lamina cribrosa, and is perforated by the escaping bundles of nerve-fibres. I am unable to offer you any account of its functions. Before the ciliary muscle was known, it was formerly thought to be the factor of accommodation, against which, however, is the absence of muscularity. H. Müller suggested that it may subserve the nutrition of the vitreous humour in the absence of a retinal vascular system. But there are very large eyes with correspondingly bulky vitreous humour and no retinal vascular system, without pectens, and this throws doubt on Müller's suggestion.

Although reaching its maximum development in birds, the pecten is not restricted to them. It is present in lizards. In the gecko, iguano and chameleon it is a little sword-like process, having an intricate structure identical with that of the bird's pecten, but externally unlike this in its surface being smooth and not plaited. Some snakes also have a pecten. I have found it in the boa constrictor and viper, but it is absent from the common snake.

On Spontaneous Generation *and* Evolution.

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The value of a theory must be measured not only by the number of known facts which it correlates and explains, but chiefly also by its capacity for adding to actual knowledge in giving new turns to investigation. Whatever may be the different estimates of the modern philosophy of evolution from the first point of view, no one can doubt that it has supplied an immense stimulus to research in often unsuspected fields, while old ones have been reattacked by the help of new ideas and with no less advantage.

Among other inquiries which evolution has more or less directly encouraged, it is not surprising to find the problem of the *de novo* production of living things. As long as spontaneous generation simply served the purpose of concealing ignorance as to the development and reproduction of many of the higher organisms,[1] it was natural that it

[1] "Even as late as 1854 we find Von Siebold stating that he had arrived at the decided conclusion that intestinal worms do not originate by "equivocal generation" from substances of a dissimilar nature—namely, ill-digested

should be more and more discredited, as exact observation gradually drove it to its resting place among those organisms which are minuter and more obscure. The long existence of a belief which was only a shifting supplement to crude and imperfect knowledge, can obviously afford no *à priori* support to the theory in its modern form in which it is directly linked with purely physical views of the nature of life. On the other hand, the very minuteness and obscurity of the organisms among which it is now believed to take place are an argument in its favour. If we agree with Mr. Darwin that "it does not seem incredible that from some low and intermediate form both animals and plants may have been developed,"[1] we are naturally disposed to speculate as to the origin from the inorganic world of such a form itself. Conceiving such a phenomenon once to have taken place, it would be difficult to believe that it has not done so again and again, inasmuch as the collocation of conditions which it requires must necessarily have from time to time recurred. Hence, it might reasonably be supposed to be within the range of possible observation, to demonstrate at least some steps in the actual genesis of life, and this is what the advocates of spontaneous generation believe themselves to have done. As a proof of its purely physical nature, nothing could be more conclusive than the *per saltum* experimental development of life from substances absolutely devoid of it, and under purely physical conditions; yet in connection with a theory of universal evolution, this involves as much difficulty as the supposition of an absolute limit between the organic and inorganic worlds. To affirm that the interval is passed over *per saltum* would not in the least diminish the discontinuity, because it would still imply the existence of an interval; yet that it is only passed *per saltum*, is after all the conclusion that must be arrived at from the statements of believers in spontaneous generation.

A comprehensive theory of evolution would fail most signally of comprehensiveness if it refused to give any account

nutriment and corrupt juices."—('On Tape and Cystic Worms,' translated by Huxley, p. 3.)

Origin from ill-digested nutriment would have been simply spontaneous generation, for which Professor Huxley has recently proposed the term Abiogenesis; the production of new organisms by modification of the living substance of another is Xenogenesis. Mr. Spencer has repudiated Heterogenesis as a synonym of spontaneous generation, and in conformity with his symmetrical terminology has used it as the correlative of Homogenesis for those cases of multiplication which have been described under the name of alternate generation, and in which there is only a cyclical recurrence of the same form.—('Principles of Biology,' i, p. 210.)

[1] 'On the Origin of Species,' 4th ed., p. 571.

of the passage from the lifeless to the living, the inorganic to the organic. But regarding the multitudinous kinds of organisms that now exist and have existed as having "arisen by insensible steps," each step a phase of the moving equilibrium, which is the result of the modification of already modified structures, and which consequently necessitates progression as a general result, it is impossible for evolution to postulate anything like an absolute or discontinuous beginning of life. The strongest claim that evolution has upon our belief is based upon its universality. There can be no breach in its continuity, and accumulated analogies compel us to think that any supposed commencement of organic life must have been as much the result of insensible gradations in something pre-existing as all subsequent developments.

It is remarkable that while on the one hand Mr. Spencer has been criticised for repudiating spontaneous generation, arguments in defence of it based on evolution, and supported by reference to his writings have been used by Dr. Bastian in a recent paper in 'Nature.'[1] The contradiction has been possible, because in neither case has the fundamental principle of evolutional continuity been properly kept in view. Dr. Bastian has no doubt felt that the balance of experimental evidence has so often swayed from one side to the other in relation to this subject, that his facts would have more weight in connection with the *à priori* arguments in their favour, and hence has so published them. No doubt, a mind saturated with the vast series of facts which evolution embraces would be disposed to be more strongly impressed with the probability of new experimental results shown to be conformable with them, and it would be quite justifiable to appeal to such a disposition if the conformity were clearly apparent. While, however, Dr. Bastian's conceptions of the way in which life originates are in reality very different from Mr. Spencer's, it would be entirely erroneous to suppose that Mr. Spencer denies the evolution of living from lifeless matter; though in admitting this, neither he nor any real evolutionist admits the occurrence of what is ordinarily meant by spontaneous generation. Continuity as much forbids us to suppose that living matter has not been evolved from lifeless matter, as to suppose that lifeless matter has ever *per saltum* flashed into life.

Graham with profound acuteness described the colloidal as a dynamical state of matter, a condition of perpetual unstable equilibrium with the environment, and therefore peculiarly sensitive to incidental disturbances. He however went further than this: "the colloid" he observed, "possesses

[1] Vol. ii, pp. 170—177, 193—201, 219—228.

ENERGIA. It may be looked upon as the primary source of the force appearing in the phenomena of vitality." Now, the energy possessed by any chemical aggregate is the force which binds together its component atoms and which is liberated when they are dissevered. The energy of a colloid is therefore nothing more than the amount which is accumulated or set free in its "continual metastasis;" the difference between its energy and that of an explosive compound like potassium picrate is, that the one is actual and the other potential. It is impossible to correlate the energy of a colloid more than of a crystalloid with vitality, because they are one and the same thing. The reason why colloids lend themselves to the exhibition of vital phenomena is not because they possess actual energy, but by reason of their capacity for undergoing small amounts of molecular rearrangements so as to adjust themselves to corresponding small rearrangements of external conditions. In a statically equilibrated crystalloid such a capacity is either impossible or very much restricted. It is easy to represent to the mind at any rate one reason of this; the chemical molecule of the colloid is voluminous compared with the chemical molecule of the crystalloid, the number of actual atoms contained in the one being probably always greatly in excess of that contained in the other. Rearrangements of the atoms as the result of recombinations of the interatomic forces must therefore be possible to a much greater extent in the larger molecule than in the smaller. Yet Graham's principle must not be pushed too far; it must be kept in view that colloidal characters are not the cause, but only a phase of that capacity for responsive molecule readjustment, of which vital properties are the highest exponent, and the exhibition of these last may never be reached at all by substances like hydrated silicic acid, which are nevertheless characteristically colloidal.

According, however, to Dr. Bastian, the life, or in other words, the aggregate set of phenomena displayed by one of the simplest bodies which we call a living thing, is as much the essential and inseparable attribute of the particular molecular collocation which displays it as the properties of the crystal are essential to the kinds and modes of aggregation of the molecules which enter into its composition.[1] But the defect of this view is its one-sidedness, an essential characteristic of life being the duality of its relations—the continued balancing of those which are internal and those which are external. It is not the internal

[1] 'Nature,' vol. ii, p. 174.

relations alone—the molecular collocation—which is never the same from one moment of time to another, but its adaptability to the environment which constitutes life. As Mr. Spencer remarks, "an individual homogeneous throughout, and having its substance everywhere continuously subject to like actions, could undergo none of those changes which life consists of."[1] Colloids which are the nearest approach in inorganic matter to that which is living, tend under uniform conditions to pass from unstable to stable or crystalloid equilibrium, and such a condition of perfect equilibrium is a condition of lifelessness or death. Life does not consist in either the internal or external relations of a body separately, but in their continuous mutual adjustment; molecular constitution is only one of the elements of life.

Dr. Bastian, nevertheless, considers that "monads and bacteria are produced as constantly in solutions of colloidal matter as crystals are produced in solutions of crystallisable matter," and that the difference between the products "may be due simply to the original difference in nature between such kinds of matter."[2] *Primâ facie* the analogy seems a strong one; in either case a substance at first diffused in solution, and therefore amorphous, finally segregates with the assumption of definite form. Further consideration, however, shows that this view is not free from difficulty, and leaves room for doubt whether it really describes the true state of the case. It seems clear that the form of an aggregate so produced must have some relation, whether a crystal or a living thing, to the form of its component units, and must be more or less implicitly determined by it. What explanation otherwise can be given of the constancy with which shapes identical, or generally similar, recur? The form of the aggregate would be purely arbitrary if the units had no influence in determining it. In the case of crystalloids there are many reasons for believing that the moderate number of atoms composing their molecules may, in obedience to their mutual but diverse polarities, arrange themselves in a definite form having a definite resultant polarity. Such a molecular system may be subject to disturbance, and finally rearrangement by the incidence of external forces, such as light or heat, so that a substance chemically the same may possess several allotropic crystalline conditions. But the molecules of colloids are highly complex, consisting of a vastly greater number of atoms, any arrangement of which must tend to be spherical, and therefore with no marked resultant

[1] 'Principles of Biology,' vol. i, p. 286.
[2] 'Nature,' vol. ii, p. 172.

polarity. Hence in an aggregate of colloidal molecules, individual molecules would not tend to any special relative disposition; in other words, could not crystallise, and consequently masses of different colloidal substances exhibit, on the whole, not very diverse external characters.

But colloidal molecules having polarities so variable, what relation can there exist between them and the vast variety of the shapes of living things? In the gelatinous state characteristic of colloids these polarities are hardly sufficient, even to differentiate their external appearances from one another. It is quite evident then that they cannot possess the property of arranging themselves into the special structures of the organisms to which they belong, since the infinite variety which these present would be inexplicable on such a supposition. We must agree with Mr. Spencer in conceiving it possessed by certain intermediate or physiological units composed of chemical units or molecules, but infinitely more complex, and possessing a more or less distinctive character, which ultimately produces a difference in the forms assumed by the aggregate. This helps us to understand the repetition in the offspring of the peculiarities of the parents if sperm cells and germ cells are essentially nothing more than vehicles of small groups of "physiological units in a fit state for obeying their proclivity towards the structural arrangement of the species they belong to.'[1] If this is true of the higher organisms, it must be equally true of the lower; and as it is impossible to say where a line could be drawn, it probably holds of all living beings, even of those lowest, so destitute of structure as to have no claim to the title of organism at all. Of course it may be objected that physiological units are mere figments of the imagination, but as much might be said against chemical units. In either case the use of symbolic terms is forced upon us by the analysis of our scientific ideas. In both cases they may be purely arbitrary conceptions; but if we use one, it is impossible to object to the use of the other. While, therefore, molecules having definite polarities aggregate into definite crystalline forms, molecules whose polarities are feeble will aggregate into amorphous colloidal masses. The shapes of living bodies are related to organic units more complex than the molecules of colloids. Between a solution of colloidal matter and a bacterium there is a distinct step in integration, which does not exist in the case of the formation of a crystal at all, and this, therefore, is not really analogous to the formation of a bacterium from such a solution, supposing it to take place in the way which is described. A

[1] 'Principles of Biology,' vol. i, p. 254.

particle of protoplasm suspended in a fluid would, just as an oil-drop does, tend to assume a spherical form, on the principle of Plateau's well-known experiments. But this would not account for the definite elongate shape of a bacterium

FIG. 1. × 2800.

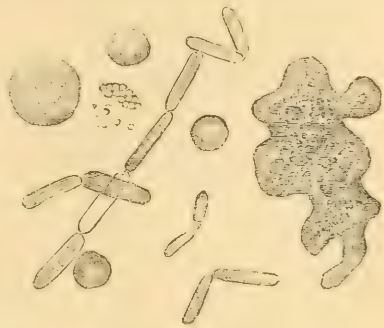

FIG. 2. × 2800.

(Figs. 1 and 2), the difference between which and the rounded outline of a monad or Torula germ must depend on difference in their component organic units.[1] These may be comparatively simple, though not necessarily so, since bacteria may be stages of more complex organisms, capable of attaining complete development in a more favorable environment.

If there is little analogy between the supposed origination of such entities as bacteria and crystallization, still less is there in the case of spores of fungi. The existence of a spore or germ implies the development of something more mature, the form of which is implicitly determined by it. Its component organic units must, therefore, be more complex than those of the simplest living bodies which run through no varied course of development. A germ apparently extremely simple in structure potentially may be rather complex, and in proportion its production by mere segregation from a solution of colloidal matter is *à priori* improbable. There is good reason for believing that even when "under the influence of pre-existing protoplasm an equivalent weight of the matter of life makes its appearance" in the place of carbon dioxide, water, and ammonia, it does so only after a process of evolution by successive integrations. It is alien to the general conception which evolution forms of the mode in which the more complex kinds of matter are derived from the less complex

[1] These figures, which are perhaps the most definite that have been published, are borrowed from Dr. Beale, 'Disease Germs,' Pl. II, fig. 13, and Pl. IV, fig. 26. Fig. 2 represents bacteria with white and red blood-corpuscles from hepatic vein of a cow which died of cattle plague.

to suppose that living matter is capable of moulding, as it were by one operation, inorganic matter, suitable, perhaps, as to ultimate composition, but of the most diverse proximate arrangement into molecular aggregation like its own. It is inconceivable that any given stage of evolution can be reached with equal facility from any inferior stage selected at haphazard.

Compare what takes place in plant nutrition. Under the influence of solar light the inorganic materials appropriated by the tissues of plants undergo successive modifications, which, weakening the stability of their molecular constitution, "give collateral affinities the power to work a rearrangement, which, though less stable under other conditions, is more stable in the presence of these particular undulations."[1] But an essential element in these changes is their occurrence in connection with the colloidal materials of vegetable tissues which bring the carbon dioxide, ammonia, and water, on which the rearrangement is effected in a condensed state into close contiguity. As soon as the bonds which unite their component elements are weakened by the unequal effect of the undulations propagated by solar light on their unequal atomic activities, they are ready to enter into new and interchanged combinations. In this way, by mutual actions and reactions, substances independently more unstable are elaborated from the more stable, and these being more and more like the materials in the presence of which the changes are effected, tend finally to be integrated with them, every stage in the process being a position of equilibrium between the molecular constitution and environing influences. By processes of which this is a rough outline, the conversion of inorganic into living matter is effected; but the presence of pre-existing living matter is a very potent factor in the change. It brings the component materials together, effects their joint exposure to solar influence, shapes the final form of their combinations, and gives them a stability they could not possess apart from it. Probably the merely physical and colloidal properties of the living matter in conjunction with solar action effect the union of the ultimate into proximate constituents, and then the "coercive polar force" of the component molecules of the living matter cause them to aggregate into similar molecules. There is plenty of analogy to induce a belief in such a coercive influence of an aggregate on integrable units; a broken crystal, for example, tends first to restore its shape before receiving further additions.[2] In this

[1] 'Principles of Biology,' vol. i, p. 32.

[2] Another good illustration of the tendency of similar molecules to

case, however, the crystalline units already in existence are coerced merely as to position; living matter being more complex than crystalline, the coercion it exerts is more complex. But if this view be probable, how can we understand inorganic substances, such as carbon dioxide, water, and ammonia or ammonium nitrate, or any such grouping of the necessary materials resisting their normal tendency to crystalline aggregation, and guiding themselves into the complex constitution of living matter.

It cannot be said that synthetical chemistry affords much more support to Dr. Bastian's views. They seem to imply that any collocation of carbon, hydrogen, nitrogen, or oxygen is capable of spontaneous rearrangement into the material substratum of a living thing. Whether the quaternary compound be crystalloid or colloid, stable or unstable, in solution or even a solid, is apparently indifferent. The tendency of all colloids is to settle down into stable crystalline forms; of the reverse passage of crystalloids into colloids Dr. Bastian only assigns a single instance, the metameric change of ammonium cyanate into urea. And urea can hardly be described as a colloid, since it has a comparatively low equivalent, crystallises in slender striated prisms, is not particularly unstable, as it may be boiled without undergoing decomposition, and must, as it is not found in the kidneys, be diffusible to be present in the urine. The general mode by which a chemist attempts to construct substances of high molecular complexity is exactly what evolution would lead us to expect. Modifications are introduced into the simplest types, and these, when so modified, are modified again. Every step must necessarily be a position of equilibrium, since otherwise progress would be impossible, and the structure must necessarily be commenced again from the beginning. If proteinaceous compounds are ever constructed in the laboratory, it will be by such a general process as this. Any substances isomeric with these would be quite as complex, and would be, therefore, as laborious a production. Certainly in no sense can such a substance as ammonium tartrate be an isomer of living protoplasm, and even supposing that that

aggregate is afforded by the pectization of colloid solutions from the gradual withdrawal of the colloid from the crystallized water. A solution of silicic acid divides into 'a clot and serum,' ending in the production of a stony mass which may be anhydrous, or nearly so. According to Graham, 'the intense synæresis of isinglass, dried in a glass dish over sulphuric acid in vacuo, enables the contracting gelatin to tear up the surface of the glass. Glass itself is a colloid, and the adhesion of colloid to colloid appears to be more powerful than that of colloid to crystalloid.'—('Proceedings of Royal Society,' vol. xiii, p. 336.

particular compound were one, ammonium carbonate, and all the other materials of diverse composition from which Dr. Bastian believes life to be evolved, could not be isomeric as well.

Evolution must be consistent with itself. A truly monistic conception of nature, to use Haeckel's word, repudiates the spontaneous appearance of life, as it repudiates everything else spontaneous; but it implies the continuity of what is living with what is lifeless, of what is called vital with what is called physical. The evolution of proteinaceous matter, as Mr. Spencer observes, must have taken place in the early world according to the same laws as those to which chemists have unconsciously conformed, and it is impossible to indicate more clearly than in his words the further changes which, in harmony with general modes of evolution, this proteinaceous matter must have undergone.

"Exposed," he says, "to those innumerable modifications of conditions which the earth's surface afforded, here in amount of light, there in amount of heat, and elsewhere in the mineral quality of its aqueous medium, this extremely changeable substance must have undergone, now one, now another, of its countless metamorphoses. And to the mutual influences of its metamorphic forms, under favouring conditions, we may ascribe the production of the still more composite, still more sensitive, still more variously changeable portions of organic matter, which, in masses more minute and simpler than in existing *Protozoa*, displayed actions verging little by little into those called vital—actions which protein itself exhibits in a certain degree, and which the lowest known living things exhibit only in a greater degree."

It is evident that what is here described requires an amplitude in the range and varieties of the conditions which could not possibly be realised in an experiment. It becomes, in fact, almost as difficult in such conceptions to imagine the evolution of a new plant species in a flower-pot as life in a sealed flask.

Dr. Bastian's first observations deal with the changes of the so-called "proligerous pellicle" of Burdach, and he certainly does not overstate the opposition to "generally received biological notions" involved in the transformation of aggregations of monads and bacteria into larger and higher kinds of living things. His description of the way this takes place is, however, very materially different from that which has been given by other writers, and the difference consists, not

[1] 'Principles of Biology,' vol. i, App., pp. 483, 484.

so much in the forms which he states were produced, as in the general changes preceding the production. The pellicle consists of an aggregation of monads and bacteria, or "granules," in a transparent jelly-like stratum.[1] It is this last which seems really to be of fundamental importance. "Areas of differentiation" are *gradually* formed in it, which are lighter in aspect from an increase of the jelly-like material between the granules (Fig. 3). These areas undergo a kind of segmentation, finally breaking up into "unicellular organisms," one of which exhibited partial amœboid movements. But in all these changes the granules apparently take little if any part, and it would seem far from unreasonable to suppose that they have no more than an accidental connection with the "areas," which it is just as likely are distinct living things, originating, like the bacteria, from germs, and increasing in size by regu-

Fig. 3.

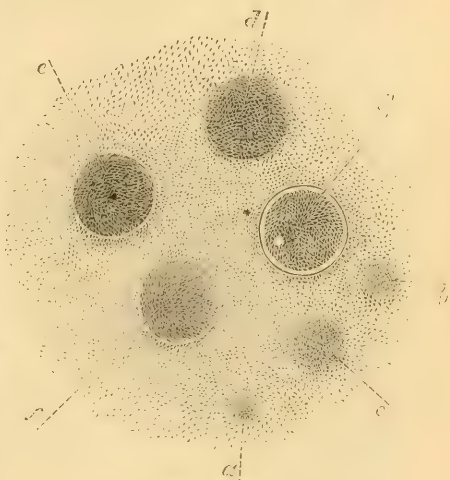

Fig. 4.

lar nutrition, and finally breaking up into individualized segments. According to Pouchet, who is endorsed by Dr. Hughes Bennett,[2] the development of new forms takes place

[1] Portions of a jelly-like material with granules, are represented in figs. 14*b* and 15*a*, and more unsatisfactory objects it is difficult to conceive. Fig. 11*b* seems to be not a spore case but a similar portion. One of the greatest difficulties in seeing clearly any result connected with spontaneous generation is the apparent inseparableness with it of crude and uninterpretable figures.

[2] "On the Molecular Origin of Infusoria," 'Popular Science Review,' vol. viii, p. 55. This is practically a revival of Buffon's 'Theory of Organic Molecules.'

quite differently in a *second* molecular pellicle produced by the disintegration of the bacteria of the first. The molecules aggregate into these masses, standing out sharply on a lighter ground (Fig. 4). The "spores of fungi" figured by Dr. Bastian in their mode of origin have a very considerable resemblance to the "unicellular organisms," differing only in the segmentation occurring more than once. More definite conclusions might have been arrived at about them if their mentioned production of "ordinary mycelial filaments" had been figured as well.[1]

Results like these, and those of Pouchet, which involve the origination of the most varied forms, in reality prove too much. Granting that the vital phenomena of the simplest living things are purely physical consequences of their material composition, it is still *à priori* incomprehensible that uniform conditions should produce so many diverse and definite specific shapes, since evolution can only conceive of these arising very gradually in response to a varied environment. Conviction in science, as in other matters, results from a balancing of probabilities. To an evolutionist it is much more probable that different specific forms should be genetically related to series of such forms from which they have derived their gradually accumulated distinctive characters, than that they should, within a very short space of time, originate from quite formless matter. Mr. Spencer remarks: "If there can suddenly be imposed on simple protoplasm the organization which constitutes it a *Paramæcium*, I see no reason why animals of greater complexity, or, indeed of any complexity, may not be constituted after the same manner."[2] And a vitalist like Dr. Beale uses language which is little different when he states that he "should as soon think of believing in the *direct* formation from lifeless matter of an oak, a butterfly, a mouse, nay, man himself, as in that of an amœba or a bacterium."[3]

The analogy between the origin of crystals and the *de novo* origin of living things which Dr. Bastian presses so strongly, might have some force if we were quite as much in the dark about the absolute beginning of *all* living as of all crystalline forms. But a belief that specific form is inherited rests on too wide a basis of facts not to afford the very strongest presumption that it is true in the case of any and every living

[1] The bodies represented by fig. 7 *b*, the 'flattened bits of protoplasmic-looking material' of Exp. 4, and the protoplasmic-looking masses of Exps. 9 and 12, are probably to be referred to the 'unicellular organisms.'

[2] 'Principles of Biology,' vol. i, App., p. 480.

[3] 'Disease Germs,' p. 59. The italics are not Dr. Beale's.

thing possessing specific form. Besides, after all, what real resemblance could there be between the earliest stage of a living thing and of a crystal? Whenever a crystalline molecule detaches itself from aqueous adhesion, either by cohering with another molecule, or by adhering to any minute foreign particle, there exists a starting-point for the building up, by successive external accretions, of a predetermined crystalline form. But a germ, even if invisible to the highest powers of existing microscopes, is a fundamentally different thing: no mere aggregation about it of external nutritive matter will be sufficient for its growth; before that can proceed its food must be absorbed, reconstituted, integrated, completely differentiated from that which is outside. A crystal begins with a merely statically disposed foundation; but a germ is from the first potentially as complex as the mature living thing which is finally evolved from it. Nor does mere minuteness abridge the interval between a particle of living and a particle of lifeless matter. The former may possess all its characteristic properties, though practically invisible. Dr. Beale has frequently observed the subdivision of living particles of the higher as well as of the lower forms of life, which could only be seen with difficulty when magnified 5000 diameters. And it is difficult to resist his conclusion that "even if the magnifying power could be increased to 50,000 diameters there would still be seen only more minute living particles growing and dividing and giving rise to particles like themselves."[1] Such particles would convert external lifeless into living matter, and so grow. There is nothing more inconceivable in this than that young plants should be developed from the scales of the stem and leaves of a *Begonia;* the minutest particles of living matter are doubtless equally able with the largest aggregates to exert some degree of that coercive polar force which has already been mentioned. Dr. Hughes Bennett, it is true, argues against the production of the immense numbers of particles of living matter found in solutions by the subdivision of similar particles derived from the atmosphere, in the first place, because "no one has ever seen this remarkable phenomenon," and secondly, because "the idea of their rapid multiplication by division is opposed to that of their power of elongating into bacteria and vibrios."[2] But Dr. Beale has shown, in the case of the yeast cell at any rate, as no doubt in other cases, that the "very minute particles divide and subdivide independently, producing still more minute particles capable of growth and division like

[1] 'Disease Germs,' p. 58.
[2] 'Popular Science Review,' vol. viii, p. 60.

themselves, not one of which, however, may be developed into an ordinary yeast cell,"[1] except it meet with an appropriate medium, when its further development is accomplished, terminating in the formation of a fungus with aërial fructification.[2]

The argument that we may dispense with pre-existent germs in the case of monads and bacteria because we do not suppose them in the formation of crystals, of course derives its force from the assumed identity of the modes in which the matter of either is built up. But even a bacterium is something very different from an aggregation of its lifeless constituent matter. The minutest crystals, on the other hand, differ in no respect from the largest except in size, and give as definite angles and measurements; their shape, in fact, is implicitly determined by that of their constituent molecules, to which it must be simply related. These molecules can be separated by solution and reaggregated again without limit, always with the production of identical crystalline forms. It cannot be imagined, however, that bacterium elements pre-existed in a solution ready to come together into a bacterium; nor is there any reason to suppose that the particles of a bacterium are capable of undergoing separation with the subsequent reconstitution of the bacterium form like a crystal. And even supposing that this were possible it would only show that a bacterium was very different from other living things. Moreover, if a bacterium underwent such a disintegration the fragments would be nothing more than "germs," and would almost certainly reproduce bacteria, not by coalescing, but severally, by growth at the expense of surrounding nutritive matters; and if they did not do this it is difficult to see how they could aggregate into anything more than an amorphous mass.

To Pasteur belongs the credit of having demonstrated the existence of germs and spores in the air, and the method by which he achieved this is well known. It was not, perhaps, perfectly adapted to demonstrate their existence optically, as the minuter particles of living matter could hardly fail to be altered if not destroyed by the treatment with different liquids necessitated by the process, even if they were visible to the low powers which were used. According to Dr. Hughes

[1] 'Disease Germs,' p. 20.

[2] By a curious coincidence, on the next page following the first instalment of Dr. Bastian's memoir, is an abstract of a paper by Dr. Polotebnow, "On the Origin and Development of Bacteria." He finds an unbroken series of forms between the minute round cells which form the mycelium of *Penicillium*, and probably other fungi and fully-developed Bacteria, which he thinks can only occur from these cells.—(*Loc. cit.*, p. 178.)

Bennett, the drawings of dust obtained from air filtered through gun-cotton are only magnified 180 diameters.[1] They demonstrate at any rate the presence in it of fungus spores, a result easily credible to any one who has watched the myriads of them which may be discharged from a single puff ball, darkening the air for a moment or two and then invisibly dispersed, or to any one who remembers the ubiquity of cryptogamic forms. *A fortiori* if the spores of the larger fungi are present in atmospheric dust, the germs of minuter and obscurer organisms cannot be concluded to be absent, especially if we remember that Professor Tyndall has shown the particles of atmospheric dust to be almost wholly destructible by heat, although of course a large proportion are lifeless matter. In his other investigations Pasteur employed a magnifying power of 350 diameters; but Dr. Child pointed out that it is quite possible that living particles might exist in a solution and yet not be detected by this, and a certain degree of uncertainty has been supposed to attach to Pasteur's results on that account.[2] It must, however, be remembered that there is no limit to the extent to which this objection may be urged. Dr. Beale thinks that whatever be the magnifying power we employ we should still be able to see particles more and more minute of living matter, and experience, as far as it has hitherto gone, appears to justify him. With the $\frac{1}{50}$th of an inch object-glass, which with the low eye-piece magnifies nearly 3000 diameters, "particles too transparent to be seen by $\frac{1}{25}$th are distinctly demonstrated."[3] It is difficult to see how, this being the case, we can ever state with absolute certainty from microscopic observations alone, that any given liquid does not contain living matter. But, on the other hand, it may be fairly presumed that in Pasteur's experiments, carried over, in some instances, a year and a half, the minute fragments of living matter would have grown during that time into visible dimensions; so that this objection is not really of so much practical importance. At any rate there was never any difficulty in detecting the existence of life in solutions which had been previously inoculated with germ-containing dust.

Dr. Bastian's results are perhaps the most remarkable of any that have hitherto been published, and they could not fail to have attracted the most serious attention, perhaps even more so, on their own merits alone, apart from any *à priori* discussions. In all the experiments liquids were

1 'Popular Science Review,' vol. viii, p. 58.
2 'Proceedings of the Royal Society,' vol. xiv, p. 184.
3 Loc. cit., p. 36.

hermetically sealed in flasks after all air had been expelled by boiling. These conditions would seem to preclude all chance of the subsequent appearance of life in the flasks; yet in only three out of twenty cases did this fail to take place; it is this which is so astonishing. If success is apparently almost inevitable, why have previous observers failed? Why they should do so, in fact, it is hardly possible to explain, dismissing the inadmissible supposition of bad faith, if Dr. Bastian's methods are free from flaw. Dr. Bastian has felt this difficulty, and suggests that in Pasteur's experiments the severity of "the restrictive conditions" produced the negative results; but this explanation, as will be seen afterwards, hardly proves very satisfactory. Professor Huxley has in fact remarked, that "it is probable there must be some error in these experiments, since others similar to them are performed on an enormous scale in the preservation of various kinds of food in tin cases, and with a totally different result."[1] And this suggests what may prove to be a source of error. The tin cases are finally sealed while standing in a bath of calcium chloride. Steam issues from a minute hole in the cover, driving before it all the enclosed air; when this has completely taken place, the whole is closed by first dropping from a sponge a drop of water upon it, which momentarily condensing the steam, is then instantly followed by a plug of molten solder.[2] Dr. Bastian, however, moderated the boiling of his flasks by turning down the lamp-flame at the time of sealing;[3] so that there would seem to be just the possibility of an indraught of air having taken place.

Of the first sixteen experiments, in eight infusions of organic matter were used, and in the other eight saline solutions. Only one of the first set and two of the last gave negative results, in all the rest living things of a rather varied kind were found when the flasks were opened, after the lapse of periods varying from five to sixty-one days, but on the average of twenty. The saline substances were all selected to contain carbon, hydrogen, oxygen, and nitrogen, but were of course on any point of view farther removed than the organic substances employed from the composition of living matter. Notwithstanding this, however, the remarkable result was obtained, that the most evolved organisms were produced by the solutions of the saline, and

[1] "Address to British Association.'

[2] The whole method is described by Dr. Wynter in 'Our Social Bees,' p. 194.

[3] Loc. cit., p. 176.

therefore least evolved substances, certainly from the evolutionist's point of view a most anomalous result. In the concluding part of his paper Dr. Bastian attributes a low evolutional capacity to acid liquids yet in Exp. 13, where no bacteria were found, but a well-marked *Penicillium*, with fructification, and in Exp. 15, where an equally characteristic fungus, with but few bacteria occurred, the solutions were both acid. And in comparing the experiments with organic infusions, the first four, which were alkaline or neutral, show on the whole less "evolutional capacity" than the last four which were acid. So also, all the four cases where spores were observed were in saline solutions, three of which were acid.

Assuming that fungi, which are comparatively high up in the scale, are produced *de novo*, it would be reasonable to expect that they would require the most favorable conditions; and it is therefore remarkable to find them occurring under what on Dr. Bastian's own data we must conclude to be unfavorable.[1] Nor is this the only difficulty about their occurrence. That they should commence with anything resembling a specialised product of ultimate differentiation like a spore, with even, as Mr. Worthington Smith remarks, a neck-like prolongation, which would ordinarily be interpreted to indicate the point of detachment from the parent,[2] is, *à priori*, very improbable. Supposing one of the *Myxogastres* about to be evolved, it would be more reasonable to suppose that the undifferentiated amœboid body, which results from the rupturing of the spore-wall, and which gives rise to the divisionless plasmodium-like mycelium, should be first evolved, rather than the spore, with its distinction of cell-wall and cell contents.

In four cases in solutions of ammonium tartrate and sodium phosphate masses of spiral fibre were met with (fig. 13 *a*). It is almost impossible to resist the conclusion suggested to Dr. Bastian that this is a non-living accidental product, altered by boiling; and it was in all probability introduced with one of the two saline ingredients, as it was only met with when these particular ones were used. It is significant to notice that in two cases (p. 197) foreign bodies were found in the solutions, and that minute shreds of cotton or paper-fibre have often been noticed with surprise, notwithstanding

[1] The reaction of the liquid, as indeed Dr. Bastian admits, has in many instances little influence on the development of living things in it. *Sarcinæ*, for example, are developed indifferently in urine which is alkaline, neutral, or acid ('Neubaer and Vogel,' p. 134).

[2] 'Nature,' vol. ii, p. 276; conf. figs. 11*e*, 13*c*, and 18*b*.

every precaution in cleansing the flasks, and using fresh distilled water (p. 220).

In the four concluding experiments tubes containing one infusion of turnip, the other three saline solutions, were after the exhaustion of the air sealed, and then exposed to a temperature of about 150° C. for four hours. In every case evidence of life was eventually found, the saline solutions as before giving the most conspicuous results. In a solution of ammonium, tartrate, and disodium phosphate, a *Penicillium* appeared, as in Exp. 13, with the same solution, after merely boiling. Spores and *sarcina* were also found in a solution of ammonium carbonate, and disodium phosphate.

What interpretation is to be given to these results? It is evident that one or other of two conclusions must be accepted;—either the fungi origiated *de novo*, or they were produced from germs, which were subjected to a temperature of 150 C. for four hours. It would certainly seem as if these last experiments were absolutely conclusive, especially when, as Dr. Bastian tells us, no fungus spore, or bacterium has been hitherto supposed to be able to withstand mere boiling without disintegration. All that can be said on the other side is that we must keep in mind that fungi, like *Penicillium*, are only ultimate forms, of which yeast globules are a peculiar condition, *Leptorthix*, probably a submerged confervoid form, and from which, if we may believe Dr. Polotebnow, even bacteria are derived. Dr. Beale has stated from observations made with his $\frac{1}{50}$, that minute yeast-cells are capable of throwing off buds or gemmules, much less than the $\frac{1}{100000}$th of an inch in diameter;[1] and these he thinks capable of subdivision practically, *ad infinitum*. All the organisms found in the solutions might have been ultimate stages in the development of such minute atoms of living matter; and as to the influence of temperature upon these nothing can properly be asserted. Disintegration is the result of heterogeneousness of parts; but these particles must be much less liable to alteration in that respect than comparatively large structures, such as spores, the formed material of whose envelope would be acted upon differently from the living matter within. There seems good evidence to show that bodies allied to germs may survive boiling; and perhaps in proportion to their minuteness. Dr. Heisch has described quite lately to the Chemical Society spherical cells found in water contaminated with sewage. They were too minute to be removed by filtration through the finest

[1] 'Disease Germs,' p. 20.

Swedish filtering paper, and boiling for half an hour or more in no way destroyed their vitality.[1]

Not merely does Dr. Bastian believe that from the re-arrangements of the particles of various saline substances in solution living beings may *de novo* originate; but he also states that this may actually take place within crystals of such compounds. He gives the details of his observations in these, as in other cases, with a minuteness which is most conscientious, especially as it is often hardly possible to avoid thinking that they suggest quite a different explanation. Crystals of ammonium tartrate were found after being kept some time to have undergone certain changes, the external portions became more or less opaque, and less soluble, and gradually increasing bubbles are seen in internal cavities, especially in those crystals which are not perfect in shape, and which present a more or less opaque appearance in their interior.[2] It is by no means improbable that a crystal presenting these characters is not homogeneous throughout, and a porosity of the interior, which would explain the opacity, would be likely to be increased by the unequal effects upon a crystal so constituted of changes of temperature. Some of the crystalline layers of agates are pervious to the colouring liquids, which are used to stain them; and these "air bubbles" might be in actual communication with the exterior, although they would be retained during solution by mere adhesion, and would be finally disengaged with all the appearance of being liberated from closed cavities. The existence of these cavities is, however, rather adduced as an evidence of changes which have taken place in the crystals, and which are the result of the development in them of the organic structures, found to be liberated by solution from their very centres. Granting that the development actually takes place, it might have been supposed that the centre of the crystal would have been its least likely seat, as being the part least subject to external influences, and therefore with less determining causes for the necessary re-arrangement of its constituent molecules. Another explanation seems more probable; the presence of foreign bodies, or "nuclei," in a solution at the point of crystallization, when adhesion and cohesion are nearly balanced, often, by making the cohesion preponderate, determines the formation of crystals about them. The character of the bodies which were liberated is not incompatible with this,

[1] 'Nature,' vol. ii, p. 179.

[2] Ammonium tartrate is known to effloresce when kept, from the loss of ammonia.

they consisted principally of fragments of cotton, or paper fibre, mucoid matter, confervoid filaments, and fungus spores. Now, supposing all these present in the original mother liquor, which is allowed to be possible, they would certainly tend to aggregate together on the same principle that a precipitate granulates. Hence a collection of them would be not unlikely to occur at the centre of some crystals, though it would not necessarily be found to do so in every one. Such an explanation is at any rate not improbable, and would seem more easy of acceptance, than the belief that a molecular re-arrangement of the crystals of ammonium tartrate into living, and even organized protoplasm could take place. Dr. Bastian supposes that as ammonium cyanate passes into urea, so ammonium tartrate "may undergo a more or less similar isomeric transformtion."[1] The transformation could hardly be an isomeric one in any case; no proteinaceous body could be properly described as an isomer of ammonium tartrate, at least in the same sense that ammonium cyanate is isomeric, or more properly metameric, with urea. Nor is it easily intelligible how "spontaneously" a crystalline molecule statically stable could pass into the unstable colloidal state. Even supposing that possible, it is difficult to understand its becoming living matter, inasmuch as the molecular structure of living matter must be different from that of similar matter not living, which is all that could arise from any isomeric modification,—otherwise in what does the difference consist? It is still more difficult to understand the "coalescence and re-arrangement" of such matter into definite organisms, such as fungus spores.

All this is something infinitely beyond the scope of evolution, properly so called, which sees in the changes any object undergoes only the correlatives of the changes of the environment, and which banishes the word "spontaneity" from its vocabulary altogether. Here, however, the cycle of changes, with ammonium tartrate at one end, and fungus spores at the other, may for ought that can be seen to the contrary, take place spontaneously in the space of a few months, and with an environment absolutely constant. Indeed, if we may trust the observations of M. Trécul (p. 195), the operation may be still further abridged, as he has seen a tetrahedral crystal within the cells of the bark of the common elder gradually converted into a short fungoid filament.

Dr. Bastian admits that Pasteur's results "seem at first

[1] *Loc. cit.*, p. 222.

sight to be all-convincing," but as soon as the first impressions are got over, he is on the contrary amazed at his "utter one-sidedness." Looking at the origination of life as a purely physical phenomenon, he compares it to evolution, and remarking that the "restrictive influence" of atmospheric pressure at the temperature at which ether boils for example is sufficient to prevent boiling in alcohol or water; he conceives that the "evolutional capacity" of liquids is something similar, and may be possible under restrictive conditions in one case when it is entirely destroyed in others. These restrictive conditions appear to be according to Dr. Bastian pressure, acid reaction, and the influence of a high temperature. He considers, therefore, that it is a quite reasonable supposition to attribute the nonappearance of life in some of Pasteur's experiments, not to the absence of germs in a liquid in which those possibly previously contained had been destroyed by boiling, but to the effect of restrictive conditions. It is to be regretted that Dr. Bastian did not repeat some of Pasteur's experiments with the 'eau de levûre sucrée,'[1] as it would be important to have seen whether they would in his hands have yielded negative results. If so, an explanation of the discrepancy could not have been sought in "restrictive conditions," but must have been looked for in some unseen source of error. As to the presence of air, it must be remarked, that in the experiments of Dr. Child, and in those of other observers quoted in Dr. Bastian's paper, this certainly did not prevent the appearance of life accompanied as it no doubt generally is with considerable gaseous tension arising from putrefactive changes.[2] It is very probable, as Dr. Bastian suggests, that high temperatures might have a more destructive effect on organic matter if contained in acid solutions, but this could only approximate them to those solutions of purely inorganic matter, the evolutional capacity of which seems to have been so high. But an argument is supplied by Dr. Bastian himself which seems suffi-

[1] The composition of this liquid was, water 100, sugar 10, albuminous and mineral matter derived from the yeast of beer 0·2 to 0·7 parts. It was at first slightly acid, becoming more so from gradual oxidation during the experiments.

[2] Dr. Child found that in the presence of carbon dioxide, hydrogen, nitrogen, and possibly of oxygen, no organisms were produced in fluids in sealed flasks, although they were when heated air was introduced. The 'restrictive influence' of pressure in each case would be the same, and it is difficult to see what other repressive influence the gases themselves could have had if life can be produced in vacuo. If the gases were not thoroughly washed the case would of course be different, as chemical substances, inimical to life might have found their way into the solution with them. (See 'Proceedings of the Royal Society,' vols. xiii, pp. 313—314; xiv, pp. 178—186.)

cient to demolish his criticisms of Pasteur's experiments. "The presumption," he remarks, "is a fair one, that solutions which are favorable to the growth and development of certain organisms, would also be favorable to the evolutional changes which more especially tend to the initiation of such living things." Now, the most striking of Pasteur's experiments are those[1] in which after a solution has shown even during eighteen months no trace of life (in consequence Dr. Bastian would say of restrictive conditions) a ball of dusted gun-cotton was introduced without allowing the access of any except calcined air, and in a few hours under precisely the same conditions as before, a development of living things was evident. Certainly, according to the argument above, we ought to infer that the evolutional capacity of the liquid was not really impaired.

In these criticisms of Dr. Bastian's paper, the object has been to point out the difficulties in the way of a reconciliation between spontaneous generation and the principle of evolution. Any one who is thoroughly impressed with the probability of the truth of those principles, finds little difficulty in deductively bridging the interval between a living thing so elemental in its characters as Haeckel's lowest Moner *Protamœba*, and a lifeless proteinaceous substance; but he has little cause for gratitude to observers in the guise of evolutionists who assure him that the matter is quite simple,—the molecules not merely of a proteinaceous substance, but of almost any casual collocation of carbon, hydrogen, oxygen and nitrogen, spontaneously rearrange themselves, and organisms much higher than *Protamœba* appear at once upon the scene. The interval which the evolutionist is modestly content to conceive deductively bridged, is as nothing to the leaping powers of the so-called heterogenist who boldly widens the gap and passes easily from ammonium tartrate to a *Penicillium*, or from a solution of smelling salts to a fungoid mycelium. If evolutionists adopt these results, they will certainly be guilty of inconsistency, "unless," as Dr. Beale remarks, "it be consistent to believe at the same time in the law of continuity and succession, and in a law which involves discontinuity and interruption as applied to the production of living forms at the present time."[2] A believer in spontaneous generation is not indeed really an evolutionist, but is only a vitalist minus the supernatural; the special creation which the one assumes is replaced by the fortuitous concourse of atoms of the other.

[1] 'Annales de Chimie et de Physique,' 1862, p. 42.
[2] 'Disease Germs,' p. 42.