HENRY BASTIAN

THE MODES OF ORIGIN OF LOWEST ORGANISMS

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Содержание

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PREFACE

Having been compelled by the results of my investigations on the question of the Origin of Life to arrive at conclusions adverse to generally received opinions, I found that several persons having high authority in matters of science, were little disposed to assent to these views. To a great extent this seemed due to the fact that a distinguished chemist had previously gone over some of the same ground, and had arrived at precisely opposite conclusions. M. Pasteur has been long known as an able and brilliant experimenter, and some of his admirers seem to regard him as an almost equally faultless reasoner.

Renewed and prolonged experimentation having tended to demonstrate the truth of my original conclusions, and to convince me of the utter untenability of M. Pasteur's views, it seemed

that the best course to pursue would be, at first, to endeavour to show into what errors of reasoning M. Pasteur had fallen, and also how his conclusions were capable of being reversed by the employment of different experimental materials, and different experimental methods. Then, having presented, in a connected form, evidence which might suffice to shake the faith of all who preserved a right of independent judgment, one might hope to have paved the way for the reception of new views – even though they were adverse to those of M. Pasteur. The present volume contains, indeed, only a fragment of the evidence which will be embodied in a much larger work – now almost completed – relating to the nature and origin of living matter, and in favour of what is termed the physical doctrine of Life.

The question of the mode of origin of Living Matter, is inextricably mixed up with another problem as to the cause of fermentation and putrefaction. M. Pasteur's labours were, at first, undertaken in order to solve the latter difficulty – to decide, in fact, between two rival hypotheses. It was held, on the one hand, that many ferments were mere dead nitrogenous substances, and that fermentation was a purely chemical process, for the initiation of which the action of living organisms was not necessary; whilst, on the other hand, it was also maintained that no fermentation could be initiated without the agency of living things – in fact, that *all* ferments were living organisms. The former may be called the *physical* theory of fermentation, of which Baron Liebig is the most prominent modern exponent; whilst the latter may be

termed the *vital* theory of fermentation, and this is the doctrine of M. Pasteur. All the facts which I have to adduce, so far as the subject of fermentation is concerned, are wholly in favour of the views of Baron Liebig.

And, the conclusions arrived at in this work are confirmed by the results of several unpublished experiments, in which living organisms have been taken from flasks that had, a few weeks before, been hermetically sealed and heated for a variable time to temperatures ranging from 260° F. to 302° F.

With the view of aiding some of my readers in their interpretation of the results of some of the experiments contained in this volume, I would call their attention to the following considerations. If fluids *in vacuo* (in hermetically-sealed flasks), which were clear at first, have gradually become turbid; and if on microscopical examination this turbidity is found to be almost wholly due to the presence of *Bacteria* or other organisms, then it would be sheer trifling gravely to discuss whether the organisms were living or dead, on the strength of the mere activity or languor of the movements which they may be seen to display. Can dead organisms multiply in a closed flask to such an extent as to make an originally clear fluid become quite turbid in the course of two or three days?

And if any one wishes to convince himself as to whether such turbidity can occur in a flask which is still hermetically sealed, let him take one that has been prepared in the manner I have elsewhere described, carefully heat the neck of it in a spirit-

lamp flame, and see how the rapid in-bending of the red-hot glass testifies to the preservation of a partial vacuum within. The vacuum in such cases is only partially preserved, because of the emission of a certain amount of gases within the flask – such as invariably occurs during the progress of fermentation or putrefaction.

In these experiments with heated fluids in closed flasks, nothing is easier than to obtain negative results. The same kinds of infusions which – if care has been taken to obtain them strong enough – will in a few days teem with living organisms, often show no trace of living things after much longer periods, when the solutions are weak. Again, in those cases where only a few organisms exist in a solution which has been made the subject of experimentation, nothing is easier than by a perfunctory examination of the fluid to fail in finding any of these sparsely-distributed living organisms. Experiments, the results of which are positive, may, therefore, in the absence of sufficient care, be cited as negative; and experiments which would otherwise have been crowned with unmistakeably positive results, may be rendered wholly barren by the employment of infusions which have been carelessly made.

A word of explanation seems necessary with regard to the introduction of the new term *Archebiosis*. I had originally, in unpublished writings, adopted the word *Biogenesis* to express the same meaning – viz., life-origination or commencement. But in the mean time the word *Biogenesis* has been made use of,

quite independently, by a distinguished biologist, who wished to make it bear a totally different meaning. He also introduced the word *Abiogenesis*. I have been informed, however, on the best authority, that neither of these words can – with any regard to the language from which they are derived – be supposed to bear the meanings which have of late been publicly assigned to them. Wishing to avoid all needless confusion, I therefore renounced the use of the word *Biogenesis*, and being, for the reason just given, unable to adopt the other term, I was compelled to introduce a new word, in order to designate the process by which living matter is supposed to come into being, independently of pre-existing living matter.

H. Charlton Bastian.

Queen Anne Street, W.,
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THE MODES OF ORIGIN OF LOWEST ORGANISMS

The mode of origin of *Bacteria*, and, to a less extent, of *Torulæ*, has been much discussed of late, and many different views have been advocated on this subject by successive writers.

It is of much importance to bear in mind when such views are under consideration, that a short time since nothing was positively known concerning the life-history of these organisms. However strongly, therefore, certain persons are inclined to rely upon the analogy which is supposed to obtain between these doubtful cases, and the multitudes of known cases – in which it can be shown that organisms are the offspring of pre-existing organisms – it must always be borne in mind that in many of the doubtful cases, where the simplest organisms are concerned, there is also an analogical argument of almost equal weight adducible in favour of their *de novo* origination – after a fashion, and under the influence of laws similar to those by which crystals arise. To rely too exclusively upon an argument from analogy is always perilous: it is more than usually so, however, in a case like this, where what is practically an opposing analogy may be deemed to speak just as authoritatively in an opposite direction.

There is one consideration, moreover, which deserves to be

pointed out here, and which does not seem to have occurred to most of those who so firmly pin their faith to the truth of the motto "*omne vivum ex vivo*." The every-day experience of mankind, supplemented by the ordinary observations of skilled naturalists, does pretty fairly entitle us to arrive at a wide generalization, to the effect that *some representatives of every kind of organism are capable of reproducing similar organisms*. But, whilst this is all that the actual every-day experience of mankind warrants being said, and whilst there is in reality the widest possible gulf between such a generalization and that which is expressed by the motto "*omne vivum ex vivo*," the latter formula has of late been spoken of as though it were the one which was in accordance with the daily experience of mankind, rather than the other, which gives expression to a generalization of a much narrower description. This experience, in reality, affords no evidence which could entitle us to place implicit belief in the formula "*omne vivum ex vivo*."

Whilst we do know something about the ability which most organisms possess of reproducing similar organisms, we cannot possibly say, from direct observation, that every organism which exists has had a similar mode of origin, because the cases in which organisms may have originated *de novo* are the very cases in which their mode of origin must elude our actual observation. Such a statement, too, would be all the more dangerous, in the face of the other analogy, when it can actually be shown that some organisms do make their appearance in fluids after

precisely the same fashion as crystals.

Although, therefore, there is a contradiction between the unwarrantable and ill-begotten formula, "*omne vivum ex vivo*," and the doctrines of what has been called "Spontaneous Generation"; there is no contradiction whatever between such doctrines and the only generalization which we are really warranted in arriving at, to the effect that *some representatives of every kind of organism are capable of reproducing similar organisms*.

Bacteria, *Torulæ*, or other living things which may have been evolved *de novo*, when so evolved, multiply and reproduce just as freely as organisms that have been derived from parents.

The views as to the origin of *Bacteria* and *Torulæ* which are most worthy of attention, may be thus enumerated: —

a. That they are independent organisms derived by fission or gemmation from pre-existing *Bacteria* and *Torulæ*.

b. That they represent subordinate stages in the life-history of other organisms (fungi), from some portion of which they have derived their origin, and into which they again tend to develop.

c. That they may have a heterogeneous mode of origin, owing to the more complete individualization of minute particles of living matter entering into the composition of higher organisms, both animal and vegetal.

d. That they may arise *de novo* in certain fluids containing organic matter, independently of pre-existing living things (*Archebiosis*).

I shall make some remarks concerning each of these views, though the evidence I have to adduce mainly concerns the possibility of the origin of *Bacteria* and *Torulæ* in the way last alluded to, viz., by *Archebiosis*.

The third mode of origin is what is called *Heterogenesis*; whilst the first and second modes are the representatives of more familiar processes, included under the head of *Homogenesis*. Thus, in accordance with the first view, *Bacteria* may be regarded as low organisms having a distinct individuality of their own and multiplying by a process of fission – thus affording instances of what I propose to term *direct Homogenesis*. Whilst, in accordance with the second view, *Bacteria* are supposed to represent merely one stage in the life-history of higher organisms, which are therefore reproduced by an *indirect* or cyclical process of *Homogenesis*.

The possible modes of origin of *Bacteria* and *Torulæ* may, therefore, be tabulated as follows: —

Modes of origin of *Bacteria* and *Torulæ*.	1. Homogenesis.	*a*. Direct.
		b. Indirect.
	2. Heterogenesis.	
	3. Archebiosis.	

I. Homogenetic Mode of Origin of Bacteria and Torulæ

Bacteria and *Torulæ* being already in existence, they may, undoubtedly, reproduce organisms similar to themselves by processes of fission and gemmation – in the same way that other low protistic organisms propagate their kind. Although so many reasons rendered this view probable, it was some time before I was able actually to confirm it by personal observations in the case of *Bacteria*. In the ordinary microscopical examination of portions of an infusion containing these organisms, an observer may watch for hours and never see a single instance of such fission occurring. His attention is apt to be distracted by the number of organisms which are constantly flitting before his view, and he is, moreover, perhaps apt to pay particular attention to those which seem by their movements to be most obviously alive.

I have observed the process most plainly when a few *Bacteria* have been enclosed in a single drop of fluid, pressed into a very thin stratum, in a "live-box" kept at a temperature of about 90° Fahr. by resting on one of Stricker's warm-water chambers placed on the stage of the microscope. Under these conditions, I have seen a *Bacterium* of moderate size divide into two, and each of these into two others somewhat smaller, in the course of fifteen minutes.

It is still more worthy of remark, that in all cases (so far as I have been able to observe), this, the most certain sign of vitality which such organisms are capable of manifesting, is shown by those which, from their stillness, might be considered dead. The *Bacteria* which are about to divide are generally either motionless,[1] or merely present slight oscillating movements. The separation is quickly brought about at the joint, so that the original organism divides into two equal portions; and these, lying close together, soon develop a new construction as they grow, through which a further division may occur.

That the *Bacteria* which reproduce should be in a comparatively quiescent condition, seems not difficult to understand. Such rudimentary organisms do not appear to possess cilia or other locomotory appendages: their movements are, therefore, in all probability dependent upon the mere molecular changes which are taking place within them, and upon which their life and nutrition depend. The process of fission must, however, be considered as the result of a new effort at equilibrium, which has, perhaps, been necessitated by molecular changes that have occurred during a preceding period of growth. The living matter which is no longer able to exist round a single centre, re-arranges itself around two centres, – as a result of which, fission occurs. It seems only natural, therefore, that

[1] Those which are quite motionless are always in close apposition either with the under surface of the covering glass, or with the surface of the glass on which they are situated.

whilst this active work of molecular re-arrangement is going on, those other molecular movements which occasion the actual locomotion of the organism from place to place, should be more or less interfered with.

This is the one and only mode of multiplication of *Bacteria* and of *Torulæ* which is actually known to occur; and such a limitation is in accordance with the more general fact, that processes of fission or gemmation are the only means of reproduction that are known to occur in the lower kinds of organisms, belonging to the PROTISTIC kingdom.

However well this process of fission may have been established, as a frequent mode of reproduction of *Bacteria*, such a fact does not lend any support to the notion that these are necessarily distinct and independent organisms. *Torulæ* (of which beer-yeast is the most familiar example) may similarly undergo this process of mere vegetative repetition to an indefinite extent, whilst only some of the products develop into fungi. The gonidia of lichens may also reproduce indefinitely in this fashion, and only some of the products of multiplication may go on to the production of lichens similar to that from which the gonidia had been derived.

It is a fact, however, admitted by many, and which any patient microscopist is capable of verifying for himself, that some *Bacteria* do develop into *Leptothrix* filaments, and that these are capable of passing into a dissepimented mycelial structure of larger size and undoubtedly fungus nature – from which

fructification of various kinds may be produced. Some *Bacteria* may therefore develop into some fungi, just as certainly as some *Torulæ* may develop into other fungi, or, just as surely as some multiplying gonidia may develop into lichens.

In order to prove, however, that the *Bacteria* which happen to go through this development into *Leptothrix* and thence into fungi, are strictly to be considered as *necessary* links in the life-history of fungi, it would be essential for the person holding such views, to show that *Bacteria* could not arise independently – or at least that no independently evolved *Bacteria* could develop through *Leptothrix*-forms into a fungus. And, similarly, for the other kinds of organisms: in order to establish that the *Torula* cell is a necessary link in the life-history of certain fungi, or the gonidial cell a necessary link in the life-history of lichens, it would be necessary to show that *Torulæ* or gonidial cells could not originate *de novo*– that no independently evolved *Torula* or gonidial cell could develop into a fungus or a lichen.

An easier position to establish would be, that the *Bacterium* or the *Torula* were *occasionally* links in the life-history of fungi, or that the gonidial cell was an occasional link in the life-history of a lichen. This doctrine would leave the other more difficult problems, – as to the possible existence of supplementary modes of origin for such organisms by Heterogenesis or by Archebiosis – perfectly open questions.

To establish the position that *Bacteria* are occasional links in the life-history of fungi, it would be only necessary to show

that some of the *Bacteria* which develop into fungi through *Leptothrix* have derived their origin from pre-existing fungi. This is the view which Hallier[2] has endeavoured to establish; it is also the doctrine of M. Polotebnow,[3] and one, moreover, to which Professor Huxley[4] inclines. Even this mode of origin for *Bacteria*, however, has not been so decisively established as might be desired. With regard to *Torulæ*, we do possess sufficient evidence tending to show that some of them may arise from pre-existing fungi, and we are equally certain that some gonidial cells are thrown off from lichens. The analogical evidence is, therefore, in favour of the view that minute particles which are budded off from the mycelium of certain fungi, may subsequently lead an independent existence, and multiply in the form of *Bacteria*– although many of the cases in which such buds *seem* to be given off, may be merely cases in which co-existing *Bacteria* have become adherent to fungus filaments or to *Torulæ*.[5]

But, with reference to these supposed cases of budding,

[2] Phytopathologie, 1867. Hallier seems, however, strongly inclined to disbelieve in the origin of these organisms by Heterogenesis or by Archebiosis.

[3] Sitzungsber. der K. Akad. zu Wien, 1870, Band lx., Heft iv.

[4] Quarterly Journal of Microscopical Science, Oct., 1870.

[5] Notwithstanding what Professor Huxley has said, I believe it to be almost certain that in many cases *Bacteria* exist in a solution in which there are neither *Torulæ* nor developed fungi. And, on the other hand, I have seen fungi growing in a simple (boiled) solution of tartrate of ammonia, for weeks together, without the appearance of *Bacteria* or the occurrence of any turbidity of the solution; and on two or three occasions I have seen *Torulæ* swarming in an infusion without the presence of *Bacteria*.

and also to those others in which the contents of a spore or sporangium break up into what Professor Hallier calls "micrococci" (which are generally incipient *Bacteria*), it would be difficult for us to decide whether such processes are normal or abnormal. When we have to do with such organisms, in fact, there may be the nicest transitions between what is called Homogenesis, and what, when occurring in other organisms, we term Heterogenesis. It may be that the production of such "micrococci" from the spore or sporangium of the fungus is not an invariable incident in the life-history of the species, but rather an occasional result of the influence of unusual conditions, or of failing vigour on the part of the organism. In this latter case we should have to do with a process of Heterogenesis; although, as I have just stated, in respect to such low and changeable organisms, scarcely any distinct line of demarcation can be drawn between Homogenesis and Heterogenesis.

The evidence seems, therefore, against the notion that *Bacteria* or *Torulæ* are ordinary, independent living things, which merely reproduce their like.

That some *Bacteria* are produced from pre-existing *Bacteria*, just as some *Torulæ* are derived from pre-existing *Torulæ*, may, it is true, be considered as settled. But, so far as we have yet considered the subject, there may be just as good evidence to show that *Bacteria* and *Torulæ* are capable of arising *de novo*, as there is that some of them are capable of developing into fungi.

If this were the case, such types could only be regarded as

the most common forms assumed by new-born specks of living matter; and, by reason of their origin – which would entail an absence of all hereditary predisposition – they might be supposed to be capable of assuming higher developmental forms.

Now, as a matter of fact, worthy of arresting our attention, we do find that some *Bacteria* are capable of growing into *Leptothrix*, whilst this is able to develop continuously into a fungus; just as we also know that some *Torulæ* are capable of growing into other fungi.

Should it be established, therefore, that *Bacteria* and *Torulæ* are capable of arising *de novo*, the facts concerning their mutability are harmonious enough with theoretical indications.

But, as I have before indicated, although it is quite true that some *Bacteria* develop into fungi, such forms may constitute no necessary links in the life-history of other fungi. I have suggested that in those (occasional) cases in which they do occur as links in the life-history of fungi, there is room for doubt whether these *Bacteria* are to be considered as normal products, or as abnormal results (heterogeneous offcasts), brought about by some unusual conditions acting upon the parent fungus. That is to say, we may be doubtful whether in such a case their origin ought to be considered Homogenetic or Heterogenetic. It may be that many of the lower fungi are such changeable organisms, and so prone to respond to the various "conditions" acting upon them (which would be almost certainly the case if they had been developed from a *Bacterium* in two or three days – the *Bacterium*

itself having been evolved *de novo*) that no very valid distinction can here be drawn between Homogenesis and Heterogenesis. Our whole point of view, in fact, concerning such fungi as are seen to develop through *Leptothrix* forms from *Bacteria* must be entirely altered, if it is once conceded that *Bacteria* may arise *de novo*. Such simple *Mucedineæ* would then have to be regarded as mere upstart organisms only a few removes from dead matter, and – in view of the greater molecular mobility of living matter – capable of being modified in shape and form even more than the most changeable crystals under the influence of altering "conditions." We should have no longer to do with the members of a stable species, which had been reproducing its like through countless geologic ages anterior to the advent of man upon the earth. Indeed, in order to reconcile such a possibility with the seemingly contradictory fact of the known extreme changeability of these lower forms of life, we hear only vague hints thrown out about our imperfect knowledge of the "limits within which species may vary." As if, in the face of what we do know concerning hereditary transmission, this changeability did not make it almost impossible to conceive that there should have been an unbroken series of such organisms since that remote epoch of the earth's history, when the first organisms of the kind made their appearance. It does not seem to me that the presumed permanence of a very changeable organism is consistent with, or rendered more explicable by, the supposition that *some* representatives of the species *have*

constantly been undergoing progressive modifications which have been successively perpetuated by inheritance, in the shape of distinct specific forms. Why should some be presumed to have undergone so much change, whilst others (presenting an equal and an extreme degree of modifiability, even to the present day) are supposed to have preserved the same specific form through a countless series of changing influences?

2. Heterogenetic Mode of Origin of Bacteria and of Torulæ

It has been long known that *Bacteria* and *Torulæ* are frequently to be found within vegetable cells, taken even from the central parts of plants, whenever these are in a sickly condition or are actually dying. They are apt to exist also within epithelial cells taken from the inside of the mouth; and the frequency and abundance with which such organisms are met with in these cells, is almost in direct proportion to the malnutrition and lack of vital power in the individual who is the subject of observation. Then, again, in persons who have died of adynamic diseases, in the course of twenty-four or thirty-six hours (during warm weather) *Bacteria* may be found in abundance within the blood-vessels of the brain and of other parts, although no such *Bacteria* were recognizable in the blood of the individual during life.

In these cases we must, in order to account for the presence of the *Bacteria* and *Torulæ*, either suppose that such organisms, in an embryonic state, are almost universally disseminated throughout the various textures of higher organisms, both animal and vegetal (though they are only able to develop and manifest themselves when the higher organisms, or the parts of them in which the *Bacteria* or *Torulæ* are met with, are on the eve of death), or else we must imagine that when the vital activity of any organism, whether simple or complex, is on the wane, its

constituent particles (being still portions of living matter) are capable of individualizing themselves, and of growing into the low organisms in question. Just as the life of one of the cells of a higher organism may continue for some time after the death of the organism itself, so, in accordance with this latter view, may one of the particles of such a cell be supposed to continue to live after even cell-life is impossible.

Now, to many persons, the latter seems to be a much simpler hypothesis than the former, and one, moreover, which is more in accordance with known facts. People's views, however, on this subject are likely to be much influenced by their notions as to the possibility of *Bacteria* arising by a process of Archebiosis. Although some may be inclined to accept the doctrine of Heterogenesis, the same persons, being "vitalists," may not readily believe in the doctrine of Archebiosis, because this implies the vivification of dead matter – the conversion of not-living elements into a living combination. Those, however, who do believe in Archebiosis will – if the necessary evidence be forthcoming – all the more readily yield their assent to the doctrine of Heterogenesis, because it is a much less novel thing to have to believe in the mere transformation of living matter, than in the possibility of its origin *de novo*.

Evidence of a tolerably satisfactory nature, however, is forthcoming, which may speak independently in favour of the doctrine of *Heterogenesis*.

It has been affirmed by Crivelli and Maggi[6] that they have actually seen the particles within granular epithelial cells (taken from the back of the tongue of a patient suffering from diabetes) grow and elongate, so as to give rise to *Bacteria*, or fuse in longitudinal series, so as to form a *Vibrio*.[7] And, moreover, as I have myself ascertained, if one takes healthy-looking epithelial scales scraped from the inside of the mouth, which appear to contain nothing but the finest granules, and places them with a little saliva in a "live-box" (and this within a damp chamber kept at a temperature of about 90° Fahr.), in the course of from 5 to 10 hours, the cells may be found to be studded throughout with motionless *Bacteria*. Of course it may be said that the granules originally seen in the cells were offcasts from pre-existing *Bacteria*[8] which had gained access to the cell. And although, to many, this may seem an extremely improbable supposition, it is, nevertheless, one which it would be very difficult to disprove. The improbability of the notion is increased, moreover, when we find that *Bacteria*, and even *Torulæ*, will develop just as freely

[6] Rendiconti del R. Istit. Lombardo, Ser. II. Vol. 1, p. 11.

[7] However novel such a mode of origin of independent *Bacteria* and *Vibriones* may appear to some, it will seem much less strange and unlikely to others who have seen, as I have done, an *Amœba*, or an *Actinophrys*-like body, originate from the progressive molecular modifications taking place in a mass of chlorophyll and protoplasm within the filament of an alga. Many independent observers have watched all the stages of this process, and some have even seen Ciliated Infusoria originate by such a metamorphic change.

[8] Or offcasts from pre-existing fungi, – constituting the "micrococci" of Professor Hallier.

within closed cells taken from the very centre of a vegetable tuber, as they will in the midst of the more solid epithelial cell from the inside of the mouth. If it be urged that in this latter situation, there is the greatest chance of the cells being brought into contact with *Bacteria*, and that it must be considered possible for imaginary minute offcasts from these *Bacteria* to make their own way into the substance of the epithelial cell, I am quite willing to grant the desirability of taking such possibilities into consideration. But, at the same time, it seems all the less likely that the actual occurrence of the *Bacteria* is explicable on these grounds, because we find them developing just as freely within the cells freshly cut from the centre of a tuberous root, or we may find them already developed within these cells, if the root has begun to decay. To suppose that actual germs of *Bacteria* and of *Torulæ* are uniformly distributed throughout the tissues of higher organisms, is to harbour a hypothesis which would appear to many to be devoid of all probability – more especially when the heterogenetic mode of origin of larger and higher organisms is a matter of absolute certainty.

3. Origin of Bacteria and of Torulæ by Archebiosis

The evidence on this part of the subject is, I think, sharply defined and conclusive. Simple experiments can be had recourse to, which are not admissible in the discussion of the question as to the origin of *Bacteria* and *Torulæ* by Heterogenesis. There, we wish to establish the fact that living matter is capable of undergoing a certain metamorphosis, and consequently, we must deal with living matter. Here, however, with the view of establishing the fact that living matter can arise *de novo*, if we are able, shortly after beginning our experiment, to arrive at a reasonable and well-based assurance that no living thing exists in the hermetically sealed experimental vessel – if the measures that we have adopted fully entitle us to believe that all living things which may have pre-existed therein have been killed – we may feel pretty sure that any living organisms which are subsequently found, when the vessel is broken, must have originated from some re-arrangements which had taken place amongst the not-living constituents of the experimental solutions, whereby life-initiating combinations had been formed.

The possibility of the *de novo* origination of *Bacteria*, *Torulæ*, and other such organisms, is one which is intimately associated with the doctrine as to the cause of fermentation and putrefaction. With regard to the almost invariable association

of such organisms with some of these processes, almost all are agreed. There is, moreover, a very frequent association of particular kinds of organisms with particular kinds of fermentation. Hence the assumption is an easy and a natural one to many persons, that the organisms which are invariably met with in some cases are the causes of these fermentations,[9] although it is quite obvious that the facts on which this view is based, are equally explicable on the supposition that the organisms are concomitant results or products (due to new chemical combinations) of the fermentative changes. In the one case the fermentative changes are believed to be initiated by the influence of living organisms; and those who regard living things as the only true *ferments*, for the most part also believe that living things are incapable of arising *de novo*. They think that those organisms which serve to initiate the changes in question, have been derived from a multitudinous army of omnipresent atmospheric germs, which are always ready, in number and kind suitable for every emergency. This is the doctrine of M. Pasteur and others. On the other hand, fermentations and putrefactions may be regarded as sets of chemical changes, which are apt to occur in organic and other complex substances – these changes being due either to the intrinsic instability of the body which manifests them, or to molecular movements communicated to

[9] From this view the transition is also easy, though none the less illegitimate, to the doctrine that *all* fermentations are caused by organisms; just as it has been easy to start, and find converts for, the doctrine expressed by the phrase "*omne vivum ex vivo*." The distinction between *all* and *some* is only too often overlooked.

it by a still more unstable body. Baron Liebig says: – "Many organic compounds are known, which undergo, in presence of water, alteration and metamorphosis, having a certain duration, and ultimately terminating in putrefaction; while other organic substances that are not liable to such alteration by themselves, nevertheless, suffer a similar displacement or separation of their molecules, when brought into contact with the ferments."

Each substance belonging to the first class, would be at the same time, therefore, both ferment and fermentable substance; whilst a small portion of such substance, when brought into contact with a less unstable substance, might induce such molecular movements as to make it undergo a process of fermentation. With regard to the cause of such induced fermentative changes, Gerhardt[10] says, in explaining Liebig's views: – "Every substance which decomposes or enters into combination is in a state of movement, its molecules being agitated; but since friction, shock, mechanical agitation, suffice to provoke the decomposition of many substances (chlorous acid, chloride of nitrogen, fulminating silver), there is all the more reason why a chemical decomposition in which the molecular agitation is more complete, should produce similar effects upon certain substances. In addition, bodies are known which when alone are not decomposed by certain agents, but which are attacked, when they exist in contact with other bodies incapable of resisting the influence of these agents. Thus platinum alone

[10] 'Chimie organique,' 1856, t. iv. p. 589.

does not dissolve in nitric acid, but when allied with silver, it is easily dissolved; pure copper is not dissolved by sulphuric acid, but it does dissolve in this when it is allied with zinc, &c. According to M. Liebig it is the same with ferments and fermentable substances; sugar, which does not change when it is quite alone, changes – that is to say ferments – when it is in contact with a nitrogenous substance undergoing change, that is, with a ferment."

Thus, in accordance with this latter view, living ferments are not needed – mere dead, organic or nitrogenous matter suffices to initiate the processes in question.[11] Those who hold this opinion may or may not believe that organisms are capable of arising *de novo*;[12] though there can be little doubt that a belief in the truth of such a doctrine does, almost inevitably, entail a belief in the *de novo* origination of living things. No one who has looked into the evidence, doubts the fact of the association between some of these processes and the presence of organisms; the only question is, as to the relation in which they stand to one another.

[11] Those who hold this opinion do not of course deny that living ferments can initiate fermentations. Every-day experience convinces them of the truth of this. They merely affirm that the intervention of vital action is not essential: they look upon fermentation as a purely chemical process, and believe that even in those cases where fermentation is initiated by living organisms (such as beer-yeast), these – although living – act chemically upon the matter which undergoes fermentation.

[12] They may not believe this, because they may be unaware of the fact of the invariable association of some organisms with some kinds of fermentations, and may consequently have never concerned themselves with the evidence bearing upon this part of the question. (See Gerhardt, *loc. cit.*)

If organisms are not the causes of those fermentative changes with which they are invariably associated, then they are, in all probability, the results of such changes; and they must certainly have been produced *de novo* if it can be shown that fermentation or putrefaction may take place under the influence of conditions which make it certain that pre-existing living organisms could have had nothing to do with the process.

Now, in order to lend some air of probability to the former hypothesis, concerning the necessity for the existence of living ferments, it was incumbent upon its supporters to endeavour to show that the air did contain such a multitude of "germs," or living things, as were demanded by the requirements of their theory. Spallanzani and Bonnet had, as far as the imagination was concerned, done all that was necessary. They had proclaimed the universal diffusion of "germs" of all kinds of organisms throughout the atmosphere – which were ready to develop, whenever suitable conditions presented themselves. So far, however, this was but another hypothesis. To establish the doctrine that fermentation cannot take place without the agency of living ferments, we cannot receive hypotheses in evidence: facts are needed. These, no one attempted to supply in an adequate manner[13] anterior to the investigations of M. Pasteur.

[13] M. Pouchet and others had examined the dust which *settles* on objects, and amongst much débris of different kinds had found comparatively few ova or spores. He had not, however, up to this time, filtered the air, so as to see what germs might be detected floating about in the atmosphere.

Speaking of his researches, even M. Milne-Edwards says,[14] "Previous to this time, the existence of reproductive particles, or infusorial germs in the atmosphere was nothing more than a *plausible hypothesis*, put forward in order to explain the origin of such creatures in a manner conformable with the general laws of reproduction; but it was only a mere supposition, and no one had been able actually to see or to handle these reproductive corpuscles."

We have to look, therefore, to M. Pasteur's investigations, and to others which may have been since conducted, for all the scientific evidence in support of what has been called the "Panspermic hypothesis."

By an ingenious method of filtration, which is fully described in his memoir,[15] M. Pasteur separated from the air that passed through his apparatus the solid particles which it contained. This search convinced him that there were, as he says, "constantly in ordinary air a variable number of corpuscles whose form and structure declare them to be organized." Some of these, he thinks, resemble the spores of fungi, and others the ova of ciliated infusoria, though he adds: – "But as to affirming that this is a spore, much less the spore of any definite species, and that one is an egg, and belonging to such an infusorium, I believe that this is not possible." He limits himself, in fact, to the statements, that the corpuscles which he found, were (in his opinion)

[14] 'Anat. et Physiol. compar.' t. viii. p. 264.

[15] 'Annales de Chimie et de Physique,' 1862, t. lxiv. p. 24.

evidently organized; that they resembled in form and appearance the germs of the lower kinds of organisms; and that, from their variety in size, they probably belonged to many different sorts of living things. Even here, therefore, we have to do with the impressions of M. Pasteur, rather than with verified statements. All that has been established by his direct investigation as to the nature of the solid bodies contained in the atmosphere is this: that the air contains a number of round or ovoidal corpuscles, often quite structureless, which he could not distinguish from the spores of fungi[16]– some of which, being about the right size, were round or ovoidal, and structureless. In addition, however, it has been shown that the air contains other rounded corpuscles which are similarly structureless, though composed of silica or starch. It may therefore be asked, in the first place, whether the conclusion is a sufficiently safe one that many of the corpuscles found by M. Pasteur were spores of fungi; and in the next place, supposing this to have been established, whether such spores were living or dead. These questions would have been answered satisfactorily if M. Pasteur could state that he had actually watched the development of such corpuscles, in some suitable apparatus, into distinct organisms. But any such development, he distinctly states, he never witnessed. He says[17]: – "What would

[16] Those which he believed to be eggs of ciliated infusoria, may be at once dismissed from consideration, as we are not at present concerned with the origin of organisms of this kind.

[17] *Loc. cit.* p. 34, note 1.

have been the better and more direct course would have been to follow the development of these germs with the microscope. Such was my intention; but the apparatus which I had devised for this purpose not having been delivered to me at a convenient time, I was diverted from this investigation by other work." The evidence which he does adduce, in subsequent portions of his memoir, in order to prove that some of these corpuscles were really "fertile germs," is almost valueless, because all the facts are open to another interpretation, which is just as much, nay, even more, in accordance with Baron Liebig's than with his own doctrine of fermentation.

But another most important consideration presents itself. M. Pasteur's researches as to the nature of the dust contained in the atmosphere enable him to say nothing concerning the presence of *Bacteria*, although he himself admits that these are generally the first organisms which display themselves in fermentations or putrefactions, and that in a very large majority of the cases in which fermentation occurs in closed vessels they are the only organisms which make their appearance.[18] And yet, notwithstanding these facts, M. Pasteur says, in reference to the common form of *Bacterium*: – "This infusorial animal is so small that one cannot distinguish its germ, and still less fix upon the presence of this germ, if it were known, amongst the organized corpuscles of the dust which is suspended in the air."

Here, then, we have a confession from M. Pasteur himself,

[18] *Loc cit.* p. 56.

that all evidence fails, where it is most wanted, in support of his hypothesis.

If a large number of fermentations begin with the presence of *Bacteria* as the only living things, and if in a number of cases no other organisms ever occur, it is useless to adduce as evidence, in proof of the view that fermentations are *always* initiated by air-derived organisms, the fact that certain corpuscles (supposed to be spores of fungi) are recognizable in the atmosphere – capped by the distinct statement[19] that *Bacteria* or their germs are not recognizable. If *Bacteria* are not recognizable in the atmosphere, what scientific evidence is there that the fermentations in which these alone occur are initiated by *Bacteria* derived from the atmosphere, or from certain imaginary *Bacteria* germs,[20] which we are supposed to be unable to distinguish? M. Pasteur may, moreover, be reminded that when he resorts to the supposition of *Bacteria* possessing "germs" which are indistinguishable, he is again resorting to hypothesis rather than to fact, in order to prove

[19] See p. 57.

[20] M. Pasteur's use of this term, in which he is followed by others holding similar opinions, is much to be deprecated. Having said that he had found certain corpuscles which resembled spores of fungi, or ova of infusoria, he subsequently speaks of them as "germs," and also applies the same name to the reproductive particles of *Bacteria*, which he merely *assumes* to be present in the atmosphere. Thus, having only proved that corpuscles resembling spores of some fungi, are to be found in the atmosphere, he subsequently speaks of the presence of a multitude of atmospheric germs as an established fact, without at all prominently pointing out that, so far as the most important of these are concerned – germs of *Bacteria*– their existence had only been inferred, and not proved.

the truth of the particular doctrine of fermentation which he advocates. *Bacteria* are known to reproduce and multiply only by a process of fission; each of the parts into which they divide being nothing more than a part of the original *Bacterium*, and therefore endowed with similar properties of resisting heat, desiccation, and other agencies. Any resort to invisible germs to account for the multiplication of *Bacteria*

Конец ознакомительного фрагмента.