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ON THE

ATMOSPHERIC GERM THEORY

AND

ORIGIN OF INFUSORIA.

A LECTURE DELIVERED TO THE ROYAL COLLEGE OF SURGEONS OF EDINBURGH, 17TH JANUARY 1868.

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NOTE.

Since this lecture was delivered my attention has been directed to two papers, by Dr George Child, in the Proceedings of the Royal Society, vols. xiii. and xiv., in which several careful experiments are described, and in which microscopical examinations with high powers, some of which were made by Dr Beale, show that infusoria occur under circumstances altogether opposed to the statements of M. Pasteur. It would appear, also, that Dr Wyman, of Cambridge, U.S., had previously made similar experiments with like results; but I am ignorant where an account of them is to be found. In the Annales des Sciences Naturelles, Botanique, Mars et Avril 1867, will be found a Memoir by M. Trécul on Amylobacteria. He has observed bacteria forming in the interior of the cells of decomposing plants, where no previous germs could exist, and says they are frequently coloured blue by iodine. Several other recent memoirs on the origin of Infusoria may be found scattered through the scientific works and journals of Great Britain, France, and Germany. (See especially Botanische Zeitung and Archiv für Mikroskopische Anatomie.) A careful perusal of these has only tended to confirm in my mind the correctness of the views which, after long consideration and repeated investigations, I now put forth.

J. H. B.

THE ATMOSPHERIC GERM THEORY.

Mr President and Gentlemen,—The subject which I have the honour of bringing before you this evening is one which has engaged the attention of physiologists since the days of Aristotle. That distinguished man, as well as most of the naturalists who lived previous to the time of Harvey, were of opinion that dust, decomposed flesh, and other dead substances might, under the influence of heat, air, and water, give rise to living beings independently of parents. This mysterious process, by a singular perversion of language, has been called spontaneous generation. We all know the story of the shepherd recounted by Virgil, who thought he saw bees spring up in the dead body of an ox, which was implicitly believed by Pliny; and we have read accounts of rats and frogs which were supposed to be engendered by the fruitful earth or teeming waters.

Francis Redi, a physician of Florence, was the first who clearly demonstrated, in 1638, that the larvæ and worms found in a dead body were not produced by putrefaction, but originated from flies' eggs deposited in the flesh.[1] Even he, however, says that he is inclined to believe the entozoa to have a spontaneous origin. The researches into generation, which alone would have conferred undying honour on the name of William Harvey, had it not been associated with the discovery of the circulation, led him to announce the law, "Omne vivum ex ovo." Since his day the belief has been general, that all animals and plants are derived from eggs or seeds; that vitality is always transmitted, and never created; and that, where these fundamental principles cannot be recognised, the minuteness of the germs and their wide diffusion throughout nature, and more especially in the atmosphere, offer a sufficient explanation of what may appear mysterious. Nature, it was argued, must be uniform in her operations, and analogy warrants our supposing that the same law of generation which applies to the higher animals and plants, is equally applicable to the lower.[2]

[1] Experimenta circa Generationem Insectorum.

[2] Most of the arguments on the other side, that is, in favour of Heterogenesis, will be found well stated by Burdach (Physiologie, tome 1. p. 8, *et seq.*)—argu-

The cell theory of growth, which was brought forward by Schleiden and Schwann in 1840, gave a new impulse to investigation by means of the achromatic microscope. With this instrument, physiologists have now traced the changes which ova and seeds undergo during their development in a vast number of animals and plants, and have even followed the former in their wanderings from the body of one animal where they originate, into that of another where they are fully developed. In this way we have an alternate generation, as explained by Steenstrup. In the mouse, for example, we find a cystic worm which, in its body, would be no further developed, but when the mouse is eaten by the cat, the cystic is transformed into the tapeworm, which infests the last named animal. But it has also been shown that certain insects are produced from others without generation by parents. This interruption in descent Owen has called Parthenogenesis, from παρθηνεια, virginity. Thus, two winged insects will produce an animal without wings, from which ten or twelve generations of individuals may be derived without a fresh act of conception, until the last in the chain gives forth another winged insect, when the process is repeated—as in the case of the aphis.[1] These generalizations, by clearing up some very obscure points in embryology, were thought to have given the *coup de grâce* to the old notion of spontaneous generation.

The ova, seeds, and primary cells, however, which were supposed to be floating in the atmosphere, though constantly looked for, could nowhere be found, and more careful investigation of the numerous forms of life which spring up in putrescent and fermented fluids utterly failed in connecting them with pre-existing germs. It is perhaps unnecessary to remark, that our modern microscopes have reached such perfection that we can examine with the greatest accuracy particles of only one fifty-thousandth of an inch in diameter, which are much more minute than the smallest recognizable ova or seeds. As all efforts, therefore, to discover the supposed germs in the atmosphere with our best instruments have failed, many scientific men who had personally investigated the subject were once more led into the belief of, at least, an equivocal or doubtful generation of the lowest forms of animal and vegetable life. This belief was strengthened by the appearance, in 1859, of a remarkable work by M. Pouchet, Professor of Natural History at Rouen, and a corresponding member of the French Academy of Sciences, entitled "Heterogenesis, or Spontaneous Generation," as distinguished from Homogenesis, or Generation from Parents. This book contains not only a full history of all that had been previously written and thought on the subject, but gives an account of numerous original experiments and observations made by the author,

ments admitted by Allen Thomson "to throw the balance of evidence in favour of the spontaneous production of Infusoria, mould, and the like." (Todd's Cyclopædia, article Generation, vol. ii. p. 430, 1839.)

[1] Owen on Parthenogenesis.

proving, as he thinks, that infusoria originate in a finely molecular, or, as he calls it, proligerous pellicle on the surface of decomposing fluids, without pre-existing cells or germs of any kind, and therefore independently of parents.

The publication of this book has led to a controversy which has continued up to the present moment. The theory of atmospheric germs, or that of the Panspermatists, has been ably sustained by M. Pasteur, who, by new experiments, has revived the doctrine that fermentation and putrefaction are not chemical processes, as has been maintained by Liebig, but physiological phenomena dependent on living germs derived from the atmosphere. These experiments gained for him, in 1859, the Grand Prize for Physiology annually awarded by the French Academy of Sciences. They have all, however, been since repeated by Pouchet and others, and their accuracy as well as the correctness of his conclusions are by them utterly denied. An extraordinary amount of research, ingenuity, and talent has been displayed by the advocates on either side, the results of which will be found recorded in numerous communications printed in the "Comptes Rendus" of the proceedings of the Academy.

Such, then, is an outline of the history of this matter. Before stating more particularly the arguments advanced by the controversialists, on one side or the other, I propose describing shortly the results of some investigations undertaken by myself, with a view of determining precisely the facts of the case, and the nature of the phenomena which have excited so much discussion.

My attention was originally directed to the origin of fungi in 1841, when examining the parasitic plants found growing on living animals, an account of which was published in the Transactions of the Royal Society of this city in 1842. In October 1863, after studying the memoir of M. Pasteur, I commenced a series of observations, with the aid of my former assistant, Dr Argyll Robertson, which were directed (1st) to determine with exactitude, by means of the microscope, the changes which occurred on the surface of infusions during the development of plants and animals there; and (2d) to ascertain what influence the air treated in various ways exercised on such growths. These observations were carefully repeated and extended in October 1864. They have again been recently repeated, and numerous other experiments have been performed, with the aid of my present assistant, Dr Rutherford. These investigations naturally divide themselves into (1st) observations by means of the microscope as to the development of infusoria; and (2d) experiments directed to destroy the supposed germs in the atmosphere so as to prevent putrefaction.

1. *Mode of Development of Infusoria.*

On making a cold or hot infusion of any vegetable or animal substance, covering the vessel with a piece of paper so as to exclude the dust, and then watching it every twelve hours, the first change visible to the eye is a slight opalescence, and the formation of a thin scum or pellicle that floats upon the surface. This appears at times, varying from a few hours to several days, according to the temperature of the atmosphere or the nature of the infusion. On examining the pellicle or film under high magnifying powers,[1] it is seen to be composed of a mass of minute molecules, varying in size from the minutest visible point to that of one thirty-thousandth of an inch in diameter. These molecules are closely aggregated together, and must exist in incalculable numbers. They constitute the primordial mucous layer of Burdach,[2] and the proligerous pellicle of Pouchet.[3] The same pellicle, examined six hours later, shows the molecules to be somewhat enlarged, and these separated by the pressure of the upper glass are already seen here and there to be strongly adhering together in twos and fours, so as to form a little chain. Many twos, also, have apparently melted together so as to form a short staff or filament—*bacterium.* (Fig. 1, *b.*) Twelve hours after this, it may be seen that the grouping of the molecules in twos, threes, and fours has become more general, and that several of these form new groups of eight lengthways. Many of them have melted together to produce longer bacteria. At the edges of the molecular mass, and in the fluid surrounding it, may now be seen a vibratile movement in the shorter bacteria and a serpentine movement in the longer ones, whereby they are propelled forwards in the fluid—*vibrio.* (Fig. 1, *c, d.*) From the second or third to the

Fig. 1.

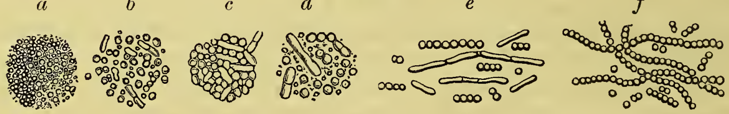


Fig. 1.—*a*, Molecular structure of the proligerous pellicle on its first appearance in a clear animal infusion. *b*, Molecular structure of the same, six hours afterwards. The molecules have been separated, and several are seen grouped together in twos and fours. Some of these have melted together so as to produce bacteria, which exhibit a trembling movement. *c*, The structure of the proligerous pellicle on the second day. *d*, The same separated. The molecules are coalescing in rows and melting together to form longer bacteria or vibrios, which move rapidly across the field of the microscope. As their development proceeds, they present the appearance seen in *e*, and in Fig. 2. *f*, Long filaments composed of adhering molecules. 800 *diameters linear.*

[1] The powers I have employed for the investigation were an excellent lens of $\frac{1}{8}$th of an inch focus, by Ross; but I also frequently used a $\frac{1}{12}$th of an inch by the same maker, and the immersion lens No. 10 of Hartnach, with varying powers of from 600 to 800 diameters linear. Occasionally I confirmed my observations with a lens recently made for me by Messrs Powell & Lealand, of $\frac{1}{25}$th of an inch focus, whereby I obtained an enlargement varying from 1250 to 2000 diameters linear.

[2] Physiologie, par Jourdan, tome ii. p. 123. [3] Hétérogénie, p. 383.

fifth or seventh days, the vibrios are lengthened, evidently by apposition of groups of other molecules, to their ends. These melt together to form a filament, which may extend a third or half, and in a few cases entirely across the field of the microscope. (Fig. 2.)

Fig. 2.

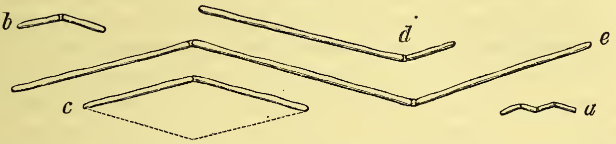


Fig. 2.—(*a*) Vibrio with a serpentine movement. (*b*) Vibrio with one flexure, evidently formed by the union of two bacteria. (*c*) Elongated vibrio with one flexure, the area of the movement marked by a dotted line. (*d*) An elongated vibrio, not moving; a bacterium evidently added at one extremity. (*e*) An elongated vibrio with two flexures, moving rapidly across the field of the microscope. An observation of these vital structures evidently indicates aggregations of bacteria and vibriones of a certain length endways, the flexures occurring at the points of junction. 800 *diameters linear.*

The movements visible in the molecules and filaments vary according to the amount of development. At first the molecules which float loose in the fluid exhibit gyrations which cannot be distinguished from Brunonian movements. When short bacteria are formed, these exhibit peculiar vibrations,—often turn round on their own axis in various directions, and slowly change their place. They rarely dart rapidly through the fluid, or exhibit a serpentine motion. But when the vibrio is formed, the filament is pushed forward with greater or less velocity, at first presenting a wriggling, but, as it becomes longer, a more decided serpentine motion. A distinct flexure can be seen at certain points in the filaments, between the groups of molecular chains or filaments. Dumas says he has seen the molecules and bacteria uniting endways, a statement the correctness of which Pouchet doubts.[1] On two occasions, however, I was fortunate enough to see this occurrence as represented in the accompanying figures. (See Figs. 3 and 4.)

Fig. 3.

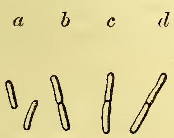


Fig. 4.

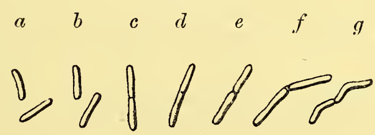


Fig. 3.—*a*, Position of two short bacteria. *b*, The lower bacterium was seen to sink down and unite itself to the upper, and then the two turned round in unison as in *c* and *d*.

Fig. 4.—*a*, Position of two bacteria. *b*, Altered position of the same. *c*, The lower one adhering to the upper. *d*, The two turning together to *e*. *f*, Vital flexure at the middle; and *g*, four flexures, when I saw the vibrio so formed move forward out of the field of the microscope. 800 *diameters linear.*

Pouchet thought that the vibriones exuded a mucous matter, whereby one stuck to the other. If so, such exudation can only

[1] Nouvelles Expériences, p. 115.

be poured out at their extremities, as they always unite lengthways, never crossways. I feel satisfied, however, that the reason the actual union has so seldom been seen is, 1*st*, That it only occurs at certain periods of development, and can only be followed by the eye, when the movements are slow; 2*d*, That amidst such a multitude of minute moving bodies it requires a long time before two can be found exactly on one plane, and can be brought so accurately into focus that they can be watched for a sufficient time. Having, however, in the two instances described and figured, actually seen the coalescence, I can have no doubt whatever that such is the true method of elongation. Numerous other facts seen among elongated vibriones support this view (see Fig. 2).

It may frequently be seen, on again examining the fluid in which these bodies have been moving actively, that they are all motionless, evidently dead. This occurs at various periods. They now rapidly disintegrate, and thus a second molecular mass or pellicle is produced. In this, rounded masses may be seen to form, which strongly refract light not unlike pus corpuscles, or the colourless corpuscles of the blood. These soon begin to move with a jerking motion dependent upon a vibratile cilium attached to one of its extremities—*Monas lens*. In a day or two other cilia are produced, the corpuscle enlarges, is nucleated, and swims through the fluid evenly. Varied forms may now occur in the molecular mass, dependent on the temperature, season of the year, exposure to sun-light, and nature of the infusion, all having independent movements. They have been denominated *Amœbœ*, *Paramecia*, *Vorticellœ*, *Kolpoda*, *Keronœ*, *Glaucoma*, *Trachelius*, *etc.*, *etc.*[1]

Pouchet describes the *Paramecium* as originating in the proligerous pellicle, formed by the breaking down of the primary bacteria and vibriones. It is the secondary histolytic mass of molecules which arrange themselves as seen in Fig. 5.

It would occupy too much time to follow the development of all the forms that may arise. They originate always long after the primary vibrios are produced, in the secondary, tertiary, or even later molecular masses, resulting from the disintegration of previous forms.

It frequently happens that soon after some of these higher infusoria are seen, that the pellicle falls to the bottom of the fluid, where it constitutes a dense precipitate, and slowly breaks down; then another scum forms on the surface, and molecules, bacteria, and vibrios are again produced.

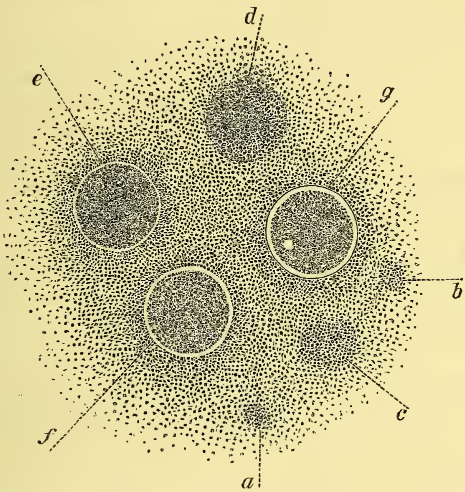
The varied forms produced are spoken of by Ehrenberg and other naturalists as being different species;[2] but I think it will be found that the laws, not only of molecular but of alternate generation and parthenogenesis, prevail among them, and one frequently passes into another. Their production is largely dependent on temperature,

[1] See Ehrenberg's Infusoria. [2] Ibid.

state of the atmosphere, light,—especially the sun's rays,—and other physical conditions.

At other times, it happens that the molecular mass, instead of being transformed into animalcules, gives origin to minute fungi. In this case the molecules form small masses, which soon melt together to constitute a globular body, from which a process juts out on one side. These are *Torulæ*, which give off processes which are soon transformed into jointed tubes of various diameters,

Fig. 5.



Formation of Ova in the proligerous Membrane.

a, Coalescence of molecules. *b*, The same more advanced. *c*, Still larger mass. *d*. The same assuming a rounded form. *e*, A membrane formed externally. *f*, Complete differentiation of the now perfect ovum from the surrounding molecular mass. *g*, A nucleus apparent. —*Pouchet.* 250 *diameters.*

terminating in rows of sporules (*Penicillium*), or capsules containing numerous globular seeds (*Aspergillus*). Occasionally filaments are formed from the direct melting together of molecules arranged longways (*Leptothrix*) (See Fig. 1, *f.*)

Here also I think various forms regarded as distinct plants pass into one another—especially torulæ, which are only embryonic forms of higher fungi. In all these cases no kind of animalcule or fungus is ever seen to originate from pre-existing cells or larger bodies, but always from molecules.

That we should sometimes have animalcules, and at others fungi, is a well-known fact, the exact causes or conditions producing which are not yet explained. The Panspermatists, of course, are of opinion that the germs in the atmosphere are of many kinds, and that as they fall into various infusions they produce different results, in the same manner that varieties in ova or seeds develop themselves in peculiar localities or special soils. This assumption, however, seems to me opposed by the following experiment:—

If an infusion be placed in a deep glass vessel, which again stands in the centre of a shallow vessel containing the same infusion, and the whole covered with a large bell glass, it will be found in eight days that on the surface of the former are numerous ciliated animalcules, while on that of the latter only bacteria and vibrios exist. The experiment may be reversed, for if the shallow vessel be filled to the brim, and the deep vessel has only its bottom covered, then the ciliated microzoa will appear in the former, and the non-ciliated in the latter.[1]

As a result of these experiments, Pouchet has formularized a law to the effect that the production of ciliated animalcules is in an inverse ratio to the square of the surface, and that the production of monads is in a direct ratio to the cube of the mass of the same fluid.[2] To this law I have met with some exceptions, animalcules having been produced in some of our recent experiments in the shallow dish, and vegetations in the deep vessel, and *vice versa.*

It is difficult to explain how germs falling from the air on the same infusion, under identically similar conditions, with the exception that the fluid is in vessels of different forms, can vary the results. Whereas the fact that the higher infusoria are formed secondarily out of the disintegrated mass of the simpler ones, which can only take place where that mass is considerable and floating on the surface of deep fluids, directly confirms the molecular theory of growth, and offers an illustration of how successive disintegrations give origin to different formations.[3]

That the infusoria originate and are developed in the molecular pellicle which floats on the surface of putrefying or fermenting liquids, has been admitted by all who have carefully watched that pellicle with the microscope, more especially by Kutzing,[4] Pineau,[5] Nicolet,[6] Pouchet,[7] Jolly and Musset,[8] Schaafhausen,[9] and Mantegazza.[10] The question therefore is, are the molecules that constitute that pellicle derived from the air or the fluid,—are they precipitated from above, or do they float to the surface from below, like the globules of the milk which produce cream?

Now, it was in consequence of having professed to demonstrate

[1] Pouchet's Nouvelles Expériences, etc., pp. 135, 243–245. Paris, 1864.

[2] Nouvelles Expériences, p. 134.

[3] See On the Molecular Theory of Organization, by the Author. Proceedings of Royal Society of Edinburgh, 1861.

[4] See Schaaffhausen, Comptes Rendus, tome liv. p. 1046.

[5] Annales des Sciences Naturelles, 3me série, tome iii., p. 182. This observer thinks he saw disintegrated fibres of meat and of other substances formed directly into vibriones,—in this he was incorrect.

[6] Arcana Naturæ, tome i. p. 2.

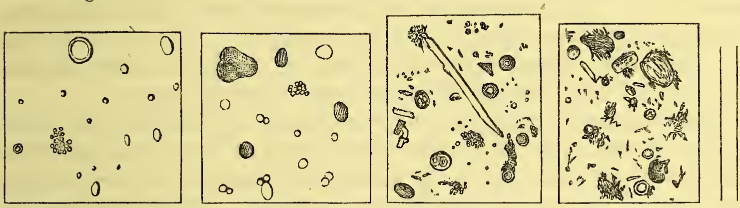
[7] Hétérogénie, p. 353. Nouvelles Expériences, p. 111.

[8] Comptes Rendus, tome l., p. 934. [9] Ibid. tome liv. p. 1046.

[10] Institut Lombard, 1852, tome iii.

what had escaped all previous observers,—viz., the germs in the air,—that M. Pasteur has made his name so famous. He tells us[1] that he did this by causing a current of air to pass through a glass tube in which a pledget of gun-cotton had been placed. This was then dissolved in ether, and the sediment allowed to collect in a watch-glass. This sediment, after being repeatedly washed, and allowed to remain in distilled water for twenty-four hours at a time, is allowed to dry. A portion of the dried matter is then put upon a slide moistened with a weak solution of potash, and, being covered with another glass, is examined with the microscope. The results he has figured; and, very properly, he has given the scale of magnifying power under which they were drawn (Fig. 10), and which, by careful measurement, I have ascertained to be 180 times linear. I show you his drawings, carefully copied.

Fig. 6. Fig. 7. Fig. 8. Fig. 9. Fig. 10.



Exact copies of the figures given by M. Pasteur of the dust he collected on gun-cotton, magnified 180 diameters. These should be compared with Fig. 1, magnified 800 diameters, showing what is seen to take place when infusoria are forming. Fig. 10, scale of one hundredth of a millimètre.

He says Figs. 6 and 7 represent organized corpuscles from dust collected in twenty-four hours, from the 16th to 17th November 1859. The manner in which these drawings, giving the volume and outline of the bodies, were made, is as follows:—"After the dust has been prepared in the manner described, I took a portion of it from the watch-glass, and diluted it with a solution of potash, consisting of 5 parts of potash in 100 of water. As soon as I perceived a globule evidently organized under the microscope, I drew it. This is how figure 4 was drawn."[2] This description leaves it uncertain whether an exact copy was taken of any portion of the field of the microscope, and, therefore, whether the figure represents the exact number of corpuscles present, and their relation to each other. It only gives their form. But, assuming that the same kind of demonstration was made in each case, we have the relative numbers of these bodies taken from the gun-cotton in Fig. 6. Fig. 7 is another demonstration of the same after the addition of an aqueous solution of iodine. Fig. 8 represents the organized corpuscles associated with amorphous particles obtained on the 25th and 26th of June 1860; Fig. 9, the dust of an intense fog in the month of February 1861. In all these demonstrations he admits the organ-

[1] Annales des Sciences Naturelles, 4me série, tome xvi. p. 25. [2] Ibid.

ized corpuscles are comparatively scarce, because, he observes (p. 31), it is frequently necessary to change the field in order to see one of them, whilst at other times several could be seen together.

M. Pasteur thinks that these drawings indicate the number of organized corpuscles that may be arrested in a small mass of cotton through which 1500 litres of air, in one of the less-frequented streets of Paris, have passed in twenty-four hours, about three or four yards from the ground. These he estimates at several millions in a litre (p. 29).

Now, it must be remembered that M. Pasteur is a chemist, and it will be admitted by every histologist that no method could be more unsatisfactory for determining either the nature or the number of the corpuscles than the one he adopted. The solution of the cotton in ether, the frequent soakings in water, the desiccation, and then the addition of a solution of potash, must completely alter the character of any living corpuscles in the atmosphere. Then the forms he assumes to be organic, are not necessarily so. They are exceedingly frequent among mineral substances, and siliceous rounded forms are common, which of course resist sulphuric acid.

Numerous investigations have been made, both before and since M. Pasteur wrote, to determine the nature of dust floating in the atmosphere—of that dust, for example, which a ray of sunlight reveals to us, when admitted into a chamber. It consists, for the most part, of different kinds of starch corpuscles; the debris of clothing, especially filaments of cotton, silk, and wool; the results of different kinds of combustion, whether of coal or of wood; various mineral bodies, globular or ovoid, amorphous or crystalline; and minute fragments of insects and vegetables; very rarely small seeds and microscopic animalcules.

These constituents vary to such an extent in different localities, as to enable the observer, in some cases, to determine whence the dust was collected. Starch corpuscles abound in the neighbourhood of flour-mills and bakeries; fragments of clothing where there have been crowded assemblies of persons, cotton and wool being predominant if the persons belong to the poorer classes, and silk if the upper classes have been present; the products of combustion predominate in smoky localities; mineral particles on the roads and highways; seeds, fragments of vegetables and insects, in market places, gardens, etc., etc. But although these constituents of the air vary in different places, infusoria, produced in all of them, are identically the same.[1]

This has been tested in various ways. The dust has been ransacked to discover organic germs,—collected and carefully examined with the microscope, near the soil, and on the summits of the highest buildings, not only in frequented, but in desert places; in crowded assemblies, as well as in empty Gothic cathedrals and ancient vaults—in the ancient palace of Karnack, on the banks of the Nile;

[1] Pouchet's Nouvelles Expériences, p. 73, *et seq.*

in the tomb of Rhamses II. at the extremity of the Desert; as well as in the central chambers of the great pyramid of Ghizeh. The chief element of the dust collected in these places has been found to be starch corpuscles.[1] Large quantities of air have been drawn through tubes by aspirators, and collected on cotton, in distilled water, or projected on glass. The feathery snow, which, falling through the atmosphere, may be well supposed to collect its contents, has been melted, and the precipitate carefully collected. The emanations of marshy places, such as those of the Maremma in Tuscany, have been specially investigated.[2] The larynges and mucous pulmonary surfaces of numerous animals have been explored, even to the inmost bone cavities of birds. On the summit of Mont Blanc, amidst eternal snow; on the glaciers of the Jura and of the Pyrenees, and in the deep crevasse;[3] on the burning plains of Egypt, and in the markets of Constantinople, the dust of the atmosphere has been microscopically examined,—and in all with a like negative result as to the existence of germs. Nowhere could they be seen, or if a few, in the opinion of some, were visible, could they in any way account for the multitude of minute infusoria, which, in all these localities, not only readily spring up in putrid fluids, but in every instance are identically the same.[4]

Indeed, on examining the drawings of M. Pasteur (see Figs. 6 to 9), let us suppose that the few bodies he has figured are truly sporules, as he believes them to be, which have preserved their form—after the action of ether, several soakings of twenty-four hours each in water, the desiccation, and subsequent mixture with a weak solution of potash. How, it may be asked, could these bodies produce the incalculable millions of minute molecules in the smallest fragment of the pellicle we can transfer to our microscopes, in which, as we have seen, the infusoria originate? It has been supposed that, on falling from the air, they undergo rapid division, and spread over the surface with the greatest rapidity; but no one has ever seen this remarkable phenomenon, and the slightest consideration must show that such an assumption is completely adverse to what can be readily demonstrated on the surface of every infusion. Thus, there can be no doubt that the minute molecules are formed first, and the bacteria, vibrios, and filaments, last. Supposing that the primary molecules, figured No. 1, in Fig. 11, enlarge to a certain point, No. 2, and then divide, how is it possible to explain the formation of elongated filaments at all? Surely the idea of their rapid multiplication by division is opposed to that of their power of elongating into bacteria and vibrios, whether by aggregation or growth from their extremities. It may frequently be seen that No. 3 is composed of molecules of exactly the same

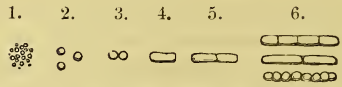
[1] Pouchet's Hétérogénie, p. 446.

[2] L. Gigot's Récherches expérimentales sur la Nature des Émanations marécageuses, Paris, 1859. Récherches sur l'Air des Maremmes de la Toscane, par M. E. Bechi. Comptes Rendus, tome lii. p. 852.

[3] Comptes Rendus, tome lvii. p. 558.

[4] Nouvelles Expériences, p. 75.

Fig. 11.



Stages in the Development of Vibriones. 800 *diameters linear.*

size as No. 2, which are floating loose,—a fact in favour of their coalescence rather than of their division, as then they would be reduced to half the size. It is more probable that although the smaller molecules may increase by imbibition of fluids, they have yet a constant tendency to aggregate together and melt into one another. No. 3 is not a proof of No. 2 dividing, but of two molecules coalescing; and when they unite, they form No. 4. Two or more of these uniting, form Nos. 5 and 6. When a similar process to this goes on in mineral bodies, as shown by Mr Rainey,[1] it cannot suggest division, but union; and this for the obvious reason, that the former would lead to disintegration, whereas, it can be seen in one case as in the other, that development is the result. In short, in the same manner as a tube is formed by a coalescence of cells, so is this minute vibratile vibrio formed by the coalescence of molecules. It may be argued, however, that each molecule elongates itself—that is, No. 2 is converted into No. 4; this into Nos. 5 and 6; and that No. 3 are sporules or ova, caused by the disintegration of No. 6. But this view is opposed by the fact that Nos. 1, 2, and 3 are seen before Nos. 4, 5, and 6 are produced. Of this all have satisfied themselves who have examined animal and vegetable infusions; and the conclusion, therefore, cannot be resisted, that the vibrios are derived from the molecules, and not the molecules from the vibrios.

But it may also be supposed, that while some have the property of dividing, others are capable of elongating or aggregating; but this view is not only opposed to observation, but is at variance with all that we know of embryonic development in plants and animals. When a plant consists of a single structural element, such as a cell or a tube, it will, I think, be admitted that growth in the sense of increased bulk, and growth in the sense of multiplication of parts by division, do not proceed at the same moment of time. Every plant and animal follows, in this respect, the same law. Nutrition is carried on up to a certain point of maturity, and then, and not till then, does generation, or the separation of parts to form new creatures, take place. When plants and animals are complex in their structure, one organ or segment may be growing, while another is disintegrating; but in individual organs there is a period for growth and reparation, and a period for division or separation. Hence, it seems to me, I am correct in thinking that if the primary molecules on the surface of an infusion possess the property of dividing, they cannot also, at the same moment, possess the property of elongating and

[1] On the Mode of Formation of Shells, etc., 8vo. London, 1855, p. 12.

forming filaments. The one function is subversive of the other. While, then, a cell or a vibrio may possess the property of growth and division, these two functions must be exercised at different periods of time,—so that, in reference to the early stage of formation, if the molecules divide, bacteria, vibrios, and filaments could not be formed. A mass of vibrionic molecules is not a compound organism; it is a mere aggregation of similar simple elements. Each of these in passing through certain phases of development may be arrested, or reach maturity at various periods, so that we frequently see different forms present at one time; but that the same forms and the same stages of growth should exhibit directly opposite functions, is surely not in accordance with physiological knowledge.

The conclusion we must arrive at therefore is, that the molecules seen on the surface of infusions out of which animalcules and fungi are produced, are not derived from the air.

Neither can they be supposed to pre-exist in the fluid, as then they would be readily seen, which they never are at the commencement. On this point nothing can be clearer than the microscopical evidence, so that it results from the facts and arguments I have brought before you, that the more simple infusoria do not originate from cells or minute germs at all, whether in the atmosphere or in the fluid. This is the almost universal conviction of histologists who have carefully investigated the matter.

2. *Chemical Experiments which have been directed to destroy the supposed Germs in the Atmosphere, so as to prevent Fermentation and Putrefaction.*

Schutze, in 1837,[1] after heating an infusion to the boiling point, connected it with two of Liebig's bulbs, one containing sulphuric acid, and the other concentrated solution of potash. The air forced through these liquids he thought capable of destroying the atmospheric germs.

Schwann also, in 1837,[2] forced air, with the same view, through metallic tubes heated to redness; and found, when so calcined, it occasionally prevented infusorial growth. He thought that oxygen alone was not the cause of fermentation, but some substance in the air capable of being destroyed by heat.

Schræder and Dusch, in 1859, filtered the air through cotton before bringing it in contact with organic fluids. They found that some did and others did not undergo putrefaction, and were induced to believe that the presence of oxygen, and the formation of an acid, were the cause of fermentation. Schræder afterwards found that the yolk of an egg, milk, and the juice of meat without water,

[1] Poggendorf's Annalen, 1837, p. 41; and Edinburgh New Philosophical Journal, 1837.

[2] Poggendorf's Annalen, 1837, p. 184.

putrefied in air filtered through cotton, and supposed it to contain an active substance, the nature of which was unknown.[1]

The experiments of Schutze, Schwann, Schræder, and Dusch have been frequently repeated without preventing the growth of fungi.[2]

Again, it is almost universally considered that the heat of boiling water or cold at zero will destroy all kinds of animal and vegetable life. Indeed, to imagine that the minute molecules or vibrios of which we have been speaking, or small ova and sporules consisting of oleo-albuminous matter without any envelope, would remain in boiling water for hours and retain their vitality, must be regarded as a violent assumption. Three or four minutes' boiling of a hen's egg not only kills it, but converts its whole substance into a hard mass. There is no seed known which, when taken out of its indurated shell or case, is capable of germinating after being boiled for a short time.[3] Yet nothing is more certain than that long ebullition of various infusions has wholly failed to prevent the formation in them of animal and vegetable growths.

Pouchet and others have frequently performed the following experiment:—An open flask was plunged into and filled with a decoction of barley which had been boiling for six hours. A stopper was introduced into it below the liquid, and on taking it out the whole neck of the flask was immediately plunged into melting sealing-wax, and hermetically closed. In six days some yeast was observed in it, at a temperature of 18° Cent. The following day the temperature was raised suddenly to 27°, when the flask burst, and then it was seen by the naked eye, and by the microscope, that it contained a notable quantity of yeast.[4] Now, yeast is a plant, which was thus proved to have grown in an infusion that had long been boiling, and from which all atmospheric air had been expelled. As, therefore, neither calcined air, sulphuric acid, liquor potassæ, gun-cotton, or a boiling temperature have failed to prevent the production of infusoria, or destroy the supposed germs in the air or infusion, I determined, in 1863, to try the effects of all these destructive agents, with the exception of the first, at once, and with the greatest possible care.

First Series of Experiments.

On the 17th and 18th of October 1864, I performed the following experiments in my laboratory, with the assistance of Dr Argyll Robertson:—

[1] Quoted by Pasteur, *opus cit.*, p. 16.

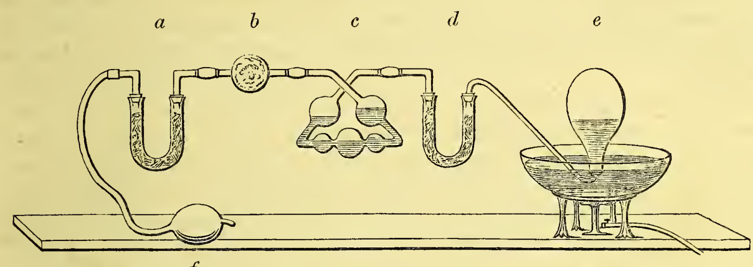
[2] See Pasteur, *opus cit.*, pp. 34, 35. Pouchet's Hétérogénie, pp. 252, *et seq.*

[3] See some conclusive experiments recently performed on this subject by Meunier. Comptes Rendus, tome lxii. p. 992. See also Pouchet's Experiments on the Seeds of Medicago from Brazil. Comptes Rendus, tome lxii. p. 941.

[4] Hétérogénie, p. 629.

Decoctions of liquorice root, of tea, and of hay were kept at the boiling temperature in a porcelain basin, over a gas flame. Flasks filled with and inverted in the boiling fluid, had air pumped into them to the extent of three-fourths of their volume, which had passed through (1st) a U-shaped tube containing liquor potassæ; (2d) Liebig bulbs, containing sulphuric acid; (3d) a hollow glass ball containing gun-cotton; and (4th) another U-shaped tube with

Fig. 12.



Arrangement of the Apparatus employed.

a, Bent tube containing liquor potassæ and pumice-stone. *b*, Hollow glass ball containing gun-cotton. *c*, Liebig's bulbs, containing sulphuric acid. *d*, Bent tube containing sulphuric acid and pumice-stone. *e*, Infusion kept at the boiling point, when air was pumped into it. *f*, Caoutchouc pump.

sulphuric acid. All the bent tubes were filled with fragments of pumice-stone, to break up the air, so as to prevent the possibility of any germs passing through in the centre of bubbles. The bent glass tube leading from the last U-shaped tube, filled with sulphuric acid and pumice-stone, was also filled with the acid, so as to destroy any germs that might be supposed to adhere to the interior. After the air so prepared had entered the flask, corks, which had been for some time boiled in the infusion, were by means of iron forceps inserted in the necks of the flasks, and the entrance of fresh air prevented. Further, on removing the flask from the boiling infusion, the cork and neck were hermetically closed by plunging them into melted sealing-wax. At the same time bottles or flasks containing the same infusion, but having a similar proportion of ordinary air, were sealed or corked up, so as to be contrasted with the influence of the prepared air.

The results were that the infusions in all the flasks in contact with ordinary air, five in number, were rendered turbid, or covered with fungi in from six to twelve days; whereas all the infusions, with one exception, which were exposed to the prepared air, eight in number, also became turbid and contained fungi, but at periods varying from four to nine months.

Some of these flasks I have recently opened (January 7, 1868), and found that they contained deposits of bacteria and vibriones, with molecular debris of an indeterminate character.

Second Series of Experiments.

About a year afterwards, on the 3d and 13th of October 1865, the experiment was repeated in exactly the same manner, with separate decoctions of liquorice and dulcamara roots, and with tea. Again, flasks holding similar decoctions with ordinary air were contrasted with those which held the prepared air. In these last, three in number, fungi appeared in from six to twelve days; and in the former, all, nine in number, with one exception, contained fungi, or were very turbid, in periods varying from four to nine months.

The exception, which I now show you in the first series of experiments, was a decoction of hay; and in the second series, a decoction of dulcamara root. It will be observed that both these infusions in the flasks remain clear and without the slightest sediment or pellicle, although the prepared air they contain, the fluid, the flask itself, and the mode of closing and sealing it, were identically the same as in all the other flasks which were filled and closed at the same time. To suppose that these two flasks were the only ones from which atmospheric germs were excluded, must appear to those who will examine them all, a strong assumption. Neither can it be true that in all the flasks, except one in each series, air could have entered since they were closed. I have placed the whole in vacuo under the bell-glass of an air-pump, and from none could any air whatever be extracted.

Three of the flasks in the second series of experiments I have opened, and found in two the same molecules and indeterminate debris as in the others; but in the third, a white cottony fungus, the size of a small pea, was floating in a tolerably clear decoction of dulcamara, which, on microscopic examination, was composed of minute jointed filaments forming a dense network.

Third Series of Experiments.

A third series of experiments was performed with the assistance of Dr Rutherford, October 14, 1867. In this case glass stoppered bottles were used, it having been suggested that sporules or germs might have been concealed in the corks formerly employed, although they had been well boiled. In this case we employed weak decoction of tea, of beef, and of hay, taking care as before, that one bottle having ordinary instead of prepared air introduced, should be kept for the purpose of comparison. In all the bottles with common air, six in number, fungi appeared in from one to two months and a half. In one bottle air was passed through carbolic acid only, and you see that it presents a distinct proligerous membrane; all the others as yet remain unchanged, but we cannot as yet regard the experiments as complete.

Fourth Series of Experiments.

In all these cases it will be observed, that the prepared air was passed through a boiling fluid and was consequently rarefied. It occurred to me to allow the fluid in the inverted bottles to get cold before introducing the prepared air. This was done in four cases, December 28, and I show you that a fungus has appeared in all. It would seem, therefore, that rarefaction of the air greatly influences the result. It may easily be conceived that air subjected to a boiling temperature is so expanded as scarcely to merit the name of air, and that it becomes more or less unfit for the purpose of sustaining animal or vegetable life.

Fifth Series of Experiments.

A fifth series of experiments was performed on the 4th of the present month (January 1868), with the same precautions, and with an infusion of dulcamara only. We filled sixteen bottles. Into four of these we pumped ordinary air into a cold infusion. Into four others ordinary air was pumped into a boiling infusion, and we repeated this experiment with four bottles each of the boiling and of the cold infusion with prepared air. The result, up to the present moment is, that the fluid has become turbid only in the four which contained ordinary air not rarefied.[1] This experiment also is not yet complete, but serves to show the great influence of rarified air.

Sixth Series of Experiments.

A sixth series of experiments was performed on the 11th of the present month. It having been asserted by M. Pasteur[2] that passing air through bent tubes, by hindering the access of germs and allowing them to be deposited on the sides of the glass, prevented the growth of infusoria, twelve bottles were prepared, four of which contained an infusion of dulcamara, another four a decoction of putrid meat, and a third four, yeast water. The bottles were plunged into and filled with the infusion when boiling, inverted in it, and allowed to get cold. Through a bent tube, five feet in length, having fourteen sharp zig-zag bends, four inches long, the air was pumped gently into three bottles of each series, and ordinary air was admitted to the fourth. The result is, that already, January 17, the fluids in all are turbid, with the exception of two bottles containing infusion of dulcamara,[3] so that bent tubes appear to intercept none of the supposed germs.

These experiments, on the whole, appear to me to be totally adverse to the atmospheric germ theory, and to indicate that the pro-

[1] A fungus has since appeared in one of the bottles containing non-rarified prepared air. (Feb. 12).

[2] Comptes Rendus, tome l. p. 306.

[3] These also have since become turbid. (Jan. 30).

duction or non-production of infusoria depends, for the most part, on the temperature, chemical constitution, density, and other physical properties of the air, rather than on living organisms there, which are developed in the fluid. Still, in every series of experiments, it will be noticed that there are one or two exceptions. This has also been observed by Pasteur in most of his experiments, and he attributes them to some currents or limited portions of air being rich in germs, whilst others are free from them.[1] But that this explanation applies to my laboratory, in which all the experiments described were made, is not probable.

It is now admitted by M. Pasteur that the boiling temperature, that is 100° Centigrade, does not prevent the growth of the supposed germs in the atmosphere; but instead of considering this fact hostile to his theory, he concludes from it that the germs have the power of resisting that amount of heat, and of being most tenacious of life; but he says, 130° Centigrade always destroys their vitality. M. Pouchet, however, has shown that the air, and the organic matter placed in boiling water, will germinate after they have been exposed to a heat of even 150°, and he says it may be raised to 200° Centigrade, and yet animalcules and fungi will develop themselves.[2]

In the same manner, air and infusions exposed to intense cold still produce animalcules, but, according to Pasteur, not so readily. Twenty flasks containing boiled infusions, and from which the air was expelled, were opened by him with excessive precaution on the Mer de Glace at Montanvert on the Jura. Notwithstanding the purity and extreme coldness of the air, infusoria appeared in five of his flasks.

As an illustration of the manner in which the controversy on this subject has been carried on in the Imperial Academy of Sciences in Paris, I may give a short account of that portion of it referring to the Glacier experiments. M.M. Pouchet, Jolly, and Musset opened eight similar flasks used by M. Pasteur at Montanvert, on the Glacier of the Maladetta, in the Spanish Pyrenees, 9000 feet above the sea, and 3000 feet higher than that of Montanvert, using all the precautions required by M. Pasteur. In addition, before cutting off the ends of their hermetically sealed tubes with a file, previously heated by a lamp, they held the flasks above their heads. Notwithstanding, infusoria appeared in all the infusions a few days afterwards.[3]

To this communication, presented to the Academy, Sept. 21, 1863, M. Pasteur replies, Nov. 2,[4] saying that he is rejoiced that his learned adversaries have gone to such an altitude to repeat his experiments; but observes that they did not take the necessary precautions. They only had eight flasks, whereas he had twenty; they shook their flasks before opening them, which he took care not to do; and they had the imprudence to use a file, instead of a pair

[1] Comptes Rendus, tome li. pp. 350, 351.
[2] Ibid. tome l. p. 1015.
[3] Ibid. tome lvii. p. 558.
[4] Ibid. p. 724.

of pincers with long branches, heated in the flame of a lamp. He says that the thumb and fingers holding the file were too near the opening into the flask, and may have conveyed germs there, especially as they were not passed through the flame, as the file was.[1] He defies them, if they take sufficient precautions, to obtain infusoria in all their flasks.[2]

MM. Jolly and Musset accept the defiance of M. Pasteur, Nov. 16,[3] and, in fact, on the 13th June following, they send a memoir to the Academy, stating that they had returned to the Maladetta, this time with twenty-two flasks—that is two more than were used by M. Pasteur—fulfilled all his conditions, not forgetting the pincers with long branches, properly heated, and found that infusoria appeared in every flask without exception in four days;[4] and so ended this part of the controversy.

Numerous other important questions have been debated before the Academy; among these are the changes which take place in the air confined in the flasks, founded on numerous analyses;[5] the observations of Jolly and Musset with regard to vibrios living in distilled water;[6] the statement by M. Pasteur that neither free oxygen nor atmospheric air are necessary for the growth of infusoria,[7] and that they will develop themselves in carbonic acid gas only. Lastly, the same chemist declares that, notwithstanding his often declared opinion that ferments are living beings and not dead matter, that he can produce fermentation with the ashes of yeast.[8] Some of these statements are confirmed by Donné,[9] who found that hens' eggs became putrid without the formation of vibrios or other infusoria. This observation, while it might serve to prove that atmospheric air passing through the egg-shell separated the germs by filtration, is wholly opposed to the idea that putrefaction is necessarily caused by such germs.

The only conclusion I can draw from the numerous contradictory and ingenious communications presented to the Academy of Sciences during the last eight years on this matter is, that not the slightest proof is given by the chemists, with M. Pasteur at their head, that fermentation and putrefaction are necessarily dependent on living germs existing in the atmosphere. They rather tend to show that these are phenomena of a chemical nature, as was ably maintained by Liebig.[10] Did we indeed confine our reading to the papers of M. Pasteur—that is, to one side of the case—we could easily persuade ourselves of his correctness;[11]

[1] Comptes Rendus, p. 725. [2] Ibid. p. 726. [3] Ibid. pp. 842–845.
[4] Ibid. lvi. p. 1122. [5] Ibid. pp. 734–739. [6] Ibid. lv. p. 491.
[7] Ibid. li. pp. 345, 346; liv. p. 267; lvi. p. 1191. [8] Ibid. lvi. pp. 418, 419.
[9] Ibid. lviii. pp. 951, 952; lxv. p. 602.
[10] Letters on Chemistry, letters 18 and 19.
[11] This is what unfortunately seems to have been done by the Commission of the Academy, which made a report on this subject, Feb. 20, 1865 (Comptes Rendus, tome xl. p. 384). No histologist was on the Commission, which refused to enter upon any kind of microscopical inquiry; Messrs Pouchet,

but every one of his experiments has been repeated by several independent investigators, who have shown his imagined proofs as to the existence of atmospheric germs to be altogether erroneous. We may conclude, therefore, that living germs are not necessarily the cause of putrefaction and fermentation; neither is it necessary to believe that ferments are living at all—they may be dead. This, if not admitted, seems to be implied by Pasteur himself, who tells us he can now excite these processes, not by fresh yeast only, but by the ashes of yeast.[1] That they may be induced by dead organic matter, which has been subjected to a direct temperature of 150° or 200° Centigrade—a heat utterly incompatible with the existence of life—we have seen to have been proved by Pouchet, Jolly, Musset, and others.

The idea that these imaginary germs were the cause of putrefaction, of disease, of blights among vegetables, and other evils, originated with Kircher and the pathologists of the seventeenth century. It has been frequently revived, but always shown to be erroneous. In 1852, cholera was supposed to be occasioned by a fungus that really existed in the dejections, but which Mr Busk pointed out was the *uredo segetum* of diseased wheat, which entered the body in the form of bread. Certain well-known parasitic diseases are spread by contact, such as scabies, which, as it depends upon an insect burrowing in the skin, may be understood to crawl from one person to another. Favus, also, I succeeded, in 1841, in proving might be made to grow on diseased surfaces of otherwise healthy persons; but many of our unquestionably infectious diseases, such as smallpox, scarlatina, measles and typhus, have no such origin. It has been attempted to be proved, indeed, by Lemaire,[2] that in the condensed vapours of hospitals and other putrid localities, vibrios may be found; but that vibrios are the cause of these various diseases, is not only not proved, but from what has been stated, is highly improbable.

What, then, it may be asked, is the origin of the infusoria, vegetable and animal, that we find in organic fluids during fermentation and putrefaction? In answer to this question, I say they originate in oleo-albuminous molecules, which are formed in organic fluids, and which, floating to the surface, form the pellicle or proligerous matter. There, under the influence of certain conditions, such as temperature, light, chemical exchanges, density, pressure, and composition of atmospheric air, and of the fluid, etc., the molecules, by their coalescence, produce the lower forms of vegetable and animal life.

Hallier, describing the development of Penicillium crustaceum, tells us, that after all movement in the primary molecular mass has ceased, the molecules arrange themselves in long lines, which he

Jolly, and Musset, under such circumstances, very properly took no part in the investigation, which, consequently, was altogether one-sided, and of no scientific value.

[1] Comptes Rendus, tome lvi. pp. 418, 419. [2] Ibid. tome lix. pp. 317–428.

calls Leptothrix chains. (Fig. 1, *f*.) From the melting together of these, the delicate filaments forming Leptothrix buccalis are evidently produced; and these, according to him, by further development, pass into Penicillium crustaceum.[1] Why the molecules should sometimes arrange themselves in long rows, and at others into rounded masses (compare Fig. 1, *f*, and Fig. 5, *a*), is probably dependent on varying degrees of limpidity and viscosity. But why both these forms of molecular matter should sometimes possess an inherent power of contractility, and at others not, it is impossible as yet even to surmise. But on the determination of this point, the variations existing between the different kinds of fermentation and putrefaction are evidently dependent.

But the ultra vitalist will ask, If so, what becomes of the doctrines —"Every living thing from an egg;" "Every cell from a cell;" and "All life from life?" To this I reply, these expressions are formulæ, which only impose upon the understanding, check the search after truth, and are already completely overthrown by the advance of science. A few words with regard to each of them will, I think, show this.

I.—After what has been said, I need not dwell upon the first of these views, viz., *omne vivum ex ovo*, for the facts we have described are quite hostile to it.

II.—That every cell comes from a cell, is a recent doctrine, which, from its first enunciation, was rejected by almost all the first histologists of Europe. The illustrious founders of the cell theory, Schleiden and Schwann, clearly showed that primary cells originated in a molecular mass which they called a blastema. Max. Schultze in 1861,[2] and Bruecke in 1862,[3] demonstrated that a cell wall had nothing to do with the development of molecular masses, and now it is not considered essential to the conception of a cell by the most inveterate cellulists. Having thus discarded from this far-famed elementary organ its external wall or boundary—which can alone entitle it to be called a cell at all—they have only to carry their conceptions a step farther, and banish the nucleus also. There will then remain an organic fluid or molecular material, the true potential substance, in which all growth takes its origin.

That molecular, nuclear, and cell forms are not necessarily organic has been proved by the researches of Ascherson,[4] Rainey,[5] and Montgomery.[6] They have succeeded in producing from various viscous substances, more especially oil and albumen, different kinds of gums,[7] and a material obtained from yolk of egg called protagon, appearances resembling almost every kind of elementary texture.

[1] Gährungserscheinungen, p. 51, and fig. 12. Leipzig, 1867.
[2] Reichert and Du Bois-Reymond's Archiv., 1861, p. 9.
[3] Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften, 1862 p. 382.
[4] Muller's Archiv, 1840.
[5] Mode of Formation of Shells, etc., London, 1858.
[6] The Formation of so-called Cells, etc., London, 1867.
[7] Rainey in Medical Times, January 1868.

The idea that there are such things as histological organic forms, which are necessarily indicative of life, must now be regarded as fallacious. It has deceived many, and among the rest, as we have seen, M. Pasteur. What now interests us are the transformations which these forms undergo, and the passage of one into the other. This inquiry must lead us to the molecular theory of organization, which inculcates that all the tissues are formed by the successive production of histogenetic or formative, and histolytic or disintegrative molecules;[1] and it appears to me that its correctness is singularly confirmed by the investigations that have been brought before you this evening. In short, we have first molecular depositions in organic fluids; these aggregate to form nuclei, cells, and other tissues; and these again, by constantly breaking down and re-forming, gradually elaborate and build up the higher organisms. It would appear that nature is constantly operating in the organic and inorganic worlds by the process of molecular coalescence; that the notion of every particle being necessarily derived from its like is erroneous; and that the law of descent from parents, which we recognise in the higher animals, changes, as we descend in the scale, first to *parthenogenesis*, whereby this direct descent is broken, and ultimately to *heterogenesis*, in which it is lost.

This theory irresistibly recalls to our recollection the philosophical speculations of the Greeks, and especially of Democritus, Anaxagoras, and Empedocles, and suggests to us that history in science, like history in morals or in politics, often repeats itself, so that our predecessors may often be regarded not only as the originators of speculative opinions in the past, but also as prophets, who have foretold in the cycle of events the deductive truths of the future.

III.—The doctrine that life must necessarily spring from life is essentially connected with the view of direct descent from parents. Previous to the time of Harvey, life was considered to be an independent principle, capable of being added to or removed from inert matter. Such was the opinion of the ancient philosophers, as allegorically explained by the fable of Prometheus, who animated the marble statue by fire stolen from heaven. In later times, Buffon imagined life, like matter, to be indestructible. According to him every living molecule had a life of its own, and the method by which it manifested its function depended on its association with other molecules. Thus, the body of an animal or a plant was the aggregation of a multitude of minute living beings arranged in a particular way. The death of the complex compound was simply a dissolution of one of these associations, and the organic molecules thus set at liberty wandered about until they once more combined with a plant or animal—here with a monad, there with a quadruped. The *materia vitæ diffusa* of John Hunter was something similar.

Our modern view of life is, not that it is independent of matter,

[1] See the author's paper on the Molecular Theory of Organization. Proceedings of the Royal Society of Edinburgh, April 1, 1861; and Microscopical Journal, 1861.

but a condition of matter : in other words, that material substances found in the atmosphere, and in plants and animals, influenced by certain forces, have peculiar properties communicated to them. These properties are contractility, sensibility, the power of growth in certain directions, and mental acts ; the exercise of any one of which constitutes life. That accidental causes are capable of communicating one or more of these properties to tissues that did not previously possess them, is certain. Thus, exposure to light may influence the movement of the pigment molecules in the skin of the frog and other animals, so that it at once becomes dark or light.[1] The entrance of a spermatozoid into the ovum—that is a vibratile fibre, much like a vibrio, pushing a molecule before it—excites those changes in the yolk which produce an embryo. An unimpregnated uterus is not contractile, but if impregnated, its fibres have, at a certain period and then only, a vital property communicated to them, and they expel the fœtus. I think no one can doubt that an aggregation of molecules produces a vibrio, which, at first motionless, has contractility communicated to it, and thereby lives.

M. Onimus has shown, by careful experiment, that if the clear serum from a blister be placed in a bag of gold-beater's skin, and put below the skin of a rabbit, so as to be exposed to warmth and endosmotic currents, bodies like pus or the colourless cells of the blood, called by Robin, Leucocytes, are formed. He has even filtered the fluid, and shown that they arise spontaneously by molecular deposition, so long as the fibrin it contains is not coagulated. Hence, he says, all substances which produce coagulation—such as alcohol, corrosive sublimate, iodine, to which we may add carbolic acid—prevent such purulent formations. Their production, however, he points out, is identical with that of molecular growth. He further ascertained, that when white of egg or fresh blood was placed in a glass vaccination tube, or vessel deprived of air, they did not putrefy; but if placed in a sac of gold-beater's skin, although not exposed to fresh air, they shortly putrefied, and contained numerous vibrios. In the first case, he says there is no exchange of air between the organic substances and the fluid, under which circumstances no organic forms are produced. He therefore maintains that vibrios and fungi in fluids are not the result of atmospheric germs, but of conditions necessary to the putrefaction of organic fluids.[2]

These conclusions are confirmed by the numerous facts which have long been recognised in pathology,[3] and indicate that it is to a knowledge of these conditions that science must now apply itself. So long as the origin of infusoria was ascribed to atmospheric germs, we rested contented, and all inquiry was stopped. I

[1] See Professor Lister on the Cutaneous Pigmentary System of the Frog.—*Phil. Trans.* 1858.

[2] Journal de l'Anatomie et de la Physiologie, 1867, p. 47.

See the author's Clinical Lectures on the Principles and Practice of Medicine, 4th edition, more especially pp. 164, 182, 233, 689.

shall be satisfied if, in supporting another doctrine this evening, I have stimulated even one inquiring mind to investigate the truth.

Gentlemen, I see by the public papers that a great legal authority in this city has been lecturing on the contradictions which exist among scientific men, and the want of proper evidence leading to their conclusions. He complains, also, that he everywhere sees a tendency to place new scientific views in opposition to religious beliefs.[1] I shall not reply to his good-natured criticisms by dwelling on the glorious uncertainties of the law, but shortly point out, in conclusion, that the theory I have placed before you this evening should recommend itself to the Biblical student. Man, he learns, was created from the dust of the earth, and he may well ask,—why should such molecular matter have been chosen for the purpose? How many histogenetic and histolytic transformations it underwent before the form was perfected, he is not told; but being made, it is distinctly said that living properties were added to it. He is nowhere taught that life invariably springs from life. On the contrary, the great doctrine everywhere impressed upon him is, that life springs from death. "Thou fool," says St Paul, "that which thou sowest is not quickened unless it die." In quoting this remarkable passage, I do not seek from it scientific evidence at all, but I believe it contains a deep philosophy consistent with all kinds of development—religious, moral, political, and certainly physiological. There is a manifest inconsistency in supposing that the same kind Providence which alone can create life, should have done so once only in ages past. The idea that a seed can retain vitality for thousands of years has hitherto been a stumbling-block to physiologists. What it really possesses is a certain arrangement of dried molecular matter, which, when exposed to peculiar conditions, enables it to exhibit the vital property of growth. The truth seems to be, that new life springs from the molecular death of preexisting tissues and organisms. The vibratile and germinal molecules concerned in the process are not new productions, but old ones, and these, submitted to conditions which it is the office of the physiologist to investigate, undergo a rejuvenescence, and enter into and constitute the living substance of the new being. This theory, I venture to think, is not only consistent with all known facts, but unites in one grand conclusion the physiological science of the present day, the intellectual speculations of ancient Greece, and the direct teachings of our sacred records.

[1] Address to the Stockbridge Working-men's Institute, by James Moncreiff, Esq., Dean of Faculty, etc.—*Scotsman* Newspaper, 14th January 1868.

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