrington-Ward a syllabus of a course of botanical lectures recently delivered at Clifton College, accompanied by the following gratifying remarks :—"My lectures were attended, I should say, by 100 ladies, and I can well bear out what was said in NATURE a few weeks ago as to the ability which women display at such classes. I never met with so much diligence and real skill in pupils before, and I only wish I could send you some of the papers written by these ladies to show how well and logically they handled a scientific subject."

MR. W. CROOKES has reprinted for private circulation his article in the current number of the *Quarterly Journal of Science* on "Spiritualism viewed by the Light of Modern Science." While admitting that phenomena have come under his notice which seem inexplicable on any known physical laws, the main part of Mr. Crookes's paper is occupied by a statement of the tests to which "Spiritualists" should subject their manifestations, which they have at present failed to do.

"DER rationelle Wiesenbau, dessen Theorie und Praxis," by L. Vincent, an exhaustive treatise on arable agriculture, has now reached its third enlarged edition.

FACTS AND REASONINGS CONCERNING THE HETEROGENOUS EVOLUTION OF LIVING THINGS *

III.

THE results at which we have arrived now require to be looked at from two or three different points of view.

In the first place, with regard to these latter experiments, in which, with the help of Dr. Frankland, a perfect vacuum was procured in the experimental flasks previous to their being hermetically sealed, and before the exposure of them and their contained fluids to the temperature of 146 to 153°C. for four hours, it is desirable to know what the influence of such a temperature would be upon fungus-spores and filaments purposely exposed thereto. It is certain that, so far as all experimental observations have gone at present, no fungus-spore has been known to germinate after it has been exposed in a fluid to a temperature of 100°C. for even a few seconds.† What then would be the effect of a temperature of 150°C. for four hours? Is it possible that a fungus-spore or a fungus-filament at all similar to those which were met with in the preceding experiments could remain as such—could retain its morphological characters, in fact, after an exposure in fluid to a temperature of 150°C. for four hours? With the view of answering this question, I placed a quantity of a small fungus, consisting of mycelial filaments and multitudes of spores, closely resembling although not quite so delicate as those which were met with in the saline mixtures, into a solution (of the same strength as that which had been previously employed) of tartrate of ammonia and phosphate of soda in distilled water, and then handed it over to Dr. Frankland with the request that he would kindly treat this in the same way as he had done the other four solutions. Accordingly, on May 11, a vacuum having been produced within the flask before it was hermetically sealed, the solution was submitted in the same digester to a temperature of 146° to 153°C. for four hours When taken out from the digester, the previously whitish mass of fungoid filaments and spores had assumed a decidedly brownish colour, and it was in great part converted into mere *débris*. On the following morning the flask was broken, and some of the remains of the fungus and its spores were examined microscopically. *The plant was completely disorganised: not a single entire spore could be found;* they were all broken up into small more or less irregular particles, and the filaments were more or less empty, containing no definite contents, and being only represented by torn tubular fragments of various sizes. This utter disorganisation was in striking contrast with other specimens of the fungus, as it existed before exposure in the digester, which I had mounted in order to retain for purposes of comparison. And from the amount of destructive influence which was exercised upon the microscopic fungus in question, we may fairly imagine that the destructive influence of a similar temperature for four hours upon the still more delicate fungus represented in Fig. 17 would have been by no means less in extent. It would seem, at all events, well-nigh impossible that such a fungus could have pre-existed in the solution before its exposure in the digester, and could afterwards have retained all its morphological characters unimpaired, as they may be seen in the specimen now in my possession, from which the above-mentioned drawing was made. The plant must have been developed, therefore, within the flask itself subsequently to its exposure in the digester. What then could its origin have been? No fungus-spore has hitherto been known to germinate—no previously Living thing has been known to live—after the fluid containing it has been raised to a temperature of 100° C. for a few seconds. The fluid in Experiment 19 had however been raised to a temperature of 146° to 153° C. for four hours. We have even seen, in addition, that such a temperature completely disorganises certain closely allied fungus-spores, so that there is good reason for presuming that it would be similarly destructive to such spores as are represented in Fig. 17 if they had pre-existed in the solution. All that I have just said applies equally to the fungus-spores found in Experiments 18 and 20, and to the Ciliated Monad found in the turnip solution.

For the present, therefore, all presumptions, based upon the best available scientific evidence, are strongly in favour of the *de novo* evolution of these organisms within their respective flasks.

Whilst it seems, however, that the Living things which were found in these four experiments must have been evolved *de novo*, it does not follow necessarily that they were evolved in Experiments 19 and 20 out of the re-arranged elements of the saline substances themselves, because no proof has been offered that these substances were chemically pure. That such *may* have been the origin of these Living things seems, however, to be possible from what I have already said, and will, I think, appear even probable, after a due consideration of some of the facts which are now about to be related.

"Germs" are supposed by many to be universally diffused, more especially in the air and within organic substances. It seemed possible, however, and only reasonable, to suppose that they might exist much less abundantly in saline materials than within organic substances, and this was one reason why such materials were made use of in my later experiments. In order to ascertain whether any visible organisms or spores were to be found in the saline materials employed, portions of these have been repeatedly dissolved by distilled water in a watch-glass, and the fluid has afterwards been submitted to the most careful microscopical examination. Moreover, after sufficient time has been allowed for subsidence, the bottom of the watch-glass has then been most carefully scrutinised by a powerful immersion lens. The saline materials employed in the preceding experiments have been potash-and-ammonia-alum, tartar emetic, phosphate of soda, phosphate of ammonia, oxalate of ammonia, acetate of ammonia, carbonate of ammonia, and tartrate of ammonia. The result of repeated examinations of these substances in the manner above stated, has been that not a trace of anything like an organism—no fungus-spore, germ, or egg of any kind—has been found in solutions of any of the substances employed, except in one. This one in which such bodies have been found is that which I have named last—the neutral tartrate of ammonia.

Several of these salts—the oxalate, the acetate, the carbonate, and the tartrate of ammonia—contain within themselves all the ele-

* (Concluded from p. 201.)

† Such a temperature, also, very frequently suffices to produce a considerable amount of disintegration in fungus filaments which are submitted to its influence. It is almost impossible that a perfect organism with a mass of loose spores around it could have braved such a temperature for fifteen minutes, and could then have presented an appearance such as is represented in Fig. 14, the original of which is still in my possession. If not the result of a new evolution, therefore, this fungus must have been developed from a spore which was able to germinate after having been boiled for fifteen minutes; and if so it would be an exception to a rule which has hitherto been found to be general.

ments necessary for the building up of organic substances. Nitrogen, carbon, hydrogen, and oxygen are there, and only require to fall into other modes of collocation in order to give birth to an organisable material. The crystals of the oxalate are very small, those of the acetate are very deliquescent, and carbonate of ammonia exists generally in the form of non-crystalline cakes.* The neutral tartrate, however, exists in the form of large distinct prismatic crystals. Solutions of the first three substances showed no trace of Living things; though organisms were frequently discovered when crystals of tartrate of ammonia were examined.

Before describing these organisms more particularly, it will be well to glance for a moment at the origin or mode of preparation of this salt. The tartaric acid entering into its composition is obtained from *argol*—the crude bitartrate of potash derived from the grape. And although this latter salt is derived from the tissues of a Living plant, the processes to which it is submitted, in order to obtain the tartaric acid in an uncombined state, would most certainly destroy all living "germs" that might have been contained therein. After a solution of the bitartrate of potash has been boiled for a time, tartrate of lime is gradually precipitated by the addition of chalk and chloride of calcium. The insoluble tartrate of lime, after having been washed several times, is then brought into contact with *strong sulphuric acid, diluted with only about four times its bulk of water*, and this mixture is boiled for half an hour.† All this is necessary before a filtrate can be obtained from which the first crystals of tartaric acid are procurable. Ammonia, the other constituent of the neutral tartrate, being a product of the destructive distillation of coal tar, and itself exercising such a destructive influence upon organic matter when existing in the form of strong *liquor ammoniæ*, would not seem to be a very promising nidus for organic "germs." The neutral tartrate of ammonia is, however, prepared by mixing a solution of tartaric acid, procured as above mentioned, with an adequate quantity of liquor ammoniæ, and then evaporating the mixture at a gentle heat. Thus prepared, the crystals contain a notable quantity of water of crystallisation, and are not specially liable to contain organic impurities.

In the stock of crystals that I obtained from Messrs. Hopkin and Williams,‡ and which had been made about six months previously, some were well formed, and almost perfectly transparent, whilst others were less regular in shape, and presented an opaque appearance with more or less of striation within. When a crystal of moderate size was taken, about $\frac{1}{3}''$ in diameter, or a portion of a larger one, and was placed in a large watch-glass with some distilled water, it was frequently found that at first a certain number of opaque-white scales, having a granular aspect under a high magnifying power, dropped from the surface of the crystal to the bottom of the watch-glass. This material, which seemed to have been produced by some superficial alteration of the substance of the salt, dissolved with much more difficulty than the unaltered matter of the crystal. It remained for a long time at the bottom of the glass, and only very slowly disappeared. As the substance of the crystal slowly dissolved away, a number of large and small gaseous bubbles gradually escaped from it. When the crystal was examined with a one-inch object-glass whilst solution was taking place, these air bubbles could be seen at first within cavities, from which they were afterwards liberated by a solution of their walls. Occasionally, from the very centre of a crystal, from which bubbles of gas had been escaping, there floated out a very small and almost invisible filamentary mass, more or less thickly studded with minute air bubbles. Such masses were just visible with an ordinary pocket-lens, and when transferred on the point of a needle to a slip of glass, and examined with a magnifying power of about 600 diameters, they were found to contain more or less of the following constituents:—(1) a minute fragment of cotton or paper fibre; (2) a variable quantity of an almost transparent, insoluble plate-like substance, homogeneous, though broken up in all directions by intersecting cracks; (3) more rarely a small quantity of a tenacious mucoid matter, containing refractive protein-looking granules of various sizes; (4) a quantity of a colourless, confervoid-looking mass, some of whose smaller filaments, $\frac{1}{20000}''$ in diameter, looked like a mere linear aggregation of irregular masses of protoplasm, though in certain larger filaments continuous with these it became obvious that the irregular protoplasm masses were contained within a delicate hyaline cylinder across which dissepiments were sometimes to be seen, as in very minute fungus-filaments; (5) and lastly, certain fungus-spores in almost all respects similar to those which have been met with in so many of the saline experimental fluids. Although four or five of these were frequently interspersed amongst the confervoid-looking filaments, they did not seem to be in organic connection with them. The confervoid-looking, though really abortive fungus-filaments, were also almost precisely similar to the filaments containing irregular masses of protoplasm which were met with (in Experiments 12 and 13), in solutions containing tartrate of ammonia.

Repeated examination of crystals during their dissolution convinced me that such organic bodies invariably came from the interior of the crystal, often from its very centre, and that they were not to be met with on its surface. Seeing, however, that minute shreds of cotton or paper fibre also as frequently came from the interior of the crystal,* it was obviously possible that the organisms met with might have been engaged mechanically during the process of crystallisation, just as it must have happened with the shreds above mentioned. From what has previously been stated concerning the mode of preparation of the neutral tartrate of ammonia and the origin of its constituents, it may be considered almost certain that these organisms could not have pre-existed in the strong *liquor ammoniæ*, and that all living organisms which might by chance have been associated with the bitartrate of potash must have been hopelessly destroyed by the boiling with sulphuric acid, which occurred at one stage in the process employed for the separation of the tartaric acid from its base. During the subsequent process of crystallisation of the tartaric acid from its mother liquor, it is of course possible that any spores existing in the adjacent atmosphere might have dropped into the fluid, and have then become mechanically enclosed within the crystals; and the same chance of such a contamination with spores would exist during the process of crystallisation of the tartrate of ammonia itself. If this, however, had been the real source of the fungus-spores and masses of confervoid-looking filaments, such bodies *might* be found in freshly prepared crystals just as well as in those which had existed for six months.† I therefore asked Messrs. Hopkin and Williams to prepare for me a fresh batch of crystals of neutral tartrate of ammonia. This they were kind enough to do; obtaining them in the same place by the same process, and exposing the mother liquor in a precisely similar way.

An examination of some of these crystals, whilst they were being dissolved by distilled water in a watch-glass, showed that (unlike the older crystals) they were not at all coated on the surface by the comparatively insoluble granular plates; and that only a few very small air bubbles emerged from their interior. And at the bottom of the watch-glass, neither during dissolution nor afterwards, was there seen any trace of the confervoid-looking filaments or of the fungus-spores, though minute shreds of cotton and paper fibres were seen similar to those which were found in the older crystals. An examination of a large number of the new crystals was attended with similar results to those just mentioned.

This absence of the *confervoid-looking filaments* and of the *large fungus-spores* from the recently prepared crystals may be accounted for by either one of two suppositions:—

First. It may be supposed that in the case of the older crystals, the spores and filaments had dropped as such into the solutions in which the tartaric acid alone, or the tartrate of ammonia was crystallising; that they were mechanically engaged in the crystals, and were subsequently liberated unchanged (without having undergone any growth or development) on the dissolution of the crystal.‡ Whilst, on the other hand, in the case of the recent crystals, it may have happened that no such filaments or spores were floating in the atmosphere at the time of *their* formation, and that, consequently, none could have dropped into the solutions. Hence none of these could have been enclosed within the crystals.

* Obtained by a process of sublimation at high temperatures.

† The boiling point of such a solution would be several degrees above 100° C. Heat and acid combined exercise a most powerfully destructive influence upon organic matter, though even *very* dilute sulphuric acid, at *ordinary* temperatures, has been found to be peculiarly destructive to all Living things.

‡ Of New Cavendish Street.

* I had often been surprised at finding such shreds when I submitted some of my experimental fluids to microscopical examination, knowing that I had frequently used freshly prepared distilled water, and had taken every precaution thoroughly to cleanse the flasks which were employed.

† I have been unable to obtain crystals of the neutral tartrate of ammonia of an older date than this, and I should feel much obliged to any one who could send me such specimens, or who could furnish me with a few *crystals* of carbonate of ammonia.

‡ If they had been engaged within the crystals of tartaric acid, they must have been liberated from these during the preparation of the neutral tartrate, only to be re-entangled whilst the crystals of this salt were forming.

This supposition is, I think, unlikely to be the real explanation of the difference between the two sets of crystals. My reasons for so thinking will, however, appear more fully during the discussion of the other supposition.

Second. It may be supposed, on the other hand, that the confervoid looking *filaments* and the *spores* are organisms which have assumed their existing forms and dimensions by a process of growth and development within the crystal, and that *the starting-point of each alike was a mere speck of Living Matter.*

By this supposition we give the panspermatists the full benefit of our microscopical researches, and so narrow their real requirements in the matter of pre-existing spores. It becomes a much simpler case for them, if instead of being compelled to calculate upon the pre-existence of fully formed fungus-spores, and of confervoid-looking filaments, they need only presume upon the pre-existence of a mere speck of Living matter less than $\frac{1}{50000}''$ in diameter. I most candidly confess, however, that the pre-existence of such specks of living matter is all that is really necessary for them.* Most of those who have worked much at the microscopic investigation of the organisms met with in organic infusions, must have come to the conclusion that there is no break in the continuity of that developmental series which commences with the mere speck of living matter—the primordial *Monad*—and thence proceeds through such forms as the *Bacterium*, the *Vibrio*, the *Leptothrix* filament, and the mycelial filament of a microscopic fungus. I do not mean to say that this is a necessary order of development, which invariably occurs—far from it, but rather that, as *Bacteria* commence their visible existence in the form of Monads, so *Vibrios* are but the developed representatives of certain *Bacteria*, just as the various kinds of *Leptothrix* filaments grow from certain pre-existing *Vibrios*, and just as certain of these *Leptothrix* filaments themselves may perchance become modified into larger segmented fungus-filaments, which, under favourable conditions, may fructify and produce spores, each of which is capable of developing into a plant like its parent in its latest phase of evolution. Originating, then, in the form of the minutest visible Living speck, we may find an organism passing more or less rapidly through the *Bacterium* and the *Vibrio* phase in order to grow into a *Leptothrix* thread, which, in its turn, by further growth and development, may give rise to a microscopic fungus producing large and definite spores. These fungus-spores, under similar influences, are capable of developing at once into a mycelium similar to that from which they have been produced. They do not again go through the lower terms of the series, but are veritable spores, serving only immediately to reproduce a fungus. It is an undoubted fact, on the other hand, which although often stated, is not generally known or admitted, that *Torula* cells and other fungus-spores may also originate as minutest visible *Living specks, which grow and develope at once into fungus-spores*, instead of passing through the intermediate stages of *Bacterium, Vibrio, Leptothrix*, and fungus-mycelium.

* Although this supposition is so far favourable to the views of the panspermatists, since it makes their real requirements so much more simple, still I am afraid they will find it a most troublesome and unorthodox supposition, unless they are disposed at the same time to become out-and-out developmentalists. Their position would be a much more easy one than it is at present if they chose to maintain that such specks of living matter—whatever their precise origin may have been—are practically mere specks of indifferent living matter, having no inherent tendencies, but plastic to the full, and capable of growing into such forms as their environing conditions may determine. (But having thus "swallowed a camel," why should they "strain at a gnat"? Why should they not also believe that the speck of indifferent living matter itself was formable by concurrence of necessary matter and conditions?) Unless the panspermatists were to adopt some such thorough-going developmental views as that which I have just indicated, they will gain comparatively little from the concessions which science compels us to make to them. They will better be able to reconcile their position with the comparative paucity of definite spores and germs which are actually detectable in the atmosphere: but they will find it as difficult as ever to account for the fact *that the right spores or germs should always be in the right place at the right time.* Very little short of a belief that each cubic inch of air contains the germs of myriads of organisms which are known, or which may hereafter be found under previously unknown sets of conditions, would be adequate to account for all the known and observable correspondences between the organisms found, and the precise nature of the fluids employed. And although the wildness and extreme improbability of this supposition must seem patent to all who have a knowledge of such subjects, strange to say, there are very many scientific men who would rather harbour such a belief—who would even, in spite of all laws of evidence, think it more probable—than another supposition, which is, on the contrary, in thorough harmony with all the main principles of their scientific creed. That a "vitalist" should reject this other supposition I can understand; but that all those scientific men—and they are happily numerous—who have discarded the notion of a special "vital principle," should still reject the notion that Living matter is capable of being evolved under suitable conditions and yet should accept this Panspermic hypothesis seeing the nature of the evidence which is respectively adducible in favour of the two views—seems to me almost inexplicable.

There is, indeed, strong reason for believing that the spores and confervoid-looking filaments in question have not dropped as such from the atmosphere, but that they are, rather, organisms which have developed within the crystal. It is almost impossible not to be struck with the improbability of the former of these alternatives, on account of the number of such large spores and filaments which this supposition would require to have been present in the atmosphere over the pans containing the crystallising materials, as compared with the extremely limited number of such large organisms which have ever been obtainable when experimental observations have been made upon the nature of the solid particles existing in the air of all ordinary localities.* The best evidence, however, in proof of the view that they are products of a development which has taken place within the crystal would be, if it could be shown that in a given batch of recently prepared crystals no such organisms were to be found, whilst in many other crystals belonging to the same batch, after an interval of weeks or months, the spores and filaments were to be discovered. Sufficient time has not yet elapsed to enable me to speak definitely on this subject. This much, however, I can say. Certain of the crystals of the batch prepared for me by Messrs. Hopkin and Williams, when examined two days after preparation, were found to contain scarcely a trace of air within. Now, however, after an interval of three weeks, through which they have been kept during the day-time at a temperature of about 80° Fahr., certain other of these crystals do, when dissolved, give exit to a notable quantity of air bubbles. This seems to indicate pretty clearly that a change of some kind has been taking place in the material of the crystal, which has led to the liberation of some of its constituents in a gaseous condition, and also, perhaps, to a liberation of some of its water of crystallisation. Whilst this has been taking place, its other elements may have been grouping themselves anew. Although, at present, there is still no certain trace of the spores or filaments, I am strongly disposed to expect that such organisms will manifest themselves in the course of a few weeks more.

[Two weeks after writing the above paragraph, and whilst these proofs were going through the press, on June 9 I examined three more specimens from the recent batch of crystals which had been set aside for observation. The quantity of gaseous bubbles which escaped from within the crystal seemed almost equal to those which had been met with within the older crystals. One or two small fragments of cotton also emerged, and in addition several very small masses of a transparent mucoid material, containing refractive protein-looking granules of various sizes and shapes. These were almost precisely similar to masses which had been met with in the older crystal. Here and there an early stage, or short portion, of a filament was seen amongst the granules, though none of these were sufficiently long to make me certain as to their nature and affinities. Although nothing else was found, the occurrence of the very small masses of mucoid material seemed to represent a stage in advance of what was met with at the last examination. One of these small mucoid masses I saw within an elongated cavity (near the surface of a half dissolved crystal), two-thirds of which was occupied by a large bubble of gas. Whilst the crystal was still under the microscope, I saw the bubble and the small mucoid mass emerge from the cavity.]

Assuming, then, the view which seems most probable, that the spores and filaments have grown within the crystal—that they are the developed representatives of certain specks of Living matter—two views may still be taken as to the origin of such Living specks. Either (1) these are some of the pre-existing "germs" of the panspermatists which have become mechanically enclosed within the crystal, or (2) these Living specks have been therein evolved by virtue of certain changes and re-arrangements which have taken place amongst the non-living constituents of the crystalline matter.

Of these two alternative views I am, after reflection on the following considerations and evidence, strongly inclined to believe that the latter is most probably the true one:—

(*a.*) It must be remembered that however strange and unlikely a situation the interior of a crystal may appear for the evolution of organisms, there is the strongest reason for believing that cavities are formed within crystals of tartrate of ammonia,†

* In all my investigations I have never met with spores *similar* to these except in one or other of the ammoniacal solutions.

† The gases which appear in bubbles increase in quantity with the age of the crystal, and these gases have been seen to be lodged in cavities within the crystal. These cavities are, perhaps, more especially liable to

and there is almost as much reason for believing that the confervoid-looking filaments and the fungus-spores have undergone a process of *growth* and *development* within such cavities. Other facts, which seem to lend an increased probability to this supposition, will shortly be detailed. But if "the conditions" are favourable enough to permit, or even to stimulate the molecular activity of certain Living particles, and if such molecular activity, whereby the Living speck grows and developes, is but the modified manifestation of the physical forces acting thereupon, I see no theoretical reason why the self-same physical forces acting upon the self-same materials should not have been able, in the same place, to *initiate* a molecular collocation similar to that which they now help to build up from moment to moment. We have been, perhaps, only too much in the habit of looking upon this as impossible. But let us sweep away this habit of mind for the moment, let us look at the facts as they are, and will it be at all easier for us, who believe in no special "vital principle," to understand how from moment to moment non-living matter is converted into matter which lives? However little we may understand it, this process is continually taking place in all growing representatives of the vegetable kingdom, and no one ever thinks of doubting that it does take place because he is unable to understand *how* it occurs. If it were once conceded that a *de novo* evolution of specks of Living matter were possible, then I think most physiologists would at once admit that where specks of Living matter are able to grow and develope, there also they may be quite capable of originating.

(*b.*) The matter of the crystals of tartrate of ammonia is, by a re-arrangement of its atoms, quite capable of giving origin to organisable compounds. If a small quantity of tartrate of ammonia is dissolved in a watch-glass with distilled water, and is protected as much as possible from dust and evaporation by being covered with a wine-glass from which the stem has been broken, and then again with a tumbler, it will be found during warm weather, that in the course of two or three days the bottom of the watch-glass is covered by a number of minute microscopic crystals, interspersed amongst a mixed layer composed of monads, bacteria, and minute *Torula* cells.* These organisms form, in fact, almost as freely (though more slowly) in the ammoniacal solution, as they do in an ordinary infusion containing organic matter. There can be little doubt that the amount of ammonia and of tartaric acid actually diminishes, and that the elements of these enter into new combinations.†

It may be said, however, that such changes do not take place by the mere action of physical forces upon the unstable molecules of the dissolved tartrate of ammonia, and that *Living ferments* are necessary for the initiation of such molecular re-arrangements. In answer to this I can only call attention to the fact that similar changes must have taken place in the fluids within the experimental tubes which were submitted by Dr. Frankland to a temperature varying from 146° to 153° C. for four hours, and that there is not one tittle of evidence at present existing to show that any Living thing could live through such an exposure, whilst there are very strong reasons indeed which should incline us to believe that no Living thing could be subjected to such a temperature without being hopelessly destroyed. Therefore in these cases it would appear that such molecular re-arrangements must have been initiated without the intervention of Living ferments, and thus, too, they would appear to be comparable with those that are known to take place in a solution of cyanate of ammonia. Here "spontaneously," or with the aid of a little heat only, a molecular re-arrangement occurs, and the saline cyanate of ammonia is replaced by a colloidal compound, urea. In order to effect this transformation, no Living ferments are necessary—none have been even supposed to exist, and there is, really, no more reason why we should imagine their presence to be necessary in order that tartrate of ammonia may undergo a more or less similar isomeric transformation.

A careful examination of the mode in which bacteria and *Torula* cells appear at the bottom of a watch-glass containing tartrate of ammonia in solution is also rather valuable on account of its bearing upon this question. What is true of the *Torula* cells is also true concerning the mode of origin of bacteria; the facts, however, can be ascertained rather more satisfactorily concerning the *Torula* cells, and for the sake of brevity I shall now speak only of them. These *Torula* cells, like the bacteria in their earlier stages, are motionless; although, therefore, they increase rapidly after one or more have been formed by a process of pullulation and growth, the numerous quite distinct patches which may be seen scattered over the bottom of the watch-glass, often at well marked distances from one another, represent so many distinct centres of origin. In these several patches there may be seen delicate ovoid *Torula* cells of almost any size beneath $\frac{1}{6000}''$ in diameter. The larger cells exist united in little groups of twos and threes, and budding from them may be seen pullulating projections of different sizes. Separate cells, also, may be seen, smaller and smaller in size, till at last they cease to be cellular in form, and we see only peculiarly refractive dots or specks less than $\frac{1}{50000}''$ in diameter. In other places a colony of *Torula* cells seems to be about to grow up. Here there may be seen merely one or two of the smallest bodies which distinctly display the cellular form interspersed amongst a variable number of the refractive specks of all sizes down to the *minimum visible* stage.* Beyond this, of course, all is darkness. We must be guided by other evidence in forming an opinion as to the probable source or mode of origination of these specks of Living matter, which are so extremely minute that they only just come within the range of our aided vision.

Another remarkable observation made upon a simple solution of carbonate of ammonia, in a watch-glass, makes still clearer the fact of the disseminated origin of organisms in such solutions. It throws light also upon the previous question as to whether the fungus-spores were developed within the crystals of tartrate of ammonia from specks of Living matter, or whether they were mechanically enclosed in their developed form; and it is sufficiently suggestive as to the possible influence of electrical conditions in promoting evolutional changes. Referring to notes made at the time, I extract the following particulars. About eleven P.M. on the 14th of the present month (June) a small quantity of ordinary sesquicarbonate of ammonia was dissolved in some apparently pure (though not distilled) water, in a watch-glass. After solution, and in about an hour's time, the fluid was carefully examined with different microscopic powers, and lastly the bottom of the watch-glass was scrutinised in very many situations with an immersion $\frac{1}{12}''$ object-glass. No Living thing of any kind was seen, though scattered over the bottom of the glass were a large number of tiny crystals, some larger and some smaller than $\frac{1}{3000}''$ in diameter. Under the polariscope they gave the most beautiful and varied colour reactions. The watch-glass was then placed on a mantel-piece with a soft surface (covered with velvet), a wine-glass, with its stem broken off, was inverted over it, and this again was covered by a tumbler, in order, as much as possible, to prevent evaporation and keep out dust. After twenty-four hours the bottom of the watch-glass was again carefully examined, with the $\frac{1}{12}''$ object-glass, and no change was observable. There were the same minute crystals, perhaps rather more numerous than before, but no recognisable specks of protoplasm or other trace of living things. The watch-glass was then replaced as before. The next day (June 16) the weather was hot and extremely sultry. The temperature was about 85° F. in the shade, and the thunder-storm, which seemed imminent during the whole of the day, began about 7 P.M., and continued till the early hours of the morning of the following day. At about 11.30 P.M. of this 16th of June, I again examined the solution in the watch-glass—forty-eight hours after it had been prepared. Then, scattered over the whole of the bottom of the glass, fungus-spores were seen in all stages of development intermixed with the small crystals. They were quite motionless, and mostly separate, rather than in distinct groups. They varied in size from the minutest visible speck up to a spherical nucleated body $\frac{1}{3300}''$ in diameter. No moving particles or bacteria were seen. Probably more than a thousand of these bodies were developing in the one watch-glass—each growing in its own place, and showing no evidence of multiplication by division or pullulation. Until they attained the

form in those crystals which are not perfect in shape, and which present a more or less opaque appearance in their interior. These less perfect types are probably for that reason more prone to undergo molecular changes under the influence of incident forces, especially in the neighbourhood of and around some fibre-fragment which has been enclosed.

* In saline solutions I have generally seen the organisms first, and have found them accumulated principally at the *bottom* of the watch-glass or other vessel in which the solution may have been contained.

† Saline solutions in which spores of fungi were placed, having been analysed previously by M. Pasteur, were again analysed by him after the plants had grown for a time. The proportion of ammonia and of other ingredients was found to have undergone a diminution correlative with the growth of the plants.

* When such a patch is marked, and watched at different intervals, a crop of perfect *Torula* cells is soon seen to occupy this same situation. And it may be well to state here that *Sarcina* also makes its appearance after a fashion which is essentially similar.

size of about $\frac{1}{10000}''$ in diameter no nucleus was visible, though they had by this time assumed a distinctly vesicular appearance. As the spores increased in size, the thick wall gradually became more manifest—though it had a rather rough granular appearance—and a nucleus gradually showed itself within, which was also granular.* The next morning, after twelve hours, the spores seemed to be much in the same condition, though numerous small colonies (30 to 50 in each) of motionless bacteria were now visible. During the day the air was clear, and the temperature lower (76° F.); and after twelve hours more (in

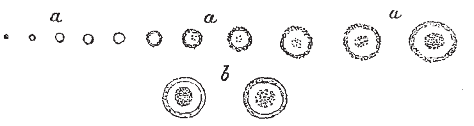

FIG 19.—Representing different stages in the development of Fungus-spores in a solution of Carbonate of Ammonia.

the evening) the bacteria were found to have considerably increased in number, and several of the fungus-spores were seen in a more devoloped condition—their thick walls being wholly or partially consolidated, and the nucleus was also more distinctly defined. In this condition they perfectly resembled the spores which were found in *Experiment* 20, and very closely resembled those which are to be met with in some of the old tartrate of ammonia crystals. The great majority of the spores were, however, still in the granular condition, and they seemed to have made no advance whatever. On the following day these spores were not quite so distinct—some of them seemed to be disintegrating, whilst none of them had undergone any further development. The bacteria, on the contrary, had decidedly increased in quantity. After two days more, minute *Torula* cells began to appear. These did not rapidly multiply, but soon began to develope into mycelial filaments.

The thick-walled spores had possibly come into existence under the influence of the high temperature and the disturbed electrical condition of the atmosphere †; and they seemed to be so much the creatures of these conditions that they were unable to survive under others which were different.

The mode, then, in which fungus-spores make their appearance in a solution of carbonate or tartrate of ammonia, seems to show that they must have originated in all parts of the solution, either by a coalescence and re-arrangement of the invisible molecules of a *pre-existing* colloidal compound,‡ or else through the development of innumerable but invisible "germs," which were disseminated through the liquid. That such invisible "germs" may have existed in the form of colloidal molecules, I am quite disposed to believe—though I am as strongly inclined to disbelieve that these fluids were saturated with "germs" of veritable fungus-spores, which had emanated from some pre-existing fungus of the same kind. We may grant that germs were there *in posse*, though not *in esse*. What warrant have we, indeed, for talking of actual though *invisible* fungus "germs"? No one can know more concerning their existence or formation than I know concerning the coalescence of colloidal molecules into minutest specks of Living matter. The necessity for the postulation of such "germs" must, therefore, seem different to different people, in accordance with the particular views which they may hold concerning Life. Those who believe in a special "vital principle" may naturally enough cling to the notion of a pre-existing germ, which may be the direct recipient of this peculiar power from some pre-existing organism; whilst those who are believers, rather, in the physical doctrines of Life will, I think, gradually find themselves contented with the pre-existence of potential "germs" in the form of colloidal molecules.

* This appearance I had not unfrequently seen before, where such spores had been developing in saline solutions, and it had always strongly suggested the notion to me that these spores were formed by a coalescence of granular particles. Here, however, there were no granules or moving particles present, the spores themselves were the only Living things, and it seemed quite certain that they could not have originated after this fashion. They obviously commenced as minute specks, and the granular appearance manifested itself so long as they were still increasing in size. When growth stopped consolidation began to take place, and an even double-contoured wall soon replaced that which was before irregular and granular.

† We may, perhaps, connect this possibility with the well-known fact that milk, beer, and other fluids are so very prone to turn sour during a thunder-storm, or whilst it is threatening.

‡ One which had existed before the organisms made their appearance, but which was the product of an isomeric modification of the carbonate of ammonia itself.

(*c*.) We find, also, associated with different sets of conditions, different kinds of Living things. In none of the crystals of tartrate of ammonia have I ever found a single distinct bacterium, and there has been the same complete absence of organisms of this kind in all my experimental fluids containing tartrate of ammonia and phosphate of soda, which have been sealed up *in vacuo*. This agreement is very striking, seeing that whenever a similar fluid, or a solution of tartrate of ammonia alone, is exposed to the air, then bacteria appear in abundance.* There is a marked accordance then between the organisms which are produced in the experimental tubes *in vacuo*, and those which come from the cavities within the crystals. There is the strongest reason for believing that the organisms which were met with within one of these experimental tubes must have been evolved *de novo*, since the existing state of our knowledge does not entitle us to believe that any such pre-existing Living thing could continue to live after it had been exposed to a temperature of from to 146° to 153°C. for four hours; and so we derive an additional presumption in favour of the *de novo* origination within the crystals, of those minutest specks of Living matter, which, as we have seen, are capable of developing into such fungus-spores as are there to be found.

In the face of this much more severe test (*Experiment* 19) it is needless to insist upon the results of other experiments in which the solutions were merely exposed to a temperature of 100°C. The fungus-spores which exist within the crystals of tartrate of ammonia do not differ, however, from all other fungus-spores that have been made the objects of experimentation. They too will not germinate after they have been exposed for one minute to a temperature of 100°C. I have taken spores and filaments from a crystal, and one half of them I have boiled for about a minute whilst the others have not been heated at all. The two patches have then been placed, at some little distance from one another, in the same growing box, with a few drops of a solution of tartrate of ammonia. The spores which had been boiled did not germinate, but those which had not been heated soon began to develope filaments. The pre-existing confervoid-looking organism, also, in the one case underwent no change, whilst in the other it grew into a distinct fungus—its filaments widening out till they became about four times as broad as they were originally. These unmistakeable fungus-filaments showed dissepiments at intervals dividing them into chambers, within which were contained large irregular blocks of protoplasm. Occasionally a filament larger than the others, might be seen terminating with a broad convex extremity, and afterwards there gradually appeared on the surface of this the minutest dot-like projections, which slowly increased in size and number. The larger of them soon became vesicular, and after a time within the vesicle granules began to cluster so as to constitute a nucleus. Thus were watched the early stages of the development of a head of fructification similar to, although much smaller than that which is represented in Fig. 17. The rate of growth was generally very slow, and after a time development ceased in my growing box, apparently because the conditions were not suitable for the evolution of such an organism as did grow luxuriantly enough within my experimental flasks. These observations were, however, extremely interesting, because I was thus able to trace all the stages in development, on one and the same plant, from mere granular abortive-looking *Leptothrix* threads, only $\frac{1}{20000}''$ in diameter, which gradually grew into a distinct confervoid-looking tube, having broken masses of protoplasm within, into slowly widening and dissepimented fungus-filaments, that were capable

* There is another difference also which deserves to be pointed out. The crystals of tartrate of ammonia or of phosphate of soda have never shown a trace of the Spiral-fibre organisms or of *Sarcina* (Fig. 13 *d*), and yet when the two have been mixed, in several of the fluids which have been kept *in vacuo*, the Spiral-fibre organism has appeared, and, similarly, on two out of three occasions when this mixture has been exposed to the air *Sarcina* has made its appearance. In one of the solutions *in vacuo* containing carbonate of ammonia and phosphate of soda, a somewhat similar Spiral-fibre has been found, and in the other *Sarcina* was met with. Both these organisms therefore seem dependent upon the presence of phosphates, and it is worthy of note that hitherto *Sarcina* has, so far as I am aware, never been known to exist except in one of the fluids of the animal body where phosphates naturally or unnaturally are present. At first *Sarcina* was discovered by Goodsir in the contents of the stomach, then it was found in the urine, and afterwards within the ventricles of the brain by Sir Wm. Jenner. And now I meet wth it in solutions containing an ammoniacal salt and a phosphate. M. Pasteur has (Ann. de Chim. et de Phys., 1862, Pl. II., fig. 27, K., and p. 80) figured, and alludes to an "Algue formée de cellules quaternaires, déposée sous forme de précipitaté," upon the walls of a flask which had contained "l'eau de levûre non sucrée," and which certainly, if not *Sarcina*, must be very closely allied thereto.

of producing a head of fructification of the *Penicillium* type. Thus, in fact, there appeared to be a strong tendency in the *Leptothrix* filaments and in the loose spores found within the crystal, to develope into the same kind of organism when either of these was placed under the influence of other and more suitable conditions. In the crystal itself, apparently, just as the conditions were not suitable for the germination of the spores, so they were not favourable for the developmental conversion of the confervoid-looking filaments into a fungus.

Whether there was any genetic relationship existing between these confervoid-looking filaments which commenced life as *Leptothrix* threads, and the few scattered spores which were frequently found with them within the crystal, is not quite certain. If any relationship did exist, however, it could only have been of one kind: the spores may have been descendants from the matter of the filaments, but the filaments were most certainly not developments from the spores. The spores existed singly or in groups of twos and threes. They were never seen in organic connection with the filaments, so that I am inclined to believe they were not even formed by a process of budding. They must, then, either have derived their origin from a minute speck of the matter of the filament which subsequently grew into a spore,* or they must have been evolved *de novo* where they were found, just as we are compelled to imagine that the similar spores must have been evolved *de novo* within the flask used in *Experiment* 19, at first by a coalescence and re-arrangement of colloidal molecules, and subsequently by a process of development similar to what is represented in Fig. 19. And, if the fungus-spore and the confervoid-looking filament both tend towards the same ultimate developmental form, we can only attribute this to the fact of the existence of a harmony between the "conditions" and such an organism. The confervoid filament and the fungus-spore are both produced within the same crystal: they seem to be but different products of what appears to us to be the same matter and the same "conditions," and if minute differences may have existed at first tending to make the initial modes of development different, the main intrinsic similarity manifests itself at last by leading them both along a line of development which terminates in a common organic form.†

For these various concurrent reasons, therefore, I deem it much more probable that the filaments and spores found within the crystals of tartrate of ammonia have been developed from specks of Living matter there evolved *de novo*, rather than that they have originated from germs of similar pre-existing organisms which had accidentally been enclosed within the crystals.

Before closing this paper, it will be necessary that I should refer more particularly to a certain part of M. Pasteur's researches, seeing that these have so strongly influenced the opinions of very many scientific men on the question of the truth or falsity of the doctrines of the heterogenists. As an experimental chemist, M. Pasteur takes a most honourable position in the foremost rank of workers, and all his investigations on this subject appear to have been conducted with the most scrupulous care. His reasonings, also, may seem at first sight to be all convincing, so that most people might be inclined to admit that he had "mathématiquement demontré," as he so frequently claims to have done, all that he had set himself to prove. The case may seem at first a poor one indeed for the heterogenists; but as soon as one gets over the first impressions produced by the various experiments, and begins to inquire whether the reasonings concerning them have been in all cases fair and logical, then it may be seen that the evidence against the occurrence of heterogenesis is very far from being so strong as it, at first sight, appeared.

On two or three occasions, when it was very important that results should be looked at from different points of view, M. Pasteur has altogether failed to do this, and has wished to interpret them only in accordance with the views of the panspermatists, *quietly ignoring the equally legitimate interpretation of the same results which might have been given by the heterogenists.* At present I shall confine myself to one instance of this kind, because I think that on this particular point the reasonings of M. Pasteur are as mischievous as they are illogical. If others were to follow his example, then certainly we could never hope to get rid of the clouds of controversy which at present obscure this subject.

The experiments of Schwann were for some time erroneously believed by very many to have upset the doctrines of the heterogenists. No organisms, it was said, were ever developed in hermetically sealed vessels when the solutions containing the organic matter had been boiled, and when all the air which was allowed access to them had been previously calcined. Schwann's experiments did yield uniformly negative results when solutions of meat were employed; though his experiments concerning alcoholic fermentation yielded results which were sometimes positive and sometimes negative. M. Pasteur also, for a time, obtained only negative results in repeating the experiments of Schwann. In these experiments, however, he had generally made use of "l'eau de levûre sucrée," of urine, or of some other fluid which was naturally unfitted to undergo evolutional changes of a high order, or even to produce lower organisms in great abundance.* But there came a time when M. Pasteur chanced to repeat his experiments, using precisely the same precautions as before, and yet the results were quite different—organisms were now found in his solutions. There was one important difference, it is true. In these latter experiments, M. Pasteur had made use of milk. Now the quantity of organic matter contained in milk is, of course, very great; it is a highly nutritive and complex fluid. It might, therefore, and ought, perhaps, to have suggested itself to M. Pasteur that the different results of his later experiments were possibly explicable on the supposition that the restrictive conditions—the boiling of the solution and the closed vessel already containing air—were too potent to be overcome by the organic matter in the one solution, whilst they were not too potent and could not prevent evolutional changes taking place in that of the other. For if, in accordance with the belief of the evolutionists, different organic fluids have different initial tendencies to undergo the changes of evolution, it may be easily understood that as the conditions favourable to evolution are more and more restricted, certain of these fluids may altogether cease to undergo such changes, others may manifest them to a meagre extent, and others still, only a little more fully. Therefore, if under the conditions peculiar to Schwann's experiments, certain fluids with low evolutional tendencies have given rise to no organisms, there is nothing whatever contradictory in the fact if it is subsequently ascertained that other fluids, with greater inherent capacities of undergoing change, will, notwithstanding all the restrictive conditions, pass through certain Life-producing changes. When subjected to a pressure of one atmosphere, water boils at 212° F., alcohol at 173° F., and ether at 96° F. The restrictive condition, or atmospheric pressure, is here in each case the same, only, having to do with differently constituted fluids, it is natural enough to look for different results under the influence of like incident forces. Ether raised to a temperature of 100° F. would rapidly disappear in the form of vapour, though no such result would follow the heating of water to a similar extent. And similarly, whilst milk might be capable of yielding organisms in Schwann's apparatus, another fluid less rich in organic matter might fail to do so. It seems almost incredible that such considerations should not have suggested themselves to M. Pasteur; but yet we have no evidence that they did occur to him.† On

* A mode of origin of spores which is, I believe, quite familiar to fungologists.

† This form of fungus-spore seems to be most prone to occur where different ammoniacal salts are employed. It has been met with not only in the tartrate of ammonia solutions, but also in those containing oxalate of ammonia and carbonate of ammonia respectively. And it has been found in no other of my experimental fluids.

* In order to avoid circumlocution in this note, I speak from the evolutionist's point of view. And whether the organisms found in a given fluid have been actually produced therein, or have only there undergone development, we may, for the sake of argument, measure the evolutional capacity of a fluid by the amount and kinds of organisms which are produced in a given quantity of it, in a definite time, and at a given temperature. We must not, however, judge of the evolutional qualities of a fluid by its mere tendency to emit a bad odour in a short space of time. A certain fluid—urine for instance—judged by these qualities, may be disagreeably putrescible, though its evolutional tendencies may be quite low. By many experimenters this difference has not been appreciated, and they seem to imagine that in employing urine they make use of a fluid which is very favourable for such experiments. But they forget that urine is an effete product containing comparatively stable compounds, which have already done their work in the body. It may after a short time swarm with bacteria, and these may be followed by fungi; but there is no comparison between the actual quantities even of these organisms, which will be developed in equal amounts of milk and urine respectively, when they are both exposed to the air for the same time in similarly-shaped vessels, and under the same bell-jar. The milk soon becomes actually solid with fungus growths. M. Pasteur's "l'eau de levûre sucrée," by his own confession (*loc. cit.* note, p. 58) is never found to contain any of the higher ciliated infusoria, and in all probability, though it produces fungi, these are met with in much smaller quantity than they would have been in an equal bulk of milk under the same conditions.

† The experiments and reasonings to which I am now alluding are detailed in pp. 58—66 of M. Pasteur's Memoir (*Ann. de Chim. et de Phys.* 1862).

the contrary, he explains the discrepancy between his earlier and his later experiments by another supposition altogether. As on other occasions, he does not even suggest to the reader that any different explanation is possible from that which he adduces. He deliberately assumes that the bacteria and vibrios which were subsequently found in the milk used in these experiments had been derived from "germs" of such organisms which either pre-existed in or had obtained access to this fluid before it had been heated, and also (contrary to the general rule which had been previously admitted) he assumed that such supposed pre-existing germs were capable of resisting the influence of the boiling temperature in milk. No direct proof of the latter assumption was ever attempted, though M. Pasteur did afterwards endeavour to bring these exceptional cases under a general law by supposing that the results obtained were due to the absence of acidity in the fluids employed. Neutral or slightly alkaline fluids might, he thought, yield positive results in Schwann's experiments, because the germs of bacteria and vibrios were not destroyed by the boiling temperature in such fluids.

Such was the very definite statement made by M. Pasteur on the faith of a chain of evidence almost every link of which is ambiguous. The most direct observations, however, which can be made upon this subject (and to the desirability of making which he does not even allude) lend not the least support to his assumption. On the contrary, they go to confirm the rule which had hitherto been generally admitted as to the inability of any of these lower organisms to live after an exposure for even a few seconds in a fluid raised to a temperature of 100°C. I have again and again boiled neutral and alkaline infusions containing very active bacteria and vibrios, and the result has always been a more or less complete disruption of the vibrios, and the disappearance of all signs of life in the bacteria. All their peculiarly vital movements have at once ceased, and they have henceforth displayed nothing but mere Brownian movements.*

M. Pasteur approaches the solution of the discrepancy in this way. His attention was arrested by the fact that milk was an alkaline fluid, because he afterwards ascertained that other alkaline fluids also yielded positive results when submitted to the conditions involved in Schwann's experiments. Thus he himself helped to overturn the strongest evidence which had hitherto been brought to bear against the heterogenists. But, this being done, it was necessary for M. Pasteur to explain such an occurrence, if he was not prepared to yield his assent to the doctrine which he had formerly rejected. He now found, truly enough, that the mere alkalinity or acidity of the solution was a matter of great importance in these experiments; he found, for instance, that his "l'eau de levûre sucrée," naturally a faintly acid fluid, was always unproductive when submitted to Schwann's conditions unaltered, though it was, on the contrary, always productive if it had previously been rendered neutral or slightly alkaline by the addition of a little carbonate of lime. Facts of this kind were observed so frequently as to make him come to the conclusion that whilst acid solutions were never productive in Schwann's apparatus, any neutral or alkaline fluids might be, if it were otherwise suitable for such experiments. Then came the question as to how this was to be explained. It should be remembered that M. Pasteur was engaged in investigating the problem of the mode of origin of certain low organisms in organic fluids, concerning which so much controversy had taken place. In this controversy, hitherto, on the one hand, it had been contended that the Living things met with derived their origin from pre-existing "germs" that had survived all the destructive conditions to which the media supposed to contain them had been subjected; whilst, on the other hand, it was contended that if the media had been subjected to conditions which (by evidence the most direct and positive) had been shown to be destructive to the lowest Living things, then such Living things as were subsequently discovered in these fluids must have been evolved *de novo*. It was a question, therefore, on the one hand, as to the degree of vitality or capability of resisting adverse conditions peculiar to the lowest Living things; and, on the other, as to the strength of the tendency to undergo changes of an evolutional character in the organic matter existing in the solutions, and on the degree to which this molecular mobility could persist, in spite of the disruptive agency of the heat to which the organic matter might be subjected. When, therefore, after having been exposed to a given set of conditions, organisms are not subsequently found in the fluids employed, this is explicable in one of two ways—that is, in accordance with either of the two opposing views. Either the heat has proved destructive to all Living things in the solutions; or else the restrictive conditions to which the organic matter in the solutions has been exposed have been too severe to permit the occurrence of evolutional changes therein. Any person seriously wishing to ascertain the truth, and competent to argue, of course would not fail to see that he was bound to give equal attention to each of these possibilities. He had no right to *assume* that the probabilities were greater in favour of the one mode of explanation than they were in favour of the other; this was the very subject in dispute—this, it was, which had to be proved. When, therefore, it was definitely ascertained by M. Pasteur that acid solutions employed in Schwann's experiments yielded negative results as far as organisms were concerned, the establishment of this fact was in reality no more favourable to the one view than to the other. It is what the Panspermatists might have expected, it is true, because—regarding it only as a question of the destruction or non-destruction of germs—even they had convinced themselves that calcining the air and boiling the fluids were adequate to destroy all Living things contained in these media; but, on the other hand, it was equally open to the Evolutionists to say that—the restrictive conditions employed being so severe—they also were not surprised at the probable stoppage of evolutional changes and at the consequent non-appearance of organisms in the solutions. When positive results were obtained, however, the case became altogether different. The rule being absolute, so far as it had gone—and founded on good evidence, to which M. Pasteur and others had assented—with regard to the inability of Living things to survive in solutions after these had been raised to the boiling temperature for a few minutes; no one should have attempted to set aside this rule, except upon evidence equally direct and equally positive, though more extensive, than that upon which the rule had been originally founded. Certainly, no one should have attempted to set it aside on the strength of *indirect evidence, which, though equally capable of explanation in accordance with either one of the two opposing views, was tacitly represented to be explicable only in accordance with one of them.* Such, however, has been the conduct of M. Pasteur. It will, perhaps, scarcely be credited by many that the investigations of M. Pasteur, which have had so much influence, and which have been looked upon by many as models of scientific method, should really contain such fallacies. On other important occasions, however, his reasoning has been similarly defective, though he himself claimed and was believed by many to have "mathematically demonstrated" what he had so plausibly appeared to prove.*

In the present case, after his experiments with milk in Schwann's apparatus, M. Pasteur ascertained that in other alkaline or in neutral fluids, even when they had been subjected to all the conditions above mentioned, inferior organisms might be found more or less quickly. But he also discovered that even such solutions no longer yielded organisms if, instead of subjecting them to a heat of 100° C. they had been exposed for a few minutes to a temperature of 110° C. And it was on the strength of two or three other links of such evidence as this that M. Pasteur sought to upset the rule with regard to the inability of inferior organisms to resist the destructive influence of a moist temperature of 100° C. On such evidence as this he attempted to raise the possible limit of vital resistance by 10° C., and sought to establish the rule that Living organisms might survive in neutral or alkaline solutions if these had not been raised to a temperature of 110° C. He did not seem to see how utterly inconclusive his conclusions were, and that he had not so much right to assume that the organisms met with in his neutral or alkaline fluids had been derived from "germs" which had resisted the boiling temperature, as he or his opponents would have had at once to fall back upon the counter assumption that the evolutional tendencies of neutral or alkaline fluids exposed to high temperatures were greater than those of similar fluids when in an acid state—and that such neutral or alkaline fluids were, as was now seen, capable of overcoming the restrictive conditions in Schwann's experiments and of giving birth to organisms, by permitting the occurrence of Life-evolving changes amongst the colloidal molecules contained therein. He had less right to explain the facts as he did, than the evolutionist would have had to explain them as above mentioned, because he was thus attempting to upset

* See what has been previously said on this subject (p. 171).

* The space at my disposal does not permit of my alluding to these other occasions at present, though I shall do so in my forthcoming work.

previously admitted facts on insufficient evidence, whilst the reasonings of the evolutionist would have been in every way legitimate. And yet M. Pasteur left his readers to imagine that the explanation which he had adduced was that which was alone admissible; he did not refer to the existence of any other mode of explanation, but at once attempted to set aside the old rule. And similarly, when he ascertained that such alkaline or neutral fluids were no longer found to contain organisms if they had been previously submitted to a temperature of 110° C. he was entitled to draw no conclusion from such facts. Nevertheless, M. Pasteur did assume that such indirect evidence entitled him to come to the conclusion that the hypothetical "germs" contained in these solutions—those which were not killed, as he supposed by a temperature of 100° C, were destroyed by a temperature of 110° C. Such two-faced evidence is, however, worthless for raising the standard of vital resistance; and to ignore the possible differences which may exist, from the evolutionist's point of view, between acid and alkaline solutions, as M. Pasteur did, is about as reasonable as if he had imagined that because water does not boil at the temperature of 100° F. the same rule must necessarily hold good for ether.

Much evidence, indeed, can be brought forward to show that even at ordinary temperatures, and under conditions in which there is a moderately free exposure to the air (and, therefore, with every facility for the entrance of "germs"), a neutral or slightly alkaline solution is not only found to contain organisms more quickly, but these are found to exist therein in much greater variety than in solutions in other respects similar, save for the fact of their being slightly acid rather than alkaline or neutral. Any of the higher forms of ciliated Infusoria *may* appear in different neutral or slightly alkaline solutions, though they never present themselves in those having an acid reaction, and *neither are their undeveloped ova or their dead bodies to be found therein.* The amount of difference capable of being produced by the mere acidity of a solution was well seen by me a few months ago. Having prepared * a mixture of white sugar and tartrate of ammonia, with small quantities of phosphate of ammonia and phosphate of soda in distilled water, whose reaction was found to be neutral, two similar wide-mouthed bottles of about three ounces capacity were filled with the fluid. Both were kept side by side in a tolerably warm place, the mouths of the bottles being merely covered in each case by a piece of glass, after glycerine had been smeared over the rim on which the cover rested. Although not hermetically sealed, these solutions were thus sufficiently protected to prevent the access of much dust from the neighbouring fire. The fluid in the one bottle was allowed to remain neutral, whilst to that of the other four or five drops of acetic acid were added, so as to make it yield a faintly acid reaction to test paper. The results were quite different in the two cases. Towards the end of the fourth day the originally unaltered neutral solution began to assume a cloudy appearance; this increased in amount during the next day, and at the close of the sixth day a thin pellicle was found on the surface, and beneath it there were some irregular, flocculent, whitish masses buoyed up by small air bubbles. Examined microscopically, the pellicles and also the flocculent masses beneath were found to be made up of medium-sized monads and bacteria, mixed with crystals of triple phosphate. There were also many scattered cells of a *Torula*, varying from $\frac{1}{5000}''$ to $\frac{1}{10000}''$ in diameter. By this time (close of the sixth day), however, the companion solution which had been slightly acidified, had undergone scarcely any appreciable change. It was still quite clear and transparent, and there was no pellicle on the surface, though there was a very slight whitish flocculent stratum at the bottom of the bottle. Even on the twenty-first day this solution continued in much the same condition—still showing no trace of a pellicle. The fluid itself was clear, and there had been only a very slight increase in the thickness of the white flocculent layer at the bottom of the bottle, which, on microscopical examination, was found to be made up mainly of a granular matter having no definite character—though mixed with this there were a small number of minute but well-formed bacteria. This acid solution had remained throughout in the same warm place, but the bottle containing the neutral fluid had not (after the examination on the sixth day) been replaced in its original place near the fire; it had continued since this time in a part of the room altogether away from the fire, and yet when this also was examined on the twenty-first day, it was found to present a very cloudy, whitish appearance throughout, there was a thick flocculent stratum at the bottom, and also a very consistent, well-marked pellicle on the surface of the fluid, made up almost entirely of large and well-formed *Torula* cells.

Although the results here detailed, as occurring in the neutral and the acidified solutions respectively, are so strikingly different, still they are by no means singular or peculiar to the particular kind of solution which was employed in this experiment. Phenomena essentially similar in kind may be observed when almost any neutral or slightly alkaline organic infusion is employed. Thus, to quote one only out of many experiments bearing upon this point. A short time ago, having prepared a pretty strong infusion of mutton, about an ounce and a half was put, after filtration, into each of two similar flasks. The one portion of the infusion was allowed to remain neutral, whilst to the other were added three drops of strong acetic acid, so as to make the whole yield a faintly acid reaction to test paper. The two flasks were then exposed side by side to a temperature of 75° to 80° F. during the day. In twenty-four hours time the neutral solution was clouded and more or less opaque, whilst the portion which was acid appeared perfectly unchanged. It was as clear as ever; and so it continued even to the end of forty-eight hours, although by this time the neutral solution was quite opaque, muddy-looking, with a pellicle on its surface, and also some flocculent deposit at the bottom of the flask. A microscopical examination of two or three drops of this fluid showed that it was teeming with most actively moving monads, bacteria, and vibrios, whilst a similar examination of the acid fluid showed not a trace of these or of any other kinds of organisms.

The difference between the results in these two sets of cases was thus extremely well marked, and the results themselves are well worth our serious attention. We had to do with equal bulks of fluid, placed under similar conditions and similarly constituted, with the exception that in each set a few drops of acid had been added to the one fluid, whilst the other was allowed to remain neutral. And it must be confessed that the difference encountered was very similar in kind to that which was observed by M. Pasteur when he made use of acid, or of neutral or alkaline solutions respectively, in repeating the experiments of Schwann. Only here we have had nothing to do with the destructive agency of heat, and germs were as free to enter into the one solution as they were into the other, so that the differences actually observed would seem now, at all events, due simply to the different qualities of the fluids themselves. Of course, such results cannot be adduced as evidence that the evolutional property of the neutral solution was higher than that of the acid solution. It may be not a case of evolution at all, but simply one of growth and development. The results, however, do show plainly enough that the neutral solution was the one most favourable to the growth and development of Living things. And if, starting from this fact, which cannot be denied, the evolutionists see reasons which induce them to assume the possibility that, in addition to mere growth and development, an actual origination of Living things may have taken place *de novo*, they would also be likely to suppose that the neutral fluid was more favourable to such evolution than that which had been acidified.† That solution which was found favourable for the processes of growth and development would also, in all probability, be favourable for evolution. A process would be most likely to be *initiated* where the conditions were suitable for its continuance. And surely the same factors would be at work in the initiation of a Living thing as would be called into play during its continuance as a growing Living thing. The presumption, therefore, is a fair one, that solutions which are favourable to the growth and development of certain organisms would also be favourable to the evolutional changes which more especially lead to the initiation of such Living things. Seeing, then, that the question of the occurrence or non-occurrence of such initiations is the very matter in dispute, it is certainly most imperative that no one engaged in investigations bearing on the subject should fail to appreciate this that these are possibilities whose probability ought to be assumed as equal. We may well be amazed, then, at the utter one-sidedness of M.

* Dec. 23, 1869. The weather being very cold and frosty. The mixture employed was another portion of the same solution as was used in *Experiment* 9.

* The reverse results, which may be produced by neutralising the acidity of a naturally acid fluid, will be exemplified further on.

† Taking it only for what it is worth, it is, at least, deserving of mention that no reason seems assignable for the presence of *Torulæ* in the one saline solution and not in the other. They were both equally exposed to the advent of "germs." It can scarcely be imagined that the *Torula* germs did obtain access to the both solutions, but that they perished in that which was faintly acid, for, as a matter of fact, *Torulæ* are much more frequently met with in acid solutions than in those which are alkaline.

Pasteur, when we find him completely ignoring one of these points of view, interpreting all his experiments by the light of a foregone conclusion, and looking upon the different solutions employed solely as fluids which are destructive or not destructive to hypothetical "germs" at a given temperature.

It should not be understood that I regard all acid solutions as having a low evolutional tendency. On the contrary, I believe I have helped to show in this paper that *some* acid solutions are most prone to undergo evolutional changes of a certain kind. These do not result in the production of Living things of a high type, but rather in an abundance of organisms of a comparatively low type. It seems to me, however, after careful observation and experiment, that a neutral or slightly alkaline solution to which a few drops of acid have been added is always found, after a given time, to contain a notably smaller number of organisms than an equal bulk of the unaltered solution. And conversely, having an acid solution whose productiveness is known, the number of organisms found in equal bulks under similar conditions, can almost always be notably increased in either one of them by the mere addition of a few drops of *liquor potassæ*, so as to render it neutral or slightly alkaline. This, as I previously pointed out, *may* be interpreted as an indication that alkalinity, or neutrality of the fluids, is more favourable than their acidity for the occurrence of evolutional changes. And thus the fact that organisms were never met with when an acid "eau de levûre sucrée" was used in repeating the experiments of Schwann, though they were met with, on the contrary, in other experiments where portions of this same fluid had been used which had been rendered slightly alkaline by the addition of chalk, might be explained without the aid of that supposition which alone seems to have occurred to M. Pasteur.

But, after reflection on this subject, it seemed to me quite within the range of probability, that the difference between acid and alkaline solutions in respect of the number of organisms which are to be found therein, when these have been simply exposed to ordinary atmospheric conditions, might be exaggerated after they had been exposed to the temperature at which water boils. It seemed quite possible that high temperatures might be more destructive to organic matter when this was contained in acid solutions than when it existed in alkaline solutions. Just as the acid seems to exercise a certain noxious influence even at ordinary temperatures, it may be conceived that this influence, whatever its nature, may be increased in intensity with the rise of temperature, and with the consequent greater facility for the display of chemical affinities. Hot acids will frequently dissolve metals which would remain unaffected by them at ordinary temperatures; and chemical affinities generally are notably exalted by an increased amount of heat. Just as the addition of an acid, therefore, to a previously neutral or slightly alkaline fluid containing organic matter in solution, appears to alter its character in some mysterious way, so may we assume that its action upon the unstable organic molecules goes on increasing in intensity as the fluid becomes hotter. So that, when two portions of a solution containing organic matter —the one neutral and the other acid—have been raised to a temperature of 100° C., whilst the organic matter of the one has been injured only by the mere action of heat; that of the other solution, which has been acidified, has not only had to submit to the deleterious influence of the high temperature, but also to the increased activity of the acid at this temperature. Thus the result would be that the amount of difference between the two solutions which existed before they had been heated, would be found more or less increased after they had been exposed to the high temperature, in direct proportion to the increase in intensity of the action of the acid produced by such high temperature. What we know concerning the precipitation of albumen in urine is quite in harmony with this view. When albumen is present, and the fluid has an alkaline reaction, mere boiling does not cause its precipitation, though, if the reaction had been acid,* the albumen present would have been precipitated, when, or even before, the fluid was raised to the boiling temperature. Or, the same result might have been brought about by the addition of a small quantity of acid to a portion of a neutral or alkaline albuminous specimen which had just been boiled without having brought about a precipitation of the albumen. Thus, the addition or presence of a small quantity of acid, in conjunction with an elevated temperature, is seen to be capable of producing results which cannot be produced by the mere elevated temperature alone. But the fact that an isomeric transformation of albumen can be brought about in this way—that albumen can be transformed so as to be no longer capable of remaining in solution—shows that a molecular change has been brought about by the influence of the acid working at high temperatures, which neither the acid nor the heat, working alone, are capable of effecting.

* Provided this was not due to the presence of a mere trace of nitric acid.

With the view of throwing further light on this subject, on March 27 of the present year I made the following experiments:—A tolerably strong infusion of white turnip was prepared and subsequently filtered.* This had a decidedly *acid* reaction. It was then divided into two portions, one of which was allowed to remain unaltered, whilst to the other a few drops of *liquor potassæ* were added, so as to give the fluid a very faintly alkaline reaction. This addition produced a slight alteration also in the naked eye appearance of the fluid; the faintly whitish opalesence which formerly existed disappeared, and was replaced by an equally faint brownish tinge. About an ounce of each of the two fluids was then placed separately in two small flasks. The fluids were not heated at all, but a piece of paper having been placed loosely in the neck of each so as to exclude dirt, they were exposed side by side to a temperature varying from 75° to 85° F. After twenty-four hours,† the unaltered acid infusion merely showed a more decided opalesence approaching to cloudiness; though that which had been rendered faintly alkaline, had a distinctly opaque whitish colour, and there was also a distinct pellicle covering more than one half of the surface of the fluid. In the three or four succeeding days the amount of opacity, of pellicle, and of deposit increased in both the fluids, though each of these continued to be more manifest in the alkaline than in the acid solution. After a week, however, the difference was scarcely appreciable, though on the whole, for about two weeks afterwards, the quantity of new matter seemed to be greater in the alkaline than in the acid solution.

But, on the same morning that these two portions of the acid and alkaline infusions had been set aside for observation, I had placed with them vessels containing two other specimens of the same fluids. These had been previously treated in the following manner: The acid fluid and the alkaline fluid, after they had been placed in their respective flasks, and the necks of these had been drawn out, were then boiled for ten minutes, and at the expiration of this time—whilst ebullition was still continuing—the drawn-out necks of the flasks were hermetically sealed in the blow-pipe flame. These vessels, therefore, were intended to show, by comparison with the other two, whether the difference produced by mere acidity or alkalinity of the solutions at low temperatures was or was not intensified by the action of heat. The flasks were all suspended in a group at the same time, and were, thenceforward, subjected to the same temperature. The results were as follows: After twenty-four hours the slightly alkaline fluid which had been boiled showed a slight though decided opalescence; it was, in fact, very similar in appearance to the acid solution which had not been boiled. The boiled acid solution was, however, as clear as when the flask was first suspended, and so it remained, apparently quite unaltered, after it had been suspended a week, though the boiled alkaline solution had by this time become decidedly opaque, and also showed some flocculent matter lying at the bottom of the vessel. And now‡, after they have been suspended more than three weeks, the acid solution still remains almost transparent, presenting only the faintest cloudiness, though with no pellicle or deposit at the bottom.§ The boiled alkaline fluid, however, presents a totally different appearance; it is whitish and quite opaque, there is a very thick pellicle covering part of its surface, and also some whitish sediment at the botton of the flask.

The difference which already exists between alkaline and acid solutions at ordinary temperatures is, then, seen to be most notably intensified after similar alkaline and acid solutions have been raised to a temperature of 100° C. And whilst these differences tend to substantiate the reality of the other mode of explanation (which I have suggested) of the discrepancies observed by M. Pasteur when he repeated Schwann's experiments with acid and with alkaline organic infusions respectively, they may also

* The turnip at this season of the year was however very poor and dry as compared with that which was employed in some of my earlier experiments (*Experiments* 4 to 9) during the winter months.

† During the whole of this time the heat only varied between the limits mentioned.

‡ April 19, 1870.

§ This solution was, therefore, much more backward in exhibiting signs of change than were the others which had been used in *Experiments* 4 to 8—a difference probably explicable by the poorer quality of the turnip used in this last experiment.

be considered to strengthen the probabilities in favour of my assumption that an acid fluid is less prone to undergo those molecular changes which lead to the evolution of Living things, than an otherwise similar fluid whose reaction is neutral or faintly alkaline. And yet this explanation was utterly ignored by M. Pasteur; he wrongly assumed that the before-mentioned discrepancies were explicable only in one way; and he moreover illogically attempted to set aside a rule to which he had previously assented, on the strength of evidence which was most ambiguous, and, therefore, inconclusive—in nature. M. Pasteur engages himself in a controversy concerning one of the most important questions in the whole range of biological science, and yet he assumes the attitude of a man who is so convinced beforehand of the error of those who are of the opposite opinion, that he will not abide by ordinary rules of fairness, he will not even, at first, assume the possibility of the truth of the opinions which are opposed to his own. Ambiguous evidence is explained as though it were not ambiguous; conclusions based upon good evidence are attempted to be set aside in favour of conclusions based upon evidence which is comparatively worthless; and, by such illogical methods, M. Pasteur proclaims that he has "mathematically demonstrated" the truth of his own views. Unfortunately for the cause of Truth, people have been so blinded by his skill and precision as a mere experimenter, that only too many have failed to discover his shortcomings as a reasoner.

But it will already have been perceived by the attentive reader, that it was not necessary for me—in my endeavour to establish as a Truth the great doctrine which M. Pasteur has striven to repudiate—to show the inconclusiveness of his reasonings on that branch of the subject to which I have just been alluding. I have striven rather to show in their true light the real nature of such modes of reasoning, which are I fear only too likely to be repeated by others. So long as people are unable readily to appreciate the worthlessness of arguments like these, they will never be likely to penetrate through the clouds of controversy which envelope this subject. Their mental vision will be blinded, and the truth will remain hidden from them. But, lured on by the success of reasonings such as these, others would have grown bolder still, and precisely as the exigencies of the case required, so would the standard of vital resistance to heat have been raised. What object can there be in laboriously ascertaining by direct experiment and observation at what temperature the lower kinds of organisms cease to live, if the information so obtained is to be studiously ignored just when it ought to be used as a kind of touchstone, or as a lamp to illumine phenomena whose explanation would otherwise be doubtful? It is a very easy process, certainly, first to start with the assumption that it is "impossible" for Living things to be evolved *de novo*, and then, every time that Living things are found under conditions where they ought not to occur (if the assumption were true, and if the generally received notions concerning vital resistance were correct), to assume that the very fact of their having been found under these conditions, of and by itself, shows that the previous notions concerning vital resistance were entirely wrong, and that the organisms which were formerly admitted to have been destroyed by a temperature of 100° C., must now be considered to be able to brave for four hours a temperature of 150° C., simply because they have been found in fluids which had been submitted to this temperature. The reasoning by which Truth is sought to be ascertained is, in fact, this:—No matter what the temperature to which the solutions and the hermetically sealed flasks have been exposed—be it even 500° C.—if Living organisms are subsequently found in the solutions, then they or their "germs" *must* have been able to resist the destructive influence of such a temperature, simply because Living things have been found, and because it is *assumed* that they cannot be evolved *de novo*. It is to be hoped that this is not the kind of reasoning which will find favour with those who are seeking for the advancement of Biological Science!

My principal objects in this paper have been to show:—

1. That there is a strong *a priori* probability in favour of the possibility of the occurrence of the heterogenous evolution of Living things, and that the most reliable scientific data which we possess do, in fact, fully entitle us to believe in this as a possibility.

2. That microscopical investigation, whilst it teaches us as much concerning the mode of origination of the lowest Organisms as it does concerning the mode of origin of Crystals, enables us to watch all the steps of various processes of heterogenous Evolution of slightly higher Organisms, such as may be seen taking place in a pellicle on a fluid containing organic matter in solution.

3. That the kinds of organisms which have been shown to be destroyed by a temperature of 100° C. may be obtained in organic fluids, either acid or alkaline, which, whilst enclosed within hermetically sealed and airless flasks, had been submitted not only to such a temperature but even to one varying between 146° and 153° C. for four hours.

4. That a new and direct evolution of organisable compounds may, in all probability,* be capable of arising, sometimes by isomeric transformation of the atomic constituents of a single saline substance such as tartrate of ammonia, and sometimes by a re-arrangement of certain of the atomic constituents belonging to two or more saline substances existing together in solution. It is not only supposed that this may occur, but that even Living things may subsequently be evolved therefrom, when the solutions have been exposed, as before, in airless and hermetically sealed flasks to a temperature of 146° to 153° C. for four hours.

On account of this *a priori* probability, and in the face of this evidence, I am, therefore, content, and as I think justified, in believing that Living things may and do arise *de novo*. Such a belief necessarily carries with it a rejection of M. Pasteur's Theory of Putrefaction, and of the so-called "Germ Theory of Disease."

H. CHARLTON BASTIAN

* It is not pretended that this is proved. The aid of the chemist and physicist must be much more extensively resorted to before such a point could be proved. I hope soon, however, to be able to bring forward additional evidence bearing upon this part of the subject.

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