

THE NATURE
AND ORIGIN OF
LIVING MATTER

H. CHARLTON BASTIAN

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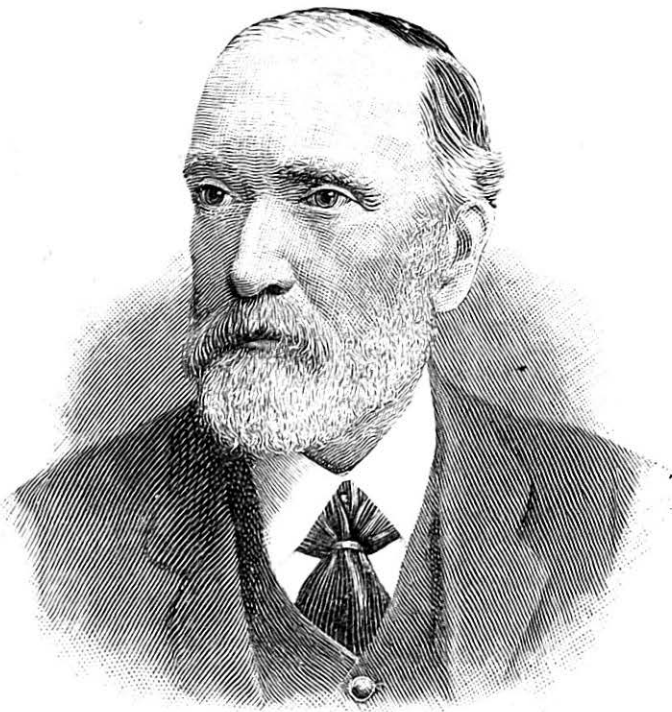
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OF
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BY

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“To experience we refer, as the only ground of all physical inquiry. But before experience itself can be used with advantage, there is one preliminary step to make, which depends wholly on ourselves: it is the absolute dismissal and clearing of the mind of all prejudice from whatsoever source arising, and the determination to stand or fall by the result of a direct appeal to facts in the first instance, and of strict logical deduction from them afterwards.”—SIR JOHN HERSCHELL: *Discourse on the Study of Natural Philosophy.*

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The Illustrations pertain to Chapters VIII., IX., and X., and the description of the several figures will be found in the text in their immediate neighbourhood.

INTRODUCTION

THE majority of biologists would be found to agree with the celebrated physiologist Max Verworn when he says¹ :—

Living substance could not exist while the earth was a molten sphere without a solid, cool crust; it was obliged to appear with the same inevitable necessity as a chemical combination when the necessary conditions were given; and it was obliged to change its form and its composition in the same measure as the external conditions of life changed in the course of the earth's development. It is only a portion of the earth's matter. The combination of this matter into living substance was as much the necessary product of the earth's development as was the origin of water.

After speaking in much the same sense, another celebrated biologist, August Weismann, follows up the argument in this way² :—

There could at first only have been inorganic substances in existence.....an albuminoid substance must therefore have arisen from inorganic combinations. No one will maintain that this is impossible, for we continually see albuminoid substances produced in plants from inorganic materials.....In order to prove "spontaneous generation," therefore, it would be necessary to try to find out from what mingling of inorganic combinations organisms could arise.

Although views of this kind on the origin of life question are in accord with, and form, as it were, the starting-point for, those expounded in this work, it seems needful to call special attention to some important points concerning which notable differences of opinion exist. To a few of these problems a brief reference may now be made.

1. *Was Archebiosis limited to the very remote past?* The actual origin of life from not-living materials—the process which I term "Archebiosis"—is commonly assumed to have taken place once only, or, rather, in one remote period only of the earth's history. This view was even from the first, and on general grounds, highly improbable, as being at variance with the "law of continuity." A great botanist, Nägeli, has said :—

If in the physical world all things stand in causal connection with one another, if all phenomena proceed along natural paths, then organisms, which build themselves up from and finally disintegrate into the substances of which inorganic nature consists, must have originated primitively from

¹ *General Physiology*, Translation, 1899, p. 319.

² *The Evolution Theory*, Translation, 1904, vol. ii., pp. 366, 369.

inorganic compounds. To deny spontaneous generation is to proclaim a miracle.¹

But if living matter came into being originally by purely natural processes, such processes should have been continually recurring, almost all over the earth, in succeeding ages as well as at the present day. To believe otherwise would, in fact, be tantamount to proclaiming that the first origin of living matter was a non-natural or miraculous event.

2. *Was Archebiosis necessarily preceded by the formation of organic matter to serve as food for the first-formed living things?* I think not; yet in a recent authoritative work the following statement may be found² :—

By whatever steps of change that high complex of organic molecules which we call protoplasm—the physical basis of life—came into existence, it very probably fed, in the first few æons of its existence, on the masses of proteid-like material which, it may be supposed, were formed in no small quantity as antecedents to the final evolution of living matter.

These are mere surmises, wholly unsupported by any attempt at evidence, either as to the original or the present evolution in nature of any such “proteid-like material.” If a material of this kind had been formed once “in no small quantity,” there is no reason why it also should not have continued to form in succeeding ages and at the present day. The view above expressed is, moreover, absolutely unnecessary, seeing that many of the lowest known living things are capable of feeding directly on inorganic materials. Thus, after referring to such facts, Professor Alfred Fischer says³ it is

probable that the first organisms did really arise from inorganic matter by “spontaneous generation,” and that, as the evolutionary theory assumes, all other living things have been gradually evolved from them in continued series. The doctrine of Evolution is, indeed, incomplete without the assumption of such a commencement. Its possibility once granted, there is no *à priori* reason to deny that it still takes place, and that, side by side with the ceaseless evolution of new species, there may actually arise new organisms by spontaneous generation.

3. *Are Bacteria some of the primordial forms of living matter, or are they derivatives from other forms of life?* This question has to be formally put, because of the statement by Sir Ray Lankester that Bacteria cannot be regarded as primordial forms of life, and that they have been “derived from chlorophyll-bearing ancestors.”⁴ This latter assertion is also made without any attempt to support it by evidence. Yet, in the absence of good evidence, any such view would seem directly negatived by the fact of the existence of the well-known group of “prototrophic Bacteria,” which build up their

¹ Quoted by Max Verworn, *loc. cit.*, p. 309.

² Ray Lankester's *Treatise on Zoology*, Part I., Introduction, 1909, p. xv.

³ *The Structure and Functions of Bacteria*, Translation, 1900, p. 50.

⁴ *Loc. cit.*, pp. xii. and xvi.

substance from inorganic materials, and concerning which Professor Fischer says¹ they are "prototrophic in the strictest sense of the word, for a simpler synthesis of proteids than theirs is scarcely conceivable."

The other two groups of Bacteria, feeding as they do on organic matter, dead or living, would naturally and easily be derived from the prototrophic group. I showed, indeed, long ago how readily one of these forms of metabolism may be exchanged for the other, by inoculating a simple solution of ammoniac tartrate in distilled water with Bacteria that have been bred in an organic infusion, and *vice versa*.² This subject is again dealt with in the Appendix to the present work.

Instead, therefore, of agreeing with Ray Lankester when he says,³ "We must look for the representatives of the most primitive forms of life among the minute Protozoa possessing the simplest methods of nourishing themselves by the digestion of already elaborated proteid," such as *Amœbæ* of one or other variety, I believe this to be an altogether mistaken view. Prototrophic Bacteria, certain *Torulæ*, and some of the simplest *Algæ*—all of them non-nucleated organisms, capable of building up their substance from inorganic materials—have the best of claims to be regarded as the real primordial types of life, and, as such, the starting-points for ever-varying forms of *Protophyta* and *Protozoa*.

This view is not based upon mere general considerations, but upon experimental evidence of the strictest kind. I claim to have proved, by evidence adduced in Chapter VIII. and in my work, *The Evolution of Life* (admitting of no other explanation consistent with existing knowledge), that Bacteria and *Torulæ* are primordial forms of life, seeing that they can now take origin in suitable inorganic media, as they probably have done in all past ages since they made their first appearance upon the earth. And, as bearing upon this latter point, it is highly important to note that, according to B. Renault, different kinds of *Micrococci* and *Bacilli*, as well as *Zooglæa*, have been found in animal and vegetal remains in the Triassic and Permian strata, in Carboniferous Limestone, and even as low as the Upper Devonian strata.⁴ Is it conceivable that such variable living things could retain the same primitive forms through all these changing ages, as Sir Ray Lankester and those who think with him would have us believe? Is it not far simpler and more probable to suppose—especially in the light of the experimental evidence now adduced—that, instead of having to do with a case of unbroken descent from ancestors through all these æons of time, we have to do, in the case of Bacteria and their allies, with successive new births of such organisms through all these ages, as primordial forms of life—compelled by their different, but recurring, molecular constitutions to take such and such

¹ *Loc. cit.*, pp. 48, 166.

² *The Beginnings of Life*, 1872, vol. ii., Appendix, pp. xlvii.—liii.

³ *Loc. cit.*, p. xv.

⁴ "*Rech. sur les Bactériacées Fossiles*," *Ann. des Sciences Nat. (Bot.)*, 1896, II., pp. 275–349.

recurring forms, just as would be the case with successive new births of different kinds of crystals?

Pfluger's "cyanogen hypothesis" may, of course, be no more necessary to account for the first beginnings of living matter on the cooling, yet still heated, surface of the earth than for its origin at the present day. It may seem, indeed, to many not a little inconsistent to assume, as he did, and as others following him believe, that great heat was needful for the original up-building of the proteid compounds of which living matter is composed, when over the whole surface of the earth at the present day this same kind of synthesis is ever taking place, in the tissues of plants, at all grades of temperature, ranging from very cold to very warm.

Then, again, as to the view that Amœbæ of one or other kind are the probable primordial types of life, strong additional evidence against this doctrine is forthcoming in Chapter X. of the present work, showing that aggregates of Bacteria (in the form of small Zooglœa masses) are capable of individualisation, and then of undergoing a series of molecular changes and segmentations, ultimately resulting in the production, now of Fungus-germs, and now of flagellate Monads or Amœbæ. So that by actual microscopical evidence, rather than by mere hypothetical imaginings, it is shown that organisms of this latter type are secondary or derivative rather than primary forms of life, as many have supposed. In order of appearance, Amœbæ (living as they do on other organisms, or on dead organic matter) have, in fact, to give place to prototrophic Bacteria, with simplest types of Mould and Algæ—none of which require to feed, in "the first few æons" of their existence, on a purely hypothetical "proteid-like material."

H. C. B.

January, 1910.

THE NATURE AND ORIGIN OF LIVING MATTER

CHAPTER I.

THE CORRELATION OF VITAL AND PHYSICAL FORCES

NEAR the close of the eighteenth century (1798) Benjamin Thompson, afterwards Count Rumford, announced to the Royal Society his conviction, based upon experimental evidence, that "heat was a mode of motion." This conception was the germ of the important doctrine known as "The Correlation of the Physical Forces," enunciated, in 1842, by Grove in this country, and almost simultaneously by Meyer, a physician, at Heilbronn—a doctrine according to which heat, light, electricity, magnetism, and chemical affinity (the so-called imponderables) were at once convertible and indestructible—and all capable of arising from or giving origin to motion either directly or indirectly. In succeeding years this doctrine was developed by many other workers, the most prominent of them being Joule, Clausius, Rankine, Thomson (afterwards Lord Kelvin), and Helmholtz.

The speculations of ancient philosophers, such as Democritus, Epicurus, Lucretius, and others, to the effect that life could be resolved into a series of physico-chemical phenomena, now began to be looked at from a more positive basis. The first real step in this direction was taken in 1845 by Meyer, of

Heilbronn, in a memoir on "Organic Movement in Relation to Material Changes," in which he showed that the processes taking place in living organisms, animal or vegetal, were produced by forces acting upon them from without, and that the changes in their composition brought about by these external agencies were the immediate sources of those modes of force apparently generated in the organisms themselves. A few years later this view was elaborated by Dr. Carpenter in a memoir published in the *Philosophical Transactions*, "On the Mutual Relations of the Vital and Physical Forces," in which he sought to show "that so close a mutual relationship exists between all the vital forces that they may be legitimately regarded as modes of one and the same force." He maintained that these so-called vital forces were evolved within the living bodies of plants and of the lower animals by the transformation of the light, heat, and chemical action obtained from without; which forces were given back to the external world again, either during the life of the living beings or after their death in terms of motion and heat, and also, to a slight extent, in the form of light and electricity. This has been

found to hold good, as we shall see later on, for plants and the lowest animals and also for the initial changes in higher animals, though all the later vital manifestations of the latter are dependent almost entirely upon the redistribution of the forces locked up in the organic substances which constitute their food, and to the various chemical changes taking place within their bodies at the expense of their own tissues.

We have thus to do with a fundamental distinction between the nutritive processes of plants and animals, for, as pointed out by Gavarret in his interesting work, *Phénomènes Physiques de la Vie* (1869), most of the physical force which, in the form of light and heat, impinges upon a plant, is consumed therein (*travail intérieur*). It is stored up as potential force in the complex organic substances entering into the composition of the plant; these organic substances being produced (under the influence of the already existing living tissues) by the action of the above-mentioned physical forces upon the not-living constituents of the earth, air, and water by which the plant is surrounded. The animal, on the contrary, liberating and using the forces which have been stored up by the plant—after assimilating a part of its substance in the form of food—expends them in the production of that *travail extérieur* which the animal's nature and the necessities of its existence compel it to manifest.

Now, as we all know, animals display, in varying proportions, three principal modes of "vital" activity which testify to the continual liberation of force within them: (1) they appear to produce heat; (2) they move, by reason of the contractility of certain tissues; and (3) they display certain nervous phenomena. Let us briefly consider each of these manifestations.

1. Very many animals constantly maintain themselves at a temperature above that of the medium in which they live; this being more especially the case with the so-called "warm-blooded"

animals—among which birds are most remarkable for the very great difference existing between their temperature and that of the air. The cause of this difference in temperature between the animal and its medium has been variously explained at different times. The true theories on this subject, however, may be said to date as far back as the close of the eighteenth century, and to have commenced with the brilliant discoveries of Lavoisier. He showed that the oxygen introduced by the respiratory passages attacks the organic substances furnished by digestion, burns them, combining with their carbon and their hydrogen to form carbonic acid and water. He showed that this slow combustion of the organic materials of the blood is an incessant source of heat. Lavoisier then instituted experiments to determine the quantity of heat abstracted from the animal by radiation, by contact with air, and by evaporation of fluids from the surface of the body. On the other hand, he measured the quantity of oxygen consumed, calculated the proportions of carbonic acid and of water produced by the combination of this oxygen with the materials of the blood, and then estimated the quantity of heat disengaged during these reactions. From a comparison of the results thus obtained in these two series of observations, he came to the conclusion that the chemical reactions carried on within the body would furnish enough heat to maintain the animal at its proper temperature. This conclusion was afterwards confirmed by many other experiments and observations. The researches of Lavoisier still left us in doubt, however, as to whether the combustion of the materials of the blood took place in the capillaries of the body generally, or in those of the pulmonary circulation. This doubt was removed by Spallanzani; and the subsequent experiments of Magnus and of Claude-Bernard tended only to confirm his conclusion, that the heat-producing chemical changes were carried on in the capillaries of the body generally. Thus

the heat evolved in animals is some of that solar heat which had previously impinged upon plants, and which was gradually locked up in the form of potential energy during the growth of the plant-tissue, subsequently taken as food by animals.

2. Turning now to the next dynamic manifestation of animals—to their power of movement—we may, for the sake of brevity, consider this as it presents itself in the higher animals only—in those in which the movements depend upon the contractility of definite structures known as “muscles.” *Contractility* is the essential attribute of the muscle, and, being one of the peculiarly vital endowments, we may now inquire how far this vital property is one which is correlatable with ordinary physical forces, or whether it can display itself independently of these.¹

After discussing the many investigations which have been made on this subject, Gavarrat summarised their results as follows: “All these experiments agree in showing that in the muscular system of an animal which accomplishes *actual work* (such as raising a weight, dragging a load, etc.) everything goes on as in an ordinary steam-engine. While the muscle *performs work*, the heat produced by the internal combustion becomes divided into two complementary portions; the one part appears as *sensible heat*, and determines the temperature of the muscle, the other *disappears*, so far as its existence in the form of heat is concerned, and, by the intervention of the muscular contraction, *becomes transformed into mechanical work*. The muscle is an *animated machine*, which, like the steam-engine, utilises the heat in order to produce work: in both cases there is necessarily an *equivalence* between the heat which disappears, or is consumed, and the external work achieved.”

In consequence of its origin, the energy manifested during the contraction of the muscle is directly comparable with the energy due to the elasticity of vapour when this is the motor power at work, as in a steam-engine. Chemical change—combustion, in fact—in each case, in muscle and in steam-engine alike, causes the liberation of heat; and in each case part of this liberated force is capable of manifesting itself anew in the form of mechanical energy. It matters not whence the heat is derived—whether it comes from the decomposition of the recently assimilated food-products, in the blood which circulates through the muscle, or whether it proceeds from the liberated energy or sun-force that may have been locked up for ages in the bowels of the earth, but which is now set free by a process of combustion in the engine fire—the result is the same, and in the muscle, as much as in the steam-engine, we have to do with a machine in which the transference of heat into mechanical energy is capable of being effected.

The muscle, it is true, is a much more subtle kind of machine, and the precise mode of its action is as yet hidden from us; we know not *how* it is—through what precise molecular changes taking place in the substance of the muscle—that the heat which disappears as heat, is, through the property of *contractility*, enabled to reappear in the form of mechanical energy while the animal performs its manifold muscular movements. That this is so, however, we know; and we know, moreover, that, as a mere machine for the conversion of heat into mechanical energy, the muscle far excels the best steam-engine which has ever been constructed.

3. Turning now to the third mode of vital activity—to that which manifests itself in the display of nervous phenomena—we find that these manifestations are also closely dependent upon the integrity of certain material structures, and that their appearance coincides with an increase in the quantity of heat

¹ For a full and admirable treatment of this question we must refer the reader to pp. 120-194 of the work of Gavarrat, already quoted.

appreciable in, or in the neighbourhood of, these structures.

It has been ascertained very definitely, by the experiments of Helmholtz and of Schiff, that the transmission of a stimulus through a nerve is marked by a rise of temperature therein; while the extremely interesting experiments of J. S. Lombard seem to show that a similar, though minute, rise of temperature takes place in the brain itself when it is in a state of activity.

The molecular motion or energy, set free in the nervous system, subserves very different purposes. Upon evidence which cannot now be gone into, it could be shown that the nervous system plays an important part (*a*) in regulating the various secretions and in influencing the nutrition of the body generally. It is nerve-force again (*b*) which initiates or calls into play the activity of the various muscles by which the countless movements within the bodies of animals are produced, and also those by which locomotion and external visible movements generally are effected. But nerve-changes also (*c*) give rise to other manifestations — manifestations altogether peculiar in kind and peculiar to the individual in whom they occur. Feeling is the basis of "Consciousness," and of "Intellectual Actions," and these are phenomena that are commonly believed to be called into existence by the activity and the occurrence of molecular changes within certain parts of the nervous system and of the brain generally.

From a consideration of these different modes of activity of the nervous system it may easily be imagined how hopeless the attempt would be to establish anything like a quantitative estimate of the amount of force answering to the above-mentioned different results of nervous action. In considering the question of muscular activity and its correlation with physical force, we have to do with a measurable effect under the form of mechanical energy. But the manifestations of the activity of the nervous system are much more subtle and

eluding. How is it possible for us to estimate the value of the energy expended in regulating the nutrition of the body? How, in a motor act, shall we separate what is due to the nerve and what to the muscle? Nay, where Feeling is aroused, where Consciousness appears, how shall we estimate the equivalent value of this, which each one knows in himself alone, and which seems to differ so absolutely from everything else in the universe? However probable it may be that what we know as "Sensation" and "Thought" are as truly the direct results of the molecular activity of certain nerve-centres, as mechanical energy is the direct result of the activity of a muscle, this cannot be proved. Still, conscious states or feelings are admitted to be an appanage only of nerve actions; and their mode of appearance, their increase in intensity, and their modifiability by agents modifying the nerve-tissues, together with the limitation by which they occur only in association with certain nerve actions taking place in the highest and most complex of an animal's Nerve Centres—all these facts harmonise with the notion that they are in some way an actual outcome of such Nerve Actions, no more capable of being dis severed from the physical conditions on which they depend than is Heat to be dis severed from its physical conditions. Matter and force are inseparable—neither can exist alone. But to show how these particular motions in Nerve-Tissue arise which underlie Conscious States, and how they again subside into mere ordinary nerve actions, must, from the very nature of the problem, ever remain insoluble.

Having briefly glanced at these higher manifestations of life taking place in the bodies of animals, and having indicated how intimate are the relations existing between such higher manifestations and the physico-chemical processes ever going on in their various organs during the assimilation of food, we must now, in our attempt to form an estimate of

the nature of life and of living matter, look at the simpler problems presented by plant life.

Plants, in fact, are the active agents ever ministering to the wants of animals. They, in fashioning their own structures, are continually giving birth to organic substances which are to constitute the materials necessary for the maintenance of animal life. Thus Dumas, in an interesting little work by himself and Boussingault on *The Chemical and Physiological Balance of Organic Nature* (1844), says:—"M. Boussingault has demonstrated that plants in full growth always take carbon from the carbonic acid of the air, hydrogen from the water which bathes them, and frequently azote from the air.....The soil he used for the growth of his plants, the subjects of experiment, was a siliceous sand, which was first sifted, then kept at a red heat for some time, in order to destroy every trace of organic matter within it. It was then moistened with distilled water, and the seeds sown; after an interval of a few days, the seeds which did not germinate were removed.....Peas planted in a soil absolutely barren, and watered with pure water, may attain to complete maturity, passing through all the phases of their natural growth, and bearing flowers and ripe seeds. During this process they fix a large quantity of azote, which they must derive either from the air dissolved in the water which they absorb by their roots, or from the air that surrounds their stalks and leaves."

Animals, as a rule, are powerless for the creation of organic matter; they can assimilate and modify the organic substances that have been built up for them in the tissues of plants; but they cannot abstract from earth, air, and water the elementary constituents of organic matter, and force them to enter into such and such combinations. They use the materials which have been elaborated for them by plants, since they all feed either directly upon members of the vegetable kingdom, or else indirectly by living upon animals which have been so

nourished. Plants, then, are the great factors of organic matter—the vegetable kingdom is nature's laboratory, within whose sacred precincts not-living matter is coerced into more elevated and complex modes of being, and is made to display those more subtle characteristics which we find in living tissues. Using only the great forces of nature—availing themselves only of the subtle motions emanating from the Sun under the names of heat, light, and actinism—plants compel carbonic acid to yield up its carbon, water its hydrogen, and nitrate of ammonia its nitrogen.

It might be thought that plants derive the principal part of the ingredients with which they build up their own structures from the soil; but the experiments of Boussingault just referred to have long since disproved this formerly favoured assumption, and shown that the atmosphere is the great storehouse for the pabulum of plants. Carbon is the most fundamental ingredient of the vegetable kingdom; all plants fix this substance, and all obtain it from carbonic acid—either abstracting it directly from the air by their leaves, or obtaining it through their rootlets. In the latter case they may obtain it from rains which have fallen to the earth impregnated with the carbonic acid of the atmosphere, or else they procure it from that which is liberated by the gradual decomposition of organic particles in the soil. But that the air is the great storehouse whence, either mediately or immediately, plants procure their carbon, is rendered more and more obvious to us by the consideration of such facts as those to which Schleiden refers in his *Biography of a Plant*, when he says:—"From forests maintained in good condition we annually obtain about 4,000 lbs. of dry wood per acre, which contains about 1,000 lbs. of carbon. But we do not manure the soil of the forests, and its supply of humus, far from being exhausted, increases considerably from year to year, owing to the breakage by wind and the fall of the leaf. The haymaker of Switzerland and

the Tyrol mows his definite amount of grass every year on the Alps, inaccessible to cattle, and gives not back the smallest quantity of organic substance to the soil. Whence comes this hay if not from the atmosphere? The plant requires carbon and nitrogen, and in the woods and on the wild Alps there is no possibility of its acquiring these matters save from the ammonia and carbonic acid of the atmosphere."

As Dumas says:—"It is in plants, consequently, that the true laboratory of organic nature resides; carbon, hydrogen, ammonium, and water are the elements they work upon; and woody fibre, starch, gums, and sugars, on the one hand, fibrine, albumen, casein, and gluten on the other, are the products that present themselves as fundamental in either organic kingdom of nature—products, however, which are *formed in plants, and in plants only, and merely transferred by digestion to the bodies of animals.*"

Such, then, is the mighty round of things, such are the interchanges ever taking place on the surface of our globe. The inorganic is continually being fashioned into the organic, and this, after passing through successive changes, and after having displayed the manifestations of life, is ever passing again into the inorganic, ever again giving up its fashioning forces. "The crude and formless mass of the air, gradually organised in vegetables, passes without change into animals, and becomes the instrument of sensation and thought; then, vanquished by this effort, and, as it were, broken, it returns as crude matter to the source whence it had come." "Thus," Dumas also says, "is the mysterious circle of organic life upon the surface of the globe completed and maintained! The air contains or engenders the oxidised substances required—carbonic acid, water, nitric acid, and ammonia. Vegetables, true reducing apparatus, seize upon the radicals of these—carbon, hydrogen, azote, ammonium; and with them they

fashion all the variety of organic or organisable matters which they supply to animals. Animals, again, true apparatuses of combustion, reproduce from them carbonic acid, water, oxide of ammonium, and azotic or nitric acid, which return to the air to reproduce the same phenomena to the end of time."

Thus it would appear that the most simple not-living or mineral constituents, coming into relation with one another *in the presence of pre-existing protoplasm*, appear, for aught we know to the contrary, to fall at once into those subtle combinations which constitute the basis of "living" matter. The rapidity of the process mocks and defies all theoretical explanation. Here, at all events, there seems to be no laborious process of synthesis—no long chain of substitution compounds—before the final product is evolved.

But the property of decomposing ammonia and of feeding upon elementary mineral substances is by no means confined to the higher plants. The same power is possessed by *Conferva vulgaris* and other low Algæ, as was demonstrated by Bineau more than fifty years ago;¹ while nearly ten years afterwards it was ascertained by Pasteur that some of the lowest kinds of fungi, the Mucedineæ, were capable of growing and multiplying in a solution of sugar and tartrate of ammonia, to which a trace of some phosphate had been added. Referring to Pasteur's observations, Baron Liebig said:—"It is astonishing that this discovery has not attracted more attention in regard to a special point, for it comprises a fact of very great significance for physiology—viz., the formation of albuminates in plants, respecting which we are in possession of scarcely anything beyond conjecture; hitherto this has been regarded as one of the greatest mysteries of organic nature..... If yeast cells, placed in a mixture of

¹ *Mém. de l'Acad. des Sciences de Lyon*, t. iii., 1853.

ammonia, tartaric acid, sugar, and phosphate, could propagate and multiply, it is evident that an albuminate must have been formed from the elements of this mixture, since one of the chief constituents of the yeast fungus is an albuminoid substance."

All that is here said by Liebig becomes even still more striking after my own observations as to the freedom with which *Bacteria* and *Torulæ* multiply, not only in solutions of ammoniac tartrate to which a phosphate has been added, but also in solutions of ammoniac tartrate alone.² The fact that this occurs shows that these simple saline substances, together with water, not only contain the elements necessary for the formation of living matter, but that the passage must be comparatively easy from the mode of collocation of the elements in such a saline solution into that by which they are converted into living protoplasm. Nay, more; seeing that the multiplication of living things takes place, as I have ascertained, with so much more energy and rapidity in a solution of ammoniac tartrate than it does in one of the oxalate, the acetate, or even the carbonate, it seems to show that the ammoniac tartrate state of combination is an especially favourable platform for the initiation of these new and more complex modes of combination.

This fact, that growth and multiplication of elementary living organisms takes place at the expense of the elements of the saline solution, shows that, under a certain influence—that of the pre-existing living matter added thereto—the elements of the saline solution are capable of reacting in such a manner as to fall into new modes of combination, whereby they give rise to "living" compounds. But if, as we contend, no special or peculiar forces are at work within the pre-existing organisms, the molecular movements constituting their "life" must be determined purely by natural

affinities, so that they can only exert some catalytic action, which is essentially physico-chemical, upon the molecules of the matter with which they are brought into contact. If, then, under the influence of these chemical actions, the molecules of the saline substances and of the water in which they are dissolved undergo a rearrangement and combination whereby they are converted into living protoplasm, we are compelled to assume the truth of what appears to be on other grounds so probable, that there is a natural aptitude for the molecules of certain compounds to fall into the more complex modes of combination that exist in living matter.

Again, the plant, as we have seen, takes not-living mineral ingredients from earth and air, and, under the influence of solar light and heat, these simple materials assume, within its tissues, those higher modes of combination which are necessary in order that they may be converted into "living" matter. The forces at work in and upon the plant are supposed to be nothing more than ordinary physical forces. Here, again, it is true, matter passes from the lifeless to the living state of combination under the influence of pre-existing protoplasm. But, as no unknown and independent forces are now believed to be at work within living tissues, we must suppose that, under the influence of the physical forces within and those without the organism, lifeless compounds fall more or less immediately into such and such living combinations. The influence of the different kinds of pre-existing protoplasm would seem to expend itself chiefly in the building up of the living matter in each case after its own likeness.

The foregoing facts lend no support to the common notion that there is some extraordinary difficulty to be overcome in order that matter may fall into living modes of combination. The further common supposition as to the necessity for long intervals of time, during which numerous intermediate stages in molecular complexity may be passed through

² *The Beginnings of Life*, 1872, vol. ii., Appendix, pp. xlvii.—liii.

before the combinations resulting in what we know as living matter can arise, seems directly opposed to the above thoroughly established facts concerning

the nutrition of plants and the free growth of Bacteria and Torulæ in comparatively simple solutions of not-living matter.

CHAPTER II.

THE FUNDAMENTAL PROPERTIES OF LIVING MATTER

SEVERAL causes tend to augment the mystery inseparable from our thoughts concerning living matter. One of these is due to the common notion of the existence of a separate entity or vital principle, called "life"—a notion which, strangely enough, has been fostered by many celebrated physiologists and philosophers, such as Bichat, De Blainville, Richerand, Schelling, and even Herbert Spencer, by their attempts to find a definition of "life."

But all things possessing qualities—that is, everything in the universe—has a "life" of its own, varying though it may in rank and relative supremacy. All bodies in nature are, in fact, known to us only as aggregates of such and such properties. They are, however, divided into two great classes—the living and the not-living—according as they do or do not possess a small group of certain qualities. These differentiating qualities are those which are generalised and included under the abstract name "life." We must not be blinded, however, by the use of such a word; we must not fall into the old error of supposing that, because by a process of generalisation we have conceived a mere abstract notion to which we attach the name "life," there is anything existing, of and by itself, answering to this term. No, each material body has properties of its own

—properties which are due to its molecular constitution, and which make it what we know it to be. Certain of the objects around us, for instance, have a power of assimilation, of growing, of developing, and of reproducing their kind. Bodies possessing such properties have been arbitrarily named "living" bodies, and the word "life" has been used as a mental symbol connoting the sum-total of the properties which distinguish such bodies from the members of the other great class whose representatives do not possess them. These properties are undoubtedly of a much higher and more subtle nature than those of not-living matter; but it should be distinctly understood that they are as much dependent upon the mere qualities and nature of the material aggregate which displays them as the properties of a metal or the properties and form of a crystal are the results of the nature and mode of collocation of the atoms of which such bodies are composed.

Putting it still more plainly, we may say that the phenomena manifested by living things are dependent upon the properties and molecular activities of a particular kind of matter known as "protoplasm," just as mental phenomena are dependent upon the properties and molecular activities of nerve-tissues, and just as magnetic phenomena are

dependent upon the properties and molecular activities of certain kinds or states of iron. And though generalised conceptions of these several kinds of phenomena have been embodied in the terms "life," "mind," and "magnetism" respectively—neither of which corresponds with any independent existence—yet, regarded as ultimate facts, we are just as impotent to "explain" the relation, or nexus of causation, existing between magnetic phenomena and the one set of molecular activities as we are to "explain" the relation between the phenomena presented by the simplest forms of "life" and the molecular constitution and activities of protoplasm.

As the writer pointed out long ago¹ the attempted definitions of "life" that have been made have been so many generalised statements, more or less distinctive, concerning the phenomena presented by living things, one of the best of which is that given by Herbert Spencer as "The continuous adjustment of internal relations to external relations." On the other hand, according to Schelling, life is the "principle of individuation, or the power which unites a given all into a whole." However unsatisfactory this may be as a definition of life, we cannot fail to recognise that it is an expression of one of the most notable tendencies revealed in all the higher forms of life.

Philosophically speaking, there can be no abrupt line of demarcation between the living and the not-living; living things being only peculiar aggregates of ordinary matter and of ordinary force, whose constituents, in their separate states, are not possessed of the qualities connoted by the word "life." *Omnia mutantur: nihil interit.* As Dumas has said,² "there is an eternal round in which death is quickened and Life appears, but in which matter merely changes its place and form."

Another cause tending to complicate popular conceptions concerning "life" is what we may call an anthropomorphic fallacy—a tendency, that is, to endow living things with such attributes as we may recognise in ourselves, or at least in higher members of the animal kingdom generally. The more we look to the higher forms of life, the more apt are we to be blinded as to the real and essential nature of the phenomena taking place, owing to the great complexity which has arisen in the various functions of these higher forms step by step with the increasing complicacy of their organisation.

But the lower we descend in the scale of living things, the less marked does the life of the organism as a whole become, in contradistinction to the life of its several parts. Schelling's "tendency to individuation" becomes less and less manifest in proportion as the structural differentiation diminishes. Look at the little polyp of our lakes and ponds—the Hydra, whose individual life is so dwarfed in comparison with the life of its several parts that you may cut it or injure it to almost any extent, and yet the separate parts will still live. It can, in fact, scarcely be said to constitute a living whole, for the one animal may be divided into two, and the two into four or more, and each part will grow into an organism like that of which it is a segment—the parts grow into wholes, and in the place of the one individual organism we get four or more others similar in kind.

These also are the kinds of phenomena and modes of life with which we are familiar throughout the vegetable kingdom—nowhere do we meet with anything like that same amount of integration or individuation which is characteristic of the higher animals. Mere fragments of plants in the form of buds, "cuttings," or portions of the root, separated from the parent organism, are, as we know, capable of reproducing plants similar to those from which they have been derived. The "tendency to

¹ *The Beginnings of Life*, 1872, vol. i., p. 71.

² *The Chemical and Physiological Balance of Organic Nature*, 1844 (transln.), p. 48.

individuation" exists here also; but even in the most perfect plant the accomplished result is small indeed when compared with what we encounter among animals. The absence of a nervous system, however, combined with the less perfect condition of the vascular system, are sufficient to account for this want of integration in the plant, and the great amount of independence shown by its individual parts.

Such are some of the principal differences in the nature of the "life," or aggregate vital manifestations, of the members of the animal and of the vegetable kingdoms; and, great as are the differences between the phenomena of the higher and of the lower forms of these, we may look for even still lower manifestations of life in a group of organisms whose characteristics, whether structural or functional, are often so little marked as to make the most philosophic naturalists unable to assign them a definite place in either the one or the other of these organic kingdoms.

It might have been expected, in accordance with the doctrines of Evolution, that the lowest living things would present characters of the most general description. They ought to be simply living things, without visible organisation, and should as yet present no special characters by virtue of which a place might be assigned to them either in the vegetable or in the animal kingdom. The older naturalists thought that every living thing must be either an animal or a plant, and they accordingly ranged all organic forms under one or other of these categories. But there were certain of them whose characteristics were so indefinite that they could really claim for themselves, as we have said, no unquestioned place in either of these kingdoms, and they were consequently placed in the one or in the other alternately, as the state of knowledge at the time varied, or almost according to the whim of successive writers. But now, at last, after this unseemly bandying to and

fro, their proper position is being generally recognised.

The merit of taking a definite step as regards the classification of these lower organisms rests with Professor Haeckel, who many years ago said¹:—"I have made the attempt in my *General Morphology* to throw some light upon this systematic chaos, by placing, as a special division between true animals and true plants, all those doubtful organisms of the lowest rank which display no decided affinities nearer to the one side than to the other, or which possess animal and vegetable characters united and mixed in such a manner that, since their discovery, an interminable controversy about their position in the animal or in the vegetable kingdom has continued. Manifestly, this controversy becomes reduced to the smallest compass if the disputable and doubtful intermediate forms are separated for the present (though only provisionally) both from the true animals and from the true plants, and united in a special organic 'kingdom.' Thereby we obtain the advantage of being able to distinguish both true animals and true plants by a clear and sharp definition; and, on the other hand, a special proportion of attention is attracted to the very low organisms hitherto so much neglected, and yet so extremely important. I have called this boundary kingdom intermediate between the animal and the vegetable kingdoms, and connecting both, the PROTISTA."

It should be understood, however, that in proposing such a classification Professor Haeckel by no means wished to establish walls of separation between these three organic kingdoms. He was and is much more disposed to believe that animals, as well as plants, have gradually arisen out of modifications which have taken place in the simplest Protista. This primordial organic kingdom he divides into ten groups, in the

¹ *Monograph of Monera*. Translation in *Quarterly Journal of Microscopical Science*, July, 1869, p. 239.

lowest of which, named Monera, are included mere naked specks of protoplasm—non-nucleated, or with no easily-recognisable nucleus.

This discovery or recognition of such simple creatures as the Monera was in many respects a matter of great importance. Formerly the lowest independent living units were thought to be "unicellular organisms"—that is, units possessing a central body known as the "nucleus," together with a distinct cell wall; and Schleiden and Schwann in 1839, as a result of their remarkable investigations, endeavoured to prove that all the tissues of both plants and animals were entirely built up of such morphological units, named "cells."

Some twenty years later Virchow announced¹ views which had an immense influence on pathological doctrines throughout all the schools of medicine, and wherever biological studies were cultivated. He maintained that "the cell is really the ultimate morphological unit in which there is any manifestation of life, and that we must not transfer the seat of real action to any point beyond the cell." He denied altogether the origin of cells *de novo* in blastemata after the fashion described by Schleiden. He held that cells can be produced only from or by pre-existing cells. He did not attempt to prove, however, that the whole bulk of the tissues is made up exclusively of cells; he admitted the existence of a large amount of intercellular material in many tissues; and so, in order to reconcile this fact with his previous doctrine, he was compelled to put forward the hypothesis that such intercellular material may be broken up into imaginary "cell territories," each of which is influenced or ruled over by the cell lying in its midst.

But important modifications as to the true conception of the nature of a cell had been growing up even before the publication of Virchow's theories. So far the cell has been spoken of as an

altogether definite structure: as a body with a distinct wall or bounding membrane and certain contents, including among other things one of the most fundamental parts of the cell, the "nucleus." But it was maintained by Nägeli,² and also by Alexander Braun,³ and then more emphatically by Max Schultze, that a distinct investing membrane or cell-wall was not an essential character.³ Afterwards the typical ultimate unit of life was still further shorn of its characteristics when it was shown (if this had not been already done by Nägeli and Braun) by Brücke,⁴ Kühne,⁵ and others, that even the nucleus was a non-essential constituent of such a body.

So that in place of the old "cell," with its definite characters, this would reduce us to a mere naked, non-nucleated bit of protoplasm as the simplest material substratum adequate to display all those "vital" manifestations which were previously considered as the essential attributes of certain formed elements known as "cells." The power of displaying vital manifestations was, in fact, in opposition to the views of Virchow, transferred from definitely formed morphological units to indefinite and formless masses of what is called protoplasm. Instead, therefore, of an obvious form of "life," we are reduced to a matter of "life" presenting no appreciable morphological characters. For such a structureless unit of protoplasm—a mere shred of plasma or living matter—Haeckel proposed the term "cytode"; reserving the term "cell" for such a

¹ *Zeitschrift für Wissen. Botanik*, 1846 (transl. Ray Soc., 1849, p. 95).

² *The Phenomena of Rejuvenescence in Nature*, 1851 (transl. Ray Soc., 1853).

³ *Reichert and Du Bois Reymond's Archiv*, 1861. A mere mass of protoplasm with a nucleus was sufficient to constitute a "cell"; and, at the same time, it was maintained that the substance of the cell (or that within the wall, where this existed) was *protoplasm*, a contractile substance answering to what Dujardin had named *sarcode*.

⁴ *Wiener Sitzungsberichte*, 1861, pp. 18-22.

⁵ *Protoplasma und die Contractilität*. Leipzig, 1864.

¹ *Cellular Pathologie*, 1858.

unit when it contains a nucleus—though it must be understood that it, as well as the cytode, may be either naked or bounded by some sort of limiting membrane.¹

The old doctrine as to the fundamental properties of the "cell" as the vital unit did well enough in those days, when the lowest known living things—the lowest plants and the lowest animals—were thought to be "unicellular organisms." But since our knowledge has increased, since we have become more familiar with the various living things now constituting the lowest groups of the third organic kingdom—the Protista—the maintenance of such doctrines (leaving aside all other reasons) had become impossible. We have seen that although there are multitudes of amœboid organisms, possessing the old cell characters—that is, having a distinct nucleus and a more or less definite limiting membrane—there are, nevertheless, adult animals entering into the composition of the lowest genus of this kingdom—the *Monera*—which may have no bounding membrane and no visible nucleus, being mere bits of protoplasm, naked, non-nucleated, and structureless; while there are others, such as the Bacteria and the blue-green Chromacea, which, though equally devoid of a nucleus, are possessed of a more or less definite limiting membrane. Yet such minute, homogeneous units of protoplasm are as capable of displaying the fundamental characteristics of "life" as are the more definite unicellular organisms, to which such attributes were previously supposed to be restricted. Without visible structure they nevertheless assimilate materials from their environment, and grow; some of them constantly vary in form, and are capable of executing more or less rapid movements; though possessing no definite or visible nucleus, they nevertheless are able to divide and to reproduce their

kind—these attributes being, as we have seen, the fundamental properties of living matter.

Thus the cytode is the elementary vital unit, and the cell is a product of a higher order which may be, and probably is, constantly developed therefrom. The first stage towards the development of a typical cell occurs when the cytode undergoes some condensation of its outer layer so as to form a limiting membrane; while the second and essential stage is shown by a differentiation of the contents of the primitive vital unit, and the gradual appearance therein of the structure known as the "nucleus," to which an enormous importance is now attached.

However Weismann and his school would interpret the fact, or reconcile it with their views, there is really no room for doubt that in multitudes of plastides of various kinds a nucleus appears where previously there was none.² It usually reveals itself, moreover, in the manner originally described by Alex. Braun, in his celebrated *Rejuvenescence in Nature*.² During the development of reproductive units in Hydrodictyon and other Algæ he observed it appearing as a mere minute "clear space"—that is, a microscopical speck of matter free from granules—in the midst of a protoplasmic unit. This mode of development of a nucleus the writer has himself often seen in plastides in which previously there was no trace of such a body. Thousands of such plastides may often be seen developing in the pellicle on a hay infusion, as will be described in a subsequent chapter (see p. 127, and Figs. 30, 31). Later, the nucleus also acquires a limiting membrane, and undergoes other important developmental changes to which we shall subsequently have occasion to refer. The "cell" is thus seen to be only a developed form—a more visibly complex

¹ Weismann goes so far as to say that a nucleus never arises *de novo* (*The Evolution Theory*, transl. 1904, vol. i., p. 309).

² Translation by Henfrey (Ray Society), 1853, p. 261.

^{*} The term "plastide" is used by some rather than "cytode," though the latter nomenclature is more commonly adopted.

condition which a simpler but already living and independent unit may assume.

But even before the discovery of the simple Monera by Haeckel it had been long previously recognised by Nägeli, and also by Braun, that nothing like a nucleus was to be found in many Algæ. Thus the latter says (*loc. cit.*, p. 174):—"In particular families of the Algæ, as, for example, in the Palmellaceæ, Chlorococcaceæ, Oscillatoricæ, and Nostochineæ, as also in the large-celled Cladophoræ, and the unicellular Algæ with unlimited growth of the cell (*Vaucheria*, *Codium*, *Caulerpa*), no trace of a nucleus has yet been discovered." Yet in these latter so-called (but wrongly so-called) "unicellular" Algæ, and in the others just mentioned, all the fundamental properties of living things—even in some of them (as in *Vaucheria*) the formation of active ciliated reproductive units—are manifested, notwithstanding the absence of a nucleus. It is useless, therefore, as was formerly the case, and as happens only too frequently now, to attribute all the important phenomena occurring within a cell to the effects of its influence. Still, though it is clear that no such structure is necessary to enable a unit of protoplasm to exist as an independent vital unit, it has been found, as we shall see later on, that where present it exercises a most important influence in the production of one of the fundamental phenomena of living matter—namely, the act of assimilation, whereby its own proper structure is built up by a living unit during its process of "growth."

We are now in a position to speak briefly concerning the various fundamental properties of elementary living units; and what we shall have to say will not concern cytodes only, but will be applicable also to "unicellular organisms," and will go to show how closely, in each case, their vital manifestations are related to, and correlated with, ordinary physico-chemical processes.

This subject has been very ably and logically treated in a little book by F.

le Dantec, entitled *La Matière Vivante*,[†] to which several references will be made.

(1) One of the first and most striking characteristics of the lowest forms of life, as viewed under the microscope, is the power possessed by many of them—though others do not possess it—of what is *apparently spontaneous or instinctive movement*. And it is precisely this appearance of spontaneous movement which has tended to foster the notion that even in such elementary units their movements are due to an "internal vital principle." But, as a matter of fact, these movements, like other elementary manifestations of life, result from the physical and chemical reactions taking place between the substance of which the living units are composed and the aquatic medium in which they exist.

It is well known in regard to their medium that more or less oxygen always exists in a state of solution in such a fluid, and that the substance of the living units is constantly undergoing oxygenation and giving off carbonic dioxide, in addition to, or as an integral part of, the other concomitant chemical processes concerned in the assimilation of materials from the medium—whereby its own substance is built up and growth occurs. Verworn has shown that, if free oxygen is withdrawn from an infusion, the movements of *Amœbæ* and other low organisms gradually cease; but that, even after many hours, the re-addition of oxygen causes their several movements to recommence. Oxygen acts like this upon all sorts of plastides and unicellular organisms; and, as le Dantec says (*loc. cit.*, p. 59), "it is very probable that the majority of the normal movements of plastides are due to the constantly changing distribution of oxygen in the infusions."

It is owing to the fact that light influences these chemical reactions that this agent seems to exert a direct effect upon the movements of many of the

[†] Paris, 1895. One of the vols of the *Encyclopédie Scientifique des Aide-Mémoire*.

lower forms of life, causing them, as the anthropomorphists say, to "seek" the light. This phenomenon is seen especially with many Bacteria, with spores of Algæ, Diatoms, Desmids, Euglenæ, and other allied organisms, together with flagellate Monads. The movements in the great majority of cases are towards the source of light—rarely away therefrom, but always in relation with the direction of the luminous radiations. The actual explanation of the movement seems to depend upon a relative exaltation in the activity of the chemical processes on the side of the organism on which the light impinges, as le Dantec shows; the general effect being, as it were, to draw the organisms towards the source of light.

Then, again, food-stuffs, as well as chemical compounds of various kinds, are now known to exercise a remarkable attractive influence upon cytodes and unicellular organisms, varying much in different cases both from the point of view of the nature of the organism and the nature of the compound, but in all cases doubtless dependent upon the particular chemical activities capable of being excited in the several kinds of living units; these differences, as well as the different kinds of movement, or the absence of movement, and the different behaviour towards light being, as le Dantec very properly insists, so many evidences pointing to the conclusion that the varieties of protoplasm in these elementary organisms are almost endless.¹

The capability shown by unicellular organisms of being attracted by food-stuffs was first carefully studied by Stahl in 1884; but, according to A. Fischer,² "At about the same time Pfeffer investigated from a more general chemical standpoint the effects of such stimuli upon the movements of bacteria, protozoa, and the spermatozooids of the

higher cryptogams, and was able to show that the nutritive value was not the sole and only determinant, but that other more recondite factors were involved—factors based on the chemical constitution of the substance employed as a stimulant. For this reason he introduced the now generally adopted expression 'Chemotaxis.'"

(2) In many cases undoubtedly these chemotactic processes are intimately associated with the nutrition of the cytodes and unicellular organisms whose movements are thus incited, and to the processes included under this head we now turn. They are *Assimilation* alone, and *Digestion followed by Assimilation*, understanding "assimilation" to be the process by means of which matter from without, of some kind, is converted within the living unit into the special kind of protoplasm of which such unit is composed.

It was, as Fischer points out, formerly an axiom of general physiology "that of all organisms the green chlorophyll-bearing plants alone are able, with the aid of sunlight, to assimilate the carbon dioxide of the atmosphere, and from it to build up carbohydrates. All other living things—that is to say, plants devoid of chlorophyll, bacteria, and all forms of animal life—were supposed to be dependent for their carbon, indirectly or directly, upon the carbon compounds already formed by green plants." But then he goes on to say (*loc. cit.*, p. 47):—"The integrity of this principle has now been destroyed by the discovery of certain bacteria in the soil, which are able, even without sunlight, to appropriate the carbon dioxide of the atmosphere." These are the nitrifying bacteria, present in every variety of soil, though first isolated in 1889 by a Russian investigator, Winogradsky, which, as Fischer says, are "characterised by an extremely primitive metabolism, a physiological humility which shows them to occupy the very lowest rung of the ladder of life."

But the fact recorded by the writer in

¹ *Loc. cit.*, pp. 31, 43, 54, 61.

² *The Structure and Functions of Bacteria*, translation, 1900, p. 78.

1871, that Bacteria were capable of multiplying freely in a simple solution of ammoniac tartrate in distilled water, as mentioned in the last chapter (p. 17), showed that other common Bacteria and some *Torulæ* possessed closely allied properties, of an even simpler order. Speaking of the nitrifying Bacteria, Fischer says:—"The materials from which they build up their cells are therefore inorganic compounds of the very simplest character—carbon dioxide and ammonia, or nitrous acid with a few mineral salts. They are thus prototrophic in the strictest sense of the word, for a simpler synthesis of proteids than theirs is scarcely conceivable." It cannot be said, however, in this case that sulphur and phosphorus were absent, as they may have been ingredients of the "few mineral salts"; but in the case made known by the writer so much earlier (in 1871) the Bacteria and the *Torulæ* have in some way to build up their substance from the elements entering into the composition of distilled water—tartaric acid and ammonia; and *this* surely must be the simplest known mode of origin of protoplasm, seeing that C, H, O, N can alone have entered into its composition. Yet any one can test its reality by adding a single drop of a turbid hay or beef infusion to an ounce of distilled water in which ten grains of neutral ammonium tartrate are dissolved. If this solution thus inoculated with common micro-organisms is kept at a temperature of about 80° F. (27° C.) for a couple of days, the clear fluid will have gradually become opalescent from the growth of myriads of Bacteria, and perhaps some *Torulæ*; and after a time it will become quite turbid as their numbers increase.¹

The assimilative processes of Chro-

macea and many unicellular Algæ and Phytozoa are only a little more complex, since they build up their substance in the main after the fashion of higher plants, owing to their power of causing other inorganic compounds to fall into those modes of combination that exist in their own particular kinds of protoplasm. In other organisms, however, as with *Euglenæ* and some of their allies, organic compounds, existing in their media in a state of solution, are in part made use of in their assimilative processes.

We have, in these cases, a transition to the assimilative processes carried on by the great majority of Bacteria, which are commonly supposed to be wholly unable to build up their protoplasm from inorganic substances, and to be dependent upon organic compounds existing in the tissues or secretions of plants and animals—either after their death or during their life—or upon products which have diffused from them into the outside world. Much the same kind of processes must also obtain with the multitudes of Protozoa that exist as parasites within the various organs and tissues of almost all kinds of animals, where they find themselves surrounded by assimilable organic compounds, which may pass into their substance by osmotic processes, but still have to be coerced in each case into the particular combinations existing in the special variety of protoplasm entering into the composition of the parasite. In this respect, however, they are only exercising the same kind of power as that which is ever being exerted in every tissue of our own bodies. The blood is the common pabulum carried to all; and from it the different kinds of epithelial cells, of gland cells, of nerve cells, and others, abstract the particular materials needful to build up their own special kinds of protoplasm. It is usually assumed that such processes are simpler and easier than those effected by the prototrophic Bacteria, which, as we have seen, are capable of using the simplest inorganic materials out of which to fashion their own peculiar life-stuff. As

¹ See *Appendix* for further details. In an experiment of this kind, recently made, in which a drop of a hay infusion was used as the inoculating agent, the tartrate of ammonia solution only became very slightly opalescent even after several days; but on examination this opalescence was found to be due almost solely to minute single and budding *Torulæ*.

to the relative simplicity of the two processes, however, we need not venture to offer an opinion; though it seems clear that in each case we have to do with mere chemical combinations brought about by contact action—"catalysis," as it is termed—a process concerning which a few words must now be said.

The remarkable discovery was made by Berzelius in 1810 that certain bodies, by their mere presence, were capable of bringing about particular chemical combinations in substances or elements brought into relation with them, without being themselves affected. Thus, under certain conditions, dilute sulphuric acid will change starch into dextrine and sugar without undergoing any alteration itself; while finely divided platinum, brought into contact with oxygen and hydrogen, will cause them to combine and form water.

Later, it was ascertained that catalysis, or "contact action," as it was termed by Mitscherlich, is also a property possessed by the great class of soluble ferments known as "enzymes," that play such an important part in fermentations and in very many of the life-processes of organisms—these enzymes being complex chemical products elaborated during the life of cells, in their own substance, where they are sometimes retained and at others excreted. The investigations of Verworn, Hoffmeister, Ostwald, and others, have shown what an important part the catalytic actions of enzymes play in the metabolic processes carried on in the body by cells of different kinds. Ostwald, for instance, in a discourse "On Catalysis," delivered in 1901 at Hamburg, said:—"We must recognise the enzymes as catalysators that arise in the organism during the life of the cells, and by their action relieve the living being of the greater part of its duties. Not only are *digestion and assimilation* controlled by enzymes from first to last, but the fundamental vital action of most organisms, the production of the necessary chemical energy by combustion at the expense of the oxygen in the air,

takes place with the explicit co-operation of enzymes, and would be impossible without them." Verworn also advocates the importance of catalysis in the work of assimilation, as carried on by the ultimate units of living matter, which he terms "biogens," and agrees with Ostwald in regarding the processes that occur as essentially physico-chemical in nature.²

So far we have been dealing with cases in which the simplest living units avail themselves of materials—either inorganic or organic—in a state of solution; but in multitudes of other cases solid food is taken which requires to be dissolved ("digested") before it is assimilated. This happens, for instance, with all kinds of Amœbæ, with Ciliated Infusoria, with Sun Animalcules, and others. Where the food masses so taken are small they are commonly received into small cavities or "vacuoles" in the body substance of such organisms, and within such vacuoles they gradually become dissolved, the products of solution ultimately passing by dialysis into the surrounding protoplasm, by which they are assimilated. All the steps of this process, including an account of the movements of Amœbæ, the mode in which they take foreign bodies into their interior, the origin of the vacuoles and what goes on therein, are well told by le Dantec (*loc. cit.*, pp. 65-104), but need not detain us here.

So far as the process of "digestion" is concerned, it is admitted by all that the phenomena are essentially chemical in all cases, and that with the aid of enzymes the process can be carried on outside the body, under suitable artificial conditions, with perfect ease. There is no mystery here—the mystery, in the sense of our ignorance of the exact steps of the process, rests rather with the subsequent assimilative processes, which, as we have indicated, are similar in kind, whether we have to do with independent

² See his *General Physiology*, 1899, translation, pp. 481-6 and 529. This subject is also fully discussed in the work of Reynolds Green on *Soluble Ferments*, 1899, chapter xxiv,

living units or with the constituent elements of higher organisms.

(3) We now have to turn our attention to the last of the fundamental characteristics of living units—namely, their power of undergoing processes of *fission*, that is the spontaneous division of a single unit into two, whereby “reproduction” in its simplest form is brought about. This mode of reproduction differs little from growth itself; it was, in fact, long ago aptly termed by Huxley “discontinuous growth.”

In regard to the problem as to the cause of this discontinuity, or what brings about the spontaneous division which is so prone to occur in living units of various kinds, the reader will be in a better position to form some idea after we have briefly dealt, in a later chapter, with the question of “The Molecular Constitution of Living Matter.” It will suffice to say here that it is commonly recognised both by chemists and biologists that protoplasm must be made up of units of the most extreme complexity, the constituents of which are in a constant state of flux and change, often merely from one isomeric state to another. A condition of unstable equilibrium, in fact, is presumed to exist, constantly varying in response to changes in the medium. And it is assumed that for each of the innumerable different varieties of what we call protoplasm there are, when in this or that medium, certain varying but not very precise limits of size beyond which the molecular activities of the plastide or cell seem no longer consistent with the existence of only one centre. Two centres, therefore, in some way become established, and the attractive influences associated therewith cause the semi-fluid substance of the protoplasm to gather round each of these centres till “discontinuity” is established. The halves thus produced then increase in size (or “grow”) by means of their own proper assimilative processes, till they attain such a size as (having regard to the qualities of the medium in which they exist) seems

again to necessitate in each, as a result of the unstable equilibrium once more established, the formation of two centres, around which the plasm gathers till discontinuity is once more brought about, and so on almost *ad infinitum*.¹

Whatever may be the exact chemico-physical processes concerned in this act of fission or segmentation, the important thing to be borne in mind at present is that it is a property which belongs to living matter itself, and that it is not dependent upon any visible form or structure. This is obvious from the fact that it is manifested by Bacteria, by amœboid Monera, and by multitudes of Algæ belonging to the family of Chromacea, as well as by other non-nucleated plastides. Further, it is now known to occur even in such units as chlorophyll corpuscles; while my researches have shown that it is again an ever-recurring process in the aggregates of Bacteria constituting “Zoogloæ” masses, which form so abundantly on the surface of a hay infusion.²

It is perfectly clear, therefore, that nuclei have no necessary relation with this process of fission, and that it is to be regarded as one of the fundamental properties of living matter itself—even where this is devoid of all visible structure or organisation.

The question is altogether a different one as to what part the nucleus plays in the process of fission as it occurs in those living units in which such a body is present. Is it in any way causative then, or is its own initiatory fission merely a concomitant result of the sum-total of molecular activities occurring within the cell, when it is approaching its natural limits of size?

¹ On the other hand, in many filamentous Algæ and Fungi, instead of complete fission, the new cells formed may remain in contact separated from one another by dissepiments. A similar incomplete segmentation is met with among mere plastides, the most familiar examples of which are to be seen in long thread-like Bacilli or Streptococci.

² See Chapter X., and *Studies in Heterogenesis*, 1903, pp. 65-75.

Disregarding, or not adequately considering, the fact that in such multitudes of cases the fission of plastides occurs where a nucleus is absent, it is common to ascribe to the nucleus a causative influence when fission occurs in a cell. And this view is undoubtedly fostered by the fact that division of the nucleus commonly commences before, and proceeds more rapidly than, the division of the substance of the cell itself. This holds good for the simple amitotic division, as well as for the vastly more complicated process of karyokinesis, to which no further reference will be made at present.

Although we know that fission need not depend upon the presence and influence of a nucleus, it does not necessarily follow that when such a body has been evolved it may not be, in the unit that possesses it, in some way causative of such a phenomenon as fission. Too much doubt and uncertainty exist at present as to the real functions of the nucleus to enable us to come to a well-founded opinion on such a point. The fact that division of the nucleus precedes division of the cytoplasm is by no means conclusive, since there is good reason for believing that its molecular composition is more complex and unstable than that of the cytoplasm; and this might be the reason why, when the sum-total of molecular changes in the cell are about to eventuate in the formation of two separate centres, such processes should take effect sooner and proceed more rapidly in the nucleus than in the body of the cell itself.

The nucleus is a developmental product resulting from differentiation of the cell contents, and it is only natural to suppose, from its comparative universality and from the complicated processes taking place in connection with it, when what is known as karyokinesis occurs, that it must be associated with important functions. The results of numerous experimental researches carried on with unicellular organisms of various kinds by Balbiani, Verworn, and

others, in which sections of such organisms have been made, have led to important conclusions. "Merotomy" is the term applied to these researches, and they have invariably shown that segments which do not contain a nucleus do not survive; while those that contain the nucleus live and speedily regenerate their primitive form. This has been shown to hold good for vegetal cells, for naked Rhizopods as well as for Foraminifera, and for Ciliated Infusoria,* so the conclusion seems inevitable that where we have to do with nucleated cells the process of "assimilation" is only carried on in those segments that chance to contain a nucleus. Growth seems impossible where the nucleus is absent—though only where it is altogether absent, as the remarkable experiments of Balbiani with specimens of Stentor have shown. This rather large trumpet-shaped Ciliate possesses a long, moniliform nucleus, and Balbiani² carefully studied the question as to the amount of this nucleus existing in a fragment of the organism which would suffice, in order that regeneration of the form and structure of the Ciliate from such a fragment might occur. The account given of these experiments is as follows:—

"A Stentor having been divided transversely into three segments, an anterior one containing a nuclear chaplet of six grains, one containing only a single grain, and a posterior segment having four grains, by the next day all three were completely regenerated into perfect Stentors; and though the middle fragment, with its single nuclear grain, had to undergo much more numerous and more profound transformations than the other two, its regeneration had taken place in the same time as that of the

* Balbiani discovered one remarkable exception among these latter organisms, since the nucleated segments of Paramecia, though they lived for a month or more, showed no evidence of regeneration.

² *Ann. de micrographie*, 1892 and 1893.

others, and it had become just as perfect a Stentor—only smaller, on account of the difference in size of the three segments at the time of section.....In other analogous experiments the single nuclear grain was contained in the posterior, or in a longitudinal, fragment of the body, and the results have always been similar. We must conclude from these facts that the particular quantity of nuclear substance existing is of no importance, either for the degree of perfection or for the progress of the regeneration, and that a single grain of the nucleus behaves in this respect just like an entire nucleus."

We are warranted in concluding from these experiments, as le Dantec does, that the nucleus seems to act like a chemical substance rather than as an organ, since, as he says, "a *portion* of the nucleus in a *portion* of the corresponding protoplasm brings about the same syntheses as an entire nucleus in an entire plastide." This is an important conclusion from the point of view of the functions of the nucleus, and is fairly compatible with the suggestion made by Herbert Spencer¹ that, having regard to its chemical complexity and relative instability, "the nucleus is a source of perpetual molecular disturbance—not a regulating centre, but a stimulating centre." Nothing further on this subject need, however, be said for the present. We may now return from this digression for a few concluding words upon the subject with which we are at present more immediately concerned—

¹ *Principles of Biology*. Revised edition, 1898, vol. i., p. 260.

namely, the property of spontaneous fission, as a fundamental characteristic of living matter.

It may be asked, in conclusion, whether this is a property *sui generis*, or whether it may be manifested by any kinds of not-living matter? And here again it must be said that even what appears to be such a peculiarly vital phenomenon as spontaneous fission may be manifested by some kinds of not-living matter.

It has been long known that "myelin," a very complex fatty compound obtainable from yolk of egg and from nerve substance, will, when immersed in water, as Montgomery² has shown, give rise to cell-like bodies, and also protube delicate tubes which bend in all directions. But Robin² long ago showed more remarkable processes in the way of actual fission and amœboid changes in shape occurring in other fatty extracts, derived from dead animal substances. These phenomena were especially marked with certain fatty extracts obtained from blood, when they were mixed with water or with albuminous fluids. Such extracts are compounds having a very high molecular complexity, so that these spontaneous divisions and changes in form can only be ascribed to internal molecular movements, of a much simpler kind, doubtless, but in some measure akin to those occurring in living matter when fission occurs or amœboid movements are being produced.

² *On the Artificial Formation of So-called Cells*, 1867.

² *Mém. de l'Acad. de Médecine*, 1859, p. 248; quoted also in his *Traité du Microscope*, 1871, p. 562.

CHAPTER III.

ON SOME PROPERTIES OF CRYSTALS, WITH OBSERVATIONS ON THEIR MODE OF ORIGIN AND ON THE MODE OF APPEARANCE OF LIVING UNITS

Just as the form and properties of the crystal are to be taken as the natural outcome of the properties of its constituent molecules under the influence of its environment, so are the forms and properties of simplest organisms to be considered as the natural outcome of the properties of their molecules, entering into combination under the influence of their environing conditions.

Views analogous to these have been more or less fully expressed by many writers. The essential similarity in the laws regulating crystalline and organic forms was even suggested by Maupertuis in 1744.¹ Crystals and organisms were spoken of by Burdach² as statical and dynamical aggregates respectively. The formation of organisms was, moreover, in 1836, definitely compared by Schwann to the formation of crystals. Cells, which were then believed to be the types of all primordial organisms, were thought by him to owe their form to a process essentially similar to crystallisation, the characteristic shapes being due, in the case of cells, to a peculiarity in the nature of the substance of which they were composed. But it was in 1863, in the first edition of his *Principles of Biology*, that this relationship was most fully pointed out by Herbert Spencer, believing as he did that the structures and shapes of lower organisms are the results of the "polarities" of their con-

stituent organic units, under the continually modifying influence of external conditions. To his views on this subject we shall subsequently refer in some detail, so nothing more need be said at present.

Although saline materials so frequently aggregate into crystalline shapes when they emerge from the state of solution, still, in many cases, the assumption or not of such a form is entirely dependent upon the conditions under which the separation takes place.

Many substances which, in the chemist's laboratory, are only seen in the form of insoluble precipitates made up of amorphous granules could have been procured in a crystalline condition if the same decomposition which had given rise to the amorphous precipitate had been allowed to take place more slowly. If, instead of pouring a certain amount of a solution of sulphate of potassium into one of chloride of barium, we allow the mixture to take place gradually by means of dialysis, then crystals of sulphate of barium are formed rather than an amorphous precipitate. It has, in fact, been ascertained by Frémy³ that insoluble compounds generally, which appear in the laboratory as a result of double decomposition in the form of amorphous precipitates, can almost invariably be obtained in a crystalline condition when the chemical reaction is allowed to take place very slowly. This may be brought about by making the

¹ See Milne-Edwards in *Physiologie et Anatom. Comp.*, t. viii., p. 247.

² *Traité de Physiologie*, translation, 1839, t. iv., p. 129.

³ *Compt. Rend.*, t. lxiii., p. 714.

saline solutions mix after osmosis through membranes, wooden vessels, or porous porcelain. By one or other of these methods he obtained many very insoluble salts in the crystalline condition—such as the sulphates of baryta, strontia, and lead, the carbonates of baryta and lead, oxalate of lime, chromate of baryta, and several sulphides.

Variation in the "conditions" under which the crystallisation of any particular substance occurs, moreover, often gives rise to the most marked variation in its crystalline form. Thus, referring to the article "Dimorphism" in Watts's *Dictionary of Chemistry*, we find the following statements:—"Many substances, both simple and compound, crystallise in forms which belong to two or three different systems of crystallisation, or which, even if they belong to the same system, yet exhibit such differences in their corresponding angles as to render it quite impossible to reduce them to the same form. This was first shown by Mitscherlich in 1823 (*Ann. Ch. Phys.* [2], xxiv., 264). Such bodies are said to be dimorphous and trimorphous. The difference of crystalline form which they exhibit is associated with difference of specific gravity, hardness, colour, and other properties. Whether a body shall crystallise in one system or another seems to depend chiefly upon temperature..... Sometimes the form of the crystal varies according to the solvent from which it separates; thus arsenious anhydride crystallises from water or hydrochloric acid in regular octahedrons, but from alkaline solutions in trimetric prisms." Taking some other specific instances, we find that, "If a solution of carbonate of calcium in water containing carbonic acid be left to evaporate at the ordinary temperature, nothing is obtained but calc-spar, in microscopical and, for the most part, truncated primitive rhombohedrons; if, on the contrary, the solution be evaporated over the water-bath, arragonite is obtained in small six-sided prisms—mixed with a few crystals of calc-spar, because the temperature of the

solution is lower at first than it afterwards becomes."

In other examples we may find not only a striking difference in physical form, but also a notable change in colour, occasioned by the molecular rearrangements which change of temperature seems to necessitate. Thus I have before me now a sheet of paper having a large circular area smeared over with a scarlet patch composed of the double iodides of mercury and copper. If I hold it over a lamp, almost immediately the scarlet patch assumes a brownish-black colour; if the exposure has been only momentary, the patch almost immediately, on withdrawal, again assumes the scarlet colour; if I allow it to remain exposed to the heat for five seconds, it takes about fifteen seconds for the brownish-black colour to disappear, and for the scarlet colour to be restored. These changes will recur over and over again invariably, and just as they did more than thirty years ago, when the specimen first came into my possession.

It has also been found that the whole nature of a crystal already in existence may be changed by the action of causes which seem the most trivial: a slight elevation of temperature, or even the most delicate touch, in some cases is capable of initiating changes which spread through their entire substance, or throughout a whole aggregate of cohering crystals. Thus, in the same article on "Dimorphism" to which we have already referred, we find the following statement:—"Crystals formed at one particular temperature, and then exposed to that temperature at which crystals of a different kind are produced, often lose their transparency, and, *without alteration of external form, become changed into an aggregate of small crystals of the latter kind.* Examples of this alteration of structure are afforded by sulphur, carbonate of calcium, mercuric iodide, and many other bodies." Again:—"Mercuric iodide separates from solution, and likewise sublimes at a very gentle heat. In scarlet tables belonging to the

dimetric system; but, when sublimed at a higher temperature in sulphur-yellow, rhombic tables of the monoclinic system. The red crystals turn yellow when heated, and resume their red tint on cooling. The yellow crystals obtained by sublimation retain their colour when cooled; but, on the slightest rubbing or stirring with a pointed instrument, the part which is touched turns scarlet, and *this change of colour extends with a slight motion, as if the mass were alive, throughout the whole group of crystals as far as they adhere together.*"

These facts, together with those already cited, seem to show clearly enough, not only that the crystalline form of any crystallisable material is variable to a remarkable extent when it is allowed to crystallise under different conditions, but that, even when formed, a crystal produced under a certain set of conditions may be compelled by its very nature, when these are changed, to undergo an entire molecular rearrangement before a polar equilibrium can be again established between the same molecules and the new influences to which they are subjected. It will be well that all such facts should be thoroughly borne in mind when we have to consider the transformations that occur, or that may be deemed possible, in simple or elementary forms of living units; for, if the combinations constituting "living matter" are very much more complicated and unstable than those of the crystals to which we have just been referring, then the forms assumed by such sensitive matter under different conditions ought to be excessively variable and capable of undergoing remarkable transformations.

Let us turn now to the question of the mode of origin of crystals, and the possible mode of origin of lowest organisms, so far as this is open to mere observation.

The question in each case is whether, by mere concurrence of certain physical conditions, aiding and abetting the inherent properties of the respective dif-

ferent material combinations, some kinds of matter can fall into modes of combination called "crystalline," while other kinds are capable of falling into modes of combination called "living"; or whether, in each case, a pre-existing "germ" of the particular kind of matter is necessary, in order to determine, in suitable media, either of these modes of combination. Are we to believe that crystals can appear in no solution whatsoever without the pre-existence in that solution of certain crystalline germs; and similarly that living things can arise in no solution whatsoever without the pre-existence in such solution of living germs?

The very mention of this question in connection with the origin of crystals may seem to some people to be quite absurd, because they may have been in the habit of believing that crystals could, and do, habitually come into being *de novo*, without the agency of pre-existing crystals. But, in spite of the fact that the majority of people are quite content to believe that crystals originate in obedience to purely physical conditions, and independently of pre-existing "crystalline force," still, facts somewhat similar to those which are to be met with in connection with the sister problem long induced many chemists seriously to question the possibility of the *de novo* origination of crystals.

The way in which this problem was at last solved has been ably brought out by Professor Léo Errera in an essay entitled *A Propos de Génération Spontanée*.¹ He points out that in 1865 Violette and Gernez demonstrated, independently, that crystallisation was brought about at ordinary temperatures in supersaturated solutions of sodium sulphate by the access thereto of microscopic crystals of this salt, which exist (almost always) as constituents of atmospheric dust. These microscopic crystals, not much larger than the germs of microbes,

¹ *Revue de l'Université de Bruxelles*, t. v., 1899-1900, Mai.

act much like germs, and lead to the separation of crystals from the mother liquor.

After careful examination of the evidence Errera came to the conclusion that, from the point of view of crystallisation, there are two classes of fluids: one (*a*) in which, under appropriate conditions, there may be a spontaneous generation of crystals, as well as a spread of the process induced by inoculation; and another (*b*) where the latter process only seems possible, and in which generation has never been known to occur. These two classes of fluids, of another order, have, of course, been long recognised also by those who believe in the "spontaneous generation" of microbes.

The "germ" theory of the origin of crystals in supersaturated solutions has, therefore, not only been in existence, but has been overthrown. This has been possible, however, only because it has been more easy to show that a given set of conditions are inimical to the existence of a crystal, than it has yet been to induce people to believe that certain sets of suitable experimental conditions are incompatible with the pre-existence of matter in the living state.

The analogy between the two problems as to the possible origin of crystals and organisms *de novo* in solutions was rendered much more obvious by the discovery of the late Professor Graham that, when dissolved, the saline substance does not remain as such in solution, but that the acid and the base exist separately, and are separable by a process of dialysis. When crystallisation occurs, therefore, we have a combination of molecules taking place similar to, though much simpler than, what may be presumed to take place in the genesis of a speck of living matter.

So far as evidence derived from *microscopical examination* can be adduced, however, it is able to speak no more decisively concerning the *de novo* origin of crystals than concerning the *de novo* origin of organisms. In the

elucidation of this point the valuable, though insufficiently known, observations of George Rainey¹ come most opportunely to our aid. In ordinary cases it is difficult to watch satisfactorily with the microscope the first stage in the appearance of crystals in solutions containing crystallisable matter, owing to the rapidity with which their growth takes place. This is one point in which crystals are strikingly different from organisms. The slower growth of organisms is, however, as Graham pointed out, quite in accordance with the general slowness of colloidal changes. But, since Rainey's discovery that crystals are produced much more slowly, and undergo very important modifications in shape, when they are formed in viscid solutions, the formation of these bodies has, in both respects, become much more obviously comparable with that of organisms.

The appearance of these modified crystals may be best watched after mixing solutions of gum and carbonate of potash in the manner carefully described by Rainey. Owing to the viscid properties of gum, a solution of this substance diffuses with difficulty, and hence, when brought into contact with a solution of carbonate of potash, the malate of lime of the gum only decomposes very slowly. The insoluble carbonate of lime, instead of appearing in its usual crystalline condition, is precipitated in the form of globules resembling calculi. Rainey thus describes what takes place when portions of the two solutions are mixed under the microscope:—"The appearance which is first visible is a faint nebulosity at the line of union of the two solutions, showing that the particles of carbonate of lime when they first come into existence are too minute to admit of being distinguished individually by high microscopic powers. In a few hours exquisitely minute spherules, too small to allow of accurate measurement, can be seen in the nebulous

¹ *On the Mode of Formation of the Shell of Animals, &c.*, London, 1858.

part, a portion of which has disappeared, and is replaced by these spherical particles. Examined at a later period, dumb-bell-like bodies will have made their appearance, and with them elliptical particles of different degrees of excentricity." These modified crystals are, therefore, not produced much more rapidly than the lowest living things appear to be in other solutions during hot weather. The shapes of the products in the two cases, judging from Rainey's figures, are also remarkably similar.

It will be interesting now to turn our attention to fluids containing organic matter in solution such as have been used in experiments on so-called "spontaneous generation," so as to ascertain what mere microscopical observation can tell us concerning the first appearance of living units therein.

The writer first made observations of this kind in 1871.¹ I have recently adopted a much simpler method for making such observations, doing away with the necessity for any apparatus. An infusion of turnip has been used, or one prepared from a small portion of fresh beef or mutton, and subsequently filtered through two layers of the finest Swedish paper. A drop of one of these fluids is allowed to fall on a cleaned microscope slip, over which a seven-eighth of an inch square cover-glass is placed, and the excess of fluid is then removed by bringing a piece of blotting-paper to its edge. It is well to see that one or more minute air bubbles exist somewhere in the film before applying some melted paraffin wax round the edge of the cover-glass.² A small speck of paint may now be placed on the cover-glass just below one of the minute air bubbles, and the space between it and the bubble then carefully examined with the microscope. If the fluid in this region is found to be free from particles, that will

be the region to be subsequently watched, and the edge of the air bubble will give the necessary guidance for bringing the film of fluid into focus.

The slip thus prepared may now be placed in an incubator, the temperature of which is maintained at about blood-heat; and after three or four hours we may begin to make observations, replacing the slip in the incubator after each observation. The time necessary for the appearance of Bacteria will vary according to the nature and strength of the infusion used; and of course will be more and more prolonged if lower incubating temperatures are employed.³

But if, in a motionless film of fluid, multitudes of living particles subsequently appear, which are themselves almost motionless, how can we account for their origin? Three hypotheses present themselves. It may be said (a) that they have arisen through the reproductive multiplication of one or more germs or organisms in the film of fluid which, though visible, had escaped observation. The difficulties standing in the way of our acceptance of this explanation are these. The film is motionless, and also those first appearing particles which gradually come into view in portions of it where no such particles had been previously visible. No multiplication by fission or other means can actually be observed to take place among the first appearing particles in question; though this ought to be easily observable if it really occurred at the rate postulated. And, lastly, if the subsequent large numbers are to be accounted for by the occurrence of a reproductive process taking place among a few visible but unobserved germs, these products of fission, being at first motionless, ought to be aggregated here and there only, while, as a matter of fact, no such

¹ Thus I have found that with a temperature as low as 85° Fahr., and the film hermetically sealed, as in this mode of observation, the Bacteria usually begin to appear somewhere between the twelfth and the eighteenth hour.

² See *The Beginnings of Life*, vol. i., p. 295.

³ I have used wax which melts at 105° Fahr.

arrangement exists—the distribution of the particles being more uniform.

These various difficulties appearing fatal to this explanation of the mode of origin of the multitudes of plastid-particles and Bacteria, we are left with only two other possible modes of origin—either (*b*) they have been developed from a multitude of pretty-evenly disseminated *invisible* germs, or (*c*) they have been produced *de novo* in the fluid by a process of Archebiosis.

Thus the solution of this great problem passes beyond the reach of actual observation. Microscopical evidence enables us to bring it to this stage now, and it may perhaps never enable us to do more. It reduces us to a consideration of two rival hypotheses, and to a careful consideration of whatever evidence may be forthcoming to influence us in our choice between these two possible explanations. Nothing that can be said about the abundance of recognisable atmospheric germs can directly affect the solution of this problem. It is one which, if it has to do with germs at all, has to do with invisible germs. But invisible germs can have only a hypothetical existence, and even to this they can lay no claim, unless observed phenomena cannot be explained without such postulation. We must not forget the old and well-approved logical rule—

“Entia non sunt multiplicanda præter necessitatem.”

The “law of parsimony” may well be quoted for the benefit of those who would ruthlessly people the atmosphere with countless myriads of “entities.”[‡]

[‡] Some of those who are so eager to demonstrate the prevalence of “germs” are frequently carried away by their enthusiasm beyond the bounds of strict logic. It suffices to show by the agency of the electric light, or by some other means, that air and water contain myriads of infinitesimally small particles, some of which are organic in nature, in order that they may at once come to the conclusion that the organic particles are “germs.” But, seeing the countless forms of life which exist upon the surface of the earth, and how these are from moment to moment, during life as well as after death,

Let us suppose, however, now, for the sake of argument, that the living units which may be seen to make their appearance in the manner just described have, in reality, been formed *de novo* by a process of synthesis from the organic molecules contained in the infusion; and, further, from the point of view of this hypothesis, let us consider what is known concerning the mutability of such forms of life, and whether—as should be the case if they are in fact new-born specks of living matter—they exhibit in a remarkable degree a capacity for change in form and nature such as we have found to be present in the case of crystalline matter.

We may suppose that Bacteria of all kinds (including Bacilli, Micrococci, Staphylococci, Streptococci, etc.) and *Torulæ* merely represent the most prevalent forms which specks of new-born living matter are prone to assume;[‡] and in conformity with this view it may be stated that all intermediate shapes are frequently to be seen between the various forms, just as all intermediate conditions are to be seen between the smallest Bacteria of some highly fermentable infusion, and the larger *Vibrio*-like or *Leptothrix* forms which are frequently met with in other fluids.

The opinion was formerly held that *Vibriones* and *Leptothrix* are higher organisms than Bacteria, and that *Torulæ* are higher than either. Both these views, however, are equally devoid of any real foundation.

It is well known that the more rapidly crystalline matter separates from a solution the greater is the number and the

undergoing a molecular disintegration, it would be strange indeed if the atmosphere, and water which has been exposed to it, did not contain multitudes of organic particles, both large and small. The great majority of such mere organic particles, however, could have no reasonable title to be called germs.

[‡] In other media, of course, we may have the simplest *Algæ* appearing as non-nucleated blue-green specks of protoplasm (the *Chromacea*). These forms, however, are just as varied and mutable as we shall find the Bacteria to be.

smaller the size and perfection of the crystals which appear. And similarly it may be found that the most fermentable solutions swarm rapidly with inconceivable numbers of small Bacteria; though, if a drop of acetic acid has been added to another portion of the same infusion, it will not become turbid till many hours later, and the Bacteria which are present in the first scum may be much larger and such as are generally termed Vibriones or Leptothrix. In other cases highly fermentable fluids which have been subjected to the influence of very high temperatures (270° Fahr. and upwards) will, even when exposed to the air, yield neither Bacteria nor Vibriones—though *Torulæ* will appear after a time, often as a sediment, and multiply only very slowly.

Now, with reference to such observations, the following considerations must be borne in mind. A highly fermentable solution is in one respect exactly comparable with a supersaturated saline solution. Both contain chemical elements which we may suppose to have a strong tendency to combine, and in both cases the products of combination are insoluble—particles of crystalline matter appear in the one case, and particles of living matter in the other. But the living matter differs essentially from the crystalline matter by reason of the complexity of its constituent molecules and its consequent more marked instability. This capacity for free internal molecular movement, which is one of the most distinctive attributes of living matter, may well be most marked in the products which separate from the most fermentable solutions; and it is precisely this attribute which is the principal factor in bringing about the self-multiplication, or “discontinuous growth,” of living units.

Thus it is that rapid growth and rapid fission frequently go on simultaneously; so that, although the total amount of living matter which separates from a solution may be large, the individual living units may be very small. Discon-

tinuous growth is in excess, and therefore the fact of the growth being really rapid is apt to be overlooked.

All the differences in size and form recognisable between Micrococci and small Bacteria, between Bacteria and Bacilli or Spirilla, between the latter and Vibriones, whether jointed or unjointed; between Vibriones and Leptothrix filaments, plain or segmented in various ways; and between Leptothrix and the very delicate mycelial filaments of many Moulds, are easily explicable in accordance with these considerations. The several forms, many of which frequently occur together in the same solution, depend, in the main, upon the frequency with which segmentation tends to occur, and upon the degree of completeness of the process.

The forms of *Torulæ*, again, are almost infinite in variety as met with in different situations, and they are often notably different even in the same solution. They may vary in size from the minutest visible speck to a vesicle $\frac{1}{1000}$ or more in diameter. They may after a time become spherical, ovoidal, ellipsoidal, or cylindrical in form, and form buds in different directions.

It is impossible for us to assign any ultimate reason why one rather than the other of these forms should manifest itself. We can only observe that in some solutions different forms of Bacteria most frequently present themselves, while in others *Torulæ* are most prone to occur. It has been known, for instance, since the time of Dutrochet that the organic forms met with in acid and alkaline or neutral solutions vary; and it has been frequently observed by others that *Torulæ* are most apt to present themselves in slightly acid solutions. Again, while the most putrescible solutions almost invariably yield Bacteria, the same fluids, after their fermentability has been greatly impaired by the influence of heat, may produce nothing but *Torulæ*. *Torulæ* are generally more frequent than Bacteria in saline solutions; and in some of these, after they have

been boiled, no Bacteria ever present themselves.

Because in some solutions different forms of Bacteria invariably reproduce their like, just as in other solutions *Torulæ* reproduce *Torulæ*—because each of these forms “breeds true”—they are commonly regarded and named as so many distinct “species.” This, however, is far from being justifiable. A fragment which detaches itself from one of the lowest living things has just the same tendency to grow into the form of its parent as the fragment detached from a given crystal has to reproduce a similar crystalline form. In each case, however, the parent form is reproduced only so long as the conditions remain the same. Placed under new conditions the crystalline fragment may grow up with a modified form; and, similarly, under the influence of new conditions, a change may overtake a portion of matter thrown off from a pre-existing living form. In order that the crystal may lapse into another form it seems necessary that the new conditions shall be capable of bringing about a new molecular arrangement (or isomeric state) of the crystalline matter. And, similarly, new conditions would probably change primordial living forms only so long as they were capable of inducing internal molecular rearrangements. It is, therefore, only to be expected that even new-born organic forms should remain constant, or “breed true,” so long as we have to do with the same fluids under unaltered conditions.

But, since it is well known that different kinds of Bacteria and *Torulæ* may frequently be seen to grow in the same solution, we are compelled to believe that some minute difference in the constitution of their ultimate units exists, and that each has the power of causing, during its acts of growth, the synthesis of similar units of living matter. The occurrence of one or the other form need not, therefore, be always or wholly attributable to mere difference of “conditions”—it ought to be mostly due to an actual, though minute, difference in the

molecular constitution of the initial units of living matter. For, although the same crystalline matter, under the influence of different conditions, may assume different crystalline forms, it is much more common for different crystallisable compounds to aggregate into different geometrical forms.

It must, moreover, be quite familiar to all who have had much experience in this particular line of research that *Torulæ* frequently exist in abundance in certain solutions, and yet show no signs of developing into Fungi. Discontinuous growth goes on rather than continuous growth. So much is this the case that it was not till 1840 that the development of *Torulæ* into Moulds or simplest Fungi was traced.

The particular forms assumed by the outgrowths from germinating *Torula* corpuscles seem subject to much natural variation. Thus Trécul found that on exposure to the air the cells of beer yeast grew partly into the form of *Mycoderma cerevisiæ*, and partly into that of a large *Penicillium*. He became convinced, therefore, that the view originally advocated by Turpin is correct—viz., that *Mycoderma* and *Penicillium* are simply two forms which may be assumed by germinating beer *Torulæ*. Nay, more, the *Mycoderma* itself is observed to be most changeable in its form, as the qualities of the fluid in which it grows alter; and an already growing *Mycoderma* is said to be capable of taking on the mode of growth characteristic of *Penicillium*.¹

We have thus endeavoured to show that if, in accordance with our hypothesis, the Bacterium and the *Torula* corpuscle are only different modes of growth that may be assumed by new-born specks of living matter, so also, as might be expected, are the varieties of each kind of

¹ See *Compt. Rend.*, t. 67, p. 1,164. This development of *Mycoderma* into forms resembling *Penicillium glaucum* was observed by Turpin in 1840, by the Rev. M. J. Berkeley in 1855 (from porter yeast), and, subsequently, by Pouchet in beer yeast and in that from cider.

growth both numerous and transitional. We have seen that each of the forms, under suitable conditions, may grow in a continuous rather than in a discontinuous fashion, the *Torula* then producing variously branched and articulated growths, which at intervals are apt again to revert to the discontinuous mode of growth, so as to produce "reproductive units"—either in single file as buds from a terminal expansion, or by segmentation of the contents of a terminal chamber. These, and many other simple variations in the mode of production of the reproductive units, variously combined with different sizes, modes of branching, articulation, and segmentation of the filaments, etc., go to produce the innumerable simpler kinds of Fungi which, like the innumerable varieties of Bacteria, instead of being lineal descendants of similar mutable organisms that lived in a pre-Adamite world, may be only different modes of growth continually being assumed by new-born living matter.

What is to be observed concerning the mutability of these low forms of life may, therefore, fairly be said to be quite consistent with the notion that they are, as they seem to be, capable of arising *de novo* in suitable fluids.

And, if true, this would again be quite in harmony with what we have attempted to show in the previous chapters. We have seen how, under the influence of the well-established doctrine concerning the Persistence of Force—and more especially since the clear recognition of the subordinate doctrine as to the correlation existing between Physical and Vital Forces—the majority of biologists have long recognised that the phenomena manifested by living beings are to be ascribed simply to the properties of the matter as it exists in such living things. No one has expressed himself more decidedly on this subject than Professor Huxley, and he may fairly be taken as an exponent of the modern

doctrines on this question. In a striking article on "The Physical Basis of Life" he said¹:—"But it will be observed that the existence of the matter of life depends on the pre-existence of certain compounds—namely, carbonic acid, water, and ammonia. Withdraw any one of these three from the world, and all vital phenomena come to an end. They are related to the protoplasm of the plant, as the protoplasm of the plant is to that of the animal. Carbon, hydrogen, oxygen, and nitrogen are all lifeless bodies. Of these, carbon and oxygen unite in certain proportions and under certain conditions to give rise to carbonic acid; hydrogen and oxygen produce water; nitrogen and hydrogen give rise to ammonia. These new compounds, like the elementary bodies of which they are composed, are lifeless. But, when they are brought together under certain conditions, they give rise to the still more complex body—protoplasm; and this protoplasm exhibits the phenomena of life."

"I see no break in this series of steps in molecular complication, and I am unable to understand why the language which is applicable to any one term of the series may not be used to any of the others....."

"It is true that there is no sort of parity between the properties of the components and the properties of the resultant; but neither was there in the case of water. It is also true that what I have spoken of as the influence of pre-existing living matter is something quite unintelligible; but does anybody quite comprehend the *modus operandi* of an electric spark, which traverses a mixture of oxygen and hydrogen?"

"What justification is there, then, for the assumption of the existence in the living matter of a something which has no representative or correlative in the not-living matter which gave rise to it?"

¹ *Fortnightly Review*, February, 1869, p. 129.

CHAPTER IV.

THE MOLECULAR CONSTITUTION OF LIVING MATTER AND ITS INNATE TENDENCY TO VARIATION

WHAT has hitherto been said goes to show that the properties of all bodies alike are due to their molecular composition and nature; and that in the case of compound substances their properties are wholly different from, and cannot even be guessed at from a knowledge of, the properties of their constituent elements.

The capacity for undergoing changes in appearance and properties under the influence of changing external conditions has been illustrated by a reference to some of the transformations that occur in different kinds of matter when aggregated into the forms of crystals and of lowest living units respectively. It was furthermore hinted that these changes and transformations could only have been induced by reason of the new physical influences having brought about some alteration in the molecular arrangement pre-existing in the several substances; and that, however easily such changes are capable of being induced in crystalline matter, they ought to be brought about still more easily in newborn living matter by reason of its far greater molecular complexity.

We must look now a little more fully at this question of molecular composition, in order, if possible, to acquire some vague notion as to the degree of its complexity in living matter.

It may surprise some to know that molecular composition is an important item even with reference to substances that are looked upon as elementary—different modes of composition or arrangement of the atoms sufficing to

produce in them what are called “allotropic” states. We are most familiar with these as they are presented to us in the various forms of carbon. The differences between the diamond, graphite, anthracite, and pure charcoal are most striking, and yet these are all different states of one and the same substance, whose ultimate atoms are differently grouped.

Oxygen exists in two different states—ordinary oxygen, and ozone which is supposed to be represented by O_3 . Sulphur crystallises in rhombic octahedrons belonging to the trimetric system, and also in rhombic prisms belonging to the monoclinic system. The latter have a deep yellow colour, and are translucent, and always exhibit a great tendency to pass by molecular rearrangement—accompanied by an evolution of heat—into the opaque, straw-yellow, octahedral crystals.

There are, again, two varieties of phosphorus, known by the name of “normal” and “red Phosphorus.” The first variety is much more poisonous than the second; it is also colourless, crystallisable in rhomboid dodecahedra, soluble in sulphide of carbon, easily oxidisable, phosphorescent, and inflammable at a low temperature. The second form is chocolate or puce coloured, amorphous, much less soluble, non-phosphorescent, and not inflammable even at high temperatures. Lemoine has shown that heat is the most available means for converting the one form into the other, and that the transformation is always only partial.

Arsenic, antimony, and other metals also exist in allotropic states; and the two states of each of these elements exhibit wholly different properties.

These phenomena of allotropism show that even simple bodies—such as carbon, phosphorus, sulphur, and the metallic elements—are made up of molecules composed of similar atoms, existing in varying though definite number and grouping in each allotropic state that may exist. An alteration of the number or grouping of the atoms in the molecules, or of both, seems indeed to be the only way of accounting for the wholly different properties and crystalline forms of one and the same substance, such as sulphur, under the influence of different physical conditions.

Thus vanishes a part of the difference between simple or elementary and compound bodies. They are all made up of molecules; only those of the simple substances are aggregates of similar atoms, while those of compound substances are aggregates of dissimilar atoms.

Different compound substances vary, of course, immensely in their degree of molecular complexity. Some, such as ordinary acids or bases, are aggregates of simply complex molecules; others are aggregates of doubly complex molecules—that is to say, two simply complex molecules combine to form a doubly complex molecule, and these include, among other compounds, the very common class of bodies known as salts. Seeing that in bodies of this class a compound radicle, such as cyanogen (CN) or ammonium (NH_4), may replace one of the simple metallic elements, and that two such salts may combine together to constitute a double salt; or that the metallic element may be replaced by a more complex radicle, such as urea ($\text{C}^2\text{N}^2\text{H}^4\text{O}^2$), or kreatinine ($\text{C}_6\text{H}_7\text{N}_5\text{O}_2$), or even by one of the still more complex bodies known as alkaloids,¹ we may be some-

what amazed at the marvellous atomic complexity which is to be attained even by the crystallisable bodies known as salts.

What has been said concerning crystallisable bodies obtains also with regard to the compounds known as “colloids.” In this class are included all the plastic elements of animals and plants—that is, the various protein compounds. They are supposed to be generally characterised by the large size and complexity of the molecules of which they are compounded. Thus Professor Graham says²:—“It is difficult to avoid associating the inertness of colloids with their high equivalents, particularly where the high number appears to be attained by the repetition of a smaller number. The inquiry suggests itself whether the colloid molecule may not be constituted by the grouping together of a number of smaller crystalloid molecules, and whether the basis of colloidalty may not really be this composite character of the molecule.”

No hard-and-fast line, however, separates the colloids from the crystalloids. Although multitudes of bodies exist which may be easily placed in one or the other class, multitudes of others are to be met with having properties of an altogether intermediate character. Nay, even the most typical colloids may undergo a rearrangement of their elements, whereby they are converted into crystalloids. Nothing could show more plainly than this that a difference between a crystalloid and a colloid is merely one of degree, and that the properties of colloids are different merely by reason of the more complex molecular arrangement which prevails—an arrangement, however, which, by the mere influence of physical conditions, the molecules of certain crystalloids are known “spontaneously” to assume. A further consequence directly flowing from this superior complexity of the colloid molecules is the greater instability which characterises them as a class. Very slight

¹ The composition of narcotine, for instance, is said to be $\text{C}_{20}\text{H}_{26}\text{NO}^{14}$, and that of morphine $\text{C}_{17}\text{H}_{19}\text{NO}_8 + 2\text{H}_2\text{O}$. Both bodies have distinct crystalline forms.

² *Phil. Trans.*, 1861.

changes in the conditions or influences to which the colloid is exposed lead to changes in its constitution, owing to the ease with which a rearrangement is brought about among its constituent atoms or elementary molecules. Its very existence, as Graham pointed out, is one of "continual metastasis."

It is owing to the great size and complexity of the molecules of which they are composed that colloids, in their ordinary state, will not pass through a parchment membrane. Their molecules are too big to be able to get through its pores; though various saline substances will pass through with the greatest ease, and thus can be easily separated by dialysis from the colloids with which they may have been intermixed.

But, under the influence of certain re-agents, more especially the digestive juices of the stomach and pancreas, proteids undergo most important modifications which enable them, like crystalloids, to diffuse through membranes. When thus modified they are known as "peptones," and in regard to these bodies Verworn says:—"It is known that they arise by the hydrolytic cleavage of the original proteid molecule, so that the peptones represent the hydrates of the original proteids. Important conclusions follow from this fact. Since the proteid molecule, which was originally not diffusible on account of its enormous size, is split up in the peptonising process into the peptone molecules, which are much smaller and therefore diffusible, but which have the chemical characteristics of proteids, it follows that the proteid molecule is not simple, but polymeric—*i.e.*, it consists of a chain-like combination of many similar groups of atoms. In the transition to the peptone condition the proteid molecule is broken up with hydration into these single, similar atomic groups, all of which, however, have the chemical characteristics of proteids, but represent much smaller molecules."

Let us look at these compounds now from another point of view.

Before Wöhler (in 1828) announced to the scientific world that he had succeeded in building up an organic compound in his laboratory with the aid of no more mysterious agencies than usually lie at the chemist's disposal, and before the labours of other distinguished chemists had been crowned with a like success, there was more reason than there is at present for the belief that the forces in living things are altogether peculiar, because it was thought that certain compounds of carbon with other elements, known as "organic" substances, were capable of being produced only within these laboratories of nature. A department of "inorganic chemistry" had hitherto existed, separated quite definitely from another known as "organic chemistry." In the former were included all those elements and their compounds that were naturally met with among, and which made up, the non-living constituents of our globe; while under the latter department were ranged such compounds as proteids, carbohydrates, fats and their derivatives, which were supposed to be produced only in plants and animals.

The so-called organic compounds were for a long time regarded as altogether peculiar; not as regards their components—for they were known to be composed of precisely the same elements as were most abundant in the inorganic world—but rather in point of origin. They were the products only of living things—had been produced under the influence of "vital" forces. The action of physical forces in the world without was deemed inadequate to give rise to such combinations, and therefore they were separated by a hard-and-fast line from all other compounds with which the chemist manipulated. Thus the popular belief of the time concerning "life" was fostered; and an argument for the special and peculiar nature of the "vital forces" could, at least, be based on the supposed fact that living

* *General Physiology*, translation, 1899, p. 106.

things produced substances—were, in fact, almost entirely composed of material combinations—which could not be evolved by the agency of mere physical forces, either in the grand laboratory of nature or under the hands of the chemist. But now all this has changed. Chemists have already succeeded in building up many hundreds of such compounds; and as each month passes the list is swelled by fresh conquests. The speciality, then, of these compounds has passed away; the difference between organic and inorganic chemistry is fast vanishing—has, in fact, well-nigh vanished.

Members of this great group of albuminous or proteid compounds always contain, as principal and fundamental ingredients, carbon, oxygen, hydrogen, and nitrogen, and to these are commonly added traces of sulphur and phosphorus. The first four elements are, however, all-essential, and it is especially worthy of remark that no less than three of them are gaseous. Herbert Spencer says¹:—"When we remember how these re-distributions of Matter and Motion which constitute Evolution, structural and functional, imply motions in the units that are re-distributed, we shall see a probable meaning in the fact that organic bodies, which exhibit the phenomena of Evolution in so high a degree, are mainly composed of ultimate units having extreme mobility." When such mobile units enter into various combinations, this initial property, though masked, is still potentially present, and must have its influence upon the molecular mobility of the compounds into which they enter.

The properties of the various proteid substances which form the all-essential constituents of living tissues are found to correspond entirely with these *à priori* requirements. This can scarcely be better shown than it has been by H. Spencer when he wrote²:—"It is, how-

ever, the nitrogenous constituents of living tissues that display most markedly those characteristics of which we have been tracing the growth. Albumen, fibrin, casein, and their allies, are bodies in which that molecular mobility exhibited by three of their components in so high a degree is reduced to a minimum.....It should be noted, too, of these bodies that, though they exhibit in the lowest degree that kind of molecular mobility which implies facile vibrations of the atoms as wholes, they exhibit in a high degree that kind of molecular mobility resulting in isomerism, which implies permanent changes in the positions of adjacent atoms with respect to each other.....In these most unstable and inert organic compounds we find that the atomic complexity reaches a maximum: not only since the four chief organic elements are here united with small proportions of sulphur and phosphorus, but also since they are united in high multiples. The peculiarity which we found characterised even binary compounds of the organic elements, that their atoms are formed not of single equivalents of each component, but of two, three, four, and more equivalents, is carried to the greatest extreme in these compounds that take the leading part in organic actions. According to Mulder, the formula of albumen is $10(C_{40}H_{31}N_5O_{12}) + S_2P$. That is to say, with the sulphur and phosphorus there are united ten equivalents of a compound atom, containing forty atoms of carbon, thirty-one of hydrogen, five of nitrogen, and twelve of oxygen; the atom [molecule] being thus made up of nearly nine hundred ultimate atoms."

On the other hand, it seems equally clear from what has previously been said (p. 25) concerning the free growth of *Bacteria* and *Torulæ* in solutions of ammoniac tartrate in distilled water, that proteids and even protoplasm can be formed readily enough from C, H, O, and N, together with the merest trace of sulphur or phosphorus which are commonly regarded as necessary and

¹ *Principles of Biology*, vol. i., chapter i., "Organic Matter." ² *Loc. cit.*, p. 12.

invariable elements in a proteid compound.²

Most of these compounds whose molecules are so very complex are known to be capable of existing under very many different isomeric modifications. Protein, for instance, according to Professor Frankland, is capable of existing under probably at least a thousand isomeric forms; and this, as we have seen, is the substance which, in one state or another, enters so largely into the fabric of living things as to be, above all else, *the* organisable material—the molecules of which are, in some at present unknown way, built up so as to form the still more complex body, protoplasm. The possibilities of change and isomeric variation in this more complex product are further enormously increased by the common presence of additional elements, which must be taken up by the proteids and are thus brought into its composition. These common additional elements, one or more of which are often found in protoplasm, are chlorine, potassium, sodium, calcium, iron, and magnesium.

Three groups of proteids, varying in their solubility in water, are known as “albumins,” “globulins,” and “vitellins”; while in addition there are many very important bodies known as “albuminoids,” which are derivatives, differing from the genuine proteids, and yet in many respects like them. They include various horny and elastic substances found in skeletal tissues and in skin developments; and also the extremely important group of “enzymes” or soluble ferments to which reference was made in the last Chapter—including ptyalin, pepsin, trypsin, pancreatin, etc.

Multitudes of other organic compounds exist as decomposition products of the proteids—results of disassimilation.

² Sir William Ramsay has been kind enough to analyse one of the solutions of ammoniac tartrate for me, and he reports that the “liquid contained an excessively minute trace of sulphur, probably as sulphate; but no phosphoric acid could be detected by the molybdate of ammonium test.”—See *Appendix*.

Many of these are thrown off as waste products, the commonest of all being carbonic dioxide and urea; others, such as “fats” and “carbohydrates,” are very important non-nitrogenous compounds, the latter including different kinds of sugar and starch met with principally in plants, and of great importance in their economy.

Although such a radical distinction exists between crystalloids and colloids, in regard to their intimate molecular constitution, as to have led Graham to say, “Every physical and chemical property is characteristically modified in each class,” so that “they appear like different worlds of matter,” yet the transition from the one state to the other seems to be comparatively easily brought about. As Graham has shown, one and the same saline substance may exist with its molecules now in the crystalloid and now in the colloidal mode of aggregation—according to the different influences to which it has been subjected—or under which it has been produced. This, for instance, is the case with silica, with the sesquioxides of chromium and iron, and with other mineral substances. Nay, more, the absence of any natural barrier between the crystalloid and the colloidal mode of aggregation may be still further seen by the fact that even the most typical colloids are capable of undergoing that kind of isomeric molecular change which converts them into crystalloids. As one of the best instances of this we may mention the fact of the change which blood pigment undergoes. Hæmatoidin is frequently met with in the form of oblique rhombic crystals, and in addition there are other crystalline forms of albuminoid substances obtainable from blood,² and also from the cells of some plants (known as aleuronegranules). Chlorophyll has likewise been observed in a crystalline state

² See an article on “Albuminous Crystallisation,” in *Brit. and For. Med. Chir. Review*, Oct., 1853.

by Trécul.¹ While Marcet,² as a conclusion drawn from certain experimental researches, expressed the opinion: "That there is a constant change, as rotation in nature, from crystalloids to colloids, and from colloids to crystalloids."

These facts sufficiently show that, notwithstanding all their differences in property, the transition is fairly easy from the one to the other isomeric state. And if simple saline substances can pass into the colloidal condition, it surely should not be difficult to imagine that molecular rearrangements may take place among the constituents of ammoniacal salts of greater complexity, whereby a colloid may be produced capable of entering into the constitution of that simplest form of protoplasm such as must be engendered by the Bacteria that grow and multiply, as I have shown, in a simple solution of tartrate of ammonia in distilled water. The simplicity of their life-processes must exceed that of the nitrifying Bacteria, just as this in its turn exceeds that of the simplest Algæ (the Chromacea); though in each case their protoplasm is directly fashioned by the synthesis of inorganic elements.

If the living unit—the unit of protoplasm—is itself constituted, as is commonly supposed, by a grouping of colloid molecules, we are introduced to a substance whose molecular diversity may well be as endless as that of the countless simple forms of life, together with the similar variety of tissue elements of which plants and animals are constituted.

In its simplest form protoplasm exists as a jelly-like, slightly granular, semi-fluid substance, without visible structure, though it is presumed to have an excessively minute sponge-like or foam-like character, and to be capable of absorbing dissolved matter from without, as a preliminary process leading on to assimilation and growth. But, as Haeckel says:³ "We have as yet no knowledge

of the fundamental features of its very variable chemical structure. The one thing that bio-chemists have told us about it is that the molecule of plasm is very large, and made up of a great number of atoms (over a thousand); and that these are combined in smaller or larger groups, and are in a state of very unstable equilibrium, so that the life process itself causes constant changes in them."

This, then, is the material that was spoken of by Huxley as "The Physical Basis of Life"; and the upholders of the Protoplasm or Sarcode theory maintain that this substance has an essential unity of nature. So that, in spite of multitudes of minute specific and isomeric differences, we have in reality to do with one and the same generic substance, whether existing as the "contents" of animal and vegetable cells, or as naked masses of protoplasm—whether as parts of higher organisms, or as single independent living units. The recognition that all these various forms are but trifling alterations of a single great genus of primitive organisable material, and that in all cases this "albuminous material is the original active substratum of all vital phenomena, may," says Professor Haeckel, "perhaps be considered one of the greatest achievements of modern biology, and one of the richest in results."

Protoplasm, then, in its most general and undifferentiated condition, in the form of a naked contractile mass, of seemingly homogeneous, semi-fluid jelly; is the variable substratum for all the life-movements of the lowest living things, even in their adult condition. A minute structureless mass of jelly, now in one now in another isomeric state, suffices for the display of all the vital phenomena of the lowest organisms. Here, without the aid of organs of any kind, are carried on the vital phenomena of "growth" and "reproduction"; here do we see the first germs of that organic irritability and contractility which attain their highest development in the conscious

¹ *Comptes Rendus*, t. lxi., p. 436.

² *Proceed. of Roy. Soc.*, vol. xix. (1871), p. 455.

³ *The Wonders of Life*, 1904, transl., p. 140.

sensibility and power of movement | stand at the head rather than at the
 possessed by those living things that | foot of organic nature.

CHAPTER V.

THE "IDS" AND "DETERMINANTS" OF WEIS-
MANN *VERSUS* THE "PHYSIOLOGICAL
UNITS" OF HERBERT SPENCER

THE general acceptance of the Doctrine of Evolution initiated by the publication of Darwin's epoch-making work, *The Origin of Species*, soon attracted enormous attention to the problems connected with Heredity; and some of these doctrines, in their turn, have sufficed to focus the labours of a vast number of workers upon the structure of cells, and upon the excessively complicated processes commonly taking place in such bodies when they divide, and more especially upon the processes occurring in those particular cells—the germ-cells and sperm-cells—upon whose union the development of different animals and plants for the most part depends.

The result has been a vast amount of scientific literature dealing, both theoretically and experimentally, with the subject of Heredity,¹ and one equally vast on the subject of Cytology—that is, concerning the structure, functions, and life-history of cells. To some of these discussions a brief reference must now be made.

What has already been said in pre-

vious Chapters should have made it perfectly clear that we must look for the fundamental properties of living matter as being possessed by certain chemical units of great complexity, though so minute as to lie altogether beyond the range of any existing microscopical powers; and, further, that such invisible units when they grow, by the continuance of synthetic chemical processes, result in the appearance of minute particles of protoplasm which, though structureless, speedily assume one or other of the well-known forms of lowest organisms. We have, moreover, seen reason to believe that such forms may be represented by the various kinds of Bacteria, of Moulds and of Chromacea.

It must be clear, from the point of view of Evolution, that Unicellular Organisms and the various developed forms of cells entering into the structure of multicellular plants and animals are products which have been evolved from structureless units of living matter such as one or other of the above-mentioned, lowest-known forms of life, or units as simple as these.

We have seen how freely the process of fission is capable of taking place in all the lowest forms of life, in which no trace of a nucleus exists; how it occurs also in non-nucleated, filamentous Algæ like *Vaucheria*, and even in chlorophyll corpuscles—showing clearly that this rudimentary "reproductive process" is

¹ The modern discussions on this subject may be said to have been initiated by Darwin's hypothesis of "Pangensis," published in *Animals and Plants Under Domestication*, 1868, vol. ii., p. 357. An improved form of this rather crude hypothesis has since been put forward by Hugo de Vries, in his *Intrazelluläre Pangenesis*, Jena, 1889.

in them essentially dependent upon the molecular properties of the living matter itself, and that it is not in relation with any visible structure. These are facts only too much disregarded by many of those who base their theories of Heredity upon what goes on in cells, and by others in their attempts to establish a causal relation between some of the processes occurring in an elaborately developed cell while it is dividing, and going through all the mysterious changes included under the term "karyokinesis."

Formerly the nucleus, for instance, was supposed to be the determining cause of this whole process; but since the discovery in 1885, by Van Beneden, of another product of evolution in the cell, whose origin is still more mysterious, namely, the "centrosome," the tendency has been to look upon this minute body as the primary directing agent in the various processes occurring during the division of highly developed cells—the changes in the nucleus and other parts occurring, as it were, at its instigation. Whatever the interpretation of the relation between the several processes may be, no one can study them without being impressed by their truly wonderful nature, and without recognising the complicity of these processes of molecular physics which take place during the life and multiplication of cells—bodies that were formerly, though wrongly, regarded as the simplest of vital units.

What has just been said may give a clue to the reason why there are now two distinct biological schools in reference to the problems connected with Heredity, and those developmental tendencies of living matter that lie at the root of Evolution—tendencies which, with other causes, have eventuated in the production of all the forms of life that now exist, or in past ages have ever appeared, upon the earth. There are those, for instance, who start in their conceptions concerning these processes (a) from a mere *matter* of life, and regard the simplest vital units as composed of more or less complex aggregates

of chemical molecules, whether they have been spoken of as "physiological units" by Herbert Spencer (1863), as "plastidules" by Haeckel (1875), as "micella" by Nägeli (1884), as "pangenes" by de Vries (1889), or as "biogens" by Verworn (1894). There are others, however, who, while professed evolutionists, base their theories upon (b) a particular *form* of life, the nucleus, and make that, which must be a product of Evolution, the starting-point for their theories. These latter views are more especially associated with the name of Weismann (1881-1904) and his gradually developed hypotheses concerning "ids" and "determinants" as constituents of the nucleus.¹

It will be best for us first to say something concerning these views of Weismann. They are, however, so complicated as to make it difficult to give any adequate account of them in a few words. They can count upon very many firm adherents; though others are more struck with the number of unproved assumptions and more or less baseless hypotheses which are most skilfully used with the appearance of explaining this or that problem as it arises. Some of Weismann's fundamental positions, however, are so absolutely hypothetical and incapable of verification, if not actually opposed to existing knowledge, as necessarily to instil the gravest doubts as to the value of his theories. They lead him to attribute to "natural selection," together with his supplementary doctrine of "germinal selection" (as causes of Evolution), an influence much more profound and far-reaching than was ever claimed by Darwin himself: they lead him to deny the evolutionary effects of "use and disuse," and, in other words, the "inheritance of acquired characters"—processes which are regarded by Herbert Spencer, Haeckel, and others,

¹ The latest presentation of his doctrines is to be found in *The Evolution Theory*, 1904, transl., two vols.

as real and very important factors in the process of Evolution.

Weismann postulates the presence in living matter of a special *hereditary substance*, and this substance he believes to exist in the "chromatin granules" which, when aggregated, form the "chromosomes" of the nucleus, as they occur in the germ-cells and sperm-cells, as well as in cells generally. These chromosomes of the nucleus are often band or rod-shaped, more rarely minute and granular, though in the former case the bands or rods are made up of aggregates of granules—the granules in each case being what Weismann calls "ids."¹ The number of "chromosomes" varies much in different kinds of animals and plants, though it "remains the same in every cell-generation throughout development, as it is the same in all the individuals of a species." Thus, according to Weismann, "in some worms there are only two or four; in the grasshopper there are twelve; in the mouse, the trout, and the lily there are twenty-four; in snails thirty-two," while in Man "the chromosomes are so small that their normal number is not certain—sixteen have been counted" (*loc. cit.*, i., p. 291).

After these well-established facts we come into the region of pure hypothesis. The minute granular "ids" are assumed to be composed of a vast number of altogether hypothetical units to which Weismann gives the name of "determinants," though he also speaks of them as "primary constituents." Each "id," of which there are usually very many in each chromosome, is represented to be a "complex of primary constituents necessary to the production of a complete individual," or, as he says a little further on, "as composed of a mass of different kinds of parts each of which bears a relation to a particular part of the perfect animal, and so to some

extent represents its 'primary constituents' (Anlagen), although there may be no resemblance between these 'primary constituents' and the finished parts." There must, he says, be as many of these "primary constituents" or "determinants" "as there are regions in the fully formed organism capable of independent and transmissible variation, including all the stages of development" —even where these latter are so very different as they are found to be in many crustacea and in insects.

Thus, according to these hypotheses of Weismann, each granule of one of the minute chromosomes existing in a nucleus (that is, each "id") must contain all the infinitely numerous entities, known as "determinants," which are assumed to be necessary for the development, let us say, in a butterfly, of the larva, the pupa, and the imago; and in the latter for the myriads of every minute part or scale of the butterfly's wing; since, as he says, "the primitive cell of the butterfly's wing must contain all the determinants for the building up of this complicated part." Divisions of the cells will continue, he adds, "until the determinant architecture of the ids is completely analysed or segregated out, so that each cell ultimately contains only one kind of determinant, the one by which its own peculiar character is determined" (p. 378).

But how, it may be asked, are the determinants always to find themselves in the right place? According to Weismann, each of them "must be guided through the numerous cell-divisions of ontogeny, so that it shall ultimately come to lie in the cell which it is to control. This presupposes that each determinant has from the very beginning its definite position in relation to the rest, and that the germ-plasm, therefore, is not a mere loose aggregate of determinants, but that it possesses a structure, an architecture, in which the individual determinants have each their definite place"—their relative positions depending, as he says, partly upon

¹ This abstract of his views is gathered from Lects. XVII., XVIII., and XIX. of his work *The Evolution Theory*, pp. 345-416.

heredity, and partly upon internal forces or "vital affinities."¹ But if "the determinants must separate from each other in the course of development, so as to penetrate singly into the cells they are to control," the "id" must have the power of dividing into daughter "ids" of identical and also of dissimilar halves. This second mode, or "differential division," is not a process that can be observed. Its existence is one of the numerous assumptions made by Weismann in support of his theories, in order to attempt to account for things otherwise inexplicable. Thus, he says, when "one of two sister cells of the embryo gives rise to the cells of the alimentary canal and the other to those of the skin and the nervous system, I infer that the mother cell divided its nuclear substance in a differential way." Yet what has been proved to occur in the ova of the Sea-urchin, of the Amphioxus, and in those of some other animals, is directly opposed to this supposition, and shows that their blastomeres are identical in composition. It is not surprising, therefore, that this supposition of a "differential nuclear division" has not been favourably received, especially seeing that Weismann has to make a further postulation of the existence of forces or "affinities" within the ids which control and regulate these identical or differential divisions.

Speaking generally, Weismann holds "that the determinants gain entrance into the cells they are to control by a regulated splitting up of the ids into ever smaller groups of determinants, by a gradual analysis or segmentation of the germ-plasm into the idio-plasms of the different ontogenetic stages." But, he says, in accordance with this view, "it must strike us as remarkable that

the chromatin mass of the nucleus does not become notably smaller in the course of ontogeny, and even ultimately sink to invisibility. Determinants lie far below the limits of visibility; and, if there were really only a single determinant to control each cell, there would be no chromatin visible in such a case." But, ever ready to meet such a difficulty, Weismann does not hesitate to make a further postulation. He accordingly assumes "that, in proportion as the number of the *kinds* of determinants lying within a cell diminishes, the number of resting determinants of each kind increases. [Why this should be so is not said.] When, finally, only one kind of determinate is present, there is a whole army of determinants of that kind"; but even then they are assumed to exist in "two different states, at least in regard to their effect upon the cell in which they lie—an active state, in which they control the cell, and a passive state, in which they exert no influence upon the cell, although they multiply." All this, again, is pure hypothesis; and now come further hypotheses, equally devoid of foundation in fact.

We have the determinants gradually "segregated" in the course of development, so that each finds itself, we must not say in different kinds of cells, but rather in cells differently situated, because the determinant itself is supposed to be the main influence in producing the different kinds of cells. The question then arises, however, as to the mode of action of the determinants; and this is what Weismann says:—"It seems to me that the determinants must ultimately break up into the smallest vital elements of which they are composed, the biophors,² and that these migrate through the nuclear membrane into the cell-substance. But there a struggle for food and space must take place between the protoplasmic elements already present and the new-comers,

¹ Though Weismann uses the term "germ-plasm" as that which possesses what he calls "an architecture," it should be observed that here, as in other places, he falls back upon the more imposing term "germ-plasm" when he is really speaking of a mere minute granule—one of his "ids."

² These "biophors" have not been mentioned before, but they are comparable with the elementary vital units of Haeckel (plastidules), or those of Verworn (biogens), referred to on p. 46.

and this gives rise to a more or less marked modification of the cell-structure." It is for this end, therefore, and with a view to the results of this "struggle for food and space," that all the apparatus of determinants exist, and that all the provisions are supposed to be made, with the aid of special "vital affinities," for the proper "architecture" of the granular ids, and for the fitting "segregation" of determinants throughout all the stages of development of the particular animal or plant!

We might go on enumerating several other requirements that would have to be taken into account, relating, for instance, to double sexual determinants; to the requirements in the case of the dimorphism of larvæ, and the polymorphism often existing in such social insects as ants and bees (see *loc. cit.*, vol. i., p. 390). Then, again, as each id is presumed to consist of a complex of determinants necessary for the production of a complete individual—as there are many ids in each chromosome, and many chromosomes in each nucleus—it follows that there must be a whole host of determinants which would have to find their way into each cell during development, so that very special "forces" would need to be called into operation to make the multitude lie low while some chosen determinant (through the "struggles" of its liberated biophors) was impressing its special characters upon the previously indifferent embryonal cell. And, looking to this work that the biophors are supposed to perform, it seems clear that there must be a further complication: they must be endowed with mysterious specific qualities. Thus Weismann says:—"In any case, the biophors which transform the general character of the embryonic cells into the specific character of a particular tissue-cell must themselves possess a specific structure different from that of other biophors, for they must keep up the continuity of structures handed on from ancestors—chlorophyll, and muscle-substance, and the like."

To be asked to accept anything of this kind as a rational interpretation of what occurs in the development of an individual must be felt by many to make serious demands upon their faith; and this strain may give place to actual incredulity when they learn that this whole agglomeration of assumptions and hypotheses has been built up upon a doctrine which is itself, to say the least, devoid of any firm foundation in fact, and may fairly enough be said to be strongly opposed to many well-known facts—I allude to the author's celebrated and fundamental doctrine as to "the continuity of germ-plasm," and the "immortality of germ-cells," which he has advocated since 1885.

He would have us believe that the ordinary cells of the body—the "somatic cells"—are always quite distinct from the "reproductive cells"; that there is a strict continuity of the latter from one generation to another, and that they never take origin afresh, in the course of an individual's development, as liver cells, kidney cells, or brain cells, are known to do.

But what are the facts? It is a matter of common knowledge, and admitted by Weismann himself, that the fertilised ovum segments into hundreds, "thousands, or even millions of cells" before, in some animals, the primary germ-cell appears in the rudiments of the ovary.² And, speaking of this latter, he says in his last work (*The Evolution Theory*, i., p. 300):—"We find that it consists of a single primitive egg-cell, from which, by division, all the other egg-cells arise. In the same way the first rudiment in the testis or spermary is formed by a primitive sperm-cell, which does not differ visibly from the primitive egg-cell. Both now multiply by division for a considerable time." Where, then, it may be asked, is the alleged continuity of the germ-plasm? Only another "assumption" is needed to bring this about.

Thus, further on (p. 410), he says the

² *The Germi-Plasm*, 1895, p. 193.

"primordial germ-cell" makes its appearance "usually during embryogenesis, and often very early in it, after the first few divisions of the ovum, but sometimes not till long after the end of embryogenesis; and not even in the individual which arises from the ovum, but in descendants which arise from it by budding. Here the primordial germ-cell is separated from the ovum by a long series of cell-generations, and the sole possibility of explaining the presence of germ-plasm in this primordial cell is to be found in the *assumption*² that in the divisions of the ovum the whole of the germ-plasm originally contained in it was not broken up into determinant groups, but that a part, perhaps the greater part, was handed on in a latent state from cell to cell, till sooner or later it reached a cell which it stamped [why that rather than any previous cell?] as the primordial germ-cell. Theoretically it makes no difference whether these 'germ-tracks'—that is, the cell-generations which lead from the ovum to the primordial germ-cell—are short or very long, whether they consist of three or six or sixteen cells, or of hundreds and thousands of cells."

For the reality of the existence of such "germ-tracks," not a tittle of evidence is given, or even attempted. Weismann's ever-ready assumptions cannot be accepted as proofs, so that many of us can only regard his doctrine of the "continuity of the germ-plasm" as one which is entirely opposed to all positive evidence; and can only further regard the superstructure he has built upon it (as to the function of the granular "ids" and all their imaginary myriads of "determinants," composed in their turn of an endless variety of "biophors") as based upon an extraordinary aggregate of assumptions and hypotheses, destined to support his particular views concerning heredity and evolution, but devoid of any independent support. His knowledge is vast, and his absolute sincerity is undoubted; but

his theories seem to have so thoroughly taken possession of him that he is unable to resist attempting to explain in detail things which it were wiser to regard as at present inexplicable.

Let us get back now a little more into the region of facts and of verifiable propositions in regard to the all-important question as to the functions of the nucleus and its relation to hereditary phenomena. The interest here undoubtedly centres in the question of the function that is to be attributed to the chromatin granules (ids) which, when aggregated, go to form the chromosomes.

An important point not yet referred to is this. The chromatin is altogether differently arranged in different states of the cells. In what has been termed the "resting stage," but which, as Herbert Spencer says, would be better termed its "growing stage," the chromatin is irregularly dispersed throughout the nucleus in the form of a granular network; and it is only when the time comes for division of the cell to occur that, as one of the first of the very complicated processes about to ensue, the chromatin granules begin to aggregate so as to form the particular number of band or rod-like "chromosomes" peculiar to the cells of the plant or animal to which they belong.

Subsequently, as it has been undoubtedly proved, each chromosome, in the process of division, becomes split longitudinally into two equal portions; the halves thus formed gradually separating from one another, and the whole of each set later on becoming aggregated so as to form one of the two daughter nuclei which now find themselves in the divided cytoplasm of the original cell. At this stage the substance of the chromosomes again becomes distributed (by the separation of their component ids) so as to re-form the nuclear network of chromatin granules, characterising the cell during its growing stage. It is commonly believed that the chromatin of each chromosome preserves its identity during this time, so that when the next

² No italics in original.

division of the cell occurs the chromosomes are re-formed from exactly the same chromatin granules as before. These phenomena of karyokinesis are well shown in Fig. 74 of Weismann's work *The Evolution Theory*, while the modification of the process that occurs in fertilised eggs is equally well shown in Fig. 75 of that work.

If attention is concentrated upon this latter process, it may be easily understood that the fusion of the male nucleus with that of the female nucleus should have given rise to the inference that these two nuclei convey respectively the paternal and maternal traits which are mingled in the offspring. And when, as Herbert Spencer says,¹ "there came to be discerned the karyokinesis by which the chromatin is, during cell-fission, exactly halved between the nuclei of the daughter-cells, the conclusion was drawn that the chromatin is more especially the agent of inheritance." There is, however, much cogency in what follows when he adds:—"But though, taken by themselves, the phenomena of fertilisation seem to warrant this inference, the inference does not seem congruous with the phenomena of ordinary cell-multiplication—phenomena which have nothing to do with fertilisation and the transmission of hereditary characters. No explanation is yielded by the fact that ordinary cell-multiplication exhibits an elaborate process for exact halving of the chromatin. Why should this substance be so carefully portioned out among the cells of tissues which are not even remotely concerned with the propagation of the species? If it be said that the end achieved is the conveyance of paternal and maternal qualities in equal degree to every tissue, then the reply is that they do not seem to be conveyed in equal degrees. In the offspring there is not a uniform diffusion of the two sets of traits throughout all parts, but an irregular mixture of traits of the one with traits of the other."

H. Spencer then proceeds to make a suggestion which is in harmony with these facts, and well worthy of careful consideration. He calls attention to the chemical composition of chromatin, and its greater complexity compared with that of the general substance of the cell, seeing that analyses have shown it to contain an organic acid (nucleic) rich in phosphorus, combined with an albuminous substance; probably a combination of various proteids. This nuclear substance, therefore, has a special instability and tendency to undergo molecular change, such as nucleic acid has been found to possess, and its presence in the cell may lead to the following results: "Molecular agitation results from decomposition of each phosphorised molecule; shocks are continually propagated around. From the chromatin, units of which are thus ever falling into stabler states, there are ever being diffused waves of molecular motion, setting up molecular changes in the cytoplasm. The chromatin stands towards the other contents of the cell in the same relation that a nerve-element stands to any element of an organism which it excites." The distribution of the chromatin so as to form a diffuse network of granules during the growing stage of the cells is in a measure explained on this hypothesis, though from the point of view of the inheritance theory it would seem to be useless. This, among other facts harmonious with his suggestion, is pointed out by Spencer when he says:—"During the interval between cell-fissions, when growth and the usual cell-activities are being carried on, the chromatin is dispersed throughout the nucleus into an irregular network, thus greatly increasing the surface of contact between its substance and the substance in which it is embedded. As has been remarked, this wide distribution favours metabolism." So that, as he says, when contemplated from the suggested point of view, karyokinesis as it occurs in the cells of almost all tissues is not wholly incomprehensible:—"For if the chromatin yields the

¹ *Principles of Biology*, 1898, vol. I, p. 259.

energy which initiates changes throughout the rest of the cell, we may see why there eventually arises a process for exact halving of the chromatin in a mother-cell between two daughter-cells."² The consequences of an unequal division in cells generally are then pointed out, and also the fact that the foregoing interpretation does not attempt to explain the special process known as karyokinesis: the argument implies merely that the tendency of Evolution is to establish *some* mode for the equal division of the chromatin.

An important fact is referred to by Verworn² which tends to support H. Spencer's view as to the stimulating functions of the nucleus. He calls attention to the existence in some cells of complicated branched nuclei, and then adds, quite irrespectively of any theory:—"It seems noteworthy that it is the nuclei of secreting cells—*i.e.*, cells characterised by lively activity in which the principle of surface enlargement by branching is especially expressed."

There seems to be no definite way for reconciling this view as to the function of chromatin with the other view more commonly received, that it is the bearer of hereditary traits; though it may be said that this second function is not necessarily in conflict with the first. "While the unstable units of chromatin, ever undergoing changes, diffuse energy around, they may also be units which, *under the conditions furnished by fertilisation*, gravitate towards the organisation of the species. Possibly it may be that the complex combination of pro-

teids, *common to chromatin and cytoplasm*, is the part in which the constitutional characters inhere; while the phosphorised component, falling from its unstable union and decomposing, evolves the energy which, ordinarily the cause of changes, now excites the more active changes following fertilisation."

We are here, indeed, introduced to a question which some of the most recent experiments on "merotomy" have again brought forward—namely, whether experimental evidence does not show that hereditary traits may inhere in the cytoplasm as well as in the nucleus. Thus, in the third of an important short course of lectures delivered by the late Professor Weldón at University College, he is reported to have said:¹ "The nucleus has been held to be the sole vehicle of heredity by Weismann. Recent experiments of Boveri and Delage have demonstrated that this is incorrect. Both these writers have shown that denucleated eggs, produced by mechanical extrusion of nuclei, can be fertilised in the sea-urchin, and that they give rise to normal blastulæ, gastrulæ, and larvæ. Hence the 'determinants' of heredity are present in the cell body of the egg. If such eggs are fertilised by alien spermatozoa (of different sub-species) the larva shows the characters of the male parent, because the determinants of these are more potent."

It seems, however, that in these experiments there is no positive evidence that the fragment of denucleated egg has communicated any of its female traits to the bastard offspring, although it has served as a matrix in, and with the aid of which (as mere nutritive material), the spermatozoon has been capable of giving rise to a larva of its own type. This must be regarded as a truly wonderful result, of a kind never before known; though it seems to be the converse of a hitherto familiar class of facts, in which the eggs of certain animals are capable of going through all

² This view as to the functions of the nucleus seems capable of explaining the facts previously mentioned concerning sections of cells, and the regeneration only of those parts in which the nucleus was contained (p. 28); and also the results of Balbiani's experiments with Stentor, showing that the presence of only a small portion of the nucleus in a section of this organism was sufficient to admit of its complete regeneration (p. 29), leading le Dantec to the conclusion that it seemed to act like a chemical compound rather than as an organ.

¹ *General Physiology*, transla. 1899, p. 89.

² *The Lancet*, February 4th, 1905, p. 308.

their phases of development altogether independently of contact and fusion with a spermatozoon.

This so-called "parthenogenesis" or "virgin reproduction" is now well known to be by no means a rare phenomenon. As Weismann says:—"It occurs regularly and normally in many cases, especially in the very diverse groups of the great series of Arthropoda. Thus, among insects it is found in certain saw-flies, gall-flies, ichneumon-flies, in the honey bee, and in common wasps, and it is particularly widespread among plant-lice (Aphides) such as the vine-aphis (*Phylloxera*), whose prodigious multiplication in a short time depends partly on the fact that all the generations, with the exception of one, consist only of females with a parthogenetic mode of reproduction. Among the lower Crustaceans, also, parthenogenesis plays a large rôle, and in many species it even occurs as the sole mode of reproduction, but more often—as is also the case among insects—it occurs alternately with bi-sexual reproduction" (*loc. cit.*, p. 303).

There is a very interesting point in connection with this occurrence of "parthenogenesis" and its relation to what occurs in ordinary bi-sexual reproduction. It is well known that the egg-nucleus, previous to fertilisation, undergoes what are known as "reducing divisions" (leading to the extrusion of the two so-called "polar bodies"), the effect of which is to leave the egg-nucleus with only half its usual number of chromosomes. A reducing division occurs also during the formation of spermatozoa, though at an earlier stage—the result being that the nucleus of the spermatozoon, as it enters the egg, also has only half the number of chromosomes peculiar to the species. The fusion of the two nuclei during the act of fœcundation restores to the egg its normal number of these bodies.

It becomes a matter of much interest to ascertain, therefore, what occurs when parthenogenesis takes place. Do the

"reducing" divisions occur as usual in the egg which develops without male influence, or does such an egg retain its full number of chromosomes? This interesting point has been settled by the researches of Weismann and others for some animals, and he is thus enabled to say that "those eggs of Aphides and Daphnids which are wholly parthogenetic retain the full number of chromosomes of their species." Why, in different species, there should be different numbers of chromosomes; and why in a developing egg, in one way or another, it seems necessary that the full number of chromosomes should always be produced, still remain absolute enigmas; though a similar confession has to be made in regard to many of the phenomena of karyokinesis generally.

In this connection other facts of extreme interest must be mentioned. In some silk-moths and hawk-moths, and in quite a number of other Lepidoptera, it sometimes happens that out of a large number of unfertilised eggs a few will develop and produce caterpillars. But, as Weismann says, these facts gain "increased importance through the investigations of the Russian naturalist, Tichomiroff, who succeeded in considerably increasing the number of unfertilised eggs that developed by gently rubbing them with a paint-brush, or by dipping them for a little in dilute sulphuric acid. It is thus possible to make eggs which would not ordinarily develop without being fertilised, capable of parthogenetic development by means of a mechanical or chemical stimulus. This sounds almost incredible; but it is beyond a doubt, and it is still further corroborated by the fact that Professor Jacques Loeb has succeeded in inciting the eggs of a sea-urchin to parthogenetic development by means of a chemical stimulus. When he added to the sea water in which the eggs were laid a certain quantity of chloride of magnesium the ova developed, and not only went through the process of

segmentation, but even reached a definite stage of development.¹

In both these latter sets of experiments it is to be presumed that the eggs had undergone the usual reducing division, and that the number of chromosomes had been reduced to half their usual number; it is extremely interesting to find, therefore, that physical or certain common chemical stimuli were in these cases capable of replacing the stimulation usually induced by (the chemical and molecular) changes emanating from spermatozoa. Then, again, there can be no doubt that in the "merotomy" experiments in which de-nucleated eggs of the sea-urchin were fertilised by the spermatozoon of another sub-species we have a related, though opposite, form of "unisexual" development taking place, notwithstanding the presence of only one-half of the ordinary number of chromosomes belonging to the species—viz., those of the spermatozoan nucleus. This latter case is, however, far more remarkable than the other variety of "unisexual" development, because of the minute size of the spermatozoon as compared with that of the ovum.

Facts very similar to those we have just been recording are known also to occur in the vegetable kingdom, and especially among ferns. But what has not hitherto been suspected is the fact that the changes witnessed in the development of cancerous tissue are identical with those occurring in the normal sexual reduction of the tissues both of animals and plants. The importance of this fact is greatly enhanced when we find Dr. Bashford, the Director of the Imperial Cancer

¹ According to Weldon (*The Lancet*, Feb. 4th, 1905), Loeh has since succeeded in bringing about similar results "by the presence of a large quantity of carbon dioxide in the sea water"—experiments which have been confirmed by Delage. The latter investigator has also recorded other remarkable results (*Compt. Rend.*, May 22nd, 1905, p. 1369) with a solution of chloride of manganese in distilled water, and as well as with a solution of monobasic phosphate of soda,

Research, saying:—"Cancer arises *de novo* in each individual attacked, by a transformation of healthy tissue; one case of cancer having no relation to any other. This general conclusion is based upon observations and experiments of very varied but confirmatory nature."

This change of certain connective tissue cells into cells of cancer, representing an incipient conversion of them into the forms of germ-cells, must be regarded as decidedly adverse to the views of Weismann as to the absolute distinction between the "somatic-cells" and the germ-cells and the "continuity" of the latter. His views, moreover, seem negated by multitudes of well-established facts concerning budding, "regeneration," and repair of injuries generally, to which we must now turn our attention.

Our knowledge concerning these processes in various animals has been greatly increased during recent years, though the power of propagating themselves by buds and cuttings possessed by all kinds of plants has long been thoroughly familiar. In some Algæ, such as *Caulerpa*, in Liverworts, and in Bryophyllum, one of the House-leeks, this power is possessed to an extreme degree, so that even minute fragments of such plants are capable of growing and developing into perfect plants. While, as we are told by Dr. (now Sir William) Hooker,² that there is "a species of *Begonia* the stalks, leaves, and other parts of which are superficially studded with loosely-attached cellular bodies," and that "any one of these bodies, if placed under favourable conditions, will produce a perfect plant similar to its parent."

In animals this power of repair is by no means so widespread—which is only what might be anticipated, having regard to the increasing complexity of organisation met with in the higher types, and to

² *Nature*, vol. lxxix., p. 263.

³ Report of Brit. Association, 1868,

the fact that they are not mere repetitions of more or less similar parts, such as obtains so largely among plants. Among lower animal organisms, however, we find the property extremely common and very remarkable. Beginning with "unicellular organisms,"¹ it is seen also among different kinds of Polyps, Medusæ, Starfishes, Planariæ, and other worms (though not among Free Nematoids); till, when we come to Crustacea, Insects, and Amphibia the power of reproduction after injury dwindles down, and is confined in the main to a power of regenerating lost limbs or appendages, and in Mammalia only to a power of repairing wounds. *

In some of these lower organisms the power of repair is just as great as it is among some plants, as Weismann himself fully admits. Thus, concerning one of the common Polyps, he says:—"Not only has Hydra been cut in from two to twenty different pieces, but it has even been chopped up into innumerable fragments, and yet each of these, under favourable circumstances, was able to grow again into a complete animal." Then, concerning certain common worms, Planariæ, he says:—"Through the experiments of Loeb, Morgan, Voigt, Bickford, and others, we know that these animals respond to almost every mutilation by complete reconstruction; that they may, for instance, as is indicated in Fig. 96, be cut transversely into nine or ten pieces, with the result that each of these pieces grows again to a whole animal, unless external influences are unfavourable and prevent it." While in regard to another little worm found in ponds, Lumbriculus, he adds:—"Certainly the power of regeneration is so great in this animal that it is out of the question to talk of localising the primary constituents of regeneration; almost every broken surface is capable of regeneration."²

¹ See the previously cited Experiments of Balbiani with Stentor (p. 28) and the results of the section of cells generally.

² *Loc. cit.*, vol. ii., pp. 4, 6, 16.

In his two lectures on "Regeneration" Weismann makes an ingenious but laboured attempt to bring all this class of facts into harmony with his views as to the "Continuity of the Germ-Plasm," and the all-powerful influence, even in lowest organisms, of "natural selection." Thus he says:—"My own view is that the regenerative capacity is not something primary, but rather an adaptation to the organism's susceptibility to injury—that is, a power which occurs in organisms in varying degrees, proportionate to the degree and frequency of their liability to injury. Regeneration prevents the injured animal from perishing, or from living on in a mutilated state; and in this lies an advantage for the maintenance of the species." But even in accordance with this view, that the regenerative capacity is a secondary and acquired, rather than a primary, property of the living matter of the organisms possessing it, he is obliged to postulate the existence in all kinds of cells, in the animals in question, of "inactive accessory idioplasm"—that is, of hosts of his determinants, usually latent, but capable of being roused into activity, as this or that accident may require; while, as regards plants, he is compelled to assume that "cells must be distributed over the whole surface of the plant, each of which can in certain circumstances become the starting-point of a bud. That is to say, each must contain, *in a latent state, the complete germ-plasm which is necessary for the production of an entire plant.*" The way in which all these vast assortments of determinants would need to be distributed for possible use, in addition to all those which are required by Weismann's theories for actual use, afford problems quite bewildering in their complexity.

Let us turn now to an opposite and much simpler mode of accounting for all these facts concerning budding, regeneration, and repair which was published by Herbert Spencer, in his *Principles of*

Biology, so long ago as 1863; and which was still maintained by him in a celebrated controversy with Weismann himself in the pages of the *Contemporary Review* for 1893-4.

After referring to some of the facts concerning "regeneration" known at the time, H. Spencer says:—"We must infer that a plant or animal of any species is made up of special units, in all of which there dwells the intrinsic aptitude to aggregate into the form of that species: just as in the atoms of a salt there dwells the intrinsic aptitude to crystallise in a particular way. It seems difficult to conceive that this can be so; but we see that it *is* so. Groups of units taken from an organism (providing they are of a certain bulk and not much differentiated into special structures) *have* this power of re-arranging themselves; and we are thus compelled to recognise the tendency to assume the specific form as inherent in all parts of the organism. Manifestly, too, if we are thus to interpret the reproduction of the organism from one of its amorphous fragments, we must thus interpret the reproduction of any minor portion of any organism by the remainder.....In the one case as in the other, the vitalised molecules composing the tissues show their proclivity towards a particular arrangement; and whether such proclivity is exhibited in reproducing the entire form, or in completing it when rendered imperfect, matters not."

He then goes on to say:—"For this property there is no fit term. If we accept the word 'polarity' as a name for the force by which inorganic units [as in crystals] are aggregated into a form peculiar to them, we may apply this name to the analogous force displayed by organic units. But, as above admitted, polarity as ascribed to atoms is but a name for something of which we are ignorant—a name for a hypothetical property which as much needs explanation as that which it is used to explain. Nevertheless, in default of another word, we must employ this;

taking care, however, to restrict its meaning. If we simply substitute the term 'polarity' for the circuitous expression, the power which certain units have of arranging themselves into a special form, we may, without assuming anything more than is proved, use the term 'organic polarity' or 'polarity of the organic units' to signify the proximate cause of the ability which organisms display of reproducing lost parts, or of their having assumed the shape and structure which is peculiar to them."

Inquiring next into the possible nature of these units "which possess the property of arranging themselves into the special structure of the organisms to which they belong," he comes to the conclusion that they cannot be any of the complex chemical compounds entering into the composition of living matter. On such a supposition the existence of endlessly varied forms of animals and plants would be inexplicable; so that he dismisses the notion of their being "chemical units." He similarly dismisses the notion that the powers in question can reside in "morphological units" or cells, as, independently of other reasons, "the formation of a cell is to some extent a manifestation of this same peculiar power."¹

He thereupon goes on to say:—"If, then, this organic polarity can be possessed neither by the chemical units nor the morphological units, we must conceive it as possessed by certain intermediate units, which we may term *physiological*. There seems no alternative but to suppose that the chemical units combine into units immensely more complex than themselves, complex as they are; and that in each organism the physiological units produced by this further compounding of highly compound atoms have a more or less distinctive character. We must conclude that in each case some slight difference of com-

¹ *Loc. cit.*, vol. i., § 65. The old numbering of the sections is preserved in the new edition of 1898.

position in these units, leading to some slight difference in their mutual play of forces, produces a difference in the form which the aggregate of them assumes."

These "physiological units" of Herbert Spencer would, therefore, correspond almost exactly with the elementary vital units of Haeckel (plastidules) and of Verworn (biogens)—the existence of which was postulated by them at later periods.

After a careful consideration of all the facts known at the time, what may be termed a "Law of Heredity" was formulated by H. Spencer, in which he says¹:—"Bringing the question to its ultimate and simplest form, we may say that, as on the one hand physiological units will, because of their special polarities, build themselves into an organism of a special structure; so, on the other hand, if the structure of this organism is modified by modified function, it will impress some corresponding modification on the structure and polarities of its units. The units and the aggregate must act and react on each other. The forces exercised by each unit on the aggregate and by the aggregate on each unit must ever tend towards a balance. If nothing prevents, the units will mould the aggregate into a form in equilibrium with their pre-existing polarities. If, contrariwise, the aggregate is made by incident actions to take a new form, its forces must tend to remould the units into harmony with this new form....."

"The assumption to which we seem driven by the *ensemble* of the evidence is that sperm-cells and germ-cells are essentially nothing more than vehicles in which are contained small groups of the physiological units in a fit state for obeying their proclivity towards the structural arrangement of the species they belong to..... We must conclude that the likeness of any organism to either parent is conveyed by the special tendencies of the physiological units derived from that parent. In the fertilised germ we have

two groups of physiological units, slightly different in their structures. These slightly different units severally multiply at the expense of the nutriment supplied to the unfolding germ, each kind moulding the nutriment into its own type. Throughout the process of evolution the two kinds of units mainly agreeing in their polarities and in the form which they tend to build themselves into, but having minor differences, work in unison to produce an organism of the species from which they are derived, but work in antagonism to produce copies of their respective parent organisms; and hence ultimately results an organism in which traits of the one are mixed with the traits of the other."²

Professor Weldon, in his lectures delivered in 1905 on "Current Theories of the Hereditary Process," also came to the conclusion that Weismann's theories concerning the "sorting of determinants" are "quite irreconcilable with the facts of the equipotential nature of the blastomeres in the early stages of cell-division [of the ovum] and with the facts of regeneration of lost parts." He adds:—"The changes which subsequently occur in the different cell lineages derived from the fertilised germ-cells are largely due to other factors—viz., the relative positions assumed with regard to each other and to the cell mass by individual cells, and the differences of the incidental stimuli of the environment on differently placed cells (gravity, light and shade, pressure, relative food supply)..... Hence for normal development and perfect embryogenesis a very constant and 'correct' environment is necessary."

¹ The degrees and modes in which the several ancestral traits are mixed are now being keenly investigated and discussed by two rival schools—one of which favours the views of Francis Galton, and the other the newly-discovered Mendelian hypothesis, which is advocated by Hugo de Vries and Bateson. The views of Galton are favoured by Weldon and Karl Pearson. He believes that the two parents contribute between them half of the total heritage, the four grandparents one-quarter, and the eight great grandparents one-eighth of the total heritage, and so on (see Weldon, *Lancet*, March 25th, 1905, p. 810).

¹ *Principles of Biology*, vol. i., § 84.

Weismann, indeed, admits that if the power of repair and regeneration is dependent upon a primary property rather than upon one that has been acquired through "natural selection," he would have to give up his position, since he says (vol. ii., p. 19):—"In truth, if the body were really able to replace, after artificial injury, parts which are

never liable to injury in natural conditions, and to do so in a most beautiful and appropriate manner, then there would be nothing for it but at least to regard the faculty of regeneration as a primary power of living creatures, and to think of the organism as like a crystal, which invariably completes itself if it be damaged in any part."

CHAPTER VI.

SOME FACTORS OF EVOLUTION: ORGANIC POLARITY AND MUTATION

We have seen how great are the tendencies to molecular changes in all kinds of bodies, and how from its very nature and the great complexity of the molecules entering into its composition these tendencies ought to attain their maximum in living matter—that is, in protoplasm, the physical basis of life. We have seen how, in simpler forms of matter of the crystalloid type, changes in molecular composition are often comparatively easily brought about under the influence of changing conditions, with the result that new crystalline forms as well as other new physical properties are assumed by the matter in question as a consequence of the altered polarities of their molecules. The same kind of thing should hold good, therefore, for the different varieties of living matter. This is frankly admitted by Herbert Spencer, who says¹:—"These truths are not limited to inorganic matter: they unquestionably hold of organic matter. As certainly as molecules of alum have a form of equilibrium, the octahedron, into which they fall when the tempera-

ture of their solvent allows them to aggregate, so certainly must organic molecules of each kind, no matter how complex, have a form of equilibrium in which, when they aggregate, their complex forces are balanced—a form far less rigid and definite, for the reason that they have far less definite polarities, are far more unstable, and have their tendencies more easily modified by environing conditions."

This statement, and many others of a similar nature in the work of Herbert Spencer, is in marked contrast with what is to be met in the last and most elaborate work of Professor Weismann, *The Evolution Theory*, which I have very carefully studied. On only one single page (vol. ii., p. 152) have I found any mention of the word "isomerism"; while there have been no references to the possibility of variations in forms of organisms being due to variations in the molecular composition of living matter; the author dwells rather upon "nutritive" variations affecting his "determinants" in the germ-plasm. Indeed, he even goes so far as to say (vol. ii., p. 204):—"In my opinion, at all events, there is

¹ *Principles of Biology*, vol. i., Appendix D.

no such thing as a 'tendency' of the protoplasm to vary."

It so happens, moreover, that none of the prominent writers on the Evolution theory seem to have devoted much consideration to the question of the extreme variability of the lower forms of life; and what is still more surprising is the fact that none of them seem to appreciate how incompatible is the existence of myriads of the lowest forms of life at the present day with the commonly accepted belief that all the forms of life that have ever lived upon the earth have gradually developed, through endless generations, from primordial forms first appearing upon its surface when this became cool enough for the existence of living things. All this astounding evolution has, as they assume, taken place by virtue of the plasticity of living matter in co-operation with multitudes of ever-varying external factors; and yet, notwithstanding its extreme molecular complexity, we find Weismann, as one of the high priests of Evolution, actually doubting whether there is a tendency in protoplasm to vary. How utterly contradictory this seems to be.

Then, again, biologists either ignore or try to explain away the cogency of the evidence pointing to the occurrence of Archebiosis and Heterogenesis, though such occurrences seem alone capable of reconciling facts and theories. Certainly, on the assumption of a natural origin of living matter occurring only in a remote geological past, no satisfactory account can be given of the present existence of swarms of lowest, highly variable, living organisms which would be at all in accord with the doctrine of Evolution.

Even Herbert Spencer for once seems to have yielded to the voice of authority, emanating from his friend Huxley, and to have been persuaded that no positive evidence could be or had been obtained by means of experiment demonstrating the *de novo* origin of living matter in the present day. And, no good evidence of Heterogenesis having been forthcoming at the time that he wrote, he remained

also unconvinced as to the reality of its occurrence. As a result, he never attempted to grapple with this striking contradiction between facts and theory to which I have just been referring.

On the supposition that all existing forms of life were, in some way, descendants of forms which had, as he fully believed, come into being *de novo*, when the surface of the earth had cooled sufficiently to permit of the material combinations needful for their origin, he must have seen himself confronted by a great difficulty. Under these circumstances, he never ventured to apply his doctrine of "organic polarity" so as to attempt to account thereby for the forms assumed by the lowest types of life. Thus, speaking of this supposed newborn living matter in the remote past, and assuming that it would have appeared as portions of protoplasm more minute and more indefinite than the lowest known form of *Amœbæ*, he says:—"The evolution of specific shapes must, like all other organic evolution, have resulted from the actions and reactions between such incipient types and their environments, and the continued survival of those which happened to have specialities best fitted to the specialities of their environment. To reach by this process the comparatively well-specialised forms of ordinary Infusoria must, I conceive, have taken an enormous period of time." He was, in 1866, even more positive in rejecting other phenomena, which, as I shall hope to show in Chapter X., and have shown far more fully in my *Studies in Heterogenesis*, occur with extreme frequency. Thus he says:—"Not only the established truths of Biology, but the established truths of science in general, negative the supposition that organisms, having structures definite enough to identify them as belonging to known genera and species, can be produced *in the absence of germs derived from antecedent organisms of the same species.*"²

² *Loc. cit.*, vol. i., Appendix D (p. 698). This was written about the year 1866.

Later on I shall claim to show that this which was looked upon as so utterly improbable by Herbert Spencer is, nevertheless, constantly occurring all around us, and that the products of such processes are continually appearing and giving rise to the myriads of those lowest forms of life whose present existence would otherwise be so impossible for the Evolutionist consistently to explain.

Nay, more; I shall claim that such processes can only receive their explanation by reference to the innate tendency of living matter to undergo isomeric changes (such as Spencer freely admits and Weismann seems disposed to deny), coupled with Spencer's doctrines of "organic polarity" and "physiological units."

The sentence quoted from Herbert Spencer near the commencement of this chapter shows plainly enough what he wished us to understand by "organic polarity." If the molecules of alum during the process of crystallisation had, under altered conditions, undergone some isomeric change, they would no longer have been able to group themselves into the old form of the octahedron; a new mode of equilibrium among their molecules would have resulted in a new crystalline form, and other new properties, as in other cases previously quoted (pp. 31, 32). But, as applied to lowest forms of living matter, the doctrine of "physiological units" and of "organic polarity" means surely that the forms of low organisms may be direct results of the attempts to establish a moving equilibrium of the particular units entering into their composition. As with the crystals, let us suppose that, under the influence of some new conditions, isomeric changes have been induced in the molecular constitution of the pre-existing physiological units of some simple organism, this should, in accordance with Spencer's doctrine, result in the appearance of a new organic form in some brief period, and not, as he strangely enough suggests, in the passage previously quoted, gradually and during a

succession of generations, extending over "an enormous period of time."

This latter cannot be the legitimate interpretation of his doctrine, as he himself shows in a later page (p. 705), when he says, in reference to his hypothesis of physiological units:—"In its complete form, then, the conception is that these specific molecules, having the immense complexity above described, and having correspondently complex polarities which cannot be mutually balanced by any simple form of aggregation, have, for the form of aggregation in which all their forces are equilibrated, the structure of the adult organism to which they belong, and that they are compelled to fall into this structure by the co-operation of the environing forces acting on them, and the forces they exercise on one another—the environing forces being the source of the *power* which effects the re-arrangement, and the polarities of the molecules determining the *direction* into which that power is turned."

Here, then, is one of the Factors of Evolution, of which no account is taken by Professor Weismann, but which my observations on Heterogenesis compel me to regard as *the* all-powerful cause of Evolution among the lowest kinds of living things. How far up in the scale of life it remains an all-powerful cause must for the present remain unknown. Certainly, my observations compel me to believe that organisms as high as Ciliated Infusoria very frequently derive their form as a direct outcome of molecular changes in masses of living matter derived from wholly different sources, as I have shown elsewhere (*Studies in Heterogenesis*). This process of organic polarity is probably all-powerful also among all kinds of Protophyta and Protozoa, in many of which the wonderful symmetry of their forms and markings is of itself strongly suggestive of the determining influence of polarities, issuing in the production of a kind of organic crystallisation. Among Protophyta look, for instance, at the forms of Desmids; at the forms and surface patterns of

Diatoms; and, among other Algæ, at the extremely symmetrical patterns or modes of grouping of the cells in such forms as Coleochæte, Pediastrum, and Volvox; while among the Protozoa evidence of the same kind of forces having been at work is to be found in the single-chambered shells of the Arcellinæ, and still more plainly in the beautiful forms of Foraminifera, and of the Radiolaria. Speaking of the latter creatures, Haeckel says:—"All the various fundamental forms that can be distinguished and defined mathematically are found realised in the graceful flinty skeletons of these unicellular sea-dwelling protozoa. I have distinguished more than 4,000 forms of them, illustrated by 140 plates, in my monograph on the Challenger-radiolaria."

No reasonable person can suppose that any distinct advantage could accrue to the possessors from these infinitely varied shapes and surface patterns. And, if so, the cause of their origin must be something altogether outside the pale of Natural Selection, upon which Weismann doubtless would wish us to rely here, as in all other respects.

But this same cause, which may be all-powerful in determining the actual shapes and superficial markings of the lowest forms of life, is one that cannot fail to be operative as a contributing morphological cause in altogether higher forms of life, and thus in part to account for the symmetry displayed in the forms of plants and animals generally, and especially for the radial or bilateral symmetry of the latter. For, as Bateson points out,¹ symmetry is "a character whose presence among organisms approaches to universality." The existence of patterns, he adds, "again and again recur, and again and again the question of their significance goes unanswered." It is not only in regard to "large and tangible structures that the question arises, for the same challenge is presented

in the most minute and seemingly trifling details." But may not a good clue as to the meaning of all this be fairly said to exist in the abiding influence of "organic polarity"?

In considering the possible influence of "physiological units" and "organic polarity" in determining the forms of living things, we must not restrict our consideration to simple cases in which the egg-substance is sufficient in mass to enable the physiological units, without the aid of additional material, to undergo equilibration, and thus to permit the specific forms of the organisms being to a considerable extent assumed within the egg-envelopes—as happens, for instance, in the development of Rotifers and Nematodes.

Much more complicated cases have also to be considered, which are thus referred to by Herbert Spencer (*loc. cit.*, p. 706):—"But among higher animals such direct transformations cannot happen. The mass of physiological units required to produce the size as well as the structure that approximately equilibrates them is not all present, but has to be formed by successive additions—additions which in viviparous animals are made by absorbing and transforming into these special molecules the organisable materials directly supplied by the parent, and which in oviparous animals are made by doing the like with the organisable materials in the 'food-yolk,' deposited by the parent in the same envelope with the germ..... Thus there is necessitated a series of compromises. Each successive form assumed is unstable and transitional: approach to the typical structure going on hand in hand with approach to the typical bulk."

This may seem very complicated; but that appears to be a condition inseparable from all attempts to explain organisation and heredity. What has to be steadily borne in mind is the fact that this hypothesis is quite in accord with all the known data as to regeneration and repair in animals; as well as with the

¹ *The Wonders of Life*, 1904, p. 178.

² *Materials for the Study of Variation*, 1894, p. 22.

phenomena of reproduction by buds, cuttings, and superficial scales in plants, to which reference has previously been made (pp. 54, 55); while Weismann's counter hypothesis concerning "ids" and "determinants" utterly fails to give any rational account of such phenomena.

And now it seems clear that what Spencer says in this last quotation may help us to understand the possibility of the occurrence from time to time of what are known as "sports," arising during the development of plants and animals—such as have been long known to occur, and have been habitually taken advantage of by breeders in their efforts to establish new races or new varieties. The occurrence of such sports has in recent years been much dwelt upon by Hugo de Vries¹ in the foundation of his doctrine of the origin of species by "Mutation," as also by W. Bateson in his work on *Discontinuous Variation*.

Just as in a gemma or egg of one of the lower organisms, when subjected to unnatural conditions, isomeric changes in the living substance may be brought about, leading to the production of an entirely different form of life (Heterogenesis); so, it seems to me, we must look to some isomeric changes occurring in particular parts of the living substance of a plant or animal, during some of its later developmental phases, in order to account for the occurrence of "sports," and, perhaps, for the origin of new species by "Mutation," upon which de Vries has thrown so much light.

It is well known that Darwin did not attach any very great importance to the existence of "sports," and that his doctrine of "Natural Selection" relied in the main on the constant occurrence of minute individual variations in all directions in the different representatives of species, some of which would be more and some less "useful" for their possessors in their struggle for existence—leading, in this way, through repeated processes of multiplication and descent,

to the gradual extinction of those possessing the least useful qualities, and, on the other hand, as Herbert Spencer termed it, to "survival of the fittest."

Some very fundamental objections have for a long time been raised against the adequacy of these particular views to account for the origin of new species, and such objections have been gathering weight during recent years. Thus, it was contended, even as far back as 1871, by St. George Mivart,² that among the minute individual variations which arise many would have no utility for the species, and that the incipient stages of a possibly useful variation could not be seized upon and developed by a process of "Natural Selection," so that these incipient variations would be liable to be swamped by crossing or to disappear by atavism.

The American workers, Packard, Cope, and Hyatt, have also dwelt upon the fact that Natural Selection is not a cause of Variation. According to Packard, it comes in as a cause of the preservation or extinction of forms that have arisen in other ways.³ And, as Cope says:—"A selection cannot be the cause of those alternatives from which it selects. The alternatives must be presented before the selection can commence."³

Then, again, looking at the question from another point of view, it has been said by J. T. Cunningham⁴ that the theory of Natural Selection is "only a theory of the origin of adaptations," while it is contended that "there is scarcely a single instance in which a specific character has been shown to be useful, to be adaptive." Bateson dwells upon a similar point of view when he

¹ *Genesis of Species*, chap. ii.

² *Life of Lamarck*, 1901, p. 391. This book is a storehouse of facts and references bearing on Neo-Lamarckism.

³ *The Primary Factors of Organic Evolution*, 1896, p. 474.

⁴ Quoted by H. Spencer, *loc. cit.*, p. 565; but see his work on *Sexual Dimorphism in the Animal Kingdom*, 1900, pp. 8-11.

says¹:—"As Darwin and many others have often pointed out, the characters which visibly differentiate species are not as a rule capital facts in the constitution of vital organs, but more often they are just those features which seem to us useless and trivial.....These differences are often complex and are strikingly constant, but their utility is, in almost every case, problematical."

Difficulties of this kind de Vries claims to meet and answer by his "Mutation theory," the details of which he has recently published after many years of laborious, though fruitful, research. But before saying anything further concerning his work and views it will be well to cite some of the old and well-attested examples of the more or less sudden cropping up of "sports" or actual new species.

One of the most remarkable of known instances of *per saltum* development is the fact recorded by Darwin² that on five separate occasions what Sclater has pronounced to be a distinct species of Peacock—the "black-shouldered kind," or *Pavo nigripennis*—has appeared suddenly in a stock of common or pied peacocks, and in two of the cases (that is, in the flocks of Sir J. Trevelyan and Mr. Thornton) the black-shouldered kind had increased "to the extinction of the previously-existing breed."

Then, again, as J. J. Murphy says³:—"The otter, or Ancon, sheep of North America was also the result of a sudden variation; and the differences in the form of its skeleton from that of the common sheep amounted to a specific, if not a generic, difference." It is true, as this writer says, these and other instances that could be mentioned have arisen under domestication.⁴ But he

pertinently asks why may not the same kind of thing be taking place in the wild state? "It may be true that we have no evidence of the origin of wild species in this way. But this is not a case in which negative evidence proves anything. We have never witnessed the origin of a wild species by any process whatever; and if a species were to come suddenly into being in the wild state, as the Ancon sheep did under domestication, how could we ascertain the fact? If the first of a newly-begotten species were found, the fact of its discovery would tell nothing about its origin. Naturalists would register it as a very rare species, having been only once met with, but they would have no means of knowing whether it were the first or last of its race."

The instances just cited are cases in which new species have suddenly appeared without known cause—no one has been able to fix upon any external determining conditions. Other instances, however, are known where, as with the famous metamorphosis of the Mexican Axolotl, a more or less definite and known change of conditions has helped to bring about the transformations in question.¹

One such case, in which a marked alteration in climate was operative, has been cited by Darwin.² Metzger obtained seeds of a tall kind of maize (*Zea altissima*) from the warm regions of America and cultivated them in Germany. During the first and second years the plants reared showed some differences from the parent stock, and "in the third generation nearly all resemblance to the original and very distinct American parent-form

and frizzled fowls, short-faced tumbler pigeons, hook-billed ducks, etc., and with plants a multitude of varieties, suddenly appeared in nearly the same state as we now see them." (Darwin, *loc. cit.*, vol. ii., p. 414.)

¹ As to the nature and significance of this transformation, see *Studies in Heterogenesis* pp. 332 and 335.

² *Animals and Plants under Domestication*, vol. i., p. 322.

¹ *Material for the Study of Variation*, 1894, p. 11.

² *Animals and Plants under Domestication*, vol. i., p. 322.

³ *Habit and Intelligence*, 1869, vol. i., p. 343.

⁴ "It is certain that the Ancon and Mauchamp breeds of sheep, and almost certain that the Niata cattle, turnspit and pug-dogs, jumper

was lost. In the sixth generation this maize perfectly resembled a European variety.....When Metzger published his book, this variety was still cultivated near Heidelberg, and could be distinguished from the common kind only by a somewhat more vigorous growth." Darwin speaks of this as the most remarkable instance known to him "of the direct and prompt action of climate upon a plant."

Then, again, a very remarkable series of sudden transformations in the character of the flower in many different kinds of plants has been long known to occur, and has been fully described by Maxwell Masters in his work on *Vegetable Teratology*,¹ under the heading of "Regular Peloria" and "Irregular Peloria."

In the first variety irregular flowers become regular, as when a Violet drops its spurs, and inequality of sepals and petals, and appears as a perfectly regular flower. On the other hand, when the flower becomes regular by the increase in number of its irregular portions (as in the Toad-flax when it develops equal sepals, and five spurs instead of only one), this is known as irregular peloria. In this latter case Masters says "the peloria is evidently not congenital, but occurs at a more or less advanced stage of development."

Sometimes this kind of peloric change shows itself in all the flowers on a plant, while at other times a plant will be found with part normal and part peloric flowers. The phenomenon is widespread, and is apt to occur in all kinds of plants. Masters says that changes of this kind are known to occur in no less than 110 plants, and he agrees with Darwin in thinking that in many cases such transformations cannot be attributed to "reversion." Some of these peloric sports are, moreover, capable of reproducing themselves by seeds, as has been shown by Darwin and others.

Referring to facts of this kind, and

some others of which I am about to speak, recorded by Darwin in the extremely interesting and important chapter xi. on "Bud Variation" in his *Animals and Plants Under Domestication*, I find that so long ago as 1872 I expressed the following opinion:²—"We think, moreover, that Mr. Darwin fails to attach an adequate importance to such instances of 'spontaneous' variation as he has recorded. These instances would seem to afford most interesting examples of the operation in higher organisms of those influences which suffice to produce such multitudes of heterogenetic changes among lower organisms; so that they supply most valuable additional testimony as to the continued influence of 'organic polarity' in determining the form and structure of higher organisms."

If looked at from this point of view, we shall be more fully able to appreciate the importance of many of the instances cited by Darwin, and which were believed by him not to be explicable by the principle of "reversion." One of the simplest, and yet most satisfactory, examples is that recorded concerning the rare and occasional production of nectarines upon peach-trees, and the reverse. Speaking of the peach, Darwin says:—"This tree has been cultivated by the million in various parts of the world, has been treated differently, grown on its own roots and grafted on a stock, planted as a standard, against a wall, and under glass; yet each bud of each sub-variety keeps true to its kind. But occasionally, at long intervals of time, a tree in England, or under the widely different climate of Virginia, produces a single bud, and this yields a branch which ever afterwards yields nectarines. Nectarines differ, as everyone knows, from peaches in their smoothness, size, and flavour; and the difference is so great that some botanists have maintained that they are specifically distinct. So permanent are the characters thus suddenly acquired that a nectarine

¹ Ray Society, 1859, pp. 219-239.

² *The Beginnings of Life*, vol. ii., p. 595.

produced by bud-variation has propagated itself by seed."

Changes of this kind—and several others which Darwin records¹—are doubtless due to some molecular modification, brought about in an unknown manner, in the tissues of the bud which varies; so that the production of the nectarine structure is the result of the altered balance and the new moving equilibrium that becomes necessitated in the tissues of the growing part. Such an explanation of these apparently "spontaneous" changes in plants may be illustrated by alterations which are liable to occur in certain parts of some animals when exposed to this or that abnormal influence. Thus Wallace says²:—"The Indians (of South America) have a curious art by which they change the colours of the feathers of many birds. They pluck out those from the part which they wish to paint, and inoculate the fresh wound with the milky secretion from the skin of a small toad. The feathers grow of a brilliant yellow colour, and on being plucked out, it is said, grow again of the same colour without any fresh operation." Although this change seems to be producible at will by a definite agent, we really know no more concerning the actual steps of the process by which it is produced than we know concerning the intimate nature of the changes by which the peach-branch is metamorphosed into a nectarine-branch.

¹ Somewhat analogous changes, for instance, occasionally occur in rose-trees, whereby a moss-rose suddenly appears upon a tree belonging to a totally different variety (*loc. cit.*, pp. 379-381). And in regard to these changes it is important to bear in mind that many of them are certainly not to be attributed to "reversion." Thus Darwin says (*loc. cit.*, ii., p. 255):—"No one will maintain that the sudden appearance of a moss-rose on a Provence-rose is a return to a former state, for mossiness of the calyx has been observed in no natural species; the same argument is applicable to variegated and lacinated leaves; nor can the appearance of nectarines on peach-trees be accounted for with any probability on the principle of reversion."

² *Travels on the Amazon and Rio Negro*, p. 294.

The possibility of the occurrence of the molecular change which occasions this metamorphosis is, moreover, by no means limited to the buds of the peach-tree. For, as Darwin tells us, "nectarines have likewise been produced from the stone of the peach, and, reversely, peaches from the stone of the nectarine." And even this is not all, since "the same flower-bud has yielded a fruit one-half or one-quarter a nectarine, and the other half or three-quarters a peach."

So much, then, for facts concerning "sports" and "discontinuous variation" which have been known for rather a long time. Let us look now to the more recent evidence tending further to show in an unmistakable manner that even in regard to higher forms of life the origin of new species may occur by sudden "mutations" rather than by the slow cumulation through many generations of minute progressive variations.

In his before mentioned work, Professor de Vries seems to have certainly demonstrated this point in regard to the "Evening Primrose" (*Oenothera Lamarckiana*) by means of observations carried on during ten years upon 50,000 of the descendants of some of these plants placed by him in the botanical gardens of Amsterdam. Preliminary experiments made with this and a number of other plants showed it to be favourable for such observations. Of the 50,000 descendants of this plant, it was found that "about 49,200 were in no respect different from the original patriarchal *O. Lamarckiana*, showing no tendency towards gradual change in any special direction, but only the common small fluctuating 'variations' as regards size and appearance on either side of a normal—in fact, resembling in that respect other plants and animals which are left to themselves without being interfered with."

"Quite otherwise with the 826 other plants. None of these, although appearing spontaneously, could be said to be representatives of *O. Lamarckiana*, from

which they were descended. De Vries arranges them in seven distinct species—viz., 1 of *O. gigas*, 56 of *O. albida*, 350 of *O. oblonga*, 32 of *O. rubrinervis*, 158 of *O. Vanella*, 221 of *O. lata*, and 8 of *O. scintillans*.¹

Then, again, the representatives of these new species produced descendants the majority of which unmistakably belonged to the same species as itself. Some of the new species like *O. gigas* are very stable forms. Thus, from the one specimen of this plant "were obtained 450 plants, all of which, with one exception, were *O. gigas*—the one exception not being a return to *O. Lamarckiana*, but belonging to a new variety." On the other hand, *O. scintillans* is "extremely unstable—i.e., possesses the property of mutation to a high degree, a large proportion of its descendants belonging to other species, especially *O. oblonga* and *Lamarckiana* itself."

All these facts concerning the "black-shouldered" Peacock, the Ancon sheep, the tall American maize, and the various species of *Oenothera*, are so many examples of transformations more or less rapidly brought about; not, of course, due to internal changes alone, but to the conjoint influence of internal tendencies and external conditions operating at the time—the latter being an aspect of the problem which Weismann ignores, or he would never have made such a statement as this:—"When by germinal metamorphosis a new form has arisen, this, from the first moment of its existence, must be adapted to the new conditions of life or it must perish." The cases above referred to would doubtless be regarded by him as cases of "germinal" metamorphosis, and yet they have been so far adapted to their conditions of life that, instead of perishing, they have increased, multiplied, and perpetuated their kind.

These abrupt transformations of

¹ See *Nature*, June 27th, 1901, p. 208.

species do not, as Weismann implies, necessarily depend on some "sudden change in the conditions of life." It has not always been so; as we have seen on different occasions, amid the old conditions, the appearance of the "black-shouldered" Peacock, and the development of new species of *Oenothera*. And when Weismann goes on to say, "If such abrupt transformation takes place, it must produce the new form instantly equipped for the struggle for existence, and adapted in all its organs and systems of organs to the special conditions of its new life," I rejoin that this does not in the least apply to the appearance, by comparatively abrupt transformations, of multitudes of the lower forms of life such as Amœbæ, flagellate Monads, Moulds, Peranemata, Ciliated Infusoria, and many other representatives of the lower types of life which are by no means so dependent upon specialised external conditions, and amid which the part played by Natural Selection seems reduced to its lowest terms.²

Statements such as these are all that Professor Weismann, in the course of eleven pages, was able to say against the occurrence of a *per saltum* development, as one of the modes in which Evolution is carried on.² His statements and arguments must be absolutely unconvincing to any open-minded critic.

So far from there being any *à priori* objections to *per saltum* development or Heterogenesis, which, as I have elsewhere shown, takes place so frequently, the fact of its occurrence ought to be considered *as thoroughly harmonious with all that we know concerning allotropic and isomeric changes in simpler forms of matter, and concerning the constitution of living matter itself.*

For the present, however, the subject

² The origin of all these forms as a result of more or less "abrupt transformation," together with many other heterogenetic transformations, are fully described in my *Studies in Heterogenesis*, 1904, as reference to index and text will show.

² See his *Studies in the Theory of Descent*, translation, two vols., 1882, pp. 697-708.

of Heterogenesis cannot be dealt with, and I must limit myself to a brief account of the views of de Vries.

After pointing out that the origin of species has hitherto been considered an excessively slow process, and one "withdrawn from actual observation, or at least from experimental treatment," he contends that now, in accordance with his views, the process should be regarded as much more rapid, and as one coming within the range of direct observation and experiment. He disbelieves in minute common variability as a starting-point for natural selection and the production of new species, and maintains that such a process "does not lead, by even the sharpest persistent selection, to any real transgression of the limits of species, much less to the origin of new and constant attributes."

"According to the Mutation theory, species have not originated by gradual selection, continued through hundreds or thousands of years, but by sudden steps, even if the changes are very small. Unlike the variations which are progressive changes in a straight line, those metamorphoses which are designated as mutation branch off in new directions. Furthermore, so far as experience goes, they occur at random—that is, in the most diverse directions."

"The new species is at once such, and originates from the former species without apparent preparation and without graduation. Each attribute, of course, arises from one previously present, not by their normal variation, but by one small yet sudden change. Provisionally one may compare these changes, but only in the simplest manner, with chemical substitution."¹

As a factor in Evolution, therefore, it seems clear that the "mutation" theory of de Vries must be intended by its author in part to replace the theory of "natural selection." To what extent it will do so cannot be decided till much

additional work has been done and many years have elapsed. In several respects the views of de Vries are in accord with those of Cope, Packard, and Hyatt, already referred to.

De Vries believes that all species and genera, while always subject to the full range of fluctuating variability, exist at times in a mutable and at times in an immutable condition, the latter state being very much the more prevalent. Indeed, so prevalent is the immutable condition among plants that only a small proportion of the species embraced in the flora of any given region may be found existing in their mutable period. Furthermore, it has been found, and was to be expected, that some plants, even when in the fulness of their mutable period, would exhibit their mutability more readily than others.

At the conclusion of an address delivered in 1904 before the University of Chicago, which the author has kindly sent me, Professor de Vries said:—"Mutability is manifestly an exceptional state of things if compared with the ordinary constancy. But it must occur in nature here and there, and probably even in our own immediate vicinity. It has only to be sought for, and, as soon as this is done on a sufficiently large scale, the study of the origin of species will become an experimental science."

All that is said in the last two paragraphs will be found to be just as applicable to Heterogenesis as it occurs among lower forms of life, except that the resulting transformations are then much more radical and complete than are those resulting from "Mutation." In these latter changes, occurring in higher plants, each newly-originated species, while possessing distinctly separate attributes, is never, as de Vries intimates, very widely different from the parent form.

The contrast between the two processes above referred to and those of Natural Selection is great in all ways, seeing that the latter never admits of actual proof and observation, depending as it does, or is commonly believed to

¹ See *The Smithsonian Report*, 1901, pp. 631-640.

do, upon the progressive accumulation of most minute useful variations along a particular line, through generation after generation, so that it leads not to random or purposeless features, but always to "ceaseless adaptations of the species to its life-conditions." The time needed for the production of all the forms of

life that have ever lived by a process so essentially slow as this would surely be prodigious, and must inevitably far exceed anything that geologists would demand, not to speak of the far narrower limits which physicists are willing to assign for the age of our globe, dating from the consolidation and cooling of its surface.

CHAPTER VII.

OTHER FACTORS OF EVOLUTION

It is important to bear in mind what has hitherto been said as to the comparatively trivial character of the differences that mostly go to separate one species from another, and the common absence of any evidence of "usefulness" in such characters. But if we regard "organic polarity" and changes in molecular constitution as lying at the root of the origin of "sports" and as the cause of "mutation" generally, there would no longer be any discrepancy in many cases, since we are expressly told by de Vries that the changes due to mutation "occur at random—that is, in the most diverse directions."

In illustration of the comparative ease with which changes are brought about in the specific characters of some organisms, reference may be made to the results of experiments with the larvæ of certain butterflies. It is now well established that the colours and markings of many of these latter, together with the size, and to a less extent the shape, of their wings, are largely dependent upon the conditions, as regards temperature and light, under which the caterpillars and pupæ have been reared. Hence the phenomena of "Seasonal Dimorphism," in which one form of butterfly is bred in the spring, and a

different one later on in the summer. But the experiments of Merrifield¹ and of Weismann have shown that one of the two seasonal forms may be bred from the larvæ of the other form by simply altering the temperature under which the larvæ are reared—the changes, in fact, though slighter in amount, are produced just in the same way that changes in the colour and forms of crystals are brought about (pp. 31, 39).

There are also other sets of changes of like kind, lying outside the pale of utility, and yet apparently connected with one another in some mysterious manner: I allude to those which were grouped by Darwin under the head of "Correlated Variability."² In reference to this subject he says:—"Correlated variation is an important subject for us; for when one part is modified through continued selection, either by man or under nature, other parts of the organisation will be unavoidably modified." This is thoroughly in harmony with what might be expected in accordance with H. Spencer's doctrines. Thus he says³:—

¹ *Nature*, December 23rd, 1897, p. 184.

² *Animals and Plants under Domestication*, 1868, vol. ii., chapter xxv.

³ *Loc. cit.*, vol. ii., p. 622.

"If, along with a striking change in a flower which the florist contemplates, there go changes all over the plant not obvious to careless observers but visible to him, we must infer that there are everywhere minute differences which even the florist cannot perceive: the whole constitution of the plant has diverged in some measure from the constitution of kindred plants. Every local variation implies a change pervading the entire organism, manifested in concomitant variations everywhere else." Yet that the facts are as H. Spencer states he shows by three quotations, one from Darwin and two from Dr. Maxwell Masters. And it is certainly true that, while such facts as these are wholly incongruous with the hypothesis of determinants, they are in accord with the hypothesis of physiological units.

In order, however, to account for all the facts concerning "sports" of all kinds, "mutation" and "correlated variability," to which we have just been referring, as well as to many other difficulties besetting his exclusive doctrine concerning the all-sufficiency of "Natural Selection," Professor Weismann relies upon a supplemental hypothesis which he calls "Germinal Selection." In addition, he appeals to it to explain the existence of non-adaptive characters in animals and plants; as a means of starting and increasing incipient variations till they can be made use of by natural and sexual selection; as a mode of accounting for the influence of new external conditions in modifying organisms; as a means of bringing about the diminution and even total disappearance of disused organs; and, finally, as yielding an "explanation of every heritable variation."¹

In the last chapter we have seen how devoid of foundation in fact is his doctrine of the "Continuity of the Germ-Plasm," and what an extraordinary web

of assumptions and hypotheses he has made use of, in conjunction with this basal doctrine, in his attempts to explain many known phenomena. And now all outstanding difficulties, he seems to think, are to be explained away by his supplemental hypothesis of "Germinal Selection"—that is, by a process of selection occurring between the determinants of each "id," and going on in all of them simultaneously.

In reference to this doctrine we have to bear in mind that the cell itself is a body of microscopic minuteness; that the bulk of the nucleus is very much smaller still; that the nucleus contains several minute chromosomes; that each chromosome is made up of a number of granular bodies termed "ids" (only visible with very high powers of the microscope); and that each granular id is assumed to be composed of many thousands of imaginary bodies named "determinants"—and that it is between these determinants that the nutritional struggle, or "selection," is taking place.

It is further assumed by Weismann that the rate of growth of these imaginary determinants within the granular id will depend "mainly on the amount of nourishment which reaches them"; and that there will be "unequal nutrition of the determinants conditioned by the chances of the food supply." Moreover, owing to their different assimilative powers, "while one determinant may be slowly becoming weaker, its neighbour, on the other hand, may be varying on an ascending scale, just because the former is, on account of its diminished power of assimilation, no longer able to exhaust completely the food stream which flows to it"—that is, within the very minute microscopic granular id. Some of these assumed changes in rate of growth or nutritive vigour are believed to be "spontaneous," while others are "induced" by changes in external conditions. Weismann's doctrine necessitates, however, the consideration of these other additional points:—"Each id contains potentially the whole organism,

¹ See *The Evolution Theory*, vol. ii., pp. 113-158.

though with some individuality of expression. The child is thus not determined by the determinants of a single id, but by those of many ids; and the variations of any part of the body do not depend upon the variations of a single determinant X, but on the co-operation of all the determinants X which are contained in the collective ids of the relevant germ-plasm. Thus it is only when a majority of the determinants have varied upwards or downwards that they dominate collectively the development of the part X and cause it to be larger or smaller."

It is not only, he tells us, that the determinants may "become larger or smaller as a whole, but some kinds of the 'biophors' of which they are made up may increase more than others under definite altered conditions; and in that case the determinants themselves will vary qualitatively.....consequently, also, qualitative variations of the organs controlled by the determinants, the determinates," will be caused.¹

Many persons will probably fail to find much security in these complicated considerations—especially when they bear in mind that the foundations on which they are based lie among shifting and delusive sands rather than upon a bed of solid rock. H. M. Vernon believes this hypothesis of "Germinal Selection" to be absolutely at variance with an important body of well-established facts. Thus he says²:—"This theory, though plausible enough, is absolutely opposed to fact in so far as it relates to the evolution of more adaptive forms.....it is no more than an hypothesis, which has not, and never can have, any experimental evidence to support it." Haeckel also rejects it. He says:—"We may admire the subtlety and depth of the speculations with which Weismann has worked out his elaborate molecular theory; but the nearer we get

to its foundations the less solid we find them. Moreover, not one of the many supporters of the theory of germ-plasm has been able to make profitable use of it in the twenty years since it was first published. On the other hand, it has had an evil influence in so far as it denied the inheriting of acquired characters, which I hold, with Lamarck and Darwin, to be one of the soundest and most indispensable supports of the theory of descent."¹

There remain now for brief consideration three other factors of Evolution—namely (1) Sexual Selection; (2) the Effects of Use and Disuse; and (3) the direct Influence of External Conditions; and in regard to each and all of them the degree of their influence as factors hinges upon the all-important question which we shall presently have to consider; that is, whether Acquired Characters are or are not inherited.² This latter question has long been debated in relation to the last two of the factors just mentioned, but it has only recently been very seriously considered in reference to Sexual Selection. For this and other reasons it will be convenient briefly to consider and make some statements in reference to this factor before dealing with the other two.

In Darwin's view the process of Sexual Selection was essentially similar to that of Natural Selection—was, in fact, a corollary therefrom, and consequently explicable, with the necessary variations, in much the same kind of way. This view, however, has not been generally accepted.

Several theories in connection with

¹ *The Wonders of Life*, translation, 1904, p. 104; see also pp. 385 and 391.

² In many collateral respects also this question is one of enormous importance, as may be gathered when Herbert Spencer says:—"More than once I have pointed out that, as influencing men's views about Education, Ethics, Sociology, and Politics, the question whether acquired characters are inherited is the most important question before the scientific world" (*Principles of Biology*, vol. I., p. 672).

¹ *Loc. cit.*, vol. ii., pp. 125 and 152.

² *Variation in Animals and Plants*, 1903, p. 396.

this subject are discussed by J. T. Cunningham in his interesting work, *Sexual Dimorphism in the Animal Kingdom*.¹

He says (*loc. cit.*, p. 30):—"The fundamental objection to Darwin's theory of sexual selection, or any other selection theory, is that it does not account for the *origin* of the variations which it assumes.....there are two almost universal peculiarities of secondary sexual characters on which the theory of sexual selection throws no light whatever: (1) the characters do not begin to appear in the individual until it is nearly adult and sexually mature; in other words, they appear when, or a little before, the animal begins to breed; (2) they are inherited only by the sex which possesses them"—or, in other words, only by the members of one sex.

Unisexual characters have, as a general rule, some function or importance in the special habits or conditions of life of the sex in which they occur, whether they are fighting weapons like the antlers of stags, allurements as in the special plumes of male birds which are erected and displayed in courtship, or love-notes as in the songs of birds generally. "So far," Cunningham says, "there can be no doubt that Darwin was perfectly right.....There is rivalry, combat, and competition. But the important truth which appears to have been generally overlooked is that in the case of each special organ its special employment subjects it to special, usually mechanical, irritation or stimulation, to which other organs of the body are not subjected." Such constantly repeated irritations or stimulations are causes thoroughly well recognised as being capable of producing physiological effects on the tissues of a distinct kind, and have been much dwelt upon by E. D. Cope in his important work, *The Primary Factors of Organic Evolution*.²

Facts seem to imply, however, that the development of these "physiological correlations" at the present stage of

evolution takes place by heredity, seeing that they appear, even though the usual irritations are partly or entirely wanting. If, therefore, we were at liberty to assume the inheritance of acquired characters, the explanation of the existence of secondary sexual characters, and their strict limitations to a particular sex and a particular period of life, and often to a particular season, would present far less difficulties than those which beset the explanations of Darwin and Weismann.

Believing as he does in such inheritance of acquired characters, the explanation of Secondary Sexual Characters adopted by Cunningham is this: "*that the direct effects of regularly recurrent stimulations are sooner or later developed by heredity, but only in association with the physiological conditions under which they were originally produced.*" He contends, in short, that the whole series of multitudinous facts agrees remarkably with the hypothesis that secondary sexual characters are due to the inheritance of acquired characters.

The further evidence in regard to this subject of the Inheritance of Acquired Characters must now be briefly considered, as it is also the all-important question in relation to Use and Disuse, and in regard to the Direct Influence of External Conditions. If the effects of these influences are confined to individuals, and not transmitted to their progeny, neither of them could be regarded as a factor of Evolution; while, on the other hand, if the effects acquired in either of these ways are capable of being transmitted to offspring, it is clear that we should have in each of them a factor of Evolution of great importance.

Doubts on this subject were first started by Weismann in 1883, and were strongly reinforced two years later by the publication of his essay on "The Continuity of the Germ-Plasm."³ In a later essay in the same volume he says

¹ See his *Essays on Heredity*, transl., 2nd edition, 1891, vol. i., pp. 67 and 163.

² 1900, pp. 24-44.

³ 1896, Chicago.

(p. 399):—"I think it is now generally admitted that a very important problem is involved in this question, the solution of which will contribute in a decisive manner towards the formation of ideas as to the causes which have produced the transformation of species. For, if acquired characters cannot be transmitted, the Lamarckian theory completely collapses, and we must entirely abandon the principle by which alone Lamarck sought to explain the transformation of species—a principle of which the application has been greatly restricted by Darwin in the discovery of natural selection, but which was still to a large extent retained by him." Weismann came to the conclusion that acquired characters cannot be transmitted, because, as he thought, there were no proofs of such transmission, and because its occurrence was theoretically improbable, or even "inconceivable," as some of his followers say.¹

It is well never to lose sight of the fact that opposition to the view that functionally acquired characters are inherited is essentially associated with Weismann's doctrine of the "Continuity of the Germ-Plasm," and the other hypotheses founded thereupon. We have seen (pp. 46-50) how void of foundation in fact this doctrine is, and how much it is actually opposed to multitudes of facts that have been made known since the time when it was first enunciated. And as to the alleged improbability or inconceivability of any means by which the germ-cells may be specifically altered as a result of changes

taking place in the soma by use or disuse, or by the direct influence of conditions, one might have thought that the last persons to start such an objection would be Weismann and his followers, looking to the extraordinarily complex mixture of assumptions and hypotheses by which their own attempted explanation of heredity is sought to be supported.

Some of Herbert Spencer's last words on this subject may well be quoted here. In his *Facts and Comments* (1902, p. 92) he says:—"The doctrine of use-inheritance is rejected because of inability to 'conceive any means' by which a modification produced in an organ can produce a correlated modification in the germ of a descendant. Yet the alternative hypothesis is accepted, notwithstanding a kindred inability, which is certainly not less, and may be held much greater. If Weismann's view is true, such a structure as a peacock's tail feather implies over 300,000 determinants. Multiply that by the number of such feathers, and add those of the body feathers, as well as those of all the parts of all the organs, and then imagine the number of determinants which must be contained in the microscopic sperm-cell. Further imagine that in the course of the developmental transformations each determinant finds its way to the place where it is wanted! Surely to 'conceive any means' by which these requirements may be fulfilled is not a smaller difficulty if it is not a greater" than that which has to be met in connection with the doctrine of physiological units.²

The question of relative conceivability

¹ In the early days of this discussion far too much importance was attached to the fact that mutilations are not inherited. But these occur for the most part once only in the life of an individual, and are mostly experienced by one sex only, so that it is as Cope says (*loc. cit.*, p. 400) "quite unreasonable to cite the history of mutilations as evidence against the inheritance of natural characters produced by oft-repeated and long-continued natural causes." Still, some well-authenticated cases of the inheritance of mutilations have been recorded by him (p. 431).

² The above is an under-statement of the case as to the inconceivability of Weismann's views, because the "sperm-cell" contains a number of chromosomes, and each of these bodies is made up of a number of granules—the ids; and each one of these mere granules is supposed to contain the total number of determinants referred to above for the building-up of the body, which Spencer speaks of, not as in the ultra-minute "id," but as contained in the entire "sperm-cell."

between Weismann's doctrine of absolutely innumerable determinants in each "id" of the germ-plasm destined to produce corresponding parts in the offspring and Spencer's doctrine of physiological units cannot by any possibility be said to be easier for the former doctrine; and when one considers how this doctrine utterly fails to explain multitudes of known facts concerning repair and regeneration in animals, and reproduction by buds, cuttings, and superficial cells in plants, with which Spencer's doctrine is quite in harmony, there ought not to be room for doubt as to which hypothesis is most in accord with facts.

It only remains, therefore, to look at some of the actual evidence tending to support the view that the Effects of Use and Disuse, and also the direct Effects produced by altered External Conditions, can be, and are frequently, transmitted to offspring.

The effects of Use and Disuse are effects that would be met with in animals rather than in plants; and apart from secondary sexual characters, to which reference has already been made, the discussion in recent years has largely been confined to questions touching the effects of use-inheritance on the frame of man himself.

It is well known that Darwin² collected a number of facts implying that functionally altered structures are transmitted by heredity. Among the facts that he adduced he showed especially that there is a changed ratio between the wing-bones and the leg-bones in the domesticated, as compared with the wild, duck; and that alterations in the bones of fowls and rabbits had also taken place. Concerning such changes he said: "From the foregoing facts there can be no doubt that certain parts of the skeleton in our anciently domesticated animals have been modi-

fied in length and weight by the effects of decreased or increased use." He also attached much importance to the increased or diminished length of the intestines, resulting, apparently, from changed diet. It has been found, for instance, that the intestines of the domestic cat are one-third longer than those of the wild cat of Europe.

But let us turn now to examples more recently made known. Some important observations as to the transmission of acquired characters in Fungi have been published by George Massee in a recently issued number of the *Philosophical Transactions* (197, B), showing that in the course of twelve to sixteen generations several ordinary saprophytic Fungi could be made to take on a parasite habit, and perpetuate themselves as parasites. Related facts have also been recorded by Klebs and L. Errera.

Haeckel regards the spiral shape of the shells of Gasteropods as "a very fine instance of the inheritance of acquired characters."¹ He likewise regards the asymmetrical shapes of flat-fishes, such as soles, flounders, and turbot, as results of changes that they have gradually acquired through many generations owing to their habit of lying on one side (right or left) at the bottom of the sea, though they are well known to have a perfect bilateral symmetry when young, like that of ordinary fishes. Cunningham also attaches much importance to these changes,² and he cites another remarkable case in which particular habits seem to have engendered a definitely corresponding change in another fish (Anableps) living in the estuaries of Brazil and Guiana, which "does not wear spectacles, but actually has its eyes made in two parts, the upper half of the lens having a different curvature from that of the lower. The pupil is also divided into two by prolongations from the iris." And the explanation of this remarkable condition seems to lie in the

² *Animals and Plants*, 1868, ii., pp. 295-303.

¹ *The Wonders of Life*, p. 185.

² *Loc. cit.*, pp. 16 and 23.

fact that "this fish is in the habit of swimming at the surface with its eyes half out of the water, and the upper half of the eye is adapted for vision in the air; the lower half for vision under water." There is no reason, he says, to suppose "that the required variations ever occurred until the ancestors of Anableps took to swimming with their eyes half out of water."

These cases ought rather to be referred to under the next head of changes wrought by alteration in the conditions of life. As examples occurring in animals, however, it is more convenient to refer to them here.

We must turn now to the various cases cited by Herbert Spencer during the celebrated discussion that took place between him and Weismann in the pages of *The Contemporary Review* in 1893-4, his contributions to which are reproduced as an Appendix to the first volume of *The Principles of Biology*. An admirable *résumé* of his own point of view will be found in his fourth article (pp. 671-695), so that only brief references to the principal instances cited will be mentioned here. One of the most weighty has reference to the co-adaptation of co-operative parts in reference to use-inheritance. He cites, for instance, the case of the extinct Irish elk, the male of which had enormously developed horns, weighing upwards of a hundredweight, "carried at great mechanical disadvantage: supported as they are along with the massive skull which bears them, at the extremity of the outstretched neck." Moreover, "that these heavy horns may be of use in fighting, the supporting bones and muscles must be strong enough, not simply to carry them, but to put them in motion with the rapidity needed for giving blows. Let us then ask how, by natural selection, this complex apparatus of bones and muscles can have been developed *pari passu* with the horns?.....It would be a strong supposition that the vertebræ and muscles of the neck suddenly became bigger at the

same time as the horns. It would be a still stronger supposition that the upper dorsal vertebræ not only at the same time became more massive, but appropriately altered their proportions, by the development of their immense neural spines. And it would be an assumption still more straining our powers of belief, that along with heavier horns there should spontaneously take place the required strengthenings of the bones, muscles, arteries, and nerves of the scapula and the fore-legs" (pp. 537-8). Through the intermediation of the inherited effects produced by use all the co-operative parts could vary together; on the other hand, it is impossible to imagine how such correlated variations in different parts could spring up spontaneously and be developed by natural selection.

Then, again, there is the case of the rudimentary hind limbs of the whale, as to which Herbert Spencer says: "Though, during those early stages of decrease in which the disused limbs were external, natural selection probably had a share in decreasing them, since they were then impediments to locomotion, yet when they became internal, and especially when they had dwindled to nothing but remnants of the femurs, it is impossible to suppose that natural selection played any part: no whale could have survived and initiated a more prosperous stirp in virtue of the economy achieved by such a decrease." It is surely, as he says, much more reasonable to attribute this extreme dwindling to the prolonged effects of disuse perpetuated through innumerable generations.

We come next to a number of characters presented by man himself which seem to exemplify changes resulting from the inheritance of functionally produced modifications. There is, for instance, the varying powers of tactile discrimination possessed by the skin and mucous membranes in different parts of the body, and especially the extreme sensitiveness of the finger-tips and of the tongue; the diminution of the size and

weight of the jaws and teeth which characterises the civilised races, as contrasted with the savage races; and the simultaneous increase in size of the great toe and diminution of the little toe, as the result of a cause "which has been operating ever since the earliest anthropoid creatures began to decrease their life in trees, and increase their life on the earth's surface," the changes being attributable to the fact that "effort is economised and efficiency increased in proportion as the stress is thrown more and more on the inner digits of the foot and less and less on the outer digits." Dr. (now Sir) Havelock Charles has also made known more than twenty differences, chiefly in the structure of the knee and ankle joints, to be met with between the leg-bones of Europeans and those of the Punjaub people—"differences caused by their respective habits of sitting in chairs and squatting on the ground." Markings and facets possessed by the latter and transmitted by heredity (since they are to be found in the new-born infant, and even in the foetus) are present owing to their habit, uniformly persisted in through innumerable generations, of squatting; while there is a total disappearance of the markings in question in the skeleton of the European, "as no advantage would accrue to him from the possession of facets on his bones fitting them for postures not practised by him" (*loc. cit.*, i., p. 689).

These points noted by H. Charles are, however, only special instances of what had previously been established by Cope. He says (*loc. cit.*, p. 467): "All the form characters of the vertebrate skeleton, and, for that matter, of the hard parts of all animals, have been produced by muscular pressures and contractions, and the friction, strains, and impacts due to these.....the characters of the skeleton can generally be shown to be inherited, because they appear before birth, and are found at some stage or another of fetal life."

When great musical talents or great

powers of calculation show themselves, such powers are also apt to be transmitted to some of the progeny. In these latter cases especially it seems much more reasonable to suppose that the unusual powers have taken *origin* in the soma rather than in the germ-plasm—that is to say, that the individuals in whom such unusual powers first show themselves owe them to changes that have taken place in particular parts of the developing brain. The degree of development of different regions of the brain is subject to constant variations in all individuals. One person has highly developed auditory centres and cerebral regions in association therewith, another has a poor development of the same parts; and the same thing holds good for the visual centres and their associated higher cerebral mechanisms. Some of those having highly-developed auditory centres may prove to have unusual musical abilities; while others, like Inaudi, may have marvellous powers in dealing with figures. His extraordinary capacity in this direction showed itself first when he was only six years old, and has continued ever since. He said to one who interviewed him a few years since: "Reading or writing figures is of little aid to me. I *must hear them*. I have no more than an average memory for colours, forms, events, places, or musical airs. Although I cannot remember for any length of time a couple of lines of poetry, I can easily remember without any effort a long series of figures, varying from twenty-five to thirty lines, after I have only heard them once." Other calculators are known to depend upon visual rather than upon auditory symbols; while one of them, George Bidder, F.R.S., who as a youth was known as the "Calculating Boy," has certainly transmitted his powers to some of his descendants, though to a minor extent.

Some striking facts illustrating the development of certain cerebral powers by use and practice carried on through generation after generation have recently

been cited by W. Woods Smyth,¹ who says: "Lately I heard a missionary at a May meeting tell of the marvellous facility with which Chinese children memorise whole books of the Bible; the four Gospels, and sometimes the Acts also, being an easy feat for children of ten or twelve years. Having carefully sought information from other authorities, I find these facts confirmed, and that the same applies to Mohammedan children. We are aware that for ages their ancestors have been compelled to memorise long portions of their sacred books, and, although occasionally we meet with a child of any nation with a gigantic memory, that differs widely from the case of a people where it has become a general characteristic."

Another point depending upon cerebral development in particular directions and its transmission by heredity is also of much importance. A child usually learns to speak slowly and with much difficulty; but I have contended elsewhere² that the child would never be able to speak at all if he had not inherited from very remote ancestors certain nervous mechanisms in the brain upon which this power depends, and that, if the child still has to learn to speak, this is due to the fact that he begins to try as soon as the appropriate nerve centres begin to develop. When in certain cases the acquirement of speech has not taken place at the usual time, it sometimes happens that years after, on the occurrence of some strong stimulus, the previously dumb person begins to talk, without going through the usual process of learning how to do it. Four cases in illustration are cited. One was a patient of my own, one a patient of the late Dr. Wigan, and the other two are matters of ancient history—cases referred to by Herodotus, which, in face of the modern instances, cannot be lightly dismissed.

Herbert Spencer, as is well known, holds that the inheritance of functionally-caused alterations has come "more to the front as evolution has advanced," and that "it has played the chief part in producing the highest types.....the action of natural selection being limited to the destruction of those who are constitutionally too feeble to live, even with external aid.....Natural selection acts freely in the struggle of one society with another; yet, among the units of each society, its action is so interfered with that there remains no adequate cause for the acquirement of mental superiority by one race over another, except the inheritance of functionally-produced modifications."³

The question whether Acquired Characters resulting from changes in the External Conditions of Life are inherited or not cannot be profitably discussed without precise references to Weismann's doctrine, so as to bring out the real difference between it and that of those who incline to the Lamarckian view. His doctrines will be found very distinctly stated in one of his *Essays Upon Heredity*,³ though scattered passages in his recent work, *The Evolution Theory*, will help to clear up doubtful points.

New characters in plants or animals are, he holds, either (a) *blastogenic* and transmissible, or (b) *somatogenic* and non-transmissible. Modifications of the latter type due to reaction of the soma under altered external influences are regarded by him as alterations limited to the individual, because *they are supposed to leave the germ-plasm unaffected*.

Thus, according to Weismann, changes in the soma cannot lead to, or be associated with, definite and correlated changes in the germ-plasm; though changes in the germ-plasm are, in some inscrutable way through the intermediation of his countless armies of determinants and biophors, supposed by him

¹ *Nature*, June 15th, 1905, p. 152.

² *Aphasia, and Other Speech Defects*, 1898, pp. 5-8.

³ *Biology*, i., pp. 553 and 560.

⁴ Edition of 1891, pp. 423-428 especially.

to be capable of leading to precise correlated changes in the soma. But, according to Spencer and others, changes in the soma induced by external conditions *may* become associated with definitely related changes in the germ-plasm. Such correlated changes are held by them to be no more impossible or inscrutable than those which we see every day actually occurring in the reverse order—namely, in the development of animals and plants from their germs.

Let us see how far Weismann is consistent in holding his doctrines, and to what extent the logic of facts may have compelled him to make admissions, which practically bring him almost into accord with Spencer and his followers.

The *blastogenic* changes with which heritable characters are correlated are, in the opinion of Weismann, due either (1) to artificial or to natural selection, or else (2) to "spontaneous" variations of the germ-plasm, from whatsoever cause arising.

In considering these views we must first come to some understanding as to the starting-points for artificial and natural selection. According to Darwin,¹ "selection does nothing without variability, and this depends in some manner on the action of surrounding circumstances on the organism." On this same subject Weismann says²: "The ordinary, never-ceasing, always active germinal selection depends, we must assume, upon intra-germinal fluctuations of nutrition, or inequalities in the nutritive stream which circulates within the germ-plasm." Such changes, however, he thinks, "have their roots in external influences." He asks, indeed, "how could the germ-plasm be changed except by the operation of external influences, using the words in their widest sense?" (*Essays*, i., p. 424). And in his recent work he says (ii., p. 137), when speaking of some of the influences of external conditions: "I call this form of germinal variation

'induced' germinal selection, and contrast it with 'spontaneous' selection, which is caused not by extra-germinal influences, but by the chances of the intra-germinal nutritive conditions."¹

Thus it is in part these "induced" changes in the ids of the germ-plasm that, according to Weismann, produce the "variability" without which, as Darwin said, "selection does nothing." This direct action of external conditions upon the germ-plasm is, in fact, freely admitted. After speaking of the effects of warmth and cold on the pupæ of certain butterflies, Weismann says (ii., p. 273): "It is thus intelligible that somatic variations like the blackening of the wings through warmth appear to be directly inherited and accumulate in the course of generations; in reality, however, it is not the somatic change itself which is transmitted, but the *corresponding variation evoked by the same external influence in the relevant determinants of the germ-plasm* within the germ-cells—in other words, in the determinants of the following generation." Elsewhere, also, he had previously said:² "Many climatic variations may be due, wholly or in part, to the simultaneous variation of corresponding determinants in some parts of the soma and in the germ-plasm of the reproductive cells." This double simultaneous action seems first to have been suggested by Francis Galton, though it was more definitely expressed later by Cope, who gave to the process the name of "Diplogenesis."³

Here we have surely the plainest admission from Weismann himself of the very occurrence of the matter in dispute—that is, the transmission of acquired variations. The thing itself is

¹ "Amphimixis as the means by which a continued new combination of variations is effected," need not be considered here, since this, as Weismann says, is not "the real root of variation" (ii., p. 194).

² *The Germ-Plasm*, Contemporary Science Series, 1893, p. 406; see also *Evolution*, ii., p. 272.

³ *American Naturalist*, December, 1889, published in 1890.

¹ *Animals and Plants*, i., p. 7.

² *The Evolution Theory*, ii., p. 266.

admitted to occur, and it is a matter of altogether minor importance whose hypotheses are capable of giving the best explanation of the mode in which it is brought about. Weismann assumes that the change must have been initiated in the germ-plasm itself.¹ But whether that is true, and whether production of the new characters in plant or animal must of necessity be *initiated* in the germ-plasm, anterior to correlated changes in the soma, rather than *induced* in the germ-plasm simultaneously as a result of the operation of the same causes which produce the definite changes in the soma, is a question, perhaps, of some theoretical interest (though practically insoluble); but it is not the question with which we are immediately concerned, and is of little or no importance, since Weismann himself now admits the diplogenic process.

Irrespective, then, of the mode in which it is brought about, we see that Weismann frankly admits the whole point in dispute—namely, that *acquired characters can be and are frequently inherited.*

The plasticity of plants has long since been shown to be most remarkable, alike from the extent and the rapidity of the variations that have been induced. References to many striking changes of this kind will be found in an article on "Recent Science," by Prince Kropotkin.² Thus, he says, Vilmorin obtained the cultivated carrot out of the wild one in five generations. Carrière did the same with the radish, and Buckman the same with parsley. While, in a previous article,³ the same writer had cited numerous experiments showing remarkable alterations induced in plants by changes in their environment: transferring them, for instance, from the lowlands to mountain elevations; subjecting

others to a deficiency of light; or exposing them to a very dry air, and so producing prickles by gradually diminishing the size and development of the leaves. In this latter case, the growth of plants in different degrees of moisture of itself seemed to produce the structures best adapted for avoiding excessive evaporation. So that, as was said by Kropotkin, "several adaptations which were considered as slowly accumulated accidental variations can be obtained very rapidly—as a direct result of environment itself." Vernon, indeed, in his work on *Variation in Animals and Plants*,⁴ feels warranted in saying that "adaptability would seem to be a fundamental property of protoplasm"; and in the pages indicated he cites numerous cases in illustration of this view from experiments of different kinds made with plants. It is true that in some of these cases the question arises whether the indirect influence of external conditions—that is, "natural selection"—may not also have been operative (*loc. cit.*, p. 391).

Apart, however, from the question of adaptability, we must not lose sight of the remarkable changes recorded in the last chapter in illustration of "mutation." These changes would, of course, be regarded by Weismann as due to what he calls "spontaneous" changes in the germ-plasm. But no distinct boundary-line can be drawn between cases of "induced" changes in the germ-plasm and those which he speaks of as being "spontaneous." Has he not himself said that *all* such changes "have their roots in external influences"? And in the case of the various new species of the Evening Primrose, as well as in that of the black-shouldered Peacock, we have notable instances of acquired characters of a "spontaneous" order being transmitted to succeeding generations.

It is, as I have said, altogether a matter of subordinate interest whether the change in external conditions acts first

¹ *Essays*, i., p. 426.

² *Nineteenth Century and After*, 1901, vol. 50, pp. 424-438.

³ *Nineteenth Century*, April, 1894, pp. 688-691.

⁴ *International Science Series*, 1903, pp. 373-378.

upon the soma, or first upon the germ-plasm, or simultaneously upon both.

We have, however, other evidence bearing upon this latter point, which must not be lost sight of. Thus, that heritable variations may be created in, and transmitted by, the soma only seems to be shown by the phenomena of "pelorism," to which reference was made in the last chapter (p. 64), and of bud-variation generally. These, or at all events the majority of them, appear to be local effects not dating from the germ-plasm at all, but from the soma, and that often at a comparatively late stage of its development. The occurrence of such mere local changes was fully recognised by Spencer as not necessarily taking their origin in the germ-plasm, since, when speaking of his physiological units, he said (i., p. 369) they must be presumed to have "such natures that, while a minute modification, representing some small change of local structure, is inoperative on the proclivities of the units throughout the rest of the system, it becomes operative in the units which fall into the locality where that change occurs." And, in regard to what is known as "irregular peloria," Maxwell Masters² makes the definite statement that this form of "peloria is evidently not congenital, but occurs at a more or less advanced stage of development."

Bud-variations generally are now known to be excessively numerous. As far back as 1895, according to Professor Bailey, of Cornell University, no less than three hundred of such bud-sports were known, and actually under cultivation, in the United States.

There seems, therefore, absolutely no room for doubt that local variations may take their origin in the soma, and may be perpetuated, selected, and increased therein, in the form of bud-variations. It seems equally clear that in other cases we may have actual new species of plants arising by "mutation," as a result of *quasi*-"spontaneous"

changes,² either in the germ-plasm or in the soma; and that in other cases still under the influence of altered External Conditions—whether acting primarily upon the soma or upon the germ-plasm, or concurrently—we may find variations arising in plants and animals which are capable of being inherited and transmitted to their progeny. The evidence, as we have seen, is equally clear also in favour of the heritable effects of Use and Disuse.

There is thus reason to believe that we have these other important factors for ever co-operating with Organic Polarity and Natural Selection as causes of Evolution, and as producers and maintainers of new organic forms. And if such causes have, during the habitable life of our globe, ever been in operation, the rate of Evolution must have been very notably greater than that which could have resulted from the sole action of a process like Natural Selection—which from its very nature is one that must of necessity produce its changes slowly and gradually, through many generations. As Darwin said, "it can act only by very short and slow steps." For such a process alone to achieve all that has been brought about in the way of organic Evolution would, as I have already intimated, require prodigious periods of time far exceeding anything that geologists would demand, or which physicists are prepared to grant.

Yet we find Weismann an implicit believer in the potency and actuality of this one cause. He holds, as we have seen, that "there is no such thing as a 'tendency' of the protoplasm to vary," and its probably varied isomeric changes are never once referred to. He appeals to chance variations in the nutrition of his "determinants," due to irregularities in the food supply that reaches them within the microscopic granules of which

² Possibly as a result of isomeric changes in the constitution of the living matter in the part in question.

² *Vegetable Teratology*, p. 229.

the chromosomes are composed, and regards "natural selection" as an all-sufficient cause of organic Evolution. I, on the other hand, am impressed with the intrinsic mutability of living matter and its tendency to assume innumerable isomeric states; and see in this fundamental property of protoplasm a quality which would cause it to vary in form under the influence of its own changing states and "molecular polarities"; which would make it sensitive to all external influences, whether acting directly or indirectly; which would enable it to respond to the effects of use and disuse,

and make it prone, from slight, often imperceptible, causes, to undergo *quasi*-spontaneous isomeric variations, leading, according as the organism is low or high in the scale of being, either to actual heterogenetic transformations or to comparatively slight specific "mutational" variations. The conjoint co-operation of causes and influences of this kind, acting not only now but during all geologic ages, would make it no longer necessary for biologists to require that between two and three thousand millions of years have passed since living things first appeared on the surface of the earth.

VIII.

CONCERNING THE PRESENT OCCURRENCE OF ARCHEBIOSIS

EVOLUTION implies continuity and uniformity. It teaches us to look upon events of all kinds as the products of continuously operating causes—it recognises no sudden breaks or causeless stoppages in the sequence of natural phenomena. It equally implies that natural events do not vary spontaneously. It is a philosophy which deals with natural phenomena in their widest sense—in the universe as a whole, and not merely in our solar system: it embraces both the present and the far-distant past. It assures us that the properties and tendencies now manifest in our surrounding world of things are in all respects similar to those which have existed in the past.

Whether we follow the philosopher or the geologist in his bold speculations concerning the past, or listen to the biologist making his predictions as to the future stages which the germ of a given animal will pass through in the

progress of its development, in each case the "uniformity of nature" is tacitly assumed. This assumption underlies almost all our thoughts and actions, even in every-day life. And without such a belief regarding the succession of events science would be impossible—the very idea of it, in fact, could never have arisen. In the absence of "uniformity" we could neither fathom the past nor illumine the future.

The properties and chemical tendencies of material bodies have, in fact, been found to be quite constant through time and space. Speaking upon this subject in a celebrated discourse on "Molecules," Professor Clerk-Maxwell said:² "We can procure specimens of oxygen from very different sources—from the air, from water, from rocks of every geological epoch. The history of these

² *Nature*, September 25th, 1873, p. 440.

specimens has been very different, and if, during thousands of years, difference of circumstances could produce difference of properties these specimens of oxygen would show it.....In like manner we may procure hydrogen from water, from coal, or, as Graham did, from meteoric iron. Take two litres of any specimen of hydrogen, it will combine with exactly one litre of any specimen of oxygen, and will form exactly two litres of the vapour of water.....Now, if during the whole previous history of either specimen, whether imprisoned in the rocks, flowing in the sea, or careering through unknown regions with the meteorites, any modification of the molecules had taken place, these relations would no longer be preserved.....But we have another, and an entirely different method of comparing the properties of molecules. The molecule, though indestructible, is not a hard, rigid body, but is capable of internal movements, and when these are excited it emits rays, the wave-length of which is a measure of the time of vibration of the molecule.....By means of the spectroscope the wave-lengths of different kinds of light may be compared to within one ten-thousandth part. In this way it has been ascertained, not only that molecules taken from every specimen of hydrogen in our laboratories have the same set of periods of vibration, but that light having the same set of periods of vibration is emitted from the sun and from the fixed stars.....We are thus assured that molecules of the same nature as those of our hydrogen exist in these distant regions, or at least did exist when the light by which we see them was emitted."

With evidence such as this before us, which could easily be multiplied to an enormous extent, we should hesitate before needlessly postulating any infringement of the uniformity of natural phenomena. But to assume, as the great majority of Evolutionists do, that Archebiosis, or the natural origin of living matter, took place once only in the remote past, and that it has not been

repeated, or, if repeated in past times, that it no longer goes on, is to look upon this process as a kind of natural miracle, and to postulate a break in "continuity" which ought only to be possible in the face of overwhelming evidence of its reality. This latter is, however, as I contend, practically absent.

For living matter to have come into being in the remote geological past, when nothing of the kind had previously existed, must have been a far more difficult thing than for it to arise *de novo* now, and during past ages, after living things had been plentiful on the face of the earth. Originally there would have been no organic compounds in the waters, diffused from pre-existing living things, such as commonly exist at the present day. Some suppose that matter of this sort would have had to come into being by natural agencies before the next step in complication could occur; that is, the formation of living matter itself. This, however, as we shall see, is extremely doubtful.

Two of the most consistent Evolutionists from my point of view have been Haeckel as zoologist and Carl Nägeli as botanist. Both of them, but especially the latter, believe that Archebiosis is a process that has been many times repeated. In regard to the original process itself and our ignorance of the steps by which it occurred, Nägeli says: "The origin of the organic from the inorganic is, in the first place, not a question of experience and experiment, but a fact deduced from the law of the constancy of matter and force. If all things in the material world are causally related, if all phenomena proceed on natural principles, organisms, which are formed of and decay into the same matter, must have been derived originally from inorganic compounds."

What the actual steps of the process were in the past, or what they are now, if living matter is still being formed from its elements, is absolutely unknown. Pflüger, in 1875, put forward the notion that cyanogen and its compounds may

have been formed when the earth was in a state of incandescence or great heat, and that changes of this kind may have formed the starting-point from which much more complex molecules were ultimately produced. The cyanogen-radicle is, in fact, believed by him to be always present in protoplasm and to be one of the things which mainly helps to confer its characteristic properties upon protoplasm. Pflüger, indeed, says that cyanic acid might almost be described "as a semi-living molecule," owing to its tendency to grow by concatenation of atoms.

But there is another much simpler possibility that must not be lost sight of. We have seen in Chapter III. that living matter grows freely in, and that it is capable of building up its own substance from, a simple solution of ammoniac tartrate in distilled water—and still more freely when a little phosphate of soda has been added to the solution. Nitrate of ammonia, moreover, is known to be produced in the air during thunderstorms. And this nitrate of ammonia, so formed, together with water, carbonic acid, and a few other simple saline ingredients, are, as is well known, the materials which plants of all kinds use for the building up of their own substance.

When we find, therefore, that Bacteria and *Torulæ* can grow freely in a simple solution of ammoniac tartrate, and still more freely with the addition of a minute quantity of sodic phosphate, the view seems forced upon us that the synthesis of living matter may be, in its lowest phasis, a much simpler process than has hitherto been imagined. We find Bacteria which have been previously living a totally different life in organic fluids, when introduced into this simple solution, which may be almost described as an inorganic mixture,* at once altering their life processes in a fundamental manner, and forming their protoplasm in some mysterious way from the mere elements of the saline solution. It

would seem, therefore, that the process in this case cannot be a very complicated one; and when we think of the intimate relations existing between Origination and Growth in simpler forms of matter, and the examples that have been given in a previous chapter (pp. 32, 38), we are encouraged to hope that some conditions may be at last discovered under the influence of which it may be freely admitted that living matter will take its origin, as well as merely grow, within some simple saline solution.

Does not the very fact that origination and growth so often occur simultaneously in the case of crystalline matter, and under the influence of the same causes, show us that the two processes are intrinsically similar, and that conditions favourable for growth are also likely to be favourable for origination? And if this be true for crystalline matter, may we not infer that it should also be true for living matter? These are questions usually neither asked nor answered in any definite manner. They are, however, by no means unworthy of an attentive consideration. When Bacteria are introduced into the ammoniac tartrate solution, unfortunately we have no means of knowing whether origination, as well as growth and multiplication, occurs. We do not know for certain whether the influence of a pre-existing nucleus is relatively more potent or more necessary in the case of living matter than it is in the case of crystalline matter; but it ought not to be assumed to be so except upon evidence of a clear and unambiguous nature.

The probabilities, therefore, would certainly seem to be altogether in favour of the continuance of a natural process like Archebiosis after it had been once initiated, especially when we see how easily the "growth" of living matter from mere inorganic elements is occurring over almost all parts of the earth's surface. Let us look, however, now to the principal adverse considerations usually brought forward.

* See *Appendix*.

Much stress has been laid by certain writers upon the fact that "the doctrine of spontaneous or equivocal generation has been chased successively to lower and lower stations in the world of organised beings as our means of investigation have been improved."¹ So that, as another eminent writer says, "if some apparent exceptions still exist, they are of the lowest and simplest forms."² And it is usually inferred from this latter fact that further knowledge and improved means of observation will prove these apparent exceptions to be no exceptions to the supposed general rule—*omne vivum ex vivo*. A consideration of this kind seems to have powerfully influenced Professor Huxley.

But much confusion exists in reference to this point which needs to be removed. The absurd ancient notions on the subject of "Equivocal or Spontaneous Generation," to the effect that rats were produced from the mud of the Nile, eels from the mud of rivers generally (Aristotle), that bees could be born from the putrefying flesh of an ox (Virgil), and other similar fancies, have, of course, no bearing upon the subject at all. Then, again, under this term "Spontaneous Generation," it has been the custom to confuse two wholly distinct processes—namely, Archebiosis and Heterogenesis—which require to be considered and dealt with altogether apart. We have nothing to say concerning Heterogenesis at present; we are considering Archebiosis only, and in regard to this process no believer in Evolution could ever suppose that it had to do with the origination of any but "the lowest and simplest" organic forms.

When it is said that a belief in "Spontaneous Generation" (Archebiosis) would tend to contradict the experience of all mankind, as we are accustomed to see living things invariably proceed or take their origin from other living things, my

reply is that Archebiosis may be occurring all round us, and that from its very nature it must be a process lying altogether outside human experience, and never likely to come within the actual ken of man. So that, even if it had been given to Professor Huxley, as he said³ in a celebrated Presidential Address, to "look beyond the abyss of geologically recorded time," it would have been excessively unlikely that he would have been enabled to witness, as he intimated, an "Evolution of living protoplasm from not-living matter." At the most he might have seen then (but only if equipped with a powerful microscope) just what he might have seen in his own day under more favourable conditions—namely, a gradual emergence into the sphere of the visible, in some suitable fluid, of the minutest specks of living protoplasm.

It is scarcely conceivable for an Evolutionist to suppose that living matter could ever take origin, either now or in the past, in any other way. So that the birth of ultra-microscopical particles, gradually coming into the region of the visible, may easily be understood as a process, in its incipient stages, lying altogether outside the range of man's experience, and as one which, even in its later stages, has been seen only by a few who have specially looked for it.

In Chapter III. (pp. 34, 35) I have told all that can be seen of what appears to be the origin of particles of living matter—the possible process of Archebiosis—and have made known an easy means by which others may, if they proceed with care, observe for themselves this gradual emergence of living particles from the region of the invisible. They do not proceed from any germs which are known to science. Science knows only visible germs. Moreover, I have shown that the very minute particles which appear speedily grow into Bacteria or into Torulæ of one or other kind, and

¹ Professor (now Lord) Lister, *Introductory Lecture* (University of Edinburgh), 1869, p. 12.

² Mr. Justice Grove (Presidential Address), *Rept. of Brit. Asscn.*, 1866, p. 71.

³ *Report of the Brit. Asscn. for the Advancement of Science*, 1870.

that these forms of life are just as plastic and modifiable as we should expect them to be if they had in fact been derived from new-born particles of living matter, this being a substance which, as we have seen, has a high intrinsic tendency to variation by reason of its extreme molecular complexity.

Everything, therefore, tends to support the doctrine of Continuity, and seems to testify to the present occurrence of Archebiosis, especially when the alternative we are asked to accept is the belief that these particles which we see gradually emerging into the region of the visible, and developing into simplest organisms whose form and mode of growth varies with every slight change in the media in which they appear, have been perpetuating themselves in this humble form through hundreds of millions of years, while other portions of the same variable living matter have gradually given rise to all the higher forms of life that have ever appeared upon the earth.

It is, of course, not to be supposed that I regard Bacteria and Torulæ as the only primordial forms of life. It is possible that the simplest forms of Algæ—especially the non-nucleated Chromacea—may also be primordial living things. In all mountainous countries, and wherever large surfaces of rock are bared, we may constantly see great black patches, often many yards in diameter, looking, at a distance, like patches of paint. On close examination we may find that such a patch is composed of a thin, blackish, granular layer, most closely adherent to the surface of the stone, which is otherwise quite bare. Portions of such a limestone rock thus coated have been brought home by me from the Pyrenees, and microscopical examination of some scrapings from the black patch, after soaking in water for a short time, has shown it to consist of an aggregation of minute purple algaoid vesicles, some extremely minute and others about $\frac{1}{100}$ in diameter, mixed with a smaller number having an orange or red-brown

tint. They were all, however, simple Algaoid cytodes, presenting no trace of a nucleus, nor were filaments of any kind to be seen among the vesicles.

We have taken the question of the present occurrence of Archebiosis as far as it can be taken from the point of view of logic, of continuity, and of actual observation; so that we now have to turn to another aspect of the question—namely, to a brief consideration of some of the latest experimental trials that have been made, in attempts actually to prove that a natural origin of living matter is still possible under this or that experimental condition.

On this subject Weismann says (*Evolution*, ii., p. 366): "After the fiery earth had so far cooled that its outermost layer had hardened to a firm crust, and after water had condensed to a liquid form, there could at first only have been inorganic substances in existence. In order to prove spontaneous generation, therefore, it would be necessary to try to find out from what mingling of inorganic combinations organisms could arise; to prove that spontaneous generation could never have been possible is out of the question.....It would be impossible to prove by experiment that spontaneous generation could *never* have taken place, because each negative experiment could only prove that life does not arrive *under the conditions of the experiment*. But this by no means excludes the possibility that it might arise under other conditions."

What he says in the last two sentences is, of course, quite true; but what he said previously narrows the conditions of the experiment overmuch. We do not now only want to prove the problem as it presented itself in the past, when there were nothing but "inorganic substances in existence." However interesting and important that problem may be, our first and more immediate interest is to know whether Archebiosis can be proved to take place now, under the easier conditions provided by the plentiful

existence of organic matter in solution.

Although experiments have been made in both directions, still the trial has, for the most part, been made with solutions or infusions containing organic matter; and, owing to the doubts and difficulties inseparable from the problem, it must be confessed that the results of these experiments have not been such as to carry conviction to the minds of men of science generally. Many have been led, in fact, to attach more weight to the supposed non-success of the experimental evidence than there was any real warrant for; and, not mindful of the strict limitations of the scope of such evidence, as indicated above by Weismann, have seemingly allowed their judgments to be unduly prejudiced in regard to the general question. From this point of view the influence in this country, during the last generation, of Huxley, of Tyndall, and of Herbert Spencer, and in France of Pasteur, has been altogether unduly to decry the very possibility of the occurrence of Archebiosis. Only one philosophic thinker in this country during that period seemed to feel the full weight of the general evidence. That was George Henry Lewes, who wrote as follows: "All the complex organisms are evolved from organisms less complex, as these were evolved from simpler forms: the link which unites all organisms is not always the common bond of heritage, but the *uniformity of organic laws acting under uniform conditions*.....It is, therefore, consistent with the hypothesis of Evolution to admit a variety of origins or starting-points."

The requirements underlying all attempts to prove the "spontaneous generation" of living matter by flask experiments with superheated fluids are of two kinds. We have (a) to ascertain as far as possible, by preliminary trials, the lowest amount of heat, and the duration of its application, which would be necessary for the destruction of all

pre-existing living things within the vessels and fluids with which experiment is to be made. And we have (b) to see whether it is possible that fluids which have been submitted to the *lowest necessary amount of heat* to ensure the destruction of all pre-existing life can still, by subsequent treatment under what are (in our experiments) at the best only very unfavourable conditions, be induced to engender living matter.

Everything, of course, turns upon these words, "lowest necessary amount of heat."

The destructive and deteriorating influence of heat upon the organic fluids with which experiments have to be made must be obvious to all; and the ultimate deteriorating results may not be very different where recourse is had to one process of heating at a high temperature, or to several successive heatings at rather lower temperatures, as in the "discontinuous process" adopted by Tyndall. No one need doubt the truth of the naïve remark of Fischer¹ when he says: "Infusions of organic substances, if they be only boiled long enough, will remain sterile for years."

The essential question is, therefore, What is the lowest necessary amount of heat to ensure, on the one hand, that all pre-existing living things shall be killed, and to avoid, as far as possible, extreme deterioration by heat of the fluids employed. It has now been thoroughly brought home to one how almost impossible it is to secure a stable and generally accepted conclusion in regard to this question. In death-point experiments, as such trials may be termed, the subsequent barrenness of the fluid *may* merely mean that the organisms are inert rather than dead. This must be admitted as a possibility; and, moreover, judging from my own experience and the past history of this question, I feel certain, even if there were a new basis of agreement in regard to the death-point of micro-

¹ *Fortnightly Review*, 1868.

² *The Structure and Functions of Bacteria*, trans., 1900, p. 51.

organisms in any particular fluid, that this common point of agreement would be immediately renounced by many directly some experimenter claimed that from such a basis he had demonstrated the "spontaneous generation" of living matter. He would doubtless be told that "spontaneous generation" was a chimera, and that he had only succeeded in resuscitating germs which all his predecessors had hitherto believed to be dead. The proof of this assumption would, for them (as believers in the "chimera" hypothesis), lie in the simple fact that living things had appeared within the experimental vessels. Such see-saw work might go on indefinitely. Conviction would probably never be brought about, because this ever-recurring objection could not possibly be met, unless the experimenter confined himself to sterilising degrees of heat which would allow of little chance of success in the second stage of the inquiry—viz., the attempt to coax such sterilised fluids to engender units of living matter. The last experiments I published on this subject with organic fluids, together with this kind of interpretation of their results given by others, were of such a nature as to convince me of the futility of further attempts in this direction. They tended to show the practical insolubility of the question by flask experiments with organic fluids, because of the immediate renunciation by opponents of previous statements and beliefs whenever results obtained would otherwise show that "spontaneous generation" had been demonstrated. For that reason I ceased to make and publish further experiments of this kind with organic fluids. As a full account of this side of the question is to be found in my recently published work, *The Evolution of Life* (1907), it will only be very briefly dealt with here.

Anterior to the publication, in 1876, of my experiments on the fertilisation of boiled acid urine by an accurately neutralising dosage with boiled liquor

potassæ,¹ it had been generally inferred by all workers on this subject (including Pasteur, Lister, Roberts, Tyndall, and others) that the continued barrenness of urine and other acid fluids previously boiled for only a few minutes was a *proof* that heat to the extent employed was adequate to kill any organisms or their germs pre-existing in such fluids.

This view was based upon the fact that living Bacteria or their germs seemed invariably to increase and multiply in suitable fluids placed under conditions favourable for fermentation, together with the invariable absence of any such evidence of life in acid urine which had been boiled even only for three or four minutes.

It must be admitted, however, that this assumed proof was fallacious. Life may persist even when there is not sufficient vigour in the living things to enable them to grow or multiply in the previously heated medium in which they find themselves.

And as long as such inert or partially devitalised organisms remain in certain fluids, they may never regain sufficient vigour to enable them either to grow or to multiply. They may continue as if dead, though not actually dead.

But in other fluids of a different chemical constitution, or nutritive value, it is certainly possible that such partially devitalised organisms may become reinvigorated (especially if their revival be further aided by more stimulating conditions) so as to enable them again to grow and multiply. In this way it must be admitted as possible that life may be made to appear in previously superheated fluids simply because organisms, hitherto assumed to be dead, have been made, by the means adopted, to show themselves as living.

Still, the belief that the barrenness of a superheated fluid (favourable in nature and favourably placed for the promotion of fermentation) was due to the fact that

¹ *Proceed. of Roy. Soc.*, 1876, vol. xxv., p. 149, and *Journ. of Linn. Soc. (Zool.)*, vol. xiv., 1877.

all its contained organisms and their germs were killed had been commonly accepted as true, and the results of experiments of such an order had been cited as an important portion of the evidence and arguments tending to foster the belief, enunciated by Pasteur, that "spontaneous generation" was a chimera. My successful experiments with boiled acid urine, subsequently neutralised by liquor potassæ, at once seemed to necessitate the rejection of this belief by those who had previously proclaimed it. I was, in effect, promptly told that the organisms which all previous experimenters had spoken of as being killed in boiled acid urine could not have been killed. I had, by subsequent neutralisation of the fluid, and by exposing it to a far more stimulating incubating temperature (50° C.) than had been employed by previous experimenters, succeeded, they in effect said, in bringing to life organisms which everyone else had supposed to have been killed. It was this, rather than Archebiosis, which accounted for the swarms of Bacteria that speedily appeared within my experimental flasks—developed, it was assumed, owing to the survival of dried germs, in the form of old Bacilli spores.

But many years ago, after what I considered to be some very extravagant statements by Tyndall concerning the powers of resisting heat possessed by, what were for him, hypothetical germs contained in "old hay," I made a long series of experiments with multitudes of dried hay germs (that is, with spores of the hay Bacillus) which had been allowed to dry in their mother liquid on glass slips five years previously. I tested the death-point of these old Bacilli spores in acid urine that had been boiled, in which I knew that Bacilli or any such organisms did not originate; though I had ascertained that the inoculation of boiled urine with these spores, would cause Bacilli to grow and multiply therein most freely. An emulsion of the old spores was made by scraping the dried scum from the glass slips into a

small quantity of water, followed by a vigorous shaking. Drops of this emulsion were then added to some acid urine, and this inoculated fluid was subsequently boiled for twenty minutes, with the result that the spores invariably *seemed* to be killed—seeing that the fluids remained unchanged.

Thus boiling for twenty minutes in a slightly acid fluid seems to destroy multitudes of the actual dried spores, while boiling for five, or even more, hours is said to be necessary in a certain neutral fluid (hay infusion) to ensure the destruction of similar spores possibly present. This may be true; but, on the other hand, spores actually existing in the hay infusion may be much more speedily killed; and it may be the power of giving birth to organisms (the "germinality" of the fluid) which requires the five hours at 100° C. for its extinction.

It is true that there is good evidence (such as Chamberland advances,¹ and of a kind to which I had long ago called attention) to show that growth and multiplication of Bacilli does not take place so freely or quickly in slightly acid as in slightly neutral solutions. But then, might not, even ought not, the same difference to hold good in regard to the possible origin of living units in the respective fluids? The conditions less favourable for growth should be likewise less favourable for origin, and *vice versa*.

The difference between twenty minutes and five hours of exposure to 100° C. is enormous; but the difference between the rate of growth and multiplication of Bacilli in slightly acid and in slightly neutral fluids respectively, however real, is comparatively insignificant.

Hence the question of the possible germinality of the hay infusion cannot be ignored. But here again, as ever, we cannot completely get rid of the

¹ *Recherches sur l'origine et le développement des organismes microscopiques*. Thèse, No. 420, 1879. (Faculté des Sciences, Paris.)

doubt that in my death-point experiments the spores were not really killed by the twenty minutes' boiling in the acid urine. Chamberland might retort that they may have been merely rendered inert by the heat so long as they remained in this fluid. To attempt to get rid of this difficulty is to come face to face with its correlative—namely, How are we to decide, when dealing with other more favourable fluids, whether an appearance of organisms therein is to be ascribed to *survival of germs*, or to *germinality of the fluids*?

The tendency with Pasteur, and with Tyndall and others, has been too much altogether to ignore this latter possibility—to regard it as a “chimera,” and one not to be seriously considered. But this, as was well said by Rücker in his Presidential Address before the British Association in 1901, concerning the one-sided attitude of some in reference to a fundamental physical problem, is “to beg the whole question at issue; to decide the cause before it has been heard.”

It is worthy of note, however, that my opponents in this question have invariably assumed, rather than proved, various points needful to give adequate warranty to their interpretation of the appearance of living organisms in critical flask experiments.

They know quite well that spores are distinctly more resistant to heat than the parent organisms; they know that the parent organisms without spores are almost infinitely more common than such organisms with spores; they know that even under most favourable conditions such spores will often refuse to develop; yet whenever living organisms appear in the guarded fluids, which have been heated far more than is necessary for the destruction of the parent organisms, they invariably assume that the much rarer spores have been present, and they further find it necessary to assume that the spores which are often most slow to develop under favourable conditions now, in spite of the injurious heating and unfavourable conditions in

which they are placed, straightway hasten to develop, to grow, and to multiply within the experimental vessels. This is surely more like begging the question at issue than judging in accordance with evidence.

In addition to the above considerations, it may not be without interest to some that attention should be called to the following additional facts, seeing that they may help to show in which direction the balance of evidence at present lies.

(a) *Thermal death-points and their relation to certain old flask experiments.*

The dicta at present given in regard to thermal death-points are these. Pasteur formerly said that a brief exposure to 110° C. was sufficient to sterilise all fluids; but now Chamberland, his former assistant, but the present sub-director of the Pasteur Institute, as a result of investigations made between 1877 and 1879, and recorded in the thesis above referred to, has been induced to put the temperature rather higher. He says, however, that a temperature of 115° C. is sufficient to sterilise all fluids “completely and very rapidly,” and therefore to kill any Bacilli spores that may be contained therein. While de Barry¹ says, in reference to the death-point of the spores of Fungi in water or watery vapour, “it has not been shown that any can, under these circumstances, survive a temperature of 100° C.”

I have, however, recorded experiments in which Bacilli and Torulæ have been plentifully found in organic fluids that had been heated within closed vessels to higher points than any of those above mentioned as lethal for such organisms and their germs.

Thus, experiments were made with neutral and faintly alkaline hay infusions of this nature.² “In each case about

¹ *Fungi, Mycetozoa, and Bacteria*, p. 347.

² See a Memoir “On the Conditions Favouring Fermentation,” *Journ. of Linn. Soc. (Zool.)*, vol. xiv., pp. 50-53.

half-an-ounce of the fluid was used, half filling a tube which was sealed when cold; so that above the fluid there was ordinary air.....In twenty-one cases they were heated to 248° F. (120° C.) for thirty minutes; and in five of these latter trials (all with the same hay infusion) no fermentation subsequently occurred. In the other instances more or less distinct fermentation supervened, though in some the signs of change before opening the vessels were only slight.¹.....Also in five other experiments in which *milk* was heated to 240° F. (115.5° C.) for ten minutes, fermentation more or less marked occurred in each case in from two to ten days."

The nature of the changes seen in the fluids and the kinds of organisms found therein are fully described in this same memoir in subsequent pages (pp. 53-60), and illustrations of some of the organisms met with are given.

Two points worthy of note concerning the changes occurring in such hay infusions are these. When an infusion of this kind has been heated only to 100° C., and subsequently becomes fertile, it may always be seen to change uniformly throughout its whole bulk. There is no cloudiness gradually spreading from centres of infection, but a uniform opalescence, gradually increasing to actual turbidity, throughout the fluid, which at the same time becomes gradually paler in colour. On the other hand, where this or any other clear infusion has been heated to 110° C. and upwards, such a change almost never occurs. We have then altogether different appearances. The bulk of the fluid remains clear, though, after days or weeks, a very slow and slight accumulation of matter occurs at the bottom of the vessel; and in this, on microscopical examination, in addition to different

kinds of Bacteria, it is most common to find *Torulæ* and other Fungus germs together with some Mycelia, such as I have figured as found in the experiments above referred to.

The finding, however, of living *Torulæ* and other Fungus germs, with Mycelia, in these superheated fluids, is a fact of great significance in view of the statement by de Barry (with whom others agree) in regard to spores of Fungi generally—namely, that they have never been shown to be capable of surviving in fluids at a temperature of 100° C.²

(b) *The production of different micro-organisms at will from healthy urine.*

One of the arguments of Pasteur, and also of Lister, William Roberts, and others, against the present occurrence of Archebiosis was this. They met the objection that in flask experiments with suitable fluids negative results are due to the degradation of the organic constituents in the fluids, owing to the heating of them many times to minor degrees or once to some major amount, by saying that milk, blood, or urine, if taken directly from the body, with all necessary precautions against external contamination, can be preserved indefinitely without change and without the appearance therein of any micro-organism. This, of course, is what ought to occur if such fluids are, as they maintained, germless, and if the present occurrence of Archebiosis is such a chimera as they imagined it to be.

Over twenty years ago I resolved to test these statements for myself, taking urine as the test fluid because of the greater facility for multiplication of experiments therewith. During a period of three months I made nearly two hundred experiments, using all the precautions

¹ These tubes, on account of their subsequent treatment, were necessarily of very thick glass, and were, therefore, thoroughly *flambés* when I prepared them, shortly before they were made use of.

² In his *Nouvelles expériences sur la Génération Spontanée* (1864, pp. 126 and 190) Pouchet records experiments in which beer-wort which had been boiling for five and six hours respectively gave rise in full and hermetically-sealed vessels to swarms of *Torulæ* in the course of a few days.

recommended by Lister as needful in such experiments. The urine was passed either into one of his receivers or else directly into the experimental vessels, and in all cases the vessels employed were thoroughly *flambés*. The urine experimented with had only a moderate amount of acidity, as it was always found to be capable of being neutralised by from 7-10 minims of liquor potassæ to the ounce.

The results that I obtained in the large majority of cases were divisible into four categories:—

(1) Urine passed into a *flambé* vessel and subsequently kept at temperatures ranging between 60° and 86° F. (16° and 30° C.) would, as Pasteur, Lister, and others said, remain clear and free from all signs of change.

(2) Urine passed into a *flambé* vessel and subsequently kept at a temperature of about 113° F. (45° C.) would almost invariably become turbid within three days, and on examination be found to swarm with Micrococci.

(3) Urine passed into a *flambé* vessel which had been *fully or two-thirds neutralised with liquor potassæ*, and subsequently exposed to temperatures ranging from 113° to 120° F. (45° to 50° C.) would generally become turbid in seventeen to twenty-four hours, and on examination be found to swarm with Bacilli only.

(4) The addition of lesser amounts of liquor potassæ—that is, *sufficient for half neutralisation*, or a little less or more—would generally lead to a mixture of Micrococci, Staphylococci, and Bacilli.

I found I could thus obtain, almost at will, urine containing Micrococci only, Bacilli only, or mixtures of these two organisms.

In all the experiments where liquor potassæ was used I had previously heated it in tubes to 105° to 120° C. for one hour, as I had formerly done to meet altogether baseless objections made by Pasteur in regard to other experiments.

I have little doubt that blood or milk taken from an animal with all the precautions possible, and subjected to such

temperatures as I have employed, would speedily show signs of change. I have, however, made no such experiments with either of these fluids.¹

From this brief indication of the present state of the question in regard to experimental trials the reader will have gathered something as to the nature of the difficulties besetting the attempt actually to prove the present occurrence of Archebiosis, and how almost hopeless it seems to convince persons, already firmly believing “spontaneous generation” to be a chimera, that any apparent success is not simply a case of “survival of germs.”

Still some may, perhaps, be inclined to think with me that the balance of evidence is, even here, very distinctly in favour of the occurrence of Archebiosis rather than adverse thereto; although they may also realise that this mode of experimentation with heat-degraded fluids in small vessels cannot in any way be regarded as a fair test of what may now be possible in free nature—that is, with unaltered organic fluids existing under the most favourable conditions.

There is another point also of great importance which has not previously been referred to—that is, the different ultimate questions left in the two modes by which I have endeavoured to throw

¹ Some kindred investigations with milk, described by me in the *Lancet* (Oct. 31, 1908), have, however, shown that one of the common Moulds (*Oidium lactis*) can be caused, by a very strict method, to develop in profusion from *any single drop of the purest milk obtainable*. “Thousands of torula-like bodies or conidia were to be found [in each drop with which trial was made] among the milk globules and Bacteria.” These observations are thoroughly in accord with the statements of Swithinbank and Newman, in their work on *The Bacteriology of Milk* (1903, pp 187-189), concerning the *natural changes* occurring in milk and cream, though they, like other authorities, do not attempt to trace the actual origin of the Moulds, which can, by the very simple procedure I have indicated, be caused to appear in any single drop of the purest milks. Mere chance contamination of the milk by air-borne spores cannot possibly suffice to explain such facts.

light on this question. We found (p. 34) that, so far as mere *observation* goes, Bacteria may be seen in a thin film of suitable fluid under the microscope gradually emerging from the region of the invisible: the question left here is, Have they taken origin from *pre-existing invisible germs*, or have they been formed as ultra-microscopic particles by a synthetic process, followed by growth and visibility?

On the other hand, in the *experimental* trials the Bacteria which appear are always assumed to be developed from *visible and well-known germs*, such as are produced by the hay Bacillus, and are known to be able to resist higher degrees of heat than the organisms in which they have been formed: the question that should be left here is, Can it be shown by any independent evidence that such germs (even if present) could possibly survive the preliminary heating had recourse to in the experiments related?

But this latter question is really, with most persons, replaced by a mere assumption that these known visible germs have been present, have survived destruction, and have given rise to any new birth of Bacteria that has appeared. The assumption, however, even then, covers only part of the ground—not being applicable to *Torulæ* and Fungus germs generally, such as are often found in the experimental vessels, since in them no highly resistant “spores” are known, and de Barry tells us “it has not been shown that any can under these circumstances [that is, in water or watery vapour] survive a temperature of 100° C.” The evidence I have adduced concerning the appearance of all these organisms is, therefore, altogether in favour of the *de novo* origin of such forms of life.

It thus seems that we have in reality two distinct and more or less independent methods for attacking the problem as to the present occurrence of Archebiosis—that (a) by *experiment* with superheated fluids in closed flasks; and (b) another less recognised method, that of mere *observation*, aided by high

microscopic powers, of what occurs in thin films of suitable unheated organic fluids.

This latter method calls attention to, and thoroughly exposes the fallacy of, the common belief that the occurrence of “spontaneous generation” is opposed to the universal experience of mankind. It shows that what is supposed to contradict this common experience lies altogether outside, and of necessity completely beyond; the range of ordinary human experience. Nobody with unaided eyes could ever have witnessed the birth from fluids of invisible particles, by which the “spontaneous generation” of living matter must always commence, if it commences at all. And even when aided by the most powerful microscope nobody could decide, when *minimum visible* particles appear in the field of view, that such particles have proceeded from invisible germs rather than from a primordial synthesis of living units.

But, in regard to this latter point, as I have already shown, absolutely no logical or consistent reason exists for a disbelief in the present occurrence of Archebiosis. There is, indeed, no vestige of evidence to show that, under favourable conditions, the process may not be continually taking place around us. The birth of invisible particles of living matter in fluids could not oppose or contradict the actual experience of anyone. Yet this, and this only, is what is meant by Archebiosis.

Since this book was first published, in 1905, I have adduced further experimental evidence of a very convincing kind in proof of the present occurrence of Archebiosis. This has been dealt with in detail in *The Evolution of Life* (1907); but I here reproduce, with only a few alterations, a brief account of these experiments, read before the Royal Medical and Chirurgical Society in December, 1906, under the following title:—

On the de novo origin of Bacteria, Bacilli, Vibriones, Micrococci, Torulæ, and Moulds in certain previously superheated saline solutions, within hermetically sealed vessels

During the year 1906, after a very long interval, I began again to occupy my leisure time in making experiments with different superheated saline solutions, in order to ascertain whether any evidence could be obtained of a *de novo* origin of living matter therein. I was induced to deal with such solutions for various reasons.

It seemed clear that, if living things originally appeared upon this earth solely as a result of natural processes, they must have taken origin from some such materials. Then, again, it seemed likely that such solutions might be less deteriorated than others containing organic matter by subjection to high temperatures. I was further tempted to use such solutions after discovering, during the previous year, that Bacteria of different kinds would grow and freely multiply in a simple solution of ammonium tartrate when exposed to diffuse daylight, even better than when the solutions were kept at a much higher temperature, but in the dark.¹

The recognition of this latter fact was something altogether new. It was well known previously that direct sunlight proves destructive to Bacteria, but diffuse daylight was also supposed to be injurious.² We shall find, however, that this is very far from being true.

It is clear that no experiments of this kind can be deemed conclusive until a general agreement has been arrived at as to the thermal death-point of such micro-organisms as are apt to be found within the experimental vessels. Fortunately, after years of research by workers in different parts of the world, there is at last much unanimity in regard to this important point.

¹ "The Simplest Kind of Protoplasm," *Knowledge and Scientific News*, August, 1905, p. 199. Now reproduced as an *Appendix*.

² In a lecture on "The effects of Physical Agents on Bacterial Life," at the Royal Institution in June, 1900, the late Dr. Allan Macfadyen is reported to have said (the *Times*, June 11th): "Direct sunlight was a most deadly bactericidal agent, and diffuse light was also injurious, though slower in action." See also *Proceedings of the Royal Institution*, vol. xvi., p. 451.

It is now well recognised that all microbes (including *Torulæ* and Fungus spores generally), when in their ordinary condition of active growth, are killed by exposure for a minute or two in fluids that are raised to the boiling point, 212° F.—almost all of them being, in fact, killed at distinctly lower temperatures, ranging from 140°–167° F. (60° to 75° C.).

The spores of Bacilli, however, constitute a well-known exception to this rule. Still, those of the hay-Bacillus and of others occasionally to be found in tap-waters have been declared by Chamberland, the present sub-director of the Pasteur Institute, to be all destroyed when immersed in fluids by exposure for a minute or two to a temperature of 115° C.¹

There are certain "Thermophilic Bacteria," found in soils and in some other sites, which are more resistant still; but these have never been met with in tap-water, and are certainly not to be found in freshly distilled water, such as has been employed in all the experiments of which I am about to speak.

Very full details concerning this part of the subject, which is of the greatest importance in reference to these experiments, may be found in my recent work, entitled *The Evolution of Life* (1907).

Trials were at first made with several different solutions containing ammoniacal and other salts, either alone or in combination with certain acids; but attention was finally concentrated upon some solutions very similar to those with which I had performed a few tentative experiments in 1871, in which carbon was ostensibly absent, though its close chemical ally silicon was always present.

My reason for making trials at that time with such solutions may be best explained by the following quotation from my work, *The Beginnings of Life*, published in 1872 (Vol. II., Appendix A, p. ix.):—

"Living matter being the result of a

¹ *Comptes rendus*, 1879, vol. i., p. 659.

chemical combination of a certain kind, there is no absolute improbability in the supposition that the carbon usually existing in the living compound might be replaced by some other element. With the hope of throwing some little light upon this very difficult subject, I made several tentative experiments with saline solutions containing—in addition to nitrogen, oxygen, and hydrogen—some other element in the place of carbon. The element with which the carbon was replaced was either silicon, boron, chromium, aluminium, or iron.* Except in those solutions in which carbon was replaced by silicon, no living things have been met with in any of these solutions (after they had been boiled and the necks of the flasks had been sealed during ebullition). This result—taking it merely for what it is worth—is extremely interesting and suggestive, since silicon is certainly the element which most closely resembles carbon, and which might, therefore, best replace it in compounds otherwise similar to those which constitute the basis of living matter."

Reference was then made to an observation of my own, in which a mass of Mould was found growing luxuriantly on the surface of some silicate of soda solution, contained in a corked bottle which had been unopened for about six months. Attention was also called to the previous observations of Messrs. Roberts (afterwards Sir Wm. Chandler Roberts) and Slack on solutions of hydrate of silica, as recorded in the *Quarterly Journal of Microscopical Science* for 1868 (pp. 105-108), in which they say: "All the specimens of silica solution supplied by Mr. Barff to Mr. Slack, whether kept in bottles nearly full and corked, in bottles containing much air, or in open vessels, exhibited the mildew threads in the course of a week or ten days." After relating some further

* As I have already stated, these experiments were merely tentative. It is not supposed that solutions were employed free from all trace of carbon, existing as an impurity.

observations and experiments, they say, in conclusion: "The preceding experiments show the facility with which moulds will grow in a solution of pure silica in distilled water."

These observations proved plainly enough that solutions of silica were favourable nourishing fluids for the *growth* of living things; and this induced me to see whether any evidence could be obtained tending to make one believe that an actual *origination* of living matter could take place in such solutions.

Some few experiments were made at that time, and the results were such as to induce me to make further trials during the early months of 1906; and these experiments soon convinced me that evidence of a remarkable kind was to be obtained.

I propose, therefore, now to refer to some of the results of experiments with three different solutions containing silica, two of which are closely alike. Their composition is as follows:—

- (A) Sodium Silicate, three minims.
Ammonium Phosphate, six grains.
Dilute Phosphoric Acid, six minims.
Distilled Water, one fluid ounce.
- (B) Sodium Silicate, four minims.
Ammonium Phosphate, four grains.
Dilute Phosphoric Acid, four minims.
Distilled Water, one fluid ounce.
- (C) Sodium Silicate, three minims.
Liquor Ferri Pernitratis, eight minims.
Distilled Water, one fluid ounce.

About half-an-ounce of one or other of such solutions has been used in each experiment. The tubes employed have been mostly composed of soft German glass, about 1 inch in diameter and three inches long, with a tapering shoulder rather more than an inch in length beyond the body of the tube.

During the process of making, such tubes would, of course, be thoroughly *flambés*, and the narrow extremity of each was always closed at this time. In this sterilised and closed condition the tubes were delivered, and were thus

always retained by me till they were about to be used.

In the case of the A and B solutions the ammonium phosphate is first allowed to dissolve, with the aid of a little heat, in the distilled water; the acid is then added, and next the sodium silicate.² Such a solution is clear, and shows no precipitate. It is introduced into one of the tubes, which has just been opened by filing off its tapering extremity; and the tube is then at once securely re-sealed in the flame of a Bunsen's burner.

In preparing the C solution a small amount of a pale yellowish precipitate of silicate of iron is produced as soon as the solution is made.

Four or five of the sealed tubes are introduced into a can of water, if they are to be heated to 100° C. only. The temperature of the water is gradually raised to the boiling point, and the boiling is prolonged for ten minutes, when the tubes are taken out with a wooden forceps and allowed to cool.

When temperatures between 115° and 130° C. (239° and 266° F.) have been employed a calcium chloride bath has been prepared which boils at the desired temperature, and a thermometer is fitted to the can so that its bulb is immersed in the mid-strata of the fluid. The tubes are placed in the bath as before when cold. The temperature of the bath is then raised to the desired point, and with a little care can be easily maintained at that temperature for ten or twenty minutes, as may be desired.

When the tubes have cooled somewhat they have been cleaned externally, labelled, and then either exposed to diffuse daylight or else placed in an incubator at about 35° C. (95° F.).

Most of these experiments were performed during the warm and bright weather which occurred in the summer

and up to nearly the end of September, 1906; and, from a comparison of their results with those of some other experiments made during the months of September and October (in which exposure to light was had recourse to as the favouring stimulus), the advantage derivable from warm and bright weather was very plain.²

As a rule, tubes containing the A or the B solution should be exposed to light or else to heat in the incubator for four to six weeks; while, when the C solution is being used, they should be exposed to light or to heat for distinctly longer periods—that is, for from three to four, or even six, months, if positive results are to be frequently obtained.

These are, in themselves, facts of great significance in regard to the interpretation of the results; since, when living organisms are found to have been taken from the tubes, the questions to be determined must always be: (1) Have the organisms descended from others pre-existing within the experimental vessels, and not destroyed by the previous high temperature to which the tubes and their contents have been submitted?—or (2), Are they organisms that have been actually engendered within the experimental vessels, after the process of heating to which they and their contents have been exposed?

In reference to these questions, it must be steadily borne in mind that where the organisms found are Bacteria, Micrococci, Torulæ, or Moulds, and the sterilised experimental vessels, together with their contents, have been exposed to temperatures of 100° C. and upwards for ten minutes, we have no right to suppose that any survival could occur. And, similarly, where the organisms found within such tubes have been

² This latter ingredient (sold as a dense liquid) is best kept for use diluted with an equal bulk of distilled water, as in that condition the proper quantities to be used (now doubled) can be more easily measured.

² The tubes were exposed during day and night inside an open north-east window, so that direct sunlight never reached them, and actinic rays were not cut off from the solutions by the window-glass over and above the loss that was inevitable during the passage of the light through the walls of the tube itself.

Bacilli, and the tubes and their contents (looking to the nature of the latter) have been exposed to 115° C. and upwards for ten minutes—and still less when they have been exposed to 130° C. for twenty minutes—we should have absolutely no right, in the existing state of knowledge, to suppose that Bacilli spores, fresh or dry, could have survived such an ordeal.

This fact of the now admitted thermal death-point of the organisms in question is the first all-important consideration in reference to the interpretation of the experimental results when living organisms are found. But another consideration which, in the light of existing knowledge, is of scarcely less importance is this. In the A and B solutions, after the process of heating, more or less silica is deposited, leaving the fluid above quite clear; while in the C solution the amount of the pre-existing deposit is slightly increased. Now, if within an hour or two, or a day or two, after these deposits have undergone the process of heating, some of the tubes are opened, as "control" experiments, and portions of the flakes of silica are withdrawn and carefully examined, no trace of organisms is to be found; yet a similar careful examination of the sediment from other tubes that have been favourably exposed during an interval of several weeks or months may reveal the fact that in such sediment different kinds of living organisms are more or less plentifully present.

The force with which such facts tell in favour of *de novo* origination as against mere "survival of germs" is extremely great, when we consider that in the case of an organic infusion, if organisms appear as a result of contamination, the fluid in the course of a few days becomes cloudy, or actually turbid, owing to the rapid growth and multiplication of the Bacteria. Yet in these sodium silicate solutions the supernatant fluid invariably remains perfectly clear, month after month, even though Bacilli, Micrococci, or *Torulæ* may be swarming

in or upon the flakes at the bottom of the tube—where at first, and for some long period, no such organisms are to be found.*

Much more time, therefore, seems needed for the origination of living matter to occur than suffices for the mere growth and multiplication of pre-existing germs.

Another very important point is this. In the organic infusions a large proportion of the organisms found will exhibit very active movements, but in the sodium silicate solutions the organisms are *invariably motionless*, scattered through or upon the flakes, and apparently originating where they are found. This holds good for *Torulæ*, as well as for Micrococci and Bacilli, and must be regarded as a very significant fact when we see these motionless organisms occurring singly, as well as in small groups, throughout the flakes. When hundreds, and often thousands, of such motionless organisms are thus to be seen; and when it is borne in mind that no organisms whatever are to be found within other of these tubes (serving as "control" experiments) if their contents are examined within a day or two after they have been heated, what conclusions are we to draw? If organisms are not there at first, after the process of heating, and if, after an interval, they are there in abundance and are invariably stationary, clearly we must have to do with living organ-

FIG. I.



Appearance of a tube after it has been heated.

isms are not there at first, after the process of heating, and if, after an interval, they are there in abundance and are invariably stationary, clearly we must have to do with living organ-

* When, however, one of these saline solutions is purposely inoculated with living organisms, they are then to be found plentifully multiplying on the flakes in the course of two or three days.

isms that have developed and multiplied in the sites where they are found.

Is there any chance of contamination having taken place after the heating? None. The procedure has been this. After an interval of weeks or months, the end of the tube has been cut off, and if the orifice is narrow some of the deposit is shaken out on to a clean microscopic slip, which has just been sterilised by passing it four or five times through the flame of a spirit lamp; and over the deposit a cover-glass is then placed which has been similarly treated. If organisms are found, they are generally photographed at once, or else after they have been stained by drawing a drop of a solution of eosin or of gentian violet beneath the cover-glass. In case the specimen is reserved for future more careful examination, or with a view to see whether the organisms found will develop further, the cover-glass is at once surrounded with some liquid paraffin melting at about 105° F.—which will sometimes prevent evaporation for several weeks, and allow Bacteria or Torulæ to multiply, or the latter to develop hyphæ. Any lingering doubt as to whether the organisms are really living may thus be removed.

Where the aperture of the tube is larger, the only variation is that some of the deposit is withdrawn from the tube by a pipette, which has just been sterilised in the flame of a spirit lamp, and from it transferred to the sterilised microscope slip.

With the solutions that I have described, when the periods of exposure to light and heat have been long enough, organisms will generally be found in the first one or two specimens taken from the tube. As I have said, the organisms will all be motionless; but their numbers, looking to the character of the fluids employed, will clearly testify to the fact that they must have developed within the tube after it had been heated; moreover, in the specimens mounted as I have described, the organisms will often be found to multiply or develop further beneath the cover-glass.

A few examples of the results obtained in these experiments may now be given, as illustrated by photomicrographs taken at the time when the tubes were opened.

In Fig. 2 a number of twisted Bacilli or Vibriones are shown that were taken from a tube containing some of the B solution which had been heated to 100° C. for ten minutes, and which was subsequently exposed to light for only three weeks, though during bright and warm weather.

Fig. 3 shows a multitude of Micrococci and Bacteria, together with a few long Bacilli, taken from another tube containing a B solution which had been heated to 115° C. (239° F.) for ten minutes, and subsequently kept in an incubator for five weeks. On the edge of another of the flakes of silica from this tube the Micrococci were found that are represented in Fig. 4.

Two incipient Moulds, such as have been very frequently met with, are shown in Fig. 5, which were found in a B solution that had been heated to 120° C. (248° F.) for ten minutes, and which was subsequently exposed to light during six weeks. Several of such Moulds were found in the one or two samples of silica taken from this tube, some of them being in a very early stage of development, and with filiform extremities—showing, that is, no evidence of commencement from a Torula corpuscle or other definite spore.

In Fig. 6 some remarkable thread-like organisms are shown that were taken from a tube which had been heated to 130° C. (266° F.) for twenty minutes, five months previously. During the first three months it, and a companion tube prepared at the same time, had been exposed to light, and then both of them were placed in the incubator. After they had been there one month I opened and examined the contents of the companion tube, and found in the flakes of silica a few Torula corpuscles, together with a number of threads such as are shown, though they were less developed and in

much smaller groups. I determined, therefore, to leave the other tube in the incubator for another month; and the result, when it was opened, was that I found the threads still more abundant, and here and there forming even much larger aggregates than that shown in Fig. 6. When examined with high powers the filaments presented just such an appearance as Lehmann and Neumann show (*Principles of Bacteriology*) in their Fig. 1, r, under the name of "Torulathreads"—that is, in appearance, something between a long Bacillus and a streptococcal chain.

In Fig. 7 a number of Bacteria and of Torulæ are shown—one of the latter germinating—which were taken from a silicate of soda solution closely resembling solution B, though its exact composition is a little uncertain, as it was one of the first experiments that I made with such a solution. The tube had been heated to 130° C. (266° F.) for twenty minutes, and had subsequently been exposed to light for only eight weeks.

I now come to some experiments which were made with solution C, containing only sodium silicate and liquor ferri pernitris in distilled water.

In Fig. 8 some Vibriones or Spirillæ are shown (some stained and others unstained), which were particularly plentiful in the granular sediment of three of the tubes charged with this solution, each of which had been heated three

FIG. 2 (× 500).

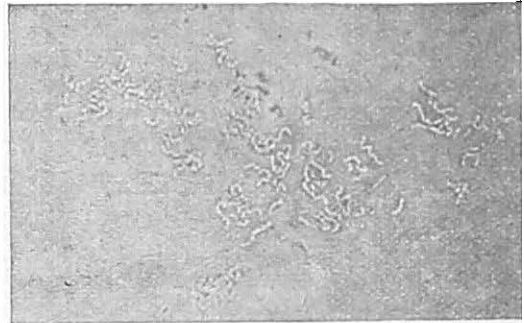


FIG. 3 (× 700).

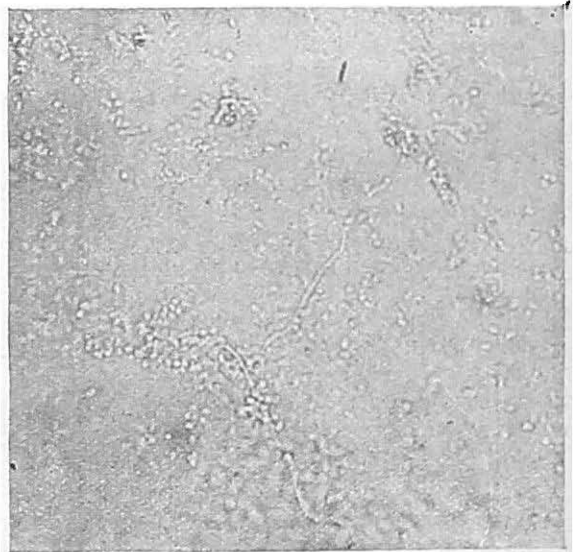
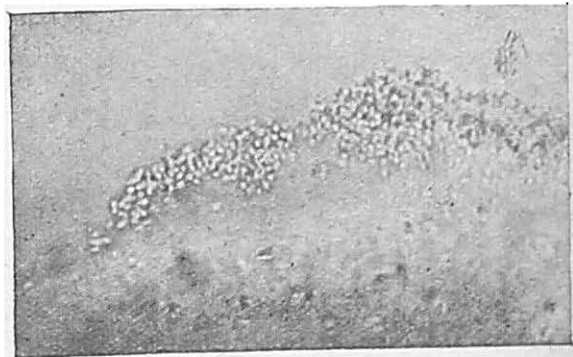
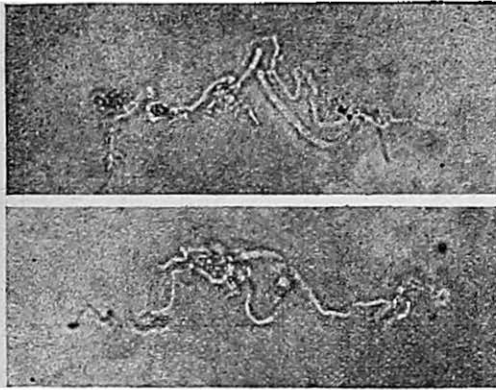
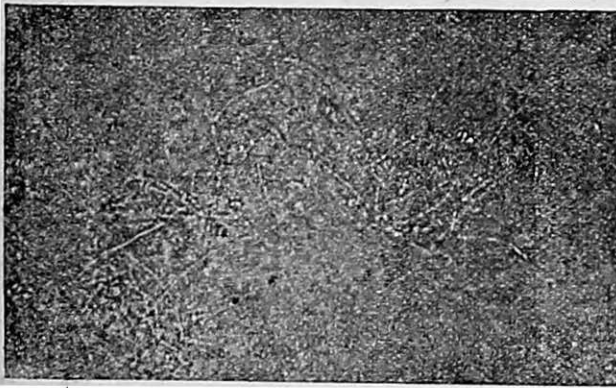
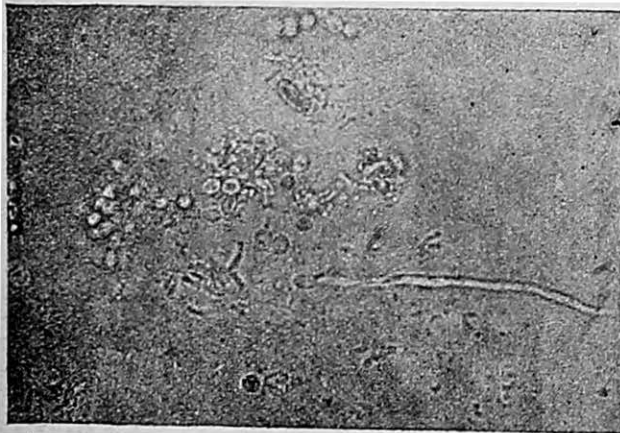


FIG. 4 (× 700).



Organisms taken from previously closed tubes which had been heated to 100° C. (212° F.) and 115° C. (239° F.) for ten minutes.

FIG. 5 ($\times 400$).FIG. 6 ($\times 500$).FIG. 7 ($\times 700$).

Organisms taken from previously closed tubes which had been heated to 120°C . (248°F .) for ten minutes and to 130°C . (266°F .) for twenty minutes.

months previously to 130°C . for twenty minutes, and during the interval had been exposed to diffuse daylight. In the same tubes a few incipient Moulds were found, together with free Micrococci, and others of a peculiar kind within loculi, of which I shall presently have to speak, as having been found within other tubes.

A large group of Micrococci, partly stained with eosin, is shown in Fig. 9, which was taken from a tube containing some of the C solution that had been heated to 130°C . for twenty minutes, six months previously. It was exposed to light for three months, and was subsequently kept in the dark for another three months. This was the longest time that any of the tubes had been left; and the organisms were more abundant and more varied than in any of the others whose contents I have examined. The groups of free Micrococci were very numerous, and they were mixed with a smaller number of others within loculi, together with the Spirillæ, Torulæ, and some well-developed Mould.

In Fig. 10 an early stage of the loculated Micrococci is shown—a group of three. Single loculi, each containing from five to seven Cocci, have often been

seen, as well as larger groups of such bodies, which ultimately tend to grow into comparatively large masses in which the walls of the loculi are no longer distinct, such as is shown in Fig. 11. In both the small and the large masses some of the Cocci seem to give origin to twisted filaments like those to be seen in Fig. 11. Similar filaments I have also found growing from some of the groups of free Micrococci. The filaments are more like those of an Actinomyces than of a Mould, but I have been able to find no description of anything like them—that is, having such an origin—in any of the bacteriological works consulted. The loculated Micrococci are comparable with *Leuconostoc* and the “frog-spawn”-like masses, to which *L. mesenteroides* gives rise, though the contents of the loculi in this latter case are always Streptococci. The organisms represented in Figs. 10 and 11 were taken from a vessel which had been heated to 100° C. for ten minutes, though almost exactly similar organisms have also been found in two tubes that had been heated to 130° C. for twenty minutes. The photographs of the organisms from the latter tubes were not quite so satisfactory, however, as those that have been here reproduced. This organism also seems to be one hitherto unknown. But it

FIG. 8 (× 700 and 500).

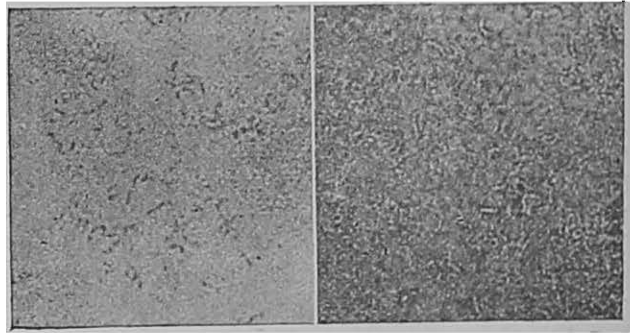


FIG. 9 (× 700).

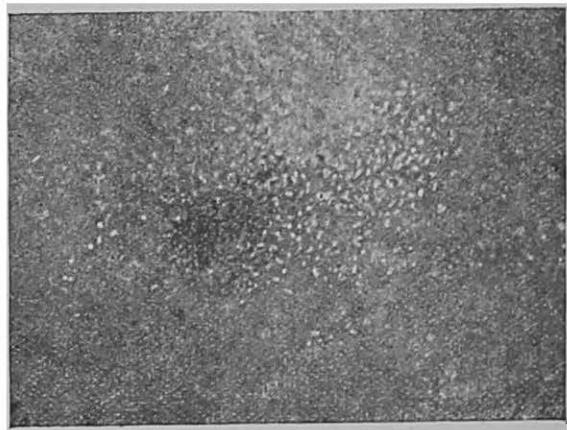


FIG. 10 (× 700).

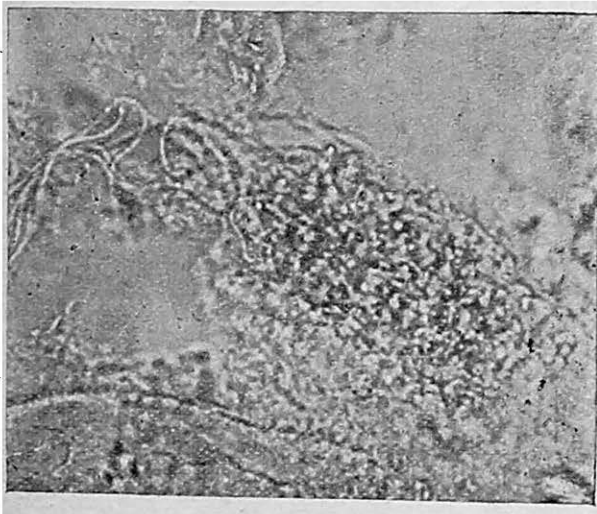


Organisms taken from previously closed tubes which had been heated to 130° C. for twenty minutes and 100° C. for ten minutes.

is interesting to find, that it, like the other organisms that I have met with in these solutions containing sodium silicate, is to be met with in fluids which have been exposed to very high temperatures, as well as in those that have been heated only to 100° C.

Before making some final statements concerning the major problem with which we have hitherto been concerned, a subordinate though highly interesting question in reference to these experiments in which silicon has been present, and carbon has been ostensibly more or less

FIG. 11 ($\times 700$).



completely absent, must again be referred to.

The question is, whether we are to consider that silicon, whose chemical properties are so very closely allied to carbon, and which, like carbon, first appears, as Sir Norman Lockyer has shown, in some of the hottest stars, can take its place, wholly or in part, in protoplasm.²

² According to Ostwald (*Inorganic Chemistry*, translation, 1904, p. 425), "The largest part of the earth's surface is composed of silicon dioxide, or of its compounds; over a quarter of the solid crust of the earth is formed by silicon."

I do not pretend that my experiments afford any distinct proof that such a substitution is possible—since carbon may have been present in the solutions, in the way of some accidental impurity, either in the distilled water or in the chemicals employed.

Still, the experiments recorded in the previous pages and elsewhere are very suggestive that such a substitution may be possible, when we bear in mind (a) the freedom with which Moulds will grow in and on the surface of colloidal silica, as originally observed by Roberts and Slack, and subsequently by myself; and when (b) we recognise that minute Moulds and different varieties of Micro-organisms will appear within closed tubes containing the solutions in question, though these have previously been exposed to high temperatures. It may be said that in such cases the organisms, if from no other source, have been capable of taking their carbon from the CO₂ of the air within the tube; but, against this supposition, in the case of the sealed tubes there is the fact (c) that the organisms are invariably found away from the air, in or upon the flakes of silica which collect at the bottom of the tube; and that for month after

month they are to be found there only, and never free in the strata of water intervening between the flakes and the air. Taken together, these facts seem to me to make out a strong presumptive case in favour of the view that silicon is capable, either wholly or partially, of taking the place of carbon in protoplasm.²

² Many organic compounds in which silicon wholly or in part replaces carbon are referred to by Professor Emerson Reynolds, F.R.S., in an important discourse entitled "Recent Advances in our Knowledge of Silicon and of its Relations to Organised Structures" (*Nature*, Aug. 11th, 1909).

INTERPRETATION OF THE EXPERIMENTAL RESULTS.

In regard to the cardinal fact of the appearance of organisms in the experiments that have just been recorded, this seems capable of receiving only one interpretation—namely, that the organisms have been evolved within the tubes. All the old objections to such an interpretation have been fairly met. The organisms found have undoubtedly been living, and they have occurred in tubes which have been *flambés*, previous to being again superheated together with their contents.

It is now generally admitted, as we have seen, that all micro-organisms in their ordinary condition are killed by the briefest exposure in fluids to 100° C.—to say nothing of lower temperatures.

The spores of Bacilli are recognised as the only products of micro-organisms capable of resisting such an exposure. Yet, as we have found, various forms (other than Bacilli) which are admitted to be killed at 100° C. can, and do, constantly appear within closed vessels that have been heated to this temperature for ten to twenty minutes. This applies, among other less known forms, to Bacteria, Vibriones, Micrococci, Streptococci, Torulæ, and other germs of Mould. When living organisms of these types appear within sealed tubes under the conditions described, we can, therefore, only conclude that they must have been evolved *de novo*.

In regard to ordinary Bacilli, the position, as we have seen, is different. These particular forms possess "spores," which, at all events after they have undergone desiccation, are capable of resisting a higher degree of heat. The extent of their powers of resistance has been differently estimated, but, after prolonged researches in the Pasteur Institute, the point was formerly fixed by the master himself at 110° C., and later, after two years' work, could only be raised, as we have seen (p. 92), by his then assistant to 115° C. At this point,

it is proclaimed, all the spores of ordinary Bacilli are completely and rapidly killed.

Noting, by the way, that even this temperature is much higher than Bacilli spores, in my very numerous direct experiments, were shown to be capable of withstanding,¹ and, further, that such bodies are little likely to be found in the freshly distilled water, or in either of the chemicals made use of in my experiments, let us, for the present, accept this temperature of 115° C. as needful to destroy all the products of ordinary Bacilli. But, as we have seen, such bodies, as well as Bacteria, Vibriones, Micrococci, Streptococci, Torulæ, and other germs of Fungi, have appeared within our experimental vessels when they have been heated, for from ten to twenty minutes, to temperatures ranging from 115° to 130° C. These organisms which we have seen to be living—which developed and multiplied—must, therefore, have been evolved *de novo*. What other answer is it possible to give?

All the bacteriologists throughout the civilised world during the innumerable researches they have carried on for the last forty years have only been able to find certain so-called "Thermophilic Bacilli" in the soil that are capable of resisting higher degrees of heat than 115° C. Yet these low organisms, which are said never to be found even in tap-waters, and *a fortiori* not in recently distilled waters, or in either of our chemicals, have, according to Christen,² never shown signs of life after they have been exposed to "125° to 130° C. for five minutes and longer; to 135° C. for one to five minutes; or to 140° C. for one minute."

The most resistant forms of these spores of the soil Bacilli, that were examined by W. H. Park, succumbed, however, at temperatures rather lower than this. He says they were destroyed "by exposure for twenty-five minutes in

¹ See *The Evolution of Life*, 1907, pp. 70-84.

² See Lehmann and Neumann's *Principles of Bacteriology*, translation, 1906, p. 53.

steam at 113° to 116° C., and in two minutes at 127° C.”¹

It comes to this, then, that all the organisms found in my experiments, with the exception of Bacilli, are such as would be killed at 100° C.; that these latter, so far as they could by any possibility be found within my tubes, should

have been killed in one or two minutes at 115° C.; yet Bacilli, as well as Bacteria, Vibriones, Micrococci, Streptococci, Torulæ, and other Fungus germs, dying under 100° C., have been taken in large numbers from tubes that had been heated to 115°-130° C., for ten to twenty minutes.

IX.

THE HETEROGENETIC ORIGIN OF BACTERIA AND THEIR ALLIES

THE term Heterogenesis is used by me in the limited sense originally given to it by Burdach—that is, as a class name for processes by which living things arise from the matter of pre-existing organisms belonging to a totally different species. Such a process I have elsewhere more briefly defined as the “production from the substance of organisms or their germs of alien forms of life.”

This doctrine now to be dealt with, and for the establishment of which I hope to bring forward very conclusive evidence, is one which is as much opposed to common belief as that dealt with in the last Chapter. That like produces like—that the offspring of any plant or animal will in all cases develop into an organism like its parent—is a belief as firmly rooted as that other which is embodied in the phrase *omne vivum ex vivo*.

If I am equally positive that Heterogenesis is a reality, I can only say that this view has been forced upon me after years of work commenced nearly forty years ago, and now again resumed during the last ten years.

In Archebiosis we are concerned with

the actual origin of living matter; while in Heterogenesis we have to do with the transformation of already existing living matter, and a consequent new birth of alien living things—as when the substance of an encysted Euglena or of an encysted Ciliate becomes transformed into a brood of Monads, Amœbæ, or Peranemata; when similar organisms are produced from a Rotifer's egg; or when even Ciliated Infusoria are produced from large encysted Amœbæ or from the great eggs of a Hydatina.² These examples will more thoroughly help to bring home to the reader what is meant by Heterogenesis.

The Heterogenetic Origin of Bacteria and their Allies.

In such instances as I have just cited it is easy to see that actual portions of the living matter of the transforming organism or germ are converted, bulk for bulk, into new organisms of a totally different kind. But in most of the cases in which Bacteria and their allies or Torulæ are produced in or from the substance of other organisms, whether

¹ *Pathogenic Micro-organisms*, second edition, 1906, p. 45.

² All such processes are described, and illustrated by photo-micrographs, in the writer's *Studies in Heterogenesis*, 1903, as a reference to the Index of that work will show.

animal or vegetal, it is often almost impossible to say for certain that their origin has been by Heterogenesis rather than by Archebiosis. Such organisms seem always to present themselves first in cells or tissues as extremely *minute motionless particles*—sometimes where no particles of any kind were previously visible, and sometimes intermixed with particles of a different order, but from which they cannot readily be discriminated. And where the particles of living matter so individualising themselves are so small as to be almost beyond the range of our most powerful microscopes, it is impossible to say until they begin to assume specific shapes, or particular modes of collocation, that they are not normal constituent granules of the tissues in which they are found. But their invariable first appearance as minute, motionless particles, rather than as active developed organisms, will be found to be a point of great importance.

Whenever Bacteria or their allies appear in the midst of the tissues or fluids of animals or plants, two related possibilities have to be thoroughly considered and excluded before their presence can be ascribed to Heterogenesis. They are as follows:—

(a) Are the bodies of plants and animals interpenetrated in all parts by visible or invisible germs of micro-organisms, or are they germless?

(b) Have the micro-organisms which may be found in the tissues or fluids of plants and animals under various conditions been produced therein by Heterogenesis (possibly in the fluids by Archebiosis), or is their presence invariably to be ascribed to "infection" from without?

(a) During the last thirty years it has been commonly held, in accordance with the teachings of Pasteur, Lister, and others, that the tissues and fluids of healthy animals and plants are germless, and altogether free from micro-organisms.

In regard to animals, however, it is clear that this position is one which cannot be accepted without very impor-

tant limitations. It is obvious that micro-organisms may, like other particles, get taken up from the mucous membranes of the alimentary canal and the respiratory system, and pass by means of lymphatics into the nearest glands. If they should get through these, and ultimately pass into the blood, the generally accepted view is that they are speedily "destroyed" in this fluid. This view is based upon the fact that bacteriologists are never able to get evidence of the existence of micro-organisms or their germs by the inoculation of different suitable and sterilised media, with blood drawn from a healthy man or from one of the lower animals similarly free from disease. Nothing more need be said now, as we shall have to return to this subject later on.

In regard to plants—that is, fruits and vegetables of different kinds—the case is not so complicated, and Pasteur was probably perfectly right in declaring that, when healthy, their cells and tissues are germless. Thus, in considering the interpretation of cases in which micro-organisms are found within the cells of certain vegetables or fruits after they have been submitted to various unnatural conditions, the question will not be whether we have had to do with the wakening up of latent pre-existing germs, but rather whether the organisms found are results of an "infection" that has recently been brought about—that is, during the exposure of the vegetables or fruits to the experimental conditions. And this brings us to the consideration of the second of the two possibilities above referred to.

(b) The second possible mode of accounting for the presence of micro-organisms in the tissues of healthy animals and plants, other than by Heterogenesis, is that they have resulted from some process of infection brought about antecedently to, or during the continuance of, the experimental conditions to which they have been subjected.

It is of great importance for the proper consideration of this possibility that we

should have some definite knowledge as to the powers possessed by such micro-organisms as Bacteria and their allies of penetrating the healthy tissues of plants and animals, as well as concerning the means by which they are enabled to do so, and the time needed for such an operation. Fortunately, one investigation, very much to the point, can be referred to, which was made by M. C. Potter in connection with a Bacterial disease of the turnip.² This investigator shows that the Bacteria which cause the disease secrete an enzyme (cytase) that tends to soften and partially dissolve the cell-wall, and also a toxin which kills the cell.

The important facts made known by this research are these: the vegetable cell is only capable of being penetrated when its walls are not thick and hard originally, and after they have been extremely softened by long contact with the cytase excreted by a number of Bacteria—the need of a conjoint attack being distinctly indicated by the author, who says: “Very soon the number of individual Bacteria becomes largely increased, each one contributes its share of toxin and cytase, and in a very short time these products have sufficiently accumulated to kill the first cell.....It is not, however, until the protoplasm has been killed and the cell-wall very much softened that the Bacteria have the power of perforating the walls and passing into the cell cavity. It would hardly be supposed that a single Bacterium, through its own excretions, could soften the wall and pierce it at one definite point after the manner of a fungus germ-tube. The extreme minuteness of the Bacteria and the rapidity of their multiplication lead them to act, as it were, in concert, and the wall becomes softened by the cumulative action of many Bacteria before the penetration of a single individual.”

The mode in which a Mould infects and penetrates a vegetable cell is very similar, allowance being made for its

greater size, which permits a single individual to do what can only be brought about by numbers of organisms in the case of Bacteria. This subject has been recently investigated by Nordhausen² while studying the parasitism of *Botrytis cinerea*. I quote from Potter, who says:—

“He has shown that the spore of this fungus excretes a powerful toxin in the initial stages of germination before any trace of the germ-tube can be detected. Its manner of effecting an entrance into a host plant is first to kill the cell by the emission of the toxin; the germ-tube then penetrates the dead cell, and is nourished saprophytically upon it; with the vigour thus gained it destroys the neighbouring cells and passes from one to another without further difficulty. The fungus-hypha has the power of penetrating the cuticle, *but only in young and tender structures*; old and hardened membranes could only be entered when the cuticle had been injured, or when it had gained strength by special saprophytic nutrition.”

These results will prove important as standards for comparison with other observations of my own which I am about to cite. We see that in the case of actual infection by Bacteria *there is need for the co-operation of many full-grown active organisms, and not mere motionless germs*, in order to bring about, by their secretions, the softening of the wall of every single cell that is penetrated; that some time is required for the operation; and that the softening produced must be considerable before any such penetration is possible by the moving organisms. It will be seen how very different is the state of things in cases which I shall cite as instances of the origin of Bacteria and their allies, in the tissues of plants and animals, by a process of Heterogenesis.

It will be found, in fact, that the presence of two characteristics, wher-

² *Proceed. of Royal Society*, vol. lxxvii., p. 442.

² *Jahrbücher für Wissensch. Botanik*, vol. xxiii., 1899.

ever they co-exist, may be regarded as strongly in favour of the interpretation of Heterogenesis as against Infection, as the following remarks will show.

(1) The means adopted by Bacteria for bringing about the penetration of cells being such as are associated with *the vital processes of adult organisms*, there is no reason to think that invisible or scarcely visible germs of such minute organisms would have the power of secreting a cytase sufficient in amount to bring about the degree of softening of a cell-wall which has been found to be a necessary preliminary to their penetration. *Yet in multitudes of cases it is minute germs of Bacteria and their allies which may be seen developing within cells or tissues.*

(2) Again, the process of infection, as described by Potter, is one brought about by *active organisms* which affix themselves to a cell-wall until it becomes softened, and then succeed, by reason of this same activity, in boring their way into the cavity of the cell. On the other hand, in very many of the cases in which, as I maintain, Bacteria and their allies may be presumed to be originating by Heterogenesis, what can often be seen is this—particles becoming visible in the midst of homogeneous protoplasm; *such particles being invariably motionless*, but followed soon by their growth and development into definite Bacteria or their allies, recognisable as such by their shapes and modes of collocation.

These two points are, therefore, of great importance, and for the purpose of interpretation it should always be borne in mind that in cases of Infection by Bacteria and their allies we have to do with *adult organisms in a state of activity*; while in cases where Heterogenesis may be presumed to be occurring we have invariably, in the first place, only to do with *motionless germs of organisms*.²

We must now see, in the first place, what evidence is forthcoming concerning the heterogenetic origin of Bacteria and their allies within the tissues of plants and other vegetal organisms.

Before citing some experimental results of this kind I may mention that Bacteria may constantly be seen developing within the living cells and filaments of various *Alge*. In *Vaucheria* and in *Spirogyra* this is commonly to be seen where the plants have been kept in unnatural conditions for a time; shut up, for instance, either in a cupboard or within a covered stone pot. In the case of *Vaucheria* they may often best be recognised in and near the growing points of the filaments where the microbes show themselves first as mere motionless specks, which gradually develop into Bacilli, and after a time take on an active existence. In the filaments presenting these appearances the cell-wall may appear quite healthy, showing no signs of softening, nor is there any indication whatever of the penetration of Bacteria from without.

The same kind of thing is often to be observed within the thick-walled resting spores both of *Vaucheria* and of *Spirogyra*. There is the appearance of motionless particles in some part of the spore, the appearance of Bacteria in the midst of these particles, and the gradual assumption by the Bacteria of swarming movements.

Again, I have frequently seen a development of motionless Micrococci and Bacilli taking place inside the thick wall of a *Nitella* cell, between it and the chlorophyll layer. Yet the normal cyclosis was still going on within this cell, showing that there could be no apertures or solutions of continuity of any kind; and all the micro-organisms to be seen in different stages of development in this layer were quite motionless.

I have also endeavoured to throw light

² Of course, by "germs" I mean here merely minute and undifferentiated stages of the organisms in question, produced by Heterogenesis,

and not the ordinary acceptance of the word—viz., a reproductive unit formed in an organism of like kind.

upon this question by repeating, with variations, some of the experiments of Lechartier and Bellamy by which they studied the fermentation that occurs in various vegetables and fruits when these are shut up within hermetically sealed vessels. They showed that the oxygen of the air was soon consumed by the vegetables or fruits, which then began to break up sugar, to give off carbonic acid, and to produce alcohol and acetic acid. They came to the conclusion, in 1872, that this fermentation might certainly occur without the production of the alcoholic ferment.¹ They, in fact, adopted Pasteur's view that the formation of alcohol in these cases was due to the altered activity of the cells of the fruit, which, in the absence of free oxygen, act after the fashion of ferments. In a later communication,² however, these investigators stated that in their experiments with potatoes and beet-root, while alcohol and carbonic acid were produced in the same way as with the fruits, and the alcoholic ferment was absent as before, Bacteria of different sizes were invariably found in the acid fluid which impregnated the softened tissues of the vegetables in question. No details on this point were given, and the authors do not appear to have made any further observations on the subject; nor did Pasteur offer any reply to such statements, though he had previously been working at the same subject himself.³

Having determined to endeavour to obtain some more definite information as to the appearance of Bacteria in this way, I have, during the last two or three years, made various experiments in which, after small *Potatoes* had been carefully washed, they were allowed to soak for a time in different germicidal fluids. First of all I employed a solution of mercury bi-chloride (1 : 2,000); while later, after the preliminary washing,

the potato was allowed to soak in a five per cent. formalin solution. The screw-top bottle in which the potato was placed was also thoroughly washed out with one or other of these fluids. In these cases organisms were found within, but also, after a time, on the surface of the potatoes thus treated; so that these particular experiments and methods were rejected as not yielding trustworthy results. This was necessary because at a rather earlier date Pasteur had stated that in experiments which he had made with fruits no ferment organisms ever appeared. He declared again that the tissues of healthy fruits and vegetables were germless, but intimated that unless care was taken they might make their way in from without.⁴

Subsequently I used a stronger solution of formalin, and have never since found organisms on or near the surface, though they have often been found within cells in the central portions of the potato. I will now, therefore, give brief details of some of the most successful of these experiments.

In July, 1901, after well washing a small new potato, it was allowed to soak in a ten per cent. solution of formalin for ten minutes, in a small screw-top bottle, and during this time the fluid was frequently shaken so as to cover the whole inner surface of the bottle. At the expiration of the time named the top was unscrewed, the fluid poured out, and the top then tightly refixed, leaving the potato itself and the inner surface of the bottle wet with the formalin solution. The bottle was subsequently left in a cupboard for seventeen weeks, the temperature of which for a long time remained about 70° F., though it afterwards fell to 50° F.

When removed from the bottle at the expiration of this time the potato was found to be quite firm and not at all shrunk. On section it was seen to be discoloured to a pale earthy tint, with mottlings here and there of a rather

¹ *Compt. Rend.*, 1872, ii., p. 1203.

² *Loc. cit.*, 1874, ii., p. 1006.

³ *Loc. cit.*, 1872, ii., p. 788.

⁴ *Compt. Rend.*, 1872, ii., pp. 788 and 981-2.

darker colour. The cut surface was moist and had a distinctly acid reaction, and there was not the least sign of softening or disintegration anywhere. Thin sections having been made, they were shaken up in a small tube with distilled water, so as to get rid of the starch-grains from many of the cells, and the sections were subsequently allowed to soak in some of Westphal's mastzellen stain, diluted with two per cent. formalin for two hours.

On microscopical examination of these sections, groups of Bacteria were found in large numbers of the cells, though not in those near the surface. The contents of one of these cells is shown in Fig. 12, B ($\times 500$); some of the Bacteria were free, and others were in, or lying on, the primordial utricle; but, as I have usually found with micro-organisms in such situations, they were not appreciably stained. Some cells, which did not contain obvious Bacteria, showed plenty of minute cocci-like bodies on the surface of the primordial utricle, also not taking the stain, which probably represent early stages of the Bacteria (Fig. 12, A, $\times 700$).

Another larger potato, about two inches in diameter, was treated in exactly the same way as the last in September, 1901; and, after the bottle was finally closed, it was left on the surface of an incubator at a temperature of about 80° F. for seven weeks.

When examined the potato was not found to have shrunk, or to be appreciably altered on the surface. On section, it was moist, of acid reaction, and showed, as before, a pale earthy colour with rather darker mottlings in places.

Sections were made and treated in the manner previously indicated; and, on examination, multitudes of Bacilli were seen here and there in cells in all parts of the section, except for about one-fifth of an inch from the surface. In places also there were fine mycelial filaments containing spore-like bodies. Some of these Bacilli took the stain fairly, as may

be seen in Fig. 13, A ($\times 500$), in which the two kinds of organisms are shown. In or on the primordial utricle, also, there were multitudes of very delicate interlacing filaments (? Bacilli), containing an abundance of spores which had taken the stain freely, as may be seen by Fig. 13, B ($\times 500$).

At the end of March, 1903, I made another slight variation in the conditions with a small new potato. After careful washing, it was placed in a small tin with a very tightly fitting cover, and allowed to soak in ten per cent. formalin for twenty minutes, the fluid having been shaken about several times so as thoroughly to wet the whole internal surface of the tin. The fluid was then poured off, leaving the surfaces wet with the solution as before; the tin cover was very tightly jammed down, and the vessel was placed *within* a copper incubator at a temperature of 75° F., and allowed to remain there for eight weeks completely cut off from all light.

When taken out and examined the cut surface of this potato presented just the same characters as in the others; the whole substance was firm throughout, there was no shrinking, and the central portion was rather darker than the other parts, which showed the usual mottling.

Sections were made, shaken up in distilled water in a small tube as before, and then placed for a short time in a dilute gentian-violet solution. On microscopical examination a large number of the cells scattered throughout the sections were found to show the most delicate branching tufts of what seemed a new kind of microphyte. They looked like a species of Cladotrix, had taken the stain slightly, and are shown in Fig. 14, B ($\times 500$). These tufts were mostly seen to be sprouting from the internal surface of the primordial utricle, or else from its external surface, where it had shrunk away from the cell-wall. At first no Bacteria were found, only tufts of this kind; but a subsequent thorough examination of other sections revealed Bacteria in abundance, and in some cells

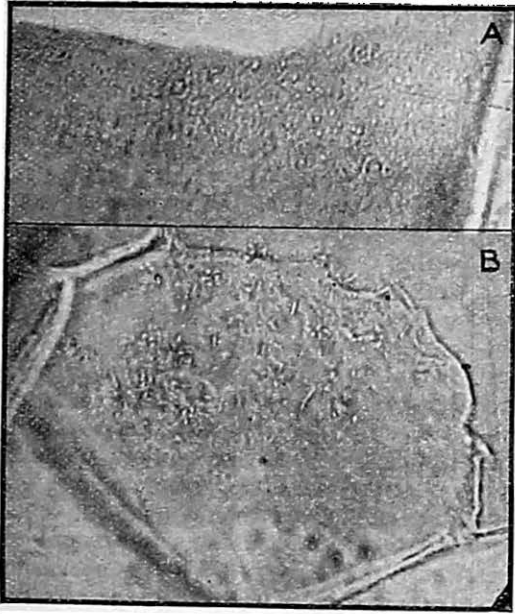


FIG. 12.

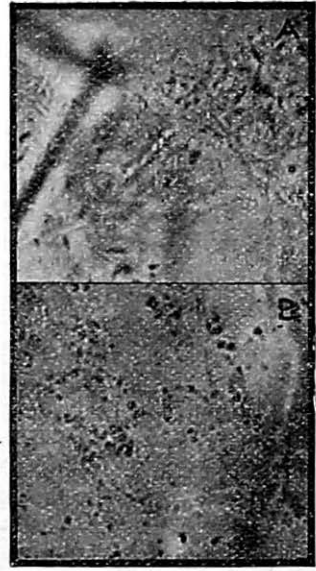


FIG. 13.

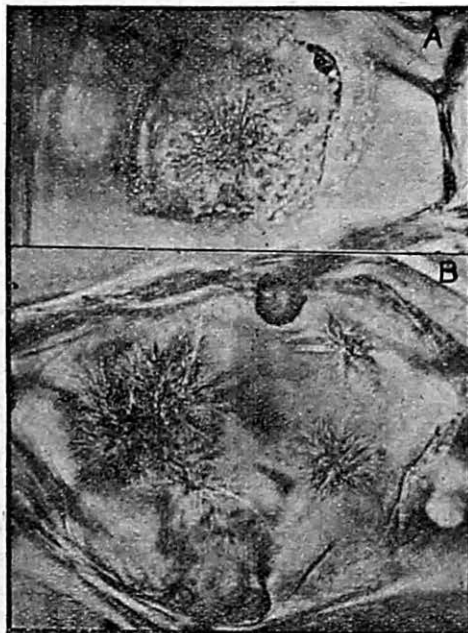


FIG. 14.

dense aggregations of them such as are shown in Fig. 14, A ($\times 500$). It seems probable that the tufts are really aggregates of Bacilli growing out from such masses of germ-like bodies. In other cells these germ-like bodies were seen apparently originating from the primordial utricle, and in places giving rise to filaments ramifying over the surface of the membrane.

A few experiments have also been made with small *Turnips* about two inches in diameter, to two of which I will now refer.

A perfectly sound turnip of the size mentioned was, in November, 1901, first well washed in water and then allowed to soak in a screw-top bottle in a ten per cent. formalin solution for ten minutes. It was subsequently treated in exactly the same manner as the potatoes had been. After the top of the bottle had been tightly screwed on it was left on the top of the incubator at a temperature of about 80° F. for seven weeks.

On examination at the expiration of this time the turnip was found to be somewhat shrivelled in its upper two-thirds. The odour of the bottle was disagreeable and pungent, though slightly aromatic and spirituous. The odour was so strong that it did not seem likely that the shrivelling was due to evaporation, owing to the screw-top not having been quite air-tight.

On section the rather shrivelled upper two-thirds was found to be much discoloured and honeycombed; the lower third being much less so. Sections were made and soaked in dilute mastzellen stain; and on examination cells here and there, not continuously, but in the upper and lower portions alike, were found to be crowded with very minute Bacteria, most of which took the stain only slightly. Some large aggregates of these organisms were seen, with others scattered about over contiguous portions of the section.

Another small turnip of the same size as the last was, at the same date, after

being well washed, placed in a screw-top bottle and stood on a small earthenware pot, in order to protect it from six drachms of pure formalin which had previously been poured into the bottle. The top was then tightly screwed on, and the bottle was placed on the incubator by the side of the other at about 80° F., where it remained for eight weeks, the turnip being in an atmosphere saturated with formalin vapour.²

On examination this turnip was likewise found to be slightly shrivelled, and it was rather soft and doughy to the touch. On section the colour was almost natural, except for a depth of about one-third of an inch round the periphery, where it was slightly discoloured, and in the centre, where there was a small area about one-quarter of an inch in diameter which had a rather gelatinous appearance.

Two sections through this central region and its neighbourhood were made, and then soaked in dilute mastzellen stain. On microscopical examination they were found to contain moderately large Bacteria, mostly in small groups, in a large number of the cells; though here and there large masses of Bacteria were found. In many of the cells the Bacteria seemed to be developing in and on the surface of the primordial utricle, and also within and on the surface of the nuclei of the cell.

The presence of the Bacteria and other organisms within the cells of the two small turnips and the potatoes that have been referred to are incapable of being accounted for by any process of infection from without; and the same may be said in reference to the delicate Fungus mycelia, found in an apple, springing up within various isolated cells

² I have not had many successes with this method, and do not recommend it, especially as the soaking for a time in ten per cent. formalin has proved to be perfectly sufficient to guard against external contamination. It is difficult to tell how far the formalin vapour penetrates into the substance of fruits or vegetables left in an atmosphere saturated with it for many weeks.

in the midst of the substance of the fruit. There is absolutely no relation between what I have found in these cases and an actual process of infection such as M. C. Potter has described (see p. 104). We have to do, in fact, in the cases that I have cited, with motionless germs of micro-organisms arising *de novo* in or on the substance of the primordial utricles of isolated cells, having intact walls, and scattered throughout the substance of the potatoes and the turnips in question—in all parts, that is, except in the superficial portions that have been saturated with the germicidal fluid in which they had been for a time soaked.

We may turn now to see whether equally cogent evidence can be obtained in regard to the origin of micro-organisms within the tissues of animal organisms.

It would be useless to multiply instances. I will, therefore, first cite a single case in which the origin of Bacteria may be actually watched within the body of a low animal organism, and then turn to their mode of appearance within some of the tissue elements of vertebrates.

Evidence of a particularly convincing nature is to be obtained from the examination of a little creature low in the scale of animal life—namely, *Cyclops quadricornis*, one of the Entomostraca so commonly to be found in ponds. It may be seen from Plate xxiv. of Baird's *Natural History of the British Entomostraca*¹ that the four pairs of abdominal feet and also the tail are furnished with a number of "plumose spines or setæ."

Examination of one of these organisms will show that within the chitinous envelope of these slender spines, which taper away to sharp points, there is nothing but structureless protoplasm to be seen (Fig. 12, A, $\times 700$). If we take one of these little creatures, put it in a drop of distilled water, on a glass slip with a fragment of a No. 2 cover-glass on each side of it, and place over all a cover-glass, it will be found that the

animal is soon killed by the weight of the latter, though the fragments of glass prevent rupture of the body. We may then place the microscope slip in a Petri dish containing a thin stratum of water (so as to prevent evaporation from beneath the cover-glass), and, fixing upon one of the tail setæ (these being larger than those on the abdominal feet), we may examine it from time to time. What may be observed is this.

After an interval of two or three days (the duration depending upon the temperature of the air at the time) we may see, under a high power of our microscope, scarcely visible motionless specks gradually appear in increasing numbers in the midst of the previously structureless protoplasm; and still later we may see some of these specks growing into Bacteria, as in Fig. 15, B, which is a representation of A after four days. At last the whole interior of the spine becomes filled with distinct Bacteria as may be seen in C, which is from a photograph of the same spine on the sixth day—the temperature during these days varying from 70–75° F. Later still, all the Bacteria, previously motionless, begin to show active swarming movements.

In such a case it is clear we have to do with no process of infection from without, but with a *de novo* origin of Bacteria from the protoplasmic contents of the spines or setæ. The fact that they appear in these situations as mere separate, motionless specks, and gradually take on the forms of Bacteria (also motionless at first), is, as I have previously indicated, just what we might expect if they had actually taken origin in the places where they appear. On the other hand, such a mode of appearance is totally opposed to what might be expected if the micro-organisms had obtained an entry from without, through the tough chitinous envelope of the spines. It will be observed, also, that the apparent origin and mode of appearance of Bacteria here is precisely similar to what is to be seen when a film of a

¹ "Ray Society" Publication, 1850.

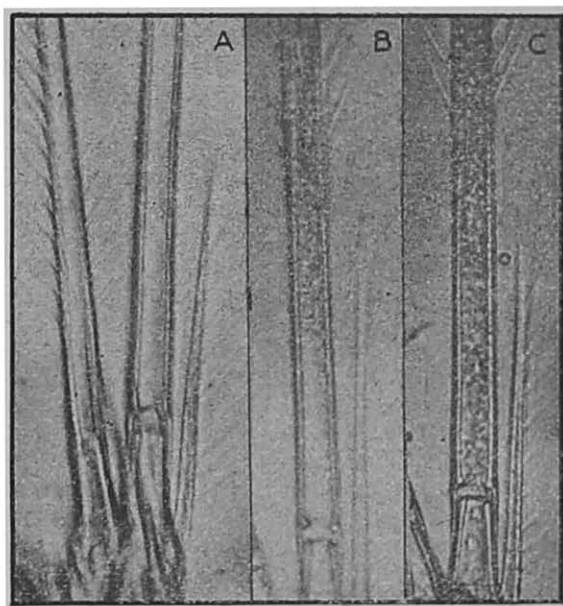


FIG. 15.

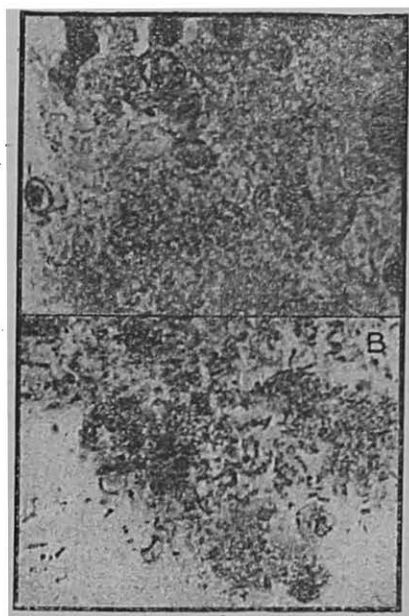


FIG. 16.



FIG. 17.

suitable organic fluid is watched under the microscope, and Bacteria gradually make their appearance therein (see p. 34).

I pass now to what may be regarded as another absolute proof of the heterogenetic origin of Bacteria, as convincing as that which has been shown to occur within the closed cells of certain vegetables.

I have already pointed out that in many parts of the bodies of man, and of higher animals generally, micro-organisms are known to exist in abundance. This is the case, for instance, throughout the whole length of the alimentary tract, and throughout a considerable extent of the mucous membranes of the respiratory tract. It is clear, also, that some of the micro-organisms may be taken up from these mucous membranes by lymphatics, and if they pass the nearest lymphatic glands some of them would ultimately find their way into the blood. When there, the view generally accepted is, as I have said, that the Bacteria and their allies are at once "destroyed." The blood of healthy animals is declared to be germless, and much importance is attached to the germicidal qualities of this fluid.

It has, however, been held by Tiegel, Burdon Sanderson, and others, that, though the blood is germless, "those parts of the animal body which are in closest proximity to absorbing mucous membranes are most liable to be found pregnant with microphytic life when tested by suitable methods."* Their experiments showed, indeed, that such organs as kidney, spleen, and liver, when removed from the body of a healthy animal immediately after its death, and suitably treated, could always be made to reveal the presence of micro-organisms.

But cutting out portions of internal organs of recently killed animals, enveloping them with superheated paraffin, and then placing them in an incubator

at a suitable temperature, followed by the finding of swarms of Bacteria in the central red and uncooked portions, is not a method that can possibly give us certain information as to the mode of origin of the organisms found. It may be, as Burdon Sanderson and others concluded, that the organisms found came from "germs which existed and retained their latent vitality in the living tissues"; but it is at least equally possible, as I maintain, that they may have had a heterogenetic origin within these tissues themselves.

It seems perfectly clear that experiments of this kind, if carried no further, could teach us nothing decisively; that their results, in fact, are of no more value than those that may be obtained by the examination of the brain and its membranes three or four days after a healthy animal has been killed. There, also, swarms of micro-organisms would be found, as I can testify; and if bacteriologists are right that organisms and their germs are, as they say, "destroyed" in the blood, we could only conclude that the organisms so found must have been produced by archebiosis or by heterogenesis. It could not reasonably be said that the organisms naturally present in the intestinal canal have been enabled to spread through the body *after death*, so as to reach its most remote parts, since many of the organisms are motionless, and others exhibit mere to-and-fro movements of a non-progressive character. The blood, again, has ceased to circulate, so that this fluid, germless during life, cannot after death be considered to act as a carrier. But if the organisms themselves cannot make their way through the tissues and travel far within brief periods, and if no carrier exists, they must have been born in or near many of the sites in which, after death, they are speedily to be found.

Speaking of the experiments made by himself, Burdon Sanderson wrote as follows¹: "Under the conditions I have

* *British Medical Journal*, 1875, vol. i., p. 199; see also 1878, vol. i., p. 119.

¹ *British Medical Journal*, 1878, vol. i., p. 119.

described to you, it seems to me quite impossible to suppose either that germs could penetrate to the organ from the outside or that any germ encountered by the organ in its transference from the body, of the animal to the basin could escape destruction. If, therefore, Bacteria be found, they or their germs must have been there before the organ was plunged into the hot liquid.....The results of all the experiments, whether with liver or kidneys, was the same. The soft, red kernel of uncooked tissue at the middle of the organ always contained Bacteria, the vigorous development of which was indicated by their large size, countless numbers, and active movements. To my mind the experiment is conclusive."

In reference to this, it is right to say that similar results have been obtained by other investigators using either similar methods or methods equally trustworthy. Such experiments have been made by Tiegel, Billroth, Nencki and Giacosa, Horsley and Mott, as well as by myself.

As I have previously intimated, the finding of organisms under these conditions is a fact so important, in view of the theories of Pasteur and Lister, and the general belief as to the germicidal qualities of the blood, that the results require to be most carefully scrutinised.

To postulate the presence of "latent germs" in these abdominal organs, and to assign as a reason for their presence the close "proximity [of such organs] to absorbing mucous membranes," and nothing else, surely cannot be regarded as a full and satisfactory explanation. It assumes, without proof of any kind, that micro-organisms, even in healthy animals, are constantly making their way out through the walls of the intestine and wandering promiscuously into this or that organ, only in the end to lapse into a condition of "latent vitality." Could anything in the way of explanation be more gratuitous and unsatisfactory?

With the view of throwing light upon this important question by the production of actual evidence, I obtained a

sheep's kidney from a recently killed animal, and saw a coating of fat nearly an inch thick stripped from it. The whole organ was then left to soak for four hours in a two per cent. solution of chromic acid, so as to destroy any organisms by which its surface may have been contaminated. It was then removed and placed in a bottle still wet with another germicidal solution—namely, a ten per cent. solution of formalin. The screw-top having been fixed so as to prevent evaporation, the bottle was transferred to an incubator, and left at a temperature of 70° F. for thirty-six hours.¹ When the organ was cut the chromic acid solution was found to have discoloured it to a depth of about a quarter of an inch; but within that margin the kidney substance was red, and only slightly softer than natural. There was no distinct odour of putrefaction. A small portion of the organ was cut out and teased in a drop of a weak solution of gentian violet; and fragments, after a short interval, were carefully examined under the microscope. A comparatively small number of Bacteria were found free between the separated and broken-up cells, and the cells were densely filled with granular matter, as may be seen in Fig. 16 A ($\times 700$); but it was impossible to identify with certainty any of the granules as germs of Bacteria. Sections that were made and carefully stained gave no more definite results.

Another sheep's kidney from a freshly killed animal was, therefore, obtained and treated in the same manner, except that it was left in the incubator at 70° F. for three and a half days, instead of merely thirty-six hours. When the organ was cut through it was deeply stained at the circumference as before with the chromic acid; but the red tissue within was much softer, and the odour was most offensive and putrid. Portions

¹ I was using the incubator for other purposes at this temperature, and, therefore, did not make use of a higher temperature.

of the organ were at once put into a ten per cent. formalin solution, with a view to obtaining sections therefrom; but a minute portion was cut off as before and teased in a drop of gentian violet. On examination with the microscope after a brief interval, I found the fragments of the tubules and kidney cells full of micrococci which had taken the stain well, together with numbers of figure-of-eight organisms and short chains (Streptococci), such as are shown in Fig. 16, B ($\times 700$).

In a few days Dr. J. S. Collier kindly sent me a number of sections, stained and unstained, cut from the portion of the kidney in the formalin solution which I had handed over to him. Some of the sections sent had been stained with methylene blue or with logwood, and then mounted in balsam; while I stained some of the plain sections with gentian violet, and subsequently mounted them in glycerine. On the whole, rather more details could be made out with these latter sections than with the specimens mounted in balsam.

Every section, however, showed, inside the area which had been affected by the chromic acid, that almost each cell within the renal tubules was full of developing or actually developed Bacteria. The former appeared as mere cocci-like particles which had taken the stain like the developed Bacteria, both being situated in the midst of cell granules comparatively unstained. The organisms were distributed through the substance of the cells, but often seemed to be most abundant in the half of the cell next the wall of the tubule. In a few of the tubules in the pyramidal portions of the kidney the organisms had developed more abundantly, so that the cells were filled with a dense mass of micrococci, such as may be seen in Fig. 17, A ($\times 700$). In sections through blood-vessels, also, a moderate number of Bacteria were seen mixed with the blood-corpuses.

Here, then, it is clear that we have again the kind of appearances which we have a

right to expect if the micro-organisms had developed in the epithelial cells of the kidney by heterogenesis. We have the cells full of particles developing into fully-formed Bacteria; and, what is more important still, we have them within almost every epithelial cell to be seen in the sections.

From the point of view of the wholly inadequate suggestion as to organisms being found in such an abdominal organ as the kidney, by reason of its "proximity to absorbing mucous membranes," it may be well to recall the fact that the sheep's kidney is encased in a mass of fat from half an inch to an inch in thickness; and that, as an additional barrier separating wandering micro-organisms from the epithelial cells of the organ, there is still the thick and tough capsule of the kidney, which I have thought it not useless to show in Fig. 17, B ($\times 700$), where it is represented, under the same magnification, by the lower half of the figure. Let anyone compare M. C. Potter's description (p. 104) of the mode in which Bacteria are enabled to effect an entry into a single vegetable cell, and then let him imagine what an army of Bacteria would be needful, with all the cytase they could excrete, to get through such a tough and thick layer as that presented by the fibrous capsule of the kidney. But surely the whole notion as to such a mode of infection of the kidney and other abdominal organs is too absurd for serious consideration.

We are, then, driven back to inquire whether it is true that the blood is germless, and whether it has, in reality, the bactericidal qualities with which it is credited. I have no evidence whatever to oppose to these beliefs; nor is it easy to see, even if bacteriologists generally should be wrong in their views as to these points, how this would suffice to explain the development of Bacteria within all the cells of a kidney treated in the way I have mentioned.

It is perfectly certain that the blood of healthy persons does not contain any appreciable number of active Bacteria.

But are bacteriologists right in supposing that such Bacteria as get into the blood-stream are "destroyed"? May they not rather be reduced to a condition of latent vitality? Their answer to this is that, if it were true, the organisms would be capable of revealing their presence when suitable media were inoculated with them and subsequently exposed to proper incubating temperatures. And it is the negative results of all such experiments with the blood of healthy animals that confirm them in their belief as to the germicidal qualities of the blood.

Nor, in fact, if the blood were assumed to be full of latent germs of Bacteria, would it be easy, as I have intimated, to

see how that would enable us to explain the development of Bacteria within almost every epithelial cell in the kidney referred to. Could organisms reduced to a condition of "latent vitality" penetrate the walls of the capillaries, and thence migrate into all the cells of a kidney tubule? The notion is again too preposterous to be entertained; so that we are compelled by evidence of a most convincing character to admit that the Bacteria have in reality been born in the individual cells of the kidney—we are compelled to believe that heterogenesis has, in fact, been taking place here as in the other instance previously cited.

CHAPTER X.

THE HETEROGENETIC ORIGIN OF FUNGUS-GERMS, OF MONADS, AND OF AMCÆBÆ FROM MINUTE MASSES OF ZOOGLEA

SOME of the changes to which reference is now to be made were briefly described by me in 1870,¹ and again more fully in 1872.² Although the changes then referred to were of a very remarkable nature, and were declared to be of such a kind as to lead to the production of flagellate Monads, of Amcæbæ, and of Fungus-germs from aggregates of Bacteria imbedded in a "more or less abundant, pellucid, gelatinous material," they seem to have attracted little serious attention, probably because they were so surprising as to be regarded as incredible. I

am not aware that any bacteriologist in Europe, America, or elsewhere, has ever repeated my observations. They seem absolutely wedded to their strict laboratory methods, and seemingly prefer to have dealings with nothing but pure cultures and sterilised media.

I have recently devoted much time to a further study of these changes occurring in what has been named the "proliferous pellicle," and have been able not only to confirm the accuracy of the results previously recorded, but also to considerably extend them.³

¹ *Nature*, No. 35, June 30, p. 172.

² *Proceed. of Royal Soc.*, 1872, vol. xx., p. 239, and *The Beginnings of Life*, vol. ii., chap. xvii.

³ At the time when my previous papers were published very little was known concerning *Zooglaa*; and that term was not employed in describing the constitution or the pellicle and of

Bacterial scums are well known to be exceedingly common in ditches and ponds—that is, in nature's laboratories—and it is a matter of great interest to know what goes on therein. Some light may be thrown upon this subject by making infusions or macerations from cut fragments of various plants, and then examining, at different periods, the scum or pellicle that forms on such fluids. What I have now to say will refer almost exclusively to infusions made from hay. The hay employed may be either fresh or old, but it does not do to substitute for hay mere unripe grasses. I have elsewhere shown how remarkably different are the products derivable from living unripe grasses and from ordinary hay.¹

In making such an infusion, I have been accustomed to cut the hay into pieces about one-half to three-quarters of an inch long, to place these in a little beaker, and then to add water so as well to cover the fragments. After maceration for two or three hours at a temperature of about 86° F. (30° C.), the infusion has been filtered through two layers of the finest Swedish filtering paper into another small beaker or covered pot. In this way all but the smallest particles, $\frac{1}{100}$ th of an inch or thereabout, will be excluded. For observation of the changes now to be described, it is best that the bacterial scum, which soon forms on the surface of the fluid, should be very thin; therefore the infusion should not be too strong, and the depth of the fluid ought not to be more than about one inch.

As all the processes with which we are concerned go on in the dark almost, if not quite, as well as in the light, one simple plan is to filter the infusion into small one-ounce earthenware pots, over which the covers are then placed till the time comes for the examination of their

contents. If three or four pots are charged at the same time, they may be opened at will on successive days, or some may be exposed to one temperature and some to another.

Otherwise the infusion may be left in a shallow open glass vessel, exposed to light, though protected from direct rays of the sun, and beneath a bell-jar, so as to exclude dust. About 65° F. (18° C.) is a favourable temperature, and, when thus exposed, in about twelve hours or so the fluid begins to show slight turbidity, and as this increases the light sherry-coloured fluid becomes gradually paler. In from twenty-four to thirty hours a thin, almost invisible, scum will have formed upon the surface of the infusion, often in the form of a coherent elastic membrane, and somewhere between thirty and forty hours multitudes of small zooglœal masses, such as I formerly termed "embryonal areas," of varying size and shape will have formed, and will be found to be imbedded in this thin scum or pellicle. These go on increasing in size and growing in number till at last they may occupy more than one-half of the pellicle. Fig. 18, A ($\times 100$) shows such masses more than usually abundant, as found on a hay infusion on the fourth day. They are rendered more distinct owing to the ground-work of the pellicle having been stained by an eosin solution. B ($\times 500$) shows portions of such a mass highly magnified.

When we attempt to remove a small portion of the early, and almost invisible, scum on the tip of a sterilised scalpel, we may often see that its point will very perceptibly depress the surface before it breaks through the membrane. This is especially the case when infusions of a greater depth than what I now recommend are employed; and then also each day that passes adds to the thickness of the pellicle, till after seven or eight days have elapsed it may have been converted into a comparatively thick pulpy layer, owing to continued accretions of fresh Bacteria from below. After this period,

the "embryonal areas" occurring therein—though the latter were referred to as aggregates of Bacteria which had formed "around themselves a certain amount of pellicid, gelatinous matter."

¹ *Studies in Heterogenesis*, 1904, p. 87.

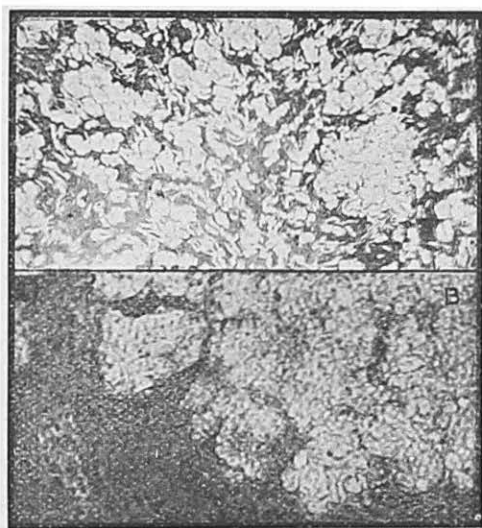


FIG. 18 ($\times 100$).

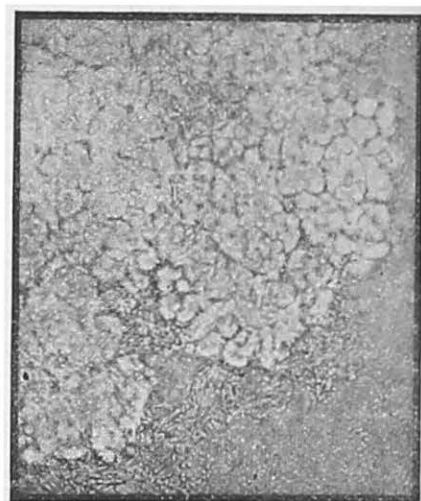


FIG. 19.

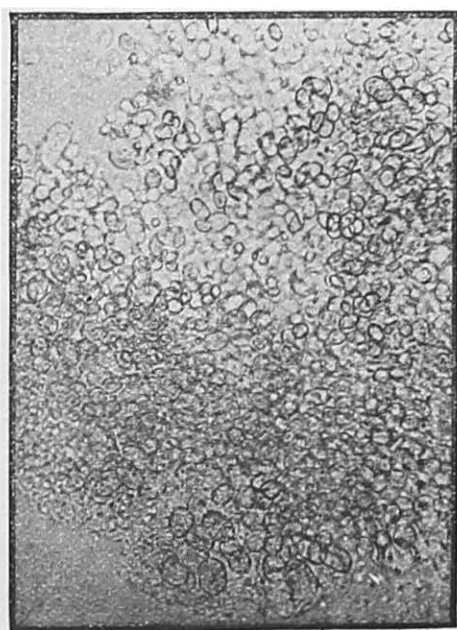


FIG. 20.



FIG. 21.

or even before, the superficial layer of the pellicle may gradually become more and more brown, while rounded, or branched and elongated, masses of zooglœa project from its under surface. Ultimately, after three or four weeks, the pellicle, if left, may break away and, in part, sink to the bottom of the vessel.

Such are the changes to be met with during the formation and growth of a pellicle on the surface of a hay infusion made and exposed in the manner I have described. One of the most notable points in connection with it is the fact that from a very early period the Bacteria which thus aggregate into a scum at the surface of the fluid, where they are freely exposed to air, excrete a transparent, jelly-like, or glœal substance, by means of which the constituent units of this scum become blended into a thin elastic membranous layer.

As may be supposed, when we examine some of the turbid fluid, or the thin scum that first appears, Bacteria of several different kinds are pretty constantly met with; but, for the most part, with hay infusions, it is Bacilli which largely predominate, such as may be seen in Fig. 30, A ($\times 500$). Toruloid corpuscles are decidedly rare, and no Monads, Amœbæ, or Ciliates are ever to be seen at this early stage. The mode and times of their appearance will be presently dealt with. In the first place, however, I would emphasise the fact that the researches which I am about to detail have no pretence to be conducted in ways that are proper and usual in the great bulk of bacteriological inquiries. We start here confessedly with a mixed association of Bacteria, tending to aggregate in a promiscuous manner on the surface of the fluid, which soon pass into a resting stage, and then secrete a transparent glœal material that binds them together. The question with which we are now concerned is, *What are the microscopical changes apt to occur in such a scum or pellicle?* The scum is confessedly a composite zooglœal

layer, and we are not concerned at present with the task of separating or identifying its constituent units. Our object will rather be to see in what way these units combine so as to produce new aggregates, small or large, which, either singly or after many processes of fission, result in the production of different kinds of organisms of higher type.

Before proceeding to answer this question in some detail, it is only right to point out that it is far from being the rule that the early organisms to appear in all vegetal and animal infusions are Bacteria of different kinds. In some infusions made from carrot or from turnip I have found that minute Torulæ are numerous from the first. Then, again, it is not the rule for all infusions that the Bacteria which accumulate at the surface should begin at once to assume a zooglœal development, as is the case with those that reach the surface of hay infusions. On the contrary, in many cases the Bacteria remain separate, for some days at least, so that no coherent membrane is formed, or, if formed, not till a later period.

Examination of one of the separate masses of Zooglœa with a high power will show its constitution, and reveal the fact that we have to do with an aggregation of separate Bacteria imbedded in a jelly-like material. The slightly-altered Bacteria within the Zooglœa mass are at an early stage plainly to be seen (as in Figs. 22 and 28, A, $\times 500$), though later on they become more or less obscured by reason of progressive molecular changes taking place in the mass during its subsequent transformation.

Some of these Zooglœa masses, as we shall see, are destined ultimately to be converted into numbers of flagellate Monads or of Amœbæ, while others become resolved into heaps of Fungus-germs. I have found it impossible to tell, in their early stages, from the mere microscopical appearance of the Zooglœa masses, whether they are destined ultimately to yield Monads, Amœbæ, or



FIG. 22.

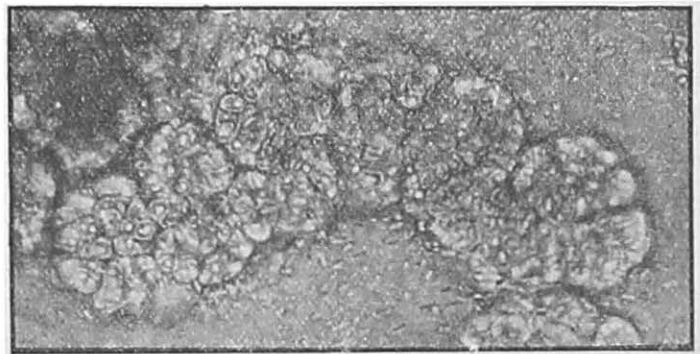


FIG. 23.

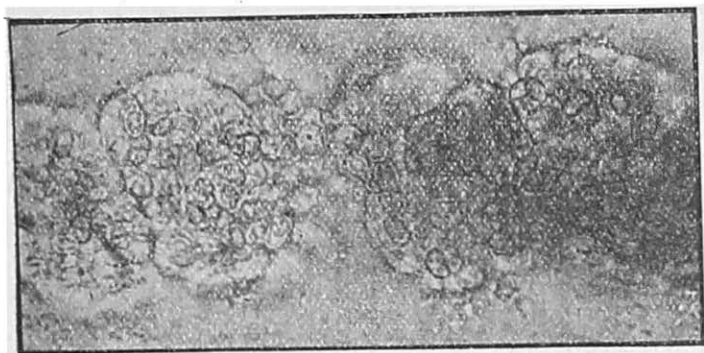


FIG. 24.

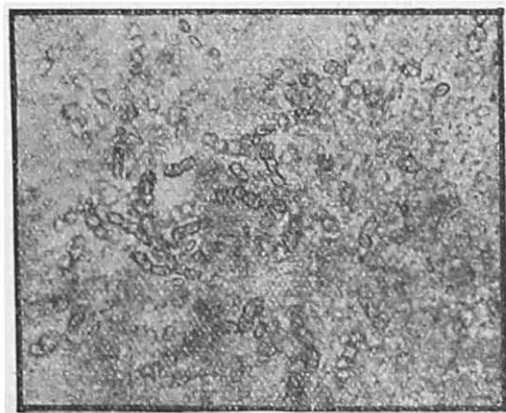


FIG. 25.

Fungus-germs; but the latter transformation is undoubtedly the more common.

It is important to bear in mind two fairly distinct aspects of the observations about to be recorded, corresponding with different stages of the processes in question. We have to do (1) with the growth, the individualisation, and the processes of segmentation taking place in these minute masses of Zooglœa. We have also to do (2) with the question of the ultimate destination, or the transformation, of the products of such segmentation. These are two parts of the subject that are to some extent distinct, and which are well worthy of separate consideration.

(1) *The Developmental Tendencies in Zooglœa.* If it be asked, what amount of knowledge bacteriologists possess on this subject? the answer must be, "extremely little," if we are to judge from the paucity of information on the subject which is to be found in any of their text-books. Yet, if they would only deign to look at what takes place in the scum on a filtered hay infusion, they could very soon satisfy themselves that Zooglœa masses not only grow rapidly, but undergo definite developmental processes, associated with marked molecular changes, as evidenced by their different behaviour at different stages to logwood or other stains, as well as by the results of microscopical examination. And while these molecular changes are taking place, the masses may segment into larger or smaller portions, and often into minute spherical or ovoid units, showing that an organising process is taking place, as may be seen in Figs. 19 and 20 ($\times 500$).

If we look at the constitution of Zooglœa masses, as shown in Figs. 22 and 23, A ($\times 700$), it may be seen that we have only to do with Bacteria imbedded in a varying proportion of glœal material. But later on, when the mass has grown, and some amount of segmentation has taken place, as in Fig. 29, A ($\times 500$), it may often be seen that

we have still only to do with aggregates of Bacteria. At other times, it is true, the molecular changes that have taken place in the mass have so altered its constitution (making the segments very refractive) that the included Bacteria are no longer recognisable, as in Figs. 19 and 26, A.

But now an important link in the proof of my views may be mentioned. The Zooglœa masses in their early stages are colourless, but a large proportion of them are, as I maintain, ultimately destined to give rise to brown Fungus-germs. The assumption of the brown colour may, however, be taken on by the segments of the Zooglœa while they are still only aggregates of Bacteria, as may be seen by Fig. 29, A ($\times 500$), showing nearly spherical segments of a large Zooglœa mass which, at the time they were photographed, were in different shades of brown, and in some of which the constituent Bacteria (now themselves coloured) are plainly to be seen. The same thing occurs not unfrequently in Zooglœa masses which have as yet undergone little or no segmentation, as in Fig. 29, D; while in B a small segmenting Zooglœa aggregate is seen becoming brown, the process being rather more advanced below than above. In the latter situation the mass seems to fuse almost imperceptibly with the contiguous portion of the pellicle. In C a similar mass is to be seen which has almost completely segmented into slightly brown Fungus-germs. In E we have two other much smaller masses of Zooglœa which have become brown, and which plainly show their constituent Bacteria; while in F a similar process is taking place in very minute units of Zooglœa, such as give rise to discrete Fungus-germs.

These facts concerning the changes taking place in large or small masses of Zooglœa, even leaving aside the question of their subsequent transformations, are facts of considerable importance, well worthy of a little attention on the part of bacteriologists.

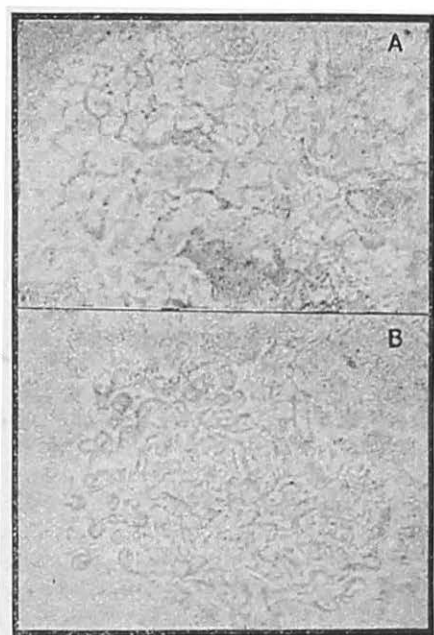


FIG. 26.

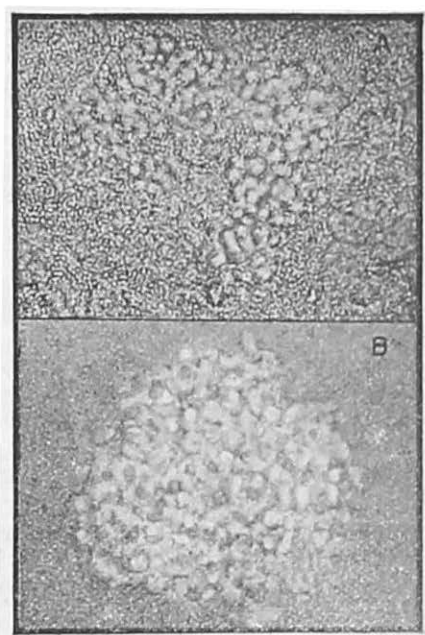


FIG. 27.

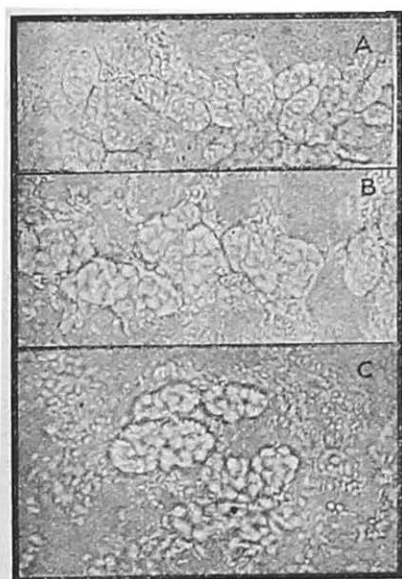


FIG. 28.

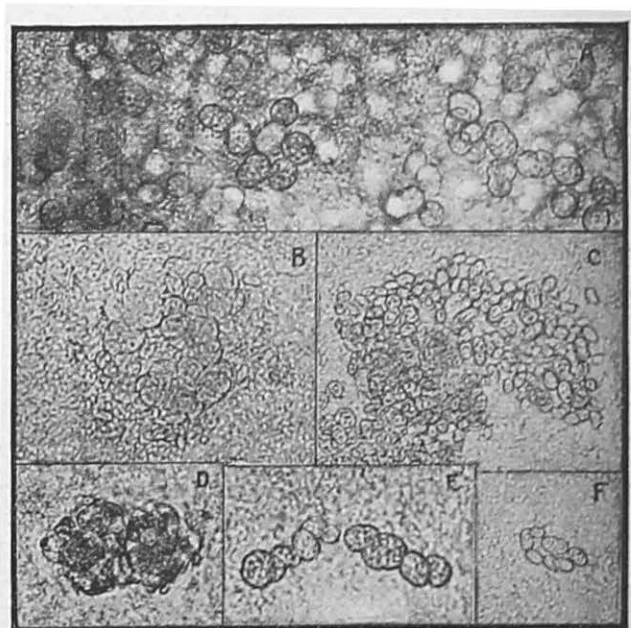


FIG. 29.

Although my more complete recent observations as to the assumption of a brown colour by the Zooglœa masses and their segments serve to make the relationship between them and the brown Fungus-germs more obvious than it was previously, still the actual proof of the development of the one from the other can now be given in a very complete way—also as a result of recent observations.

(2) *The Transformation of Zooglœa Masses into Fungus-germs or Monads.*

The very distinct specimens to which I shall first refer were taken from the scum on a hay infusion that had been exposed in a small closed pot to a temperature of 70° F. (21° C.) for seven days. Many of the Zooglœa masses had by that time become wholly transformed into brown Fungus-germs, though other masses in all intermediate stages were to be seen. Some were still in an early colourless state, as in Fig. 22 ($\times 700$). A later stage, in which the whole mass is being converted into embryo Fungus-germs, is shown in Fig. 23 ($\times 700$), parts of which are assuming a faint brown tinge; and a still later stage, in which the nucleated embryo germs are more distinct, and in which portions of the mass have assumed a still darker brown colour, is shown in Fig. 24. Some of the brown Fungus-germs into which such a mass becomes resolved may be seen (under a lower magnification) in Fig. 25 ($\times 500$), beginning to grow into short chains, preparatory to the formation of mycelia.

In the pot from which these specimens were taken at the end of the seventh day not a single hypha was to be found from which such germs could have been derived; and all such bodies were similarly absent for many days after, during which from time to time I examined the contents of the pot. It was perfectly plain, indeed, that in the thousands of small Zooglœa masses undergoing this change one had to do with no process of infection. It was clear that the Zooglœa masses were becoming organ-

ised simultaneously and throughout their whole substance, and that all the stages of the development of the Fungus-germs into which they were being transformed could be more or less plainly traced—the changes in this case taking place without any antecedent minute segmentation of the masses, such as is so common, and which may be seen in a small scale in Fig. 29, C, and in larger masses in Figs. 19 and 20.

A similar transformation of the Zooglœa masses without antecedent segmentation has been seen in other cases, though in none of them have I been able to make out the actual stages of the change anything like so plainly as in the specimens represented in Figs. 23 and 24. In one of the cases recently seen, in which the specimens were also taken from a closed pot, the Zooglœa masses, as a whole, had previously assumed a pale brown tint, and rather large Fungus-germs were formed from their substance. But, again, no hypha of any kind was ever seen among the contents taken from this pot.

Where a certain amount of segmentation has occurred in the Zooglœa mass, and the brown colour is subsequently assumed (as in Fig. 29, A, in which the constituent Bacteria are still distinct), the rather large brown segments subsequently become resolved into groups of small brown Fungus-germs, such as are shown in Fig. 25 ($\times 500$).

At other times segmentation goes on to the production of the ultimate units that are to be formed from the Zooglœa mass before any change in colour occurs. This is shown in Figs. 19 and 20 (each $\times 500$). In Fig. 19 segmentation may be seen to be progressing in a colourless mass; while in Fig. 20, and especially in the upper half, colourless ultimate segments are separating and becoming brown. In the lower half of the figure larger masses are separating and becoming brown, as in Fig. 29, A, which will subsequently divide into two to five ultimate segments. The same kind of thing occurred in the specimen shown in Fig. 21 ($\times 500$),

which was taken on the twelfth day from a pellicle on another hay infusion. In the upper and right side of the figure the principal mass of colourless segments may be seen, and some of these took on a brownish-black colour before they began to germinate, as shown in the figure. The heterogenetic Fungus-germs thus produced are minute ovoid bodies, with one or sometimes two nuclear particles, such as may be seen in this case, and also in Fig. 25 and in Fig. 29, C.²

From what has already been said it will be seen that there are various modes in which Fungus-germs originate from the Zoogloea masses. Still, these different modes are only comparatively unimportant variations, dependent upon the different states of the Zoogloea masses in regard to colour and degrees of segmentation at the time when the actual transformations take place.

As previously stated, the conversion of the Zoogloea masses into aggregates of Monads takes places less frequently than their transformation into Fungus-germs. Still, the two processes may sometimes be seen going on side by side in the pellicle. I am, indeed, disposed to think, from what I have seen, that some of the same kind of Zoogloea masses which in the early days become converted into Monads may, if they chance to remain untransformed for seven or eight days, be then converted into Fungus-germs rather than into Monads. It is difficult to be certain of this, but I am strongly disposed to believe that it is so. It is quite certain, however, that conversion into Monads, when it occurs, takes place almost always somewhere between the third and the fifth days; while after the latter date up to the tenth or twelfth day one finds Zoogloea masses either all brown or producing brown segments which are being converted into Fungus-germs. *But the Zoogloea masses and all*

the segments into which they divide invariably remain colourless where Monads or Amœbæ are to be the products.

Since a previous communication to *Nature* (Nov. 24, 1904) I have seen enormous numbers of the Zoogloea masses yielding Monads in the pellicle on an infusion of hay, which was exposed to diffused daylight in a small beaker. The Monads were met with on the fourth and fifth days, the temperature to which the infusion had been exposed having varied from about 54°-59° F. (12°-15° C.). The Zoogloea masses were in many cases very large, and presented some peculiar characters. In their early stages they always appeared as very pellucid, somewhat ramified, discontinuous areas, such as are shown in Fig. 26 ($\times 500$); A represents an early, and B a later, stage, in which separation of ultimate segments is commencing. Still later stages are shown in Fig. 27 ($\times 500$); portions of a mass beginning more fully to segment may be seen in A; while in B the segmentation of a larger mass is more complete, into motionless, colourless units, which after a time may become either active flagellate Monads or Amœbæ—sometimes the one and sometimes the other of these convertible forms of life. A similar process, taking place in other small Zoogloea masses, is plainly shown in Fig. 28 ($\times 500$): thus in A we have small masses of Zoogloea in which the constituent Bacteria are plainly seen; in B other masses becoming more refractive and about to segment; while in C segmentation is actually occurring, and the products are still more refractive. They, as well as the products in Fig. 27, B, are destined to develop either into Monads such as are shown in Fig. 30, E ($\times 500$), or else into minute Amœbæ of the same size.³

(3) On the Production in and from

² The share taken by the gloeal material in the production of these bodies is referred to in my *Studies in Heterogenesis*, p. 82, as well as the relations of this gloeal substance to protoplasm.

³ It is almost impossible to get satisfactory photographs of these organisms, because of their movements and the deforming influence of reagents on their delicate structure.

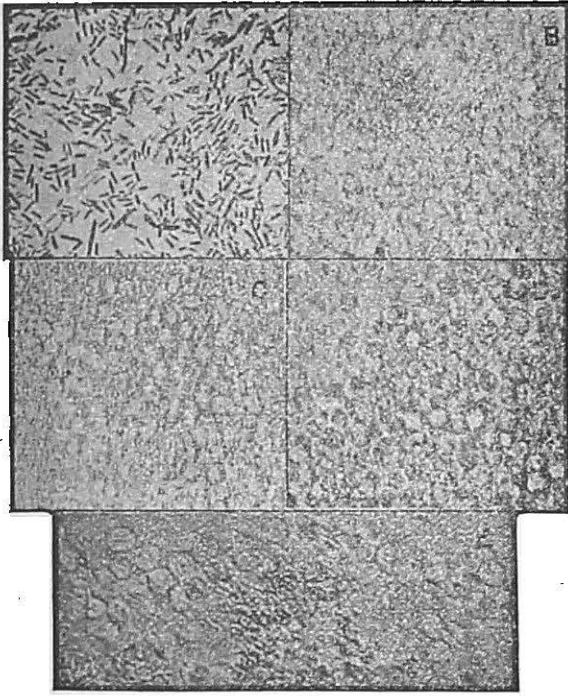


FIG. 30.

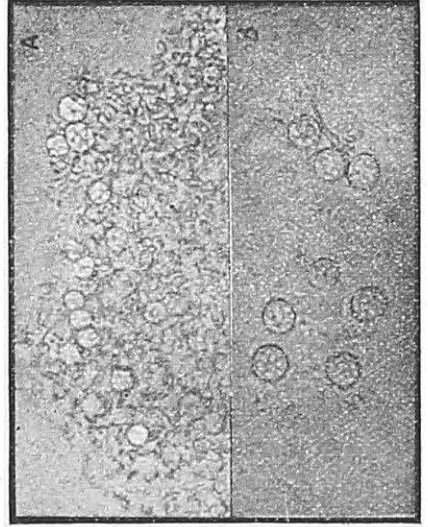


FIG. 31.

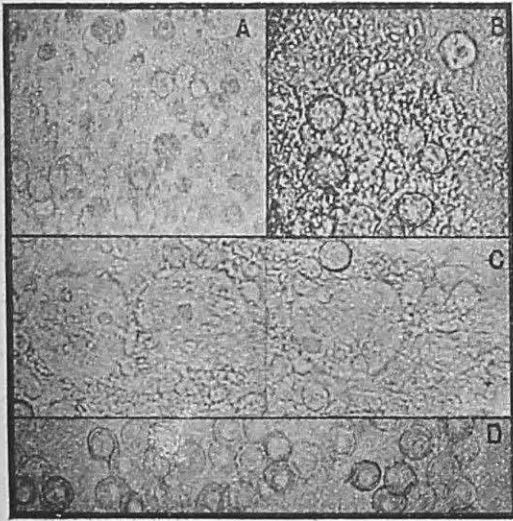


FIG. 32.

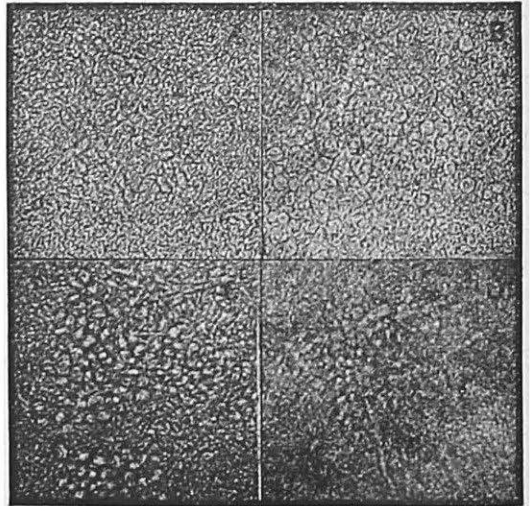


FIG. 33.

the Pellicle of discrete Corpuscles which speedily develop into Flagellate Monads or Amœbæ, or else into Fungus-germs. At the expiration of three or four days, according as the temperature to which the infusion is being exposed is up to, or some degrees below, 70° F., the first indication of the formation of discrete corpuscles may be found. When a portion of the pellicle has been taken up on the tip of a sterilised scalpel, and rotated off on to a drop of water or of some staining fluid placed on a microscope slip, and when the cover-glass has been applied, we find on examination a thin membranous, granular-looking layer surrounded by myriads of free and active Bacteria, though no free Monads or Amœbæ are to be seen. This layer will probably be already flecked by many of the minute Zoogloea masses already described; but on examination with a power of four or five hundred diameters portions of the pellicle lying between them may be seen to display a more or less evenly granular appearance, owing to the Bacteria entering into its formation having in part ranged themselves at right angles to the surface of the fluid, and being motionless, by reason of being imbedded in the viscid glœal material which they have excreted.¹ In many other places, however, especially when the pellicle is looked at slightly above the proper focal distance, appearances such as are represented in Fig. 30 B, C ($\times 500$), are to be observed. That is, the pellicle in these situations is seen to be pretty closely packed with a number of minute, motionless, and rather ill-defined whiteish corpuscles, which are almost always found to be located in the under layer of the pellicle.

I have over and over again noticed these appearances in the pellicle, and,

¹ Though motionless, they are far from being dead, as Pouchet imagined. He said (*Histogénie*, 1859, p. 354): "La pellicle prolifère étant constamment formée par les cadavres des animalcules dont les générations se sont succédé." He, in fact, invariably spoke of the Bacteria in the pellicle as "les cadavres."

owing to the corpuscles being more fully formed, even more distinctly as in Fig 30, D, when there was not a single active Monad or Amœba in or around the portions of the pellicle under the same cover-glass. But when other portions of the same pellicle have been examined twelve to sixteen hours later thousands of active Monads have been present, *all of about the same size as the motionless corpuscles previously seen to be forming*, and each moving more or less rapidly by means of a single flagellum. These active Monads, such as are poorly shown in Fig. 30, E ($\times 700$), are to be found not only around the portion of the pellicle under examination, but beneath it, where previously only motionless corpuscles existed.

For a long time, owing to the discrete corpuscles in their motionless condition being situated on the under surface of the pellicle, I found it very difficult thoroughly to satisfy myself as to their actual mode of origin. The general indications were certainly strongly in favour of their having been formed in and from the pellicle itself, as may be gathered from the following considerations:—

(1) Thousands of Monads appear comparatively suddenly of full size, and often when their appearance has been seen to be preceded by the gradual formation of multitudes of motionless corpuscles of the same size in the under layer of the pellicle.

(2) Never in their motionless condition, and only very rarely in their free active state, was any evidence of multiplication by fission to be seen, although many thousands of these Monads have been examined from time to time.

(3) In the course of five or six days, when the weather is warm, Monads are in some pellicles formed in enormous numbers, and obviously at the expense of the pellicle itself, since whole regions of this membrane actually disappear where they have been formed, leaving only intervening ridges, in the midst of

which other motionless corpuscles appear to be forming.¹

These facts and considerations alone, as I have said, seemed to indicate very forcibly that the corpuscles were formed from the very substance of the pellicle, and that they had not sprung up from multitudes of invisible germs, gradually growing till their full size had been attained, and then multiplying with great rapidity.

After a time, however, I thoroughly satisfied myself that the corpuscles are, as a matter of fact, only individualised portions of the general Zooglœa mass of which the pellicle is composed—each corpuscle containing several Bacteria. And often, when the corpuscles separate, and first begin to exhibit slow oscillating movements, they may be seen to have an exactly similar composition. They appear then as small pellucid spheres, having a single flagellum, and containing in their interior four or five Bacteria, exactly like those in the region of the pellicle from which they have been derived.² These Monads soon increase somewhat in size and become more active; the Bacteria in their interior are no longer distinguishable, and a distinct nucleus is formed, as in Fig. 30, E.

In some pellicles the discrete corpuscles that are produced in the manner indicated develop into Amœbæ rather than into Monads. This probably depends upon some differences of an unknown nature in the chemical constituents of the infusion itself favourable to the manifestation of this phase of the Monad-Amœba couple. That the two forms are rapidly and easily convertible the one into the other is very generally admitted. In an infusion made from a bunch of *Melica nutans* in full flower

minute Amœbæ were produced in myriads from the pellicles in this way. Fig. 32, A ($\times 375$) shows a number of these Amœbæ, varying much in size, which were found in the pellicle on this infusion. Again, in an infusion made from mixed grasses in full flower (largely composed of *Lolium perenne*) a thin pellicle was observed, the weather being warm, in which Monads were scarce; but vast numbers of small active Amœbæ existed as early as the second day. In regard to the appearance of this pellicle on the fifth day my note-book says: "Three-fourths of the pellicle is now apparently converted into Amœbæ, which exist in myriads. Not a single one of them has been seen to divide." In some ordinary hay infusions, also; Amœbæ are produced in great numbers from the discrete corpuscles. A number of small and large Amœbæ that were found, with multitudes of others, on the surface of a rather old pellicle in which they had had time to grow are shown in Fig. 32, C ($\times 500$); while B represents other Amœbæ beginning to encyst in another hay pellicle on the twelfth day, the upper one of which still exhibited very sluggish movements; and D ($\times 500$) shows many others completely encysted, in which the protoplasm is, in some of them, more or less contracted, and shrunk away from the cyst-wall.

An infusion of roses made from the flowers, leaves, and stems, or even from the petals only, often yields Amœbæ in great abundance. In making an infusion from the latter, the petals were allowed to macerate for about twelve hours before the fluid was filtered off. The Amœbæ found in such infusions are sometimes very short-lived. Thus, in one of them they were very abundant and active on the fourth day, but two days later they were almost all found to have become encysted.

The production of Amœbæ in abundance is, again, practically invariable in mixtures of egg and water. When one tea-spoonful of mixed white and yolk of an egg is added to eight ounces of filtered

¹ This is shown in my *Studies in Heterogenesis*, Pl. VI., Fig. 54, D.

² Sometimes, however, the Bacteria are not so clearly recognisable, as I have above indicated, and the corpuscle, as a whole, is much more refractive. Similar differences exist also, as I have shown, in regard to the segments into which embryonal areas divide.

tap-water, enormous quantities of Amœbæ are to be found after a time in the pellicle which forms on such a mixture, though they have generally been preceded by the appearance of Monads. A very thick and glistening pellicle is commonly produced, and after six or seven days Amœbæ appear therein in great abundance. Their presence may be rendered most obvious by allowing portions of the pellicle to soak in a drop or two of logwood solution for some hours. Their actual mode of origin is apt to escape observation owing to the thickness of the pellicle in which they are formed. Still, on a few occasions I have been fortunate enough to find very cogent evidence that they are formed in just the same way as the discrete corpuscles produced from the pellicles on hay infusions—that is, by individualisation and metamorphosis of minute portions of Zooglaea.

As these discrete corpuscles sometimes develop into Monads and sometimes into Amœbæ, I have been accustomed to speak of them as "indifferent corpuscles." Not unfrequently, however, after their formation they may remain for a long time quiescent, and without developing in either direction. This was the case in a hay pellicle that I had frequently examined, in which such corpuscles were found in enormous numbers, many of them being rather larger than usual. They were first seen when a small pot containing some hay infusion was opened after four days, during which it had been exposed to a temperature of 70° F. (21° C.). The edge of a portion of this pellicle is represented in Fig. 31, A ($\times 500$); while in B ($\times 700$) some of the separate corpuscles are shown more highly magnified, so as to reveal the nature of their contents. Nothing like a nucleus is to be seen, nor can one be detected by the use of any of the ordinary stains even when the corpuscles have been allowed to soak in them for many hours. Logwood, carbo-fuchsine, gentian violet, and mastzellen stain have all yielded negative results. Not a trace of a nucleus is to be found, and the cor-

puscles seem to be mere individualised portions of Zooglaea intermediate in size between the brown units on the way to the production of Fungus-germs, which are shown in Fig. 29, E and F, and, like them, containing only a few Bacteria in their interior. I have examined such corpuscles over and over again, and always with similar results. If they do not speedily develop, a limiting membrane is produced which enables their contents to resist staining for some time, and causes the corpuscles themselves to shrivel if they are mounted in glycerine and water. When, however, these corpuscles develop quickly, which is the rule, they give rise, as I have said, more or less suddenly to swarms either of Monads or of minute Amœbæ, and then, when thus developed, a delicate nucleus can generally be recognised even without the aid of stains.

While the discrete corpuscles that develop into Monads and Amœbæ seem almost always to be produced from the under layer of the pellicle, those that develop into Fungus-germs are almost invariably produced from its superficial layer. The fact of their origin from the substance of the pellicle itself is, therefore, as a rule, much more readily to be made out than that of the discrete corpuscles whose origin and nature we have previously been considering.

The discrete Fungus-germs may easily be observed to commence by the individualisation of small ovoidal or spherical portions of the pellicle, in which at first, under a high power, the contained individual Bacteria are distinct, though not to be seen in Fig. 33, A ($\times 375$), which shows the germs under a low power as they were originating on the third day on the surface of a hay pellicle; or in B on the fifth day from a pellicle on an infusion of rye grass (*Lolium*); and again in C from another infusion of the same grass which was made more than twelve months later.* In D similar

* The grass in the interval having been kept in a small cardboard box.

corpuscles taken from a pellicle on a carrot infusion on the third day are developing mycelial filaments.

It not infrequently happens that the units which originate in this way become, as they develop, more refractive, as shown in the last two figures. More frequently, however, they may notably change in colour—becoming brown, or even brownish-black, as they mature. This latter change is well seen in Fig. 21.

Of course, all that has been said in this chapter is very contrary to generally accepted beliefs. This production of different kinds of living organisms by the

fusion and differentiation of aggregates of simpler living units is something altogether unique, and hitherto unknown to biological science. But if others would only investigate this subject with care, as I have repeatedly done, there can be little doubt but that my results would speedily be confirmed. When will the bacteriologists tell us what they know about *Zoogloea*—whether they are or are not aware of its developmental tendencies, and why it should undergo processes of minute segmentation, unless such processes are a result of an organising tendency destined to have some definite outcome?

CHAPTER XI.

CONCLUSION: THE CONGRUITY OF THE EVIDENCE

IN not-living matter we have combinations of molecules of all degrees of complexity—beginning with those entering into the composition of chemical elements, and ending with the big and highly complex components of various colloids. And seeing that the so-called elements are built up by molecules composed of different numbers of similar atoms, the difference between elements and compounds simply depends upon the likeness or unlikeness of the atoms entering into the composition of their molecules.

Thus molecular composition is an important item, even with reference to substances that are looked upon as elementary—different modes of composition or arrangement of the atoms of like kind sufficing to produce what are known as “allotropic” states. It will be easily understood, therefore, that

in compound substances a greater and greater possibility of molecular rearrangement arises in proportion to their atomic complexity. Gradually, in fact, this atomic complexity becomes the all-important character of a compound, and one to which the nature of the constituent atoms is altogether subordinate. In proof of this, one has only to refer to the multitudes of “isomeric” compounds having wholly different properties made up of carbon, hydrogen, and oxygen in the same relative proportions, and to the enormous number of other isomeric compounds resulting from the varying modes of arrangement of a few definite elements.

There can be no reasonable doubt that the form of a crystal is a resultant necessity, predetermined by the molecular properties of the matter composing it, and the sum-total of conditions

acting thereupon at the time of collocation.

Similarly, there are excellent reasons for believing that the form and structure possessed by each simple organism is that which is necessitated in the medium in which it exists by the nature and properties of the complex organic molecules of which it is composed.

The fundamental difference between a crystal and an organism lies, indeed, in the fact that the one is a "statical" and the other a "dynamical" aggregate, this difference being largely dependent upon the complexity of the molecules of which the latter is composed. Organisms are dynamical aggregates because among their molecules new motions and new arrangements are continually being assumed, in the course of which, in lower organisms, there frequently arises a spontaneous division of the parent mass—that is to say, "fission" or "gemmation" takes place.

All parts of very low organisms, when thus separated from the parent, have the power of developing into living things of a similar kind. This property has points of resemblance to the process whereby a fragment *broken* from a pre-existing crystal, and thrown into a suitable solution, gradually grows into a perfect crystal, similar to that from which it has been derived. Though the fragment will reproduce a crystal similar to that from which it was taken, the crystal can undergo no spontaneous division.

If it is true, as we affirm, that *Torulæ*, and even *Bacteria* (in zoogloal aggregates), are able to develop more or less directly into simple forms of *Fungi*, and that such *Bacteria* and *Torulæ* are merely some of the primary forms most frequently assumed by certain kinds of new-born living matter, then, obviously, the form and structure of the *Fungus* would stand in the same relation to the matter of which it is composed that the form and molecular structure of the crystal does to its matter. There would be, in fact, just as much reason why the

new-born organism should develop into the form of one already in existence as there would be that the crystal of sulphate of soda which forms to-day in a solution of that substance should resemble that which formed under similar conditions twelve months or a hundred years previously. He who believes in the uniformity of natural phenomena could anticipate no other result. Living matter which we believe to be now produced *de novo* speedily shapes itself into some well-known form; and so, also, new crystalline matter, which may have been produced synthetically by the chemist in his laboratory, falls habitually into one or other of the known crystalline systems. In each of these cases there is a similarity of product, because the crystalline or the organic form produced is to be regarded as the physical expression of the harmonious actions which have led to its production—because the forms are the results of a physical necessity, and not of a mere blind chance.¹

Looked at in a general sense, the phenomena of heredity seem to be only special manifestations of the property we have spoken of as "organic polarity," as a result of which, as we have seen, parts of lower organisms are capable, under suitable conditions, of reproducing similar entire organisms, just as a part of a crystal immersed in its mother liquid will, when the conditions are suitable, reproduce the form of the parent crystal.

Neither Herbert Spencer nor Weismann alludes to the existence of transformations in organisms at all comparable with the allotropic and isomeric transformations that are so remarkable in simpler forms of matter, and the possibility of which, as the former admits (see p. 133), increases so much in frequency as its constitution becomes more complex. Yet when he comes to speak of

¹ Yet Professor Huxley once made some very sensational remarks to a large audience in opposition to such views (*Quart. Jnl. of Microsc. Science*, Oct., 1870, p. 366).

the still more complex form of matter entering into the composition of protoplasm, such transformations are no longer referred to even as possible. The matter itself is admitted to be of the most changeable kind, and the forms of living things are admitted (see pp. 56, 58, 60) to depend upon the constitution of the units of living matter from which they arise (the "physiological units" of Spencer or the "ids" of Weismann), and yet not one word is said by either of these authorities as to the possible existence among simple organisms of transformations of form akin to those which occur in such simple substances as carbon, phosphorus, salts of mercury, and other metals, as well as in the carbon compounds (see pp. 30-32, 39-41).

I, however, believe that analogous transformations of living matter are constantly occurring among lower forms of animal and vegetal life, and that these are represented by multitudinous heterogeneous transformations, some few of which only have been referred to here, though they have been much more extensively examined in my *Studies in Heterogenesis*.

These lower forms of life for the most part multiply, as I have said, by mere "discontinuous growth," and their forms, like those of crystals, seem absolutely dependent upon their molecular constitution and the environing conditions in the midst of which they exist. Changes of this or that kind in their environing conditions will, in many of them, lead to remarkable changes owing to alterations thereby induced in their molecular constitution—alterations which have as their result the establishment of new morphological tendencies. Moreover, I believe that many of the discrepant observations made by good observers as to the ordinary phases in the life-history of such pleomorphic species as *Chlamydococcus*, *Pleurococcus*, and many others, have been due to modifications occurring from time to time in their developmental phases, under the influence of special conditions of one or another kind, though either of such phases may ultimately

revert to the form from which it started. The accounts that have been given by good observers show that a regular order is not observed, and strange phases are from time to time apt to be intercalated. Many of these lower forms of life are so much the creatures of circumstances—so transitory in their duration—that I long ago proposed to speak of them as "Ephemeromorphs,"¹ in contradistinction to the regularly recurring forms comprised under and understood by the term "species."

It is the close inter-relations and interchangeability that I have shown to exist between the varied lower types of life which forces upon us the belief that the actual forms assumed are as much the immediate results of their molecular composition and their environing forces as are the various forms assumed by crystals. I am driven to the belief, in fact, that "organic polarity" is the dominating influence in the production of this or that form among these lower organisms composing the great assemblage of "Ephemeromorphs"; that heredity in any other sense is not operative among them; and that this whole world of lowest vegetal and animal forms of life must be wholly removed from the influence of Natural Selection.

I am well aware that these are very unorthodox views; that Darwin, Weismann, Poulton, and others actually believe that the rate of change is lower among lowest than among higher organisms. This most surprising view is based in the main upon the fact, dwelt upon by Huxley and others, as to the "persistence of types" through geologic ages—a fact which, with the proof of the occurrence of heterogenesis, is, as we shall see (pp. 136-137), capable of receiving a totally different interpretation, and one in no way inconsistent with the recognition of all that has been shown by multitudes of workers as to the extreme variability of these Ephemeromorphs,

¹ *The Beginnings of Life*, 1872, vol. ii., p. 559.

and the pleomorphism of such organisms as *Protococcus*, *Hæmatococcus*, *Chlamydomonas*, *Sphæroplia*, and others. The changes among these latter organisms are indeed so fitful, and vary so much in their order at different times and seasons (as witness different observers), that their varying successive changes could not be considered to be results of heredity. Like actual heterogenetic transformations, such irregular variations must be considered to depend upon slight isomeric changes in the molecular constitution of what Herbert Spencer terms their "physiological units," acting in conjunction with organic polarity as a formative principle.

The extreme variability of the organisms referred to above is well illustrated by the researches of Professor F. Cohn on *Protococcus pluvialis*; and these observations by an investigator of great repute may be cited as an example of many others.¹ He has shown, as derivatives of this apparently simple Alga, the most widely differing forms of Phytozoa, which were actually seen by him to be derived from one another by direct processes of transformation. In summing up the results of his investigations he said²: "Thus we see that a single species, owing to its numerous modes of propagation, can pass through a number of very various forms of development, which have been either erroneously arranged as distinct genera, or at least as remaining stationary in those genera, although, in fact, only transitory stages." And, after a brief enumeration of the principal changes he has witnessed among the derivatives of this simple Alga, he adds the following important statement: "A critical and comparative consideration of the foregoing facts would, therefore, appear to render untenable almost all the prin-

ciples which modern systematists have hitherto adopted as the basis for the construction of their Natural Kingdoms, Families, Genera, and Species."

It seems clear, in fact, that, so long as organisms multiply by mere "discontinuous growth" (fission or gemmation), there is absolutely no reason to appeal to heredity except in the form of "organic polarity." This would be all-sufficient for the preservation of their likeness under similar conditions; to account for their changes under dissimilar conditions; and for their reversion (as with crystals and double salts)³ to old forms on renewal of old conditions—a class of facts among organisms with which the researches of many bacteriologists have made us perfectly familiar.

It is, then, not till we get out of the great Ephemeromorphic plexus, and come to organic forms which habitually multiply by sperm-cells and germ-cells (or occasionally by the latter alone), that heredity, as postulated by Darwin and Weismann, comes into play; and it is, consequently, not till we reach such forms that it would be possible for Natural Selection to be influential as a cause of evolution. I make bold to say this, notwithstanding the opposite views held by Darwin himself, Weismann, and others, who, as is well known, maintain that the influence of Natural Selection as a producer of fitness between organisms and their circumstances is one which is applicable to all forms of life.

The evidence for the conjoint association of Organic Polarity and Heterogenesis is far more certain, and, indeed, belongs to a wholly different order of certitude from that upon which the co-operative alliance between Heredity and Natural Selection depends. On the one hand, we have the facts directly observed and recorded in Chapters viii.-x.; while, on the other, Weismann himself says:²

¹ See Transl. of his memoir, with Plate, in *Botanical and Physiological Memoirs*, Ray Soc., 1853. The reader may also consult Cook's *British Fresh Water Alga* for details as to the great variability of the other forms mentioned above.

² *Loc. cit.*, p. 559.

³ See pp. 31, 32.

² *Studies in the Theory of Descent*, trans., p. 643.

"It is true that we have never directly observed the process of natural selection"; and he then goes on to illustrate the kind of evidence that exists for it, and its cogency, by adding: "neither has anybody directly observed the mode in which the heat of the animal body is generated by the processes of combustion going on in the blood and in the tissues—nevertheless, this is believed as a certainty." We see, therefore, that for the establishment of the doctrine of Animal Heat as well as that of Natural Selection, although the amount of direct observation has been altogether inadequate to bring about conviction, the lacunæ—as in scientific investigations generally—have been filled by the exercise of reason. Yet the tendency with my critics has been to demand nothing but observation, "continuous and unbroken,"² and to allow no place whatever for reason, even to supply the smallest links in the chain of evidence. That this is no exaggerated statement anyone may see for himself who will carefully peruse Appendix II. of my *Studies in Heterogenesis*, and contrast the evidence which I adduced as to the reality of the transformation of Hydatina eggs into great Otostomata, and the incredulity with which this evidence was received not only in this country, but abroad.

So far, indeed, from there being any *a priori* objections to *per saltum* development or Heterogenesis, which, as I claim to have shown, occurs so frequently, the fact of its occurrence ought to be considered thoroughly harmonious with all that we know concerning simpler forms of matter, and with the constitution of living matter itself.

As Herbert Spencer said,² it is a cardinal fact "that proteids admit of multitudinous transformations; and it seems not improbable that in protoplasm various isomeric proteids are mingled. If so, we must conclude that

protoplasm admits of almost infinite variations in nature." But the "ids" of Weismann as well as the "physiological units" of Herbert Spencer are essentially protoplasmic in nature, and, therefore, of great molecular complexity. These are the ultimate units, the "unknown somethings which have the power of organising themselves into a structure of this or that species."² I merely contend, therefore, that these ultimate units, looking to their nature and origin, must be supposed to be *capable of undergoing isomeric changes within certain limits*; and that, such changes having been brought about, these altered units would tend to unfold into new forms—more or less different from the parent forms, and corresponding with this or that kind of heterogenetic transformation. The number of such transformations now known is considerable among lower organisms, though comparatively rare among higher plants and animals, where they are represented by "sports," "Bud variations," and the "Mutations" of de Vries.

Great differences exist with respect to the degree of variation that may be induced even in different lower organisms within similar periods, under the influence of any given changes in their environments. Changes, which may be almost inoperative in producing a modification of some organisms, may lead to profound alterations in others. And, similarly, while a very prolonged continuance of altered conditions is needful to affect some organisms, the influence of changed conditions on others may be rapid and more or less immediate.

The greater the differentiation and complexity of any organism, the less is it likely to be influenced by slight or temporary modifications in the "conditions" or influences to which it is subjected. Thus, variations which may be induced for a time in higher organisms continually tend, when the modifying influences have disappeared, to be dwarfed, and, perhaps, ultimately abo-

² *Studies in Heterogenesis*, p. 130.

² *Principles of Biology*, vol. i., 1898, p. 67.

² *Loc. cit.*, p. 370.

lished, owing to the sum-total of internal forces acting in such a manner as gradually to reproduce the former condition of equilibrium.

But how different are the phenomena when we turn our attention to lower organisms.

If the facts made known and the views that I have been advocating seem difficult of acceptance to many, as to a reviewer in *Nature*,¹ who said, in reference to some of the facts, that they were "entirely beyond credence, because so meaningless," I would say let them keep steadily in mind all that has been adduced, as matter of common knowledge, concerning the isomeric states of various saline substances, their transformations in crystalline form, in colour, and in other characters—under variations in external conditions (pp. 30–32); as well as the total transformations of such elementary substances as carbon, phosphorus, and sulphur, owing to alterations in the exact collocation of the atoms in their component molecules. Such phenomena may help to supply something in the way of "meaning."

The alleged facts concerning heterogenesis may also be said to be inconceivable. Granted; but that is no reason for doubting their validity. How far are the processes of allotropism and isomerism themselves conceivable in any true sense of the word? Our notions of inconceivability in regard to physical phenomena have received many severe shocks, even during the last few years—by revelations concerning wireless telegraphy, the "mystery of radium," and the constitution of matter itself—that is, of the so-called "simple atoms" of which elementary substances are composed.

After such revelations, who would dogmatise and attempt to set bounds to the potencies of matter, living or not-living?

Let us, then, now briefly enumerate

some of the heterogenetic transformations which even a single observer has been able to recognise in a few years, as some indication of what might be discovered by other workers if they would only be content to search for Nature's secrets and the possibility of heterogenetic transchanges, as I have done. If in many cases they did not witness similar changes to those described here and elsewhere, they would doubtless reveal many other transformations just as marvellous—possibly more so.

In the foregoing pages I have shown that Bacteria of different kinds—*Torulæ* and Moulds, together with flagellate Monads and *Amœbæ*—may, and undoubtedly do, arise *de novo* by heterogenesis; while elsewhere (*Studies in Heterogenesis*) I have shown that various simple *Algæ*, Diatoms, and Phytozoa may take their origin from alien sources; and that a varied heterogenetic origin is most frequently met with for *Amœbæ*, *Actinophrys*, flagellate Monads, *Peranemata*, and even Ciliated Infusoria.

The phenomena there recorded and illustrated can neither be explained by the facile solution that they are "results of infection," nor can it be said that the new forms of life which seem to arise by heterogenesis are in reality normal and habitual phases in the life-history of the organisms from which they proceed. These other possible interpretations have always been carefully kept in view in each case, and the evidence for and against them duly weighed.

All the observations recorded in this work in proof of the heterogenetic origin of Bacteria and their allies have been conducted under aseptic conditions, and with all necessary precautions against the possibility of infection. While, in regard to many other observations that have been made—touching the heterogenetic origin of *Amœbæ*, Monads, *Peranemata*, and Ciliated Infusoria—it must be seen that these, if made at all, have to be made on dying organisms, either in their own or in experimentally varied environ-

¹ Feb. 25th, 1904, p. 386.

ments; that in either case aseptic conditions, or absolute isolation of the organisms under observation, is always an impossibility; and that any attempts in this direction would inevitably stop the changes hitherto in progress.*

In consequence of the sceptical attitude on the part of those whose preconceptions hinder their working at such subjects for themselves, it will be well for us now to look at the question at issue from a more general point of view. We find ourselves face to face with two antagonistic doctrines: (1) the view that living things no longer arise *de novo*, and (2) the view that living things are still coming into being *de novo*, both by Archebiosis and by Heterogenesis. Let us see, therefore, which view is most in accordance with certain well-known and generally admitted classes of facts, and which is more or less completely opposed thereto; for, as Herbert Spencer has said, "There is no mode of establishing the validity of any belief except that of showing its entire congruity with all other beliefs."

The first of these classes of facts which may be regarded as a kind of touchstone for testing the relative

* Those who are sceptical concerning Heterogenesis, and have any desire to be convinced, may easily, by following my instructions, study the changes occurring in Zoogloea masses in the pellicle on a hay infusion, and the appearance of Monads, Amœbæ, and Fungus-germs therein; or they may study the origin of Diatoms from cells of Chlorochytrium in dead Duckweed, the origin of pigment Amœbæ in Vaucheria filaments or "resting spores," or some of the remarkable changes which I have described as occurring within the cells of Nitella. Some of these transformations can generally be induced or found almost at will. Others, as I have shown, are much more fitful in their occurrence, or can only be met with under conditions not always easy to realise, as with the transformation of Hydatina eggs into Ciliated Infusoria, and many other of the changes which I have recorded in my *Studies in Heterogenesis*. This latter difficulty may be due sometimes to death of the proper organisms; sometimes to unknown changes in their intimate nature not favourable to transformations previously seen—just as Vries has found in regard to "mutations."

validity of the two views is the *present-day existence of vast multitudes of lowest organisms of all kinds*. If all the forms of life that have ever existed upon the surface of the earth have been derived from the primordial forms which first took origin by natural synthetic processes only in an incalculably remote past, no adequate and consistent explanation would be forthcoming of the undoubted existence, at the present day, of the teeming multitudes of such lower organisms as have been above referred to. For if the assumed gradual development of higher forms of life during all past geologic ages has been largely due to the intrinsic mutability of living matter, as the Evolution hypothesis assumes, would it not be a stultification of that hypothesis to suppose that such primordial forms as Bacteria, Torulæ, Amœbæ, and Monads have remained practically unchanged and in these low grades for untold millions of years? To suppose that some Bacteria, Amœbæ, Moulds, and other low organisms should have lived in unbroken continuity through pre-Silurian epochs, amidst all the changes of the Carboniferous, Triassic, Oolitic, Cretaceous, and more recent geologic ages, with that mutability as an essential characteristic which they are now seen to display, and yet that they should have undergone little or no alteration, seems too incredible to be seriously entertained.

It is impossible to say that they have preserved their primitive forms by reason of their existence in unchanging environments. The very reverse of this must have been the fact; and even now such organisms are to be met with abundantly all over the face of the earth, and under the most diverse conditions.

But, in accordance with the views advanced in this work, the present-day and past existence of these organisms may be fully explained, and is just what might be expected—if they are and have ever been seething up anew by Archebiosis and by Heterogenesis.

The fact of the *wide-spread distribution*

over the earth of these and other very low types of life affords another test of the relative validity of the two views. Sollas, in his work *The Age of the Earth* (1906, pp. 176, 196, 202), calls attention to the fact that low fresh-water forms of life are similar all over the world now and far back into geologic time. And the facts brought forward in this work seem to show that the intrinsic molecular composition and properties of the different varieties of living matter have much more to do with the forms and structures of lowest organisms than mere differences in their environment. Under the dominating influence of "organic polarity," the several forms arising by Archebiosis and Heterogenesis seem to unfold, more or less immediately, into such and such simple organisms of common type.²

We may say dominating influence of "organic polarity," because it is clear that "heredity" must be in any other sense a non-existent factor for the chance products originating by heterogenesis, and for forms multiplying only by fission or mere "discontinuous growth"; and because, as the writer believes, and long ago maintained, the influence of "natural selection" as a modifying agency is either absent or extremely slight among all the lowest forms of life.

Then, again, the facts and arguments furnished by heterogenesis, coupled with Herbert Spencer's views concerning "physiological units," seem capable of affording a better explanation than has hitherto been forthcoming of the *sudden and abrupt variations in higher plants and animals* which are well known to occur—variations that have been spoken of as "discontinuous" by Bateson, and as "mutations" by de Vries, and which are often so marked as to constitute sudden origins of what appear to be actual new species of animals or plants. These phenomena, including what are

known as "Bud variations," are perhaps the nearest approaches possible among higher organisms to the still more marked transformations that occur among lower organisms. Yet such abrupt "mutations" are now admitted by biologists to be both real and frequent—"proved up to the hilt," as an authoritative writer has lately said.

We come next to some facts relating to higher organisms, and having reference not only to their distribution over the face of the earth at the present day, but also to the distribution of their fossil remains through the different strata constituting the crust of the earth.

The new views would seem to diminish to an enormous extent many of the difficulties at present existing in regard to the *geographical distribution of higher plants and animals*. Darwin, with his usual candour, said: "Undoubtedly there are many cases of extreme difficulty in understanding how the same species could possibly have migrated from some one point to the several distant and isolated points where now found." He then went on to say: "Nevertheless, the simplicity of the view that each species was first produced within a single region captivates the mind. He who rejects it rejects the *vera causa* of ordinary generation with subsequent migration and calls in the agency of a miracle." Of course, in this latter statement Darwin was referring to an upholder of the obsolete view, which he so successfully combated, of the supposed "special creation" of species. But so far from appealing to "miracle" in postulating multitudinous centres of origin as possible, I appeal to the uniformity of natural phenomena in space and in time.

This last word brings us, indeed, to another and a related test for the relative validity of the two views now under consideration, by a comparison of the aid

² This is fully in accord with the views expressed in the quotations from Herbert Spencer on pp. 56 and 58.

³ *Origin of Species*, 6th ed., p. 320. See also Sollas, *Age of the Earth*, p. 188.

they may afford in tracing what has been termed the *ancestral history of organisms from a study of their fossil records*. It was formerly thought by geologists that rocks containing similar fossils were to be regarded as contemporaneous formations. But this view was traversed by Huxley in a celebrated presidential address to the Geological Society in 1862, wherein he advanced the strongest evidence in favour of the position that "similarity of organic contents cannot possibly afford any proof of the synchrony of the deposits which contain them; on the contrary, it is demonstrably compatible with the lapse of the most prodigious intervals of time, and with interposition of vast changes in the organic and inorganic worlds between the epochs in which such deposits were formed." Nobody now contests these views and believes, as was formerly the case, that any advance in the successional series of living forms must have occurred simultaneously in different parts of the world, and, as a consequence, that strata in which similar fossils are found must have been contemporaneous formations.

But if the old view is incorrect, and if very similar faunas may be found imbedded in rocks whose formation has been separated by long-drawn ages of time, the difficulties besetting the biologists in regard to time are just as great, or even greater, than those in regard to space or geographical distribution, to which Darwin referred in the preceding quotation.

But the distribution of similar organisms through geological strata in widely different parts of the world, and in formations belonging often to widely separated geological ages, will certainly be also capable of receiving a much better explanation if, instead of supposing that all organic forms have developed and spread from one single centre, we are prepared to recognise that there have been multitudes of independent centres with wide developments in and from them. It should never be forgotten

that, as G. H. Lewes said,¹ "The link which unites all organisms is not always the common bond of heritage, but the *uniformity of organic laws acting under uniform conditions*." And if through all the life-evolving period of the history of our globe the progress of "organisation" seems to have been essentially similar (so that development may have many times gone along more or less similar lines), this seems readily explicable by the consideration that living things, both as regards their origin and their subsequent differentiation or development, are the immediate products of ever-acting natural laws and material properties.²

But the truth of the new views is still more strongly shown by the light which they are capable of throwing upon another closely related palæontological problem of the greatest importance, which has hitherto proved a stumbling-block to biologists and evolutionists alike—that is, *the fact of the existence of "persistent types" of life through long geologic ages up to the present day*. As Huxley and others have shown, this long persistence of similar organic forms has been met with both among animals and among plants of comparatively high organisations. Certain genera of Molluscs, for instance, are said to "have persisted from the Silurian epoch to the present day with so little change that competent malacologists are sometimes puzzled to distinguish the ancient from the modern species."³ This persistency is assumed by some to be due to the slow rate of change among such organisms, or to their having passed into a rigid (as opposed to a plastic) condition,

¹ *Fortnightly Review*, 1868.

² Haeckel, in the final chapter of *The Riddle of the Universe*, speaking of the planets of other suns, says: "We are justified in supposing that thousands of these planets are in a similar stage of development to that of our Earth"; and, further, that the "course of organic evolution on these other planets has been analogous to that of our own earth—always, of course, given the same limits of temperature."

³ *Proceedings of Royal Institution*, vol. iii., p. 151.

and they would have us believe that such organisms have been perpetuating their kind in an unbroken manner and in the same likeness through this long succession of geologic ages.

I have already referred to the absurdity of such a view in regard to different forms of Bacteria which have now been proved to exist in animal and vegetal remains as low down in the geologic series as the upper Devonian strata (see p. 9). Again, a similar persistency through many geologic ages is known to obtain for two sets of very low organisms closely akin to other of those which the writer has shown to take origin so frequently by heterogenesis, and which, like Bacteria, are also notorious for their high degree of variability. I refer to Foraminifera (chambered Amœbæ) and to Diatomaceæ—very low types respectively of the animal and of the vegetable kingdoms. It is the siliceous envelopes of these low unicellular organisms, with their characteristic shapes and markings, that are fortunately preserved as fossils, and which thus permit of an accurate comparison being made of ancient and existing representatives of these simple forms of life.

Concerning the former organisms, Dr. Carpenter wrote¹: "There is no evidence of any fundamental modifications or advance in the Foraminiferous type from the paleozoic period to the present day." Similar types and similar varieties from those types are, he said, to be met with in geological formations existing as far back as the triassic rocks.²

Turning now to the Diatomaceæ, whose extreme variability is no less notorious, Dr. Gregory³ wrote as follows in reference to the agreement of recent and fossil

¹ Introduction to *The Study of the Foraminifera*, 1862, p. xi.

² But if certain low organisms developed into Foraminifera in remote geologic ages, there is no reason why they should not develop in the present day into essentially similar forms; and variation may now tend to manifest itself in the same fashion as it did formerly, owing to the fact that the causes (both intrinsic and extrinsic) leading to this variation are essentially similar.

³ *Proceed. Roy. Soc. Edinb.*, 1856-7, p. 442.

forms: "I have no hesitation in saying that I believe all the forms in the Ægina clay marl, which is the oldest Diatomaceous deposit yet described, will be found living on our coast.¹.....It may also be observed that of all the forms figured by Ehrenberg from more recent strata.....the great majority are perfectly identical with existing Diatoms." Sir Joseph Hooker, Pritchard, and other writers also bear witness to the cosmopolitan distribution of these organisms. The fact that they show themselves in the most varied regions at the present day, and also in far remote ages, with identical anatomical characters, seems to indicate pretty conclusively that the intrinsic properties and varieties of the living matter of which they are composed (that is, of their "physiological units") have more to do with their forms and structures than any differences in the conditions under which they have been born and have lived.

If lineal descent without, or with, little alteration had been the rule in past ages and up to the present day, we might, at least, expect now to find such evidence of this continuity as would be represented by *fixity of habitat*. Fixity of habitat is, however, notoriously non-existent among these lower forms of life. Speaking of Rotifers and other low organisms, Pritchard says²: "One remarkable circumstance must be borne in mind by the animalcule hunter. If he happens to remember a pond where some rare species abounded last year, let him not again turn thither in search of it, as the chances will not be in his favour. These creatures rarely exist in the same water during two successive years. The reasons for this are not easily ascertainable. The remark is equally applicable to Volvox and the Desmidiæ. The search will be most productive if prosecuted on new ground."

¹ The stratum at Ægina is said to belong either to the chalk formation or to the oldest tertiary or Eocene beds.

² *Loc. cit.*, p. 655.

Statements of this kind have also been made by others, and are fully borne out by my own experience.

Ralfs, for instance, in reference to some *Desmids*, says¹: "The *Staurocarpus cœruleus* is not uncommon near Penzance, is generally in large quantity where it occurs, and, from its peculiar colour, cannot escape detection; on these accounts I made it a principal subject of my observation. Although I have yearly gathered it in several pools, and the sporangia are always abundantly produced, I have particularly noticed, during five or six years' observation, that it has never in a single instance reappeared in the same pool. At Dolgelly, where also in some years it is common, I met with the same result, with a single exception, when I gathered it in one pool for two successive years. I have noticed the same fact with regard to *Zyngema curvatum*, and I believe it holds good in regard to most if not all the other *Conjugatæ*."

In the face, then, of this absence of continuity of habitat in the present, and of the frequent extreme variability of the lower forms of life—some indications of which have been given in the foregoing quotations from F. Cohn, Carpenter, and others, together with what has been said as to the variability of *Algæ* generally (pp. 131, 137)—how is it possible to accept the notion of direct lineal descent without change as the explanation of "persistence of types"?

As I long ago pointed out,² persistence of low types of life is much more explicable on "the assumption of successive evolutions of more or less similar forms from similar starting points under the influence of like conditions, than on the assumption that such changeable forms should have produced their like through

¹ *The British Desmidea*, 1848, p. 14.

² Yet we find Huxley, for instance, saying: "In view of the facts of geology, it follows that all living animals and plants are the lineal descendants of those which lived long before the Silurian epoch."

³ *The Beginnings of Life*, vol. ii., p. 616.

such vast and unrealisable epochs of time." Persistence of types among lower forms of life is, in fact, to be expected in accordance with my views, seeing that the living things that have been constantly arising by Archebiosis and Heterogenesis have been the immediate products of ever-acting material properties or natural laws—the same in all times, however much or little the environing conditions may have varied from age to age.

Thus the continued existence of low types throughout the geologic strata from the Silurian system upwards, and, among higher types, the constant admixture of previously known forms with others altogether new, will be found quite consistent with the notion of a continual surging up through all geologic time of freshly evolved lower forms of life—representatives of which, as they become more and more highly organised, mix, in successive epochs, with those of their predecessors which still remain. In this way there may have been produced in successive geological ages, in different parts of the earth's surface, multitudes of what have been called "trees of life," branching out into animal and vegetal forms of almost inconceivable variety. Many of these trees, including all or most of their branches, may have died out during the many vicissitudes of the earth's surface and the long lapse of ever-fruitful ages; though the descendants or ultimate ramifications of other of these trees—dating back to quite different and perhaps far-distant epochs—may still survive upon the earth's surface.¹

¹ Cope, in his work, *The Primary Factors of Organic Evolution* (1896), dwells upon the fact. "that the phylogenetic lines have not been continuous." He says: "The point of departure of the progressive lines of one period of time has not been from the terminal types of the lines of preceding ages, but from points further back in the series." And, again, he says: "Many lines of variation have been at one geologic period and another discontinued. It is also true that certain divergences from the main lines have appeared.....Such variations do not seem to have had any material effect on the general Course of Evolution" (pp. 172-222).

It is only natural to expect, in accordance with these views, that, while more or less similarity would be likely to exist between the lower forms of life which have appeared at different periods of the earth's history, more and more divergence might be encountered among such higher aquatic or ærial types whose ancestors may have lived through long geologic ages. Among such forms considerable diversity may be induced by the operation of the ordinary factors of evolution. So that, if the descendants of similar organisms (derived, perhaps, from totally independent stocks) have been exposed to notably different external conditions in different ages, or if in any of them modifications have otherwise arisen, the forms ultimately produced along such lines of development may be widely different from one another, although belonging to similar types.

And at different periods in the earth's history specialisations, now of one type and now of another, have been more and more manifest or dominant. In the Silurian epoch strange crustacean Trilobites abounded in all the seas. In the Devonian epoch fishes of a remarkable structure were most plentifully represented; while the same may be said of the cup-like Encrinites in the earlier Carboniferous period, though in the later portions of this epoch they were altogether thrown into the shade by that vast tropical vegetation from which we now derive our supplies of coal. In the Oolitic period, or so-called "age of reptiles," we have a most remarkable abundance of Saurian forms, and the Amphibian type reached its highest development. Huge Ichthyosauri and Plesiosauri swam in the lakes and rivers, while strange and gigantic winged Lizards mounted into the air. In the later Tertiary period we find the Mammalian type exhibiting the greatest divergence from previously-existing forms—first, by the appearance of innumerable huge Mastodons, Megatheriums, and other unwieldy denizens of the ancient forests and plains, and subsequently by the

gradual modification of one of the ramifications of the Quadrumanous order into those beings from whom primeval Man himself seems to have been evolved.

These several types of life, however, which have from time to time become more and more specialised, need not in any sense represent the members of one progressive series. They may be rather the products of different evolutionary processes—divergences taking place now in one direction and now in another, though reproducing fundamentally similar types of organisation. The lack of synchrony in the formation of similar fossils to which Huxley called attention, and their separation often by vast periods of time, might thus be in a measure explained; though, as Cope says, "such variations do not seem to have had any material effect on the general Course of Evolution."

The facts and views brought forward in the present work will be found to have a very important bearing upon another problem of great speculative interest—namely, the question of *the time needful for the evolution of all the forms of life that have ever appeared upon the earth*, and should not be without influence in bringing much more into harmony than is at present the case the views entertained by physicists, geologists, and biologists respectively as to the probable duration of life upon our globe.

The actual age of the globe, and also the time that must be supposed to have elapsed since the first appearance of living things upon its surface, has given rise to a considerable amount of discussion since Sir William Thomson (after Lord Kelvin), in 1862, first endeavoured to show in a paper¹ on "The Secular Cooling of the Earth" that great limitations had to be put upon the enormous demands for time made by Sir Charles Lyell and his immediate followers in accounting for all the series of changes on the surface of the earth that come within the ken of the geologist.

¹ *Trans. of Roy. Soc. Edin.*, vol. xxiii.

The very positive expression of Lord Kelvin's views in this paper for a time caused some consternation among both geologists and biologists. He returned to the subject in other communications in 1865, in 1868, and again in 1899, arguing from certain physical and astronomical data. His view as to the amount of time that could be conceded as possible to have elapsed since the consolidation of the earth has varied in these different communications, though his tendency has been to limit it more and more, till in his latest utterance on this subject¹ he put it as "more than twenty and less than forty millions of years, and probably much nearer twenty than forty."

His views and reasonings have been criticised by other physicists; and especially by Professors Perry and George Darwin, who have attempted to show the uncertain nature of the data and assumptions upon which Lord Kelvin's conclusions had been founded.

Moreover, the time limits assigned by him have been considered altogether inadequate by geologists and biologists alike. The most authoritative demurrer on the former side may be found in Sir Archibald Geikie's address² to the Geological Section of the British Association in 1899, in which the whole subject is discussed in a full and interesting manner. He considered that nothing short of 100 million years "would suffice for that portion of the history which is registered in the stratified rocks of the crust" (*loc. cit.*, p. 500)—and, of course, very much more for the total duration of life upon the planet. On the other hand, Professor Poulton, three years previously, in his address³ to the Zoological Section of the British Association, had dealt in an elaborate manner with the problem from the biological standpoint, and made far greater demands for the duration of life

upon the globe, extending to over 200 million years.

As I have said, the first announcement of Lord Kelvin's conclusions undoubtedly came as a shock to biologists and geologists alike. Thus Darwin, writing to Wallace on April 14, 1869, said: "Thomson's views of the recent age of the world have been for some time one of my sorest troubles." For even the widest limit that Lord Kelvin at that time was prepared to concede for the total age of the world was altogether at variance with what would be required for the mere duration of life upon its surface, in accordance with Darwin's views as to the means by which new species had been evolved, and his supposition of a single starting-point for living matter at some one particular time and in some one particular place on the surface of the earth.⁴

Another of the principal reasons that made it necessary for Darwin to make extremely large demands upon time is to be found in his view that low forms of life change or become modified less quickly than the higher forms.⁵ This same doctrine was also most strongly enforced by Poulton in the before-mentioned address. But such a conclusion is directly opposed to a large mass of other evidence, and is, I believe, absolutely the reverse of the truth.

A similar view has since been taken by some eminent biologists. Thus Adam Sedgwick, in his address to the Zoological Section of the British Association in 1899, said that the facts of palæontology, when closely scrutinised, lend support to the view that "variation was much greater near the dawn of life than it is now, and heredity a correspondingly less important phenomenon"; while Professor Hickson, on a similar occasion in 1903, gave expression to very similar views. He went even so far as to say "that in the earliest stages of evolution the condition of extreme

¹ *Phil. Mag.*, January, 1899, p. 75.

² *Nature*, September 21, 1899.

³ *Ibid.*, September 24, 1896.

⁴ *Origin of Species*, sixth ed., p. 429.

⁵ *Loc. cit.*, p. 346.

plasticity and ready response to changing external conditions were necessary for the survival of the species."

Thus, if instead of believing with Darwin that "all the living forms of life are the *lineal* descendants of those that lived long before the Cambrian epoch," and that "all the organic beings which have ever lived on this earth may be descended from some one primordial form," it should be admitted that life originally started from multitudes of centres (as the uniformity of natural phenomena would demand); that from the earliest stages of the earth's history up to the present time new starting-points of simplest forms have been ever taking place all over the surface of the earth, we may see, not only how many of the facts concerning "persistent types" may be explained, but also how the time needed for the whole evolution of life upon the globe may have been far less prolonged than biologists have hitherto supposed. The naturalist would be no longer bound to look upon all animals and plants as members of a single imperfect series. The routes that have given rise to all the known forms of life may have been many, and several of the evolutionary series may have been developing, not only contemporaneously, but

also, to some extent, similarly from new starting-points in successive geological epochs. May not the existence of *Amphioxus* at the present day in our seas be an evidence of this? Are we to suppose that this minute organism, which is regarded as "the ancestral form of all vertebrates," has come down to us unchanged from pre-Silurian epochs?

The foregoing brief survey should suffice to show the reader how much better multitudes of well-known facts can now be explained, and how absolutely irreconcilable many of them are with the old point of view.

This great accumulation of evidence in favour of the new views, in harmony as it is with the writer's actual observations and experiments, is still further strengthened by the fact that the new view does not (as the old view does) postulate any inexplicable departure from the uniformity of natural phenomena. It assumes that the forces of nature and material properties have ever remained the same throughout the universe, and that new births of living matter have ever been taking place, both here and elsewhere, since the time when such processes first became possible.

APPENDIX

THE SIMPLEST KIND OF PROTOPLASM:

A NOTE ON THE FREE GROWTH OF BACTERIA AND TORULÆ IN A SOLUTION OF NEUTRAL AMMONIUM TARTRATE IN DISTILLED WATER.

(From "Knowledge and Scientific News," August, 1905. With slight abbreviations.)

IN his work on *The Structure and Functions of Bacteria*, Professor A. Fischer places the nitrifying Bacteria that were discovered and isolated in 1888-91 by Winogradsky among his group of "Prototrophic Bacteria." He says their life-processes are "characterised by an extremely primitive metabolism—a physiological humility which shows them to occupy the very lowest rung of the ladder of life." While on another page¹ he says the materials from which they build up their cells are "inorganic compounds of the very simplest character—carbon dioxide and ammonia, or nitrous acid, with a few mineral salts. They are thus prototrophic in the strictest sense of the word, for a simpler synthesis of proteids than theirs is scarcely conceivable." He further says: "As might be expected in the case of organisms with oxidising functions, all the nitrifying Bacteria are ærobic. They require no light, and yet, in spite of this, are able to assimilate the CO₂ of the atmosphere."

His other two groups of Bacteria are supposed to be absolutely separated from this primitive group—the "Metatrophic Bacteria," under which are included most of the known forms, because they "cannot live unless they have organic substances at their disposal, both nitrogenous and carbonaceous"; and the "Paratrophic Bacteria," because they "can exist only within the living tissues of other organisms"—that is, as true parasites.

It is the object of this article, however, to show that a sharp distinction between these first two groups does not exist, seeing that common "Metatrophic Bacteria," as

well as some Torulæ, are capable of taking on life-processes even simpler than those shown by any of the hitherto described forms of the "Prototrophic Bacteria".....

The verification of these statements can be easily made. It will only be necessary to prepare solutions of neutral ammonium tartrate in distilled water, using about 0.65 of a gramme of the salt to 30 cubic centimetres of the water (that is, 10 grains to the ounce); and, after the crystals have been dissolved, to add to one of the solutions a single drop of a recently-prepared turbid hay infusion, and to another a single drop of a recently-prepared turbid infusion made from beef or mutton. The two solutions thus inoculated with common active Bacteria may then be placed in the dark within an incubator maintained at a temperature of 30°-32° C. (86°-89° F.). In about thirty-six hours both fluids will be found to have become slightly opalescent, owing to the growth, as the microscope will show, of myriads of minute Bacteria, and occasionally of a number of very minute Torulæ.

Though these common Bacteria and Torulæ are thus capable of growing freely in the saline solution without the aid of light, I have found that light distinctly favours the process, since solutions similarly inoculated and left exposed to ordinary daylight have become turbid rather more quickly, even though the temperature to which the solutions has been exposed has been about 11° C. (20° F.) lower than that of the incubator.

In order to get rid of the complication caused by the presence even of a single drop of an organic infusion, such as was present at first, other solutions may be

¹ *Loc. cit.*, *trans.*, 1900, pp. 48 and 106.

inoculated with Bacteria taken from one of the original solutions after five or six days, when their turbidity has become more marked. As the Bacteria in these solutions are probably less numerous and less vigorous than those in the organic infusions, three drops (rather than one) are now introduced into each of two other freshly-prepared ammonium tartrate solutions, one of which may be placed in the incubator as before, and the other left in a corked flask exposed to daylight, and at the lower temperature. The growth of these less vigorous Bacteria is now decidedly less rapid, and seems only capable of occurring at all freely when aided by daylight. In the flask on the table the fluid will become slightly opalescent in four or five days; and this opalescence increases for a few days, when a sediment begins to form. But the fluid in the incubator may show no distinct opalescence, even for a couple of weeks or more, though a very minute amount of sediment will accumulate.

Examination of the sediment taken from one of these second inoculation flasks which have been exposed to daylight will show masses of Bacteria, mixed with *Torulæ* or other Fungus-germs, together with a delicate, much-twisted mycelium.[†].....

My claim that the organisms growing in this solution of ammonium tartrate in distilled water are building up protoplasm in the simplest known manner may be objected to on the ground of the ultimate organic origin of the tartaric acid, but I am told by Sir William Ramsay that "ammonium tartrate can be synthesised from inorganic material, and this substance is, so far as we know, absolutely identical with ammonium tartrate derived from tartaric acid extracted from wine-lees."

Seeing that the formula of neutral ammonium tartrate is $(\text{NH}_4)_2 \text{C}_4 \text{H}_4 \text{O}_4$, if there were no impurity in the solution, the micro-organisms would have to build up their protoplasm in some way with the aid only of C, H, O, and N—which seems almost incredible. I may say that the ammonium tartrate used was specially prepared for me, some years since, by Messrs. Hopkin and Williams, and that the solutions were made in small flasks of hard Bohemian glass. Such solutions

were formerly twice analysed for me by a skilled chemist, who reported that not the least trace of either sulphur or phosphorus could be detected. Sir William Ramsay has, however, been kind enough to analyse another specimen of the solution for me after it had been in the flask for five days,[‡] and his report is that the "liquid contained an excessively minute trace of sulphur, probably as sulphate; but no phosphoric acid could be detected by the molybdate of ammonium test."

Looking, therefore, to the fact that the nitrifying Bacteria would have at their disposal the "few mineral salts" referred to by Fischer, we may safely assume that the micro-organisms growing in this solution of ammonium tartrate, contaminated only by an "excessively minute trace of sulphur," have, in reality, been building up the simplest known variety of protoplasm. But how much the process would be aided by a little phosphorus may easily be shown by the addition of three grains of sodium phosphate to the solution. An inoculated ammonium tartrate solution with this addition will become turbid more quickly, and will soon yield a far larger amount of micro-organisms.

Looking to the nature of the primary inoculating material, it was only to be expected that several different kinds of common Bacteria would be found growing in the solutions; and this has proved to be the case. Dr. Gordon Holmes, the Director of the "Research Fund" at the National Hospital, kindly made a gelatine plate-culture from a second fluid, the first having been inoculated with a drop of a turbid hay infusion, and he reports that there were at least seven different kinds of Bacteria found—Cocci, Diplococci, Bacilli, and a kind of Streptothrix.

It certainly is very remarkable that these common micro-organisms, previously carrying on their life-processes in organic infusions, should be able so rapidly to adapt themselves to an entirely different metabolism. It is much to be desired that some skilled chemists would take the matter up and endeavour to throw some light upon the steps by which this marvellously simple synthesis of living matter is brought about.

[‡] The solution was one which had been inoculated with three drops from a first solution, and, having been in the dark incubator, it showed no trace of opalescence.

[†] A photomicrograph of these organisms was reproduced in the original paper.