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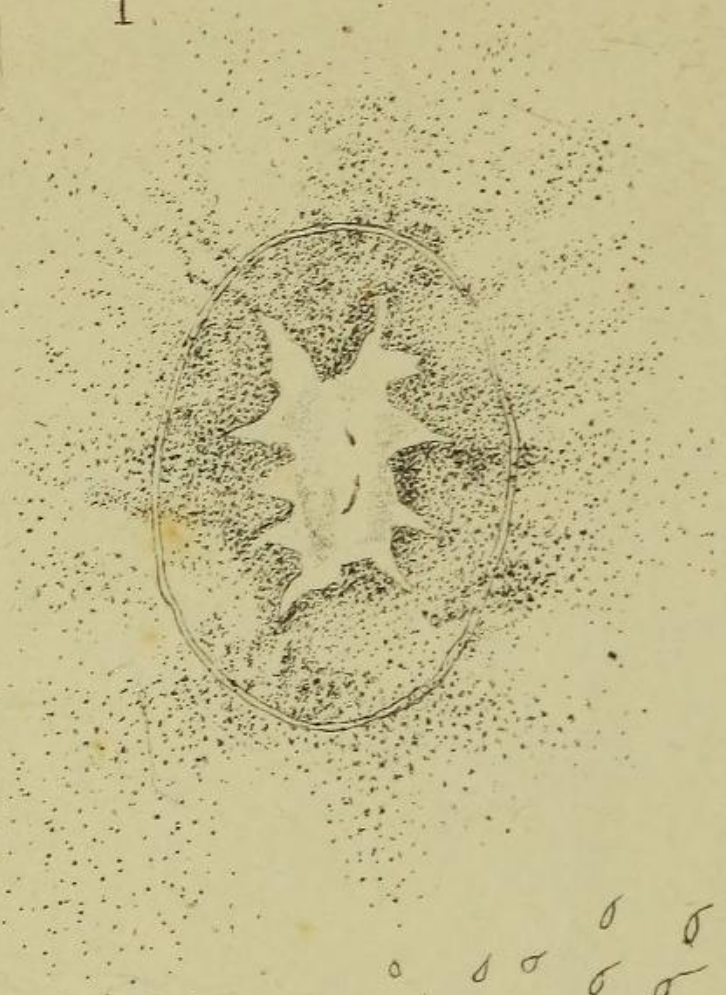

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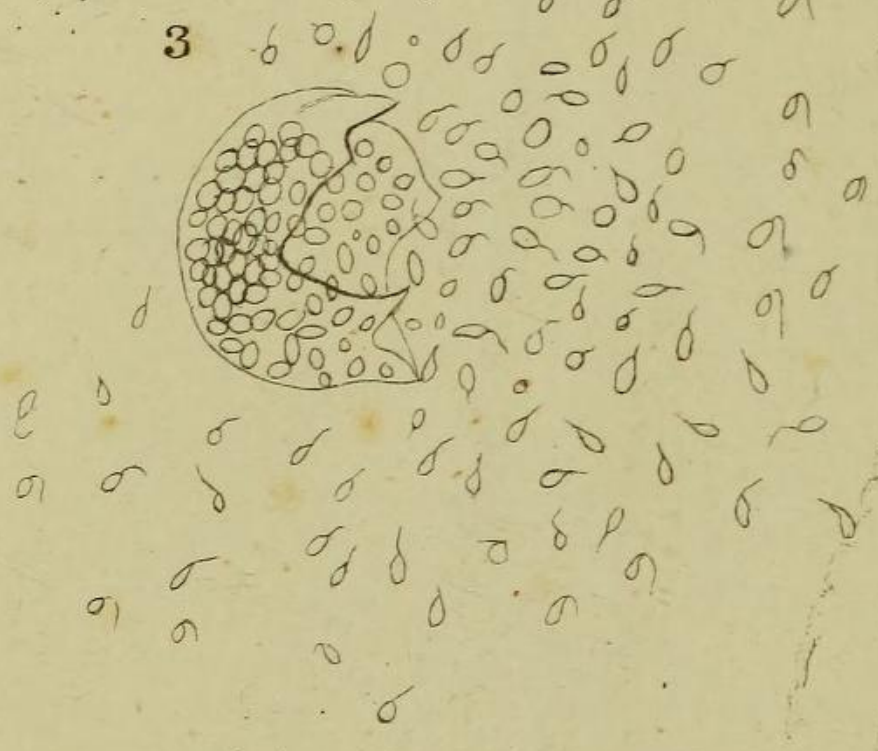

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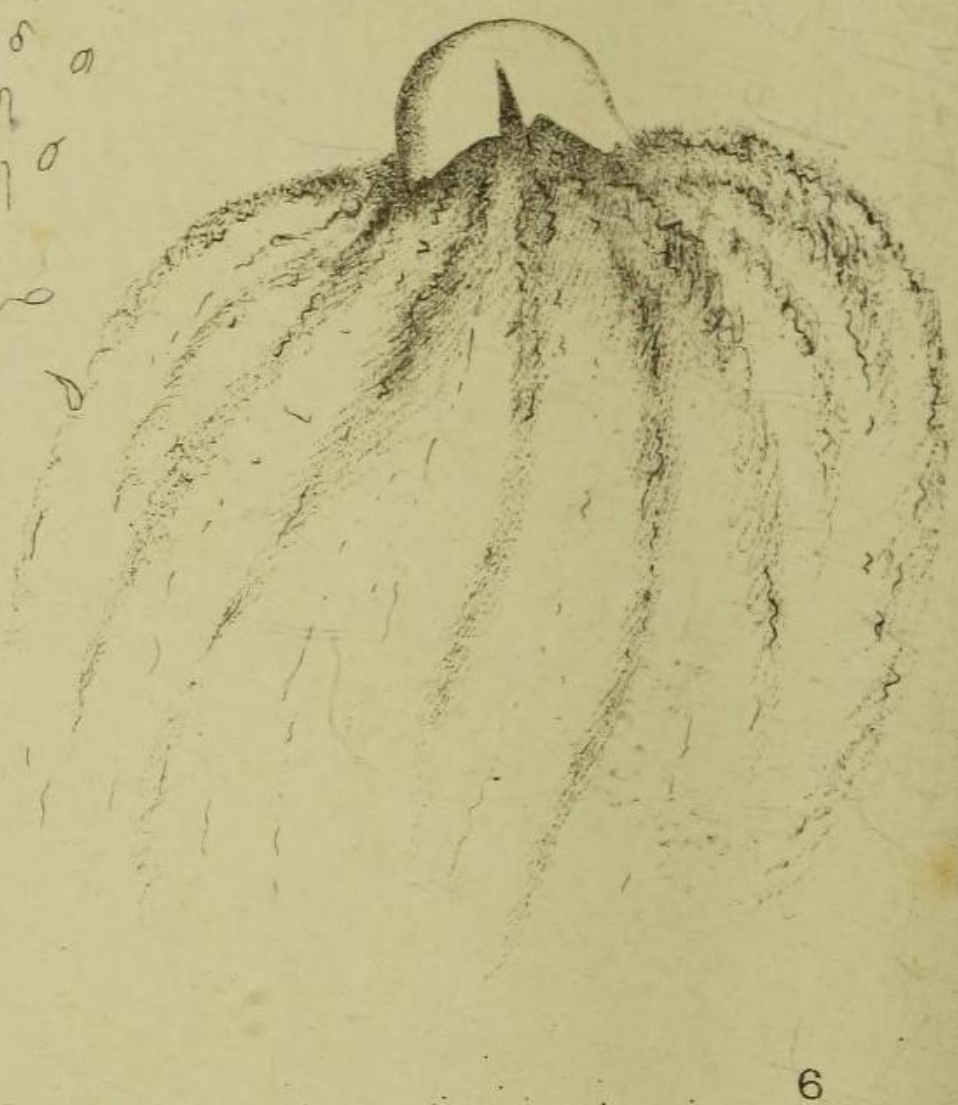

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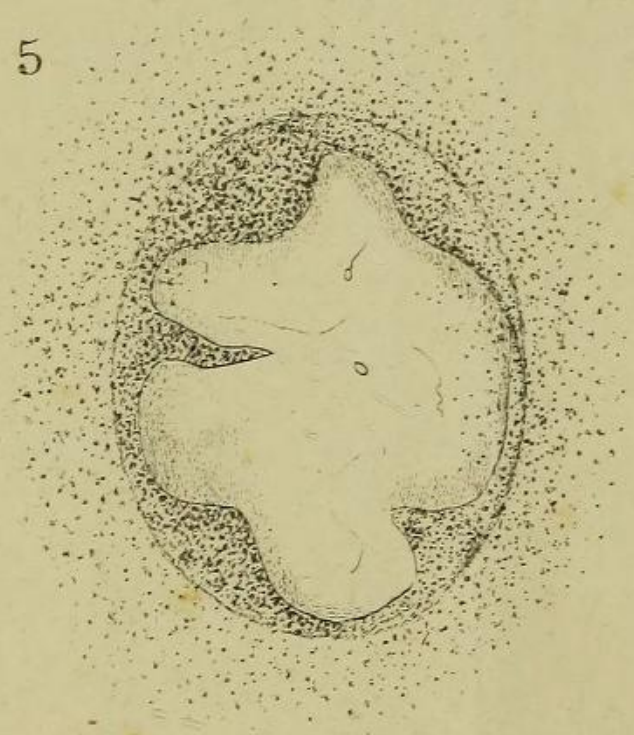

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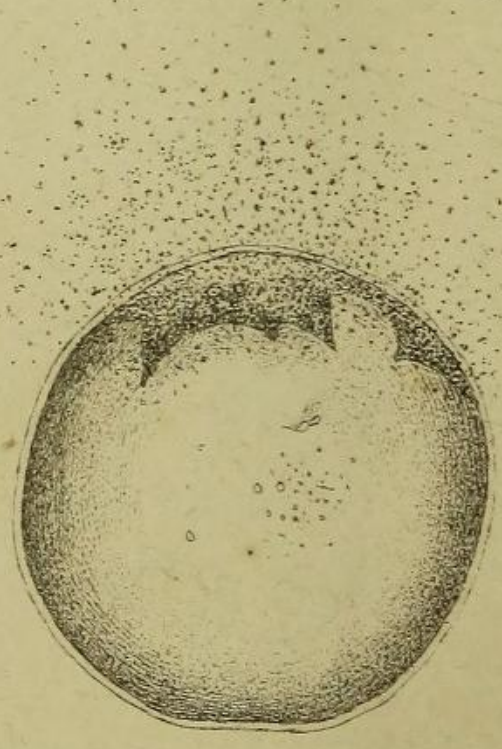

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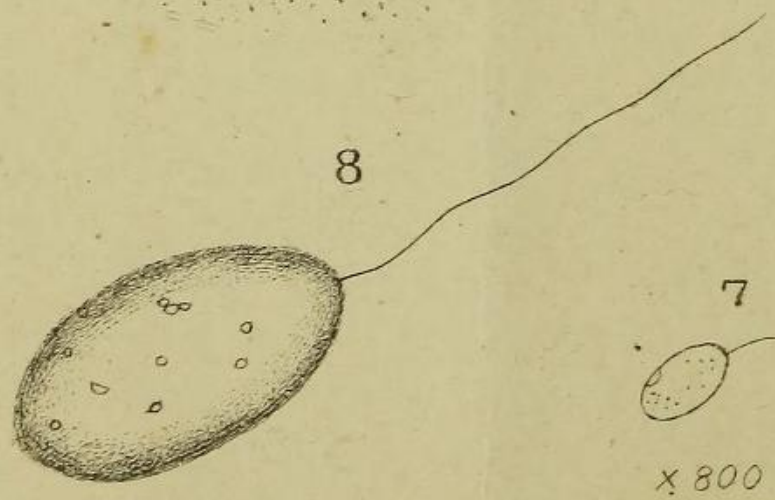

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Is there spontaneous generation?

PROFESSOR TYNDALL'S EXPERIMENTS ON SPONTANEOUS GENERATION, AND DR. BASTIAN'S POSITION.

By the Rev. W. H. DALLINGER, V.P.R.M.S.

[PLATE CXXXIII.]

IN the present position of Biological Science in relation to this important and interesting question, any positive results which have a definite bearing on the difficulties of the subject, and point hopefully to new methods of research, must be warmly welcomed. Professor Tyndall's beautiful series of experiments "On the Optical Deportment of the Atmosphere in reference to the Phenomena of Putrefaction and Infection" are precisely of this class, and will give new impulse and direction to all unbiassed labour. It is to be regretted when, in a matter so purely one of rigid science as this is, impassioned controversy is suffered to have any place. It fails utterly of its intended purpose, and simply hinders and delays the final issue. There are few but will have admired the animation, courage, and resolution manifested by Dr. Bastian in the discussion of this question during the last five years; but those who have been most capable of understanding the method, nature, and object of his experiments, and the general drift of his reasoning, are those who most earnestly disavow the perhaps unconscious, but nevertheless too palpable, advocacy of a *thesis* which his writings so freely display.

Dr. Bastian's position in relation to the origin of minute organic forms has, at the outset, the immense disadvantage of being adverse to the whole analogical teaching of nature, down to the uttermost depths of minuteness, *where our knowledge is accurate and sound.* Wherever science has put down the landmarks of possession, and is not dealing with the disputable territory of hypothesis, it is absolutely known that at some period in the cycle of development the lowliest organisms are

dependent for their propagation upon what we can only look upon as genetic products.

Manifestly, then, it must be weighty—nay, unequivocal and even irresistible—evidence that will induce the philosophical Biologist to conclude that nature's otherwise universal method is changed, in the outmost fringe of organised being. Mere reasoning could never accomplish this. It must be hard, defiant fact, which none can gainsay. But verily no such facts—nor even their most distant forecasts—are before us. The profound difficulties which bristle round the enquiry on every hand are prominent signals for caution; while the uncertainty and incompetency of the methods hitherto employed, and their conflict of results, is alive with meaning. Indeed, we are dealing with organisms so minute as to elude all but our best optical appliances; and the accurate and correct interpretation of the details they enable us to discover requires the practice and experience of years. Of the developmental history of these organisms themselves, we know from actual observation almost nothing with certainty; and the little we do know from such careful and patient observers as Cohn, Billroth, Ray, Lankester and others, is so complex and conflicting as to demonstrate the necessity of years of patient experiment and skilled research; and to plainly tell us of our ignorance of this minute and wonderful group of organic forms. And yet, forsooth, we are asked, upon the conflicting testimony of a multiplicity of boiled infusions, yielding often even in the same hands uncertain results, and in different hands conflicting ones, to believe that organic nature—whose method of reproduction is the same to the very limits of *certain* knowledge—changes its method in this uncertain and cloudy region.

Of course to "spontaneous generation" as a mode of vital reproduction there can be no *à priori* objection. Let us have it by all means, if it be a fact in nature; but not on any other terms. Is it reasonable to suppose that such men as Darwin, and Huxley, and Tyndall, and Burdon Sanderson, and Cohn, and Billroth, and Lankester, would shrink from "spontaneous generation" because of the "consequences" to which, strangely enough, it is by some supposed to lead? The very thought admits of nothing but ridicule. And yet Dr. Bastian is displeased with Darwin * because he has not definitely determined whether all living things originated in one primordial germ, or originated spontaneously in multitudinous centres scattered over the earth's surface. Both Huxley and Tyndall are in effect charged with grave inconsistency,† because, while they admit the origin of all vital forms by evolution, they

* "Evolution and the Origin of Life," pp. 13-17. † Ibid. pp. 15-16.

yet declare that they have never *seen* an instance of "spontaneous generation" of organised forms. It is asked "Why should men of such acknowledged eminence in matters of philosophy and science as Mr. Herbert Spencer and Professor Huxley promulgate a notion which seems to involve an arbitrary infringement of the uniformity of nature?" I dare not answer for them; but for myself I answer, Because the facts as presented to them on the subject—as well known to them as to Dr. Bastian, and we may venture to say as well considered—do not appear to involve the "arbitrary infringement" of nature's uniformity of which Dr. Bastian speaks. If these admittedly competent and proverbially fearless men could be led by facts to see that their teaching promulgated an "arbitrary infringement" of nature's method, is it rational to suppose that they would persist in it another hour? The very position, therefore, of the leading biologists of the day in relation to the hypothesis of "spontaneous generation" is an authoritative declaration of the invalidity of the data on which it rests.

To Dr. Bastian, nevertheless, the "facts," such as they are, have carried a different conviction. But on analysis, that conviction is evidently not wholly formed upon the bare "facts." It is influenced and stimulated by a "philosophy" which, in short, is this:—Continuity in nature is the grand outcome of all modern research; but if you are to have this in a sense wide enough to include the organic world, you must have "spontaneous generation." Give up this, and continuous evolution is impossible; *therefore* abiogenesis *must be* a great truth.

Of course continuity in nature is a profound truth. Every careful and comprehensive student of modern biology will admit that. By Dr. Bastian's own showing, Huxley, Darwin, and Spencer are its most competent expositors. But they prefer not to be hasty. They decline to determine the exact manner or line of that continuity *until they have facts of a competent kind to guide them.* There may be lines of continuity infinitely more subtle than any the subtlest minds have even conceived. At least they decline to accept one, laid down, as it appears to them, not by nature, but by Dr. Bastian; and no believer in the evolution of living things, surely, is recreant of his creed who declines a similar surrender.

The largest difficulty surrounding the question of the mode of origin of septic organisms is that of discovering their life-cycle. By dealing with them in aggregations we run told and untold risks. The conflict of results by this means, in the most accomplished hands, employing the most refined methods during the past eighteen years, is a sufficient witness. Repetitions of experiments, and conflicting results, and explanations of the reason why; and so the cycle rolls. Of course important lessons

in biology are learned, but not *the* lesson. And yet by the teachings of this complex and doubtful method *alone* Dr. Bastian is content to accept "abiogenesis" as a great fact in nature.

To those who are best acquainted with the experimental history of the subject for the last twenty—but certainly for the last six—years this is the more remarkable. For the weight of evidence is certainly not only *not* in favour of "abiogenesis," but is in the strongest sense adverse to it. The most refined, delicate, and continuous researches all point to the existence of what are at present ultra-microscopic germs. This, indeed, is directly affirmed by the authors. A single and recent instance will suffice. After a remarkable series of experiments detailed before the Royal Society Dr. W. Roberts says: "The issue of the foregoing inquiry has been to confirm in the fullest manner the main propositions of the panspermic theory, and to establish the conclusion that *bacteria* and *torulæ*, when they do not proceed from visible parents like themselves, originate from invisible germs floating in the surrounding aërial and aqueous media."*

But further, this has been remarkably sustained by analogical evidence. There are putrefactive organisms that closely approximate to the bacteria in form, structure, and size. These are the "*monads*," or, as Professor Huxley doubtless more fitly names them, the *heteromita*.† They live side by side with the bacteria in the same putrescent mass, and certainly in the later stages of the disintegration of dead organic matter are the most active and powerful agents. From their greater size they present a more promising field for microscopical research than the bacteria themselves; and the life-history of some of these could be fully mastered. I long since felt that valuable aid might thus be rendered to the discovery of the nature of the bacteria. Armed with the best and most powerful appliances which the modern optician could supply, Dr. J. Drysdale and myself ventured on the work. The results are fully detailed elsewhere.‡ It need only be remarked here that the only hope of success was in *continuous* observation of the same form, in the same drop of fluid, under the highest powers. The secret, therefore, was to find a means of keeping the same drop under examination without evaporation. This we did. § The result was that patient work enabled us to completely unravel the life-history of six of these organisms. These life-cycles cannot be here recounted. Suffice it now to say that each of them multiplied enormously

* "Phil. Trans." 1874, p. 475.

† "Macmillan's Magazine," Feb. 1876, p. 379.

‡ "Monthly Micros. Journ." vols. x. xi. xii. and xiii.

§ Ibid, vol. xi. pp. 67-69.

by self-division (fission), but that the life-cycle in *each case* began and ended in *a distinct genetic product*—call them what we choose, spores, germs, or ova.

In Pl. CXXXIII. I have drawn from nature, in the six respective cases, the condition presented by each organism at the time of emitting its spore. Fig. 1 is the genetic product of an oval monad, with a pair of flagella; it rapidly increased by fission; then in a remarkable manner a pair blended, became *one* in the form of a sac, the sac burst and poured out, as the drawing portrays, innumerable spore, which were watched continuously until they were seen to develope into the parent condition. Fig. 2 gives a similar product of another form, different anatomically and in all the details of metamorphosis, but yet passing through the states of fission, blending into a sac, and (as seen) the emission of spore; which were again watched into the parent condition. Fig. 3 shows the direct genetic product of a third, but this sac did not contain spore, but *living young*, which swam forth at once upon the bursting of the sac; and by taking in pabulum at all points of the sarcode, rapidly grew to the parent size. In fig. 4 we have new features. The organism is oval, with one flagellum. It multiplies with enormous rapidity by *multiple* fission,* and then by distinct genetic union a sac is formed and spore emitted; but they are packed in a glairy fluid, and were so minute that at first our best powers failed to reveal them. But they were afterwards seen, and their full development traced. In figs. 5 and 6 we have the same products of the two last monads. In morphological detail they greatly differed from all the preceding ones, and from each other. But the spore-sacs were produced by the same means, and the exquisitely minute spore poured forth were traced through all their stages to the adult condition.

We have here, then, important indications of fact concerning the nearest allies of the bacteria: they develope from germs.

We have besides, the weight of the best experimental evidence pointing clearly to the existence of germs in the bacteria themselves. But the microscope has failed to *demonstrate* the latter. Its finest powers and finest methods failed to reach them.

Happily at this juncture Professor Tyndall has stepped in, and, with his accustomed brilliance and precision, has opened up the path we need. *He has presented us with a physical demonstration of the existence of immeasurably minute molecules of matter—utterly beyond the reach of the most powerful combination of lenses yet constructed—which are the indispensable precursors of bacteria in sterilised in-*

* "Monthly Micros. Journ." vol. xi. pp. 69-70.

*fusions.** In short, he has opened up a new and exact method, which must lead to a scientific determination of the existence and nature of the bacteria-germs. His beautiful experiments on the decomposition of vapours, and the formation of actinic clouds by light, led him to experiment on the floating matter of the air, and with what results is widely known. Confined and undisturbed air, however heavily charged with motes, becomes at length, by their deposition, absolutely clear, so that the path of the electric beam is invisible across it. From this, and associated indications, he acutely inferred "that the power of developing life by the air, and its power of scattering light, would be found to go hand in hand;" so that a beam of light sent across the air into which infusions might be placed and examined by the eye, rendered sensitive by darkness, might be utilised with the best results in determining the existence of bacteria-germs. To bring the idea to a practical result a number of chambers were constructed with glass fronts. At two opposite sides facing each other a couple of panes of glass were placed to serve as windows, through which the electric beam might pass. A small door was placed behind, and an ingenious device was arranged to enable a germ-tight pipette to have free lateral, as well as vertical, motion. Connection with the outer air was preserved by means of two narrow tubes inserted air-tight into the top of the chamber. The tubes were bent several times up and down, "so as to intercept and retain the particles carried by such feeble currents as changes of temperature might cause to set in between the outer and the inner air."

Into the bottom of the boxes were fitted air-tight large test-tubes, intended to contain the liquid to be exposed to the action of the moteless air.

"On September 10 the first case of this kind was closed. The passage of a concentrated beam across it showed the air within it to be laden with floating matter. On the 13th it was again examined. Before the beam entered, and after it quitted the case, its track was vivid in the air, but within the case it vanished. Three days quite sufficed to cause all the floating matter to be deposited on the sides and bottom, where it was retained by a coating of glycerine, with which the interior surface of the case had been purposely varnished. The test-tubes were then filled through the pipette, boiled for five minutes in a bath of brine or oil, and abandoned to the action of the moteless air."

In this way the air in its normal condition was freely supplied to the infusions, but of mechanically suspended matter it could

* "Nature," Jan. 27, 1876, p. 252; and Feb. 3, p. 268.

be demonstrated that there was none. And it was proved, with a clearness that admits of no quibble, that infusions of every kind, animal or vegetable, were absolutely free from putrefactive organisms. "In no single instance . . . did the air which had been proved moteless by the searching beam show itself to possess the least power of producing bacterial life or the associated phenomena of putrefaction." But portions of the same infusions exposed to the common air of the Royal Institution Laboratory at a continuous temperature of from 60° to 70° Fahr. fell invariably into putrefaction; and when the tubes containing them amounted to 600 in number not one of them escaped infection—they were all "infallibly smitten." Here is irresistible evidence that there is a direct relation between a mote-laden atmosphere and bacterial development. The whole series of Dr. Tyndall's exquisite experiments is simply an irrefragable affirmation of this truth. The presence of the physically demonstrated motes is as essential to the production, in a sterilised infusion, of septic organisms, as light is to actinic action. They cannot be made to appear without the precursive motes; they cannot be prevented from appearing if the motes be there. That these are the germs of bacteria by themselves, or associated with minute specks of matter, approximates to certainty in the proportion of hundreds of millions to one.

A beautiful illustration of the minuteness and multitude of the particles is given. Let clean gum mastic be dissolved in alcohol, and drop it into water; the mastic is precipitated and milkiness is produced. Gradually dilute the alcoholic solution, and a point is reached where the milkiness disappears, and by reflected light the liquid is of a bright cerulean hue. "It is in point of fact the colour of the sky, and is due to a similar cause—namely, the scattering of light by particles small in comparison to the size of the waves of light."

Examine this liquid with the highest microscopical power, and it appears as optically clear as distilled water. The mastic particles are almost infinite in number, and must crowd the entire field of the microscope; but they are as absolutely ultra-microscopic as though they had no existence. I have tested this with an exquisite $\frac{1}{50}$ of Powell and Lealand's, employed with a new and delicate mode of illumination for high powers,* and worked up to 15,000 diameters; but not the ghostliest semblance of such particles was seen. But at right angles to a luminous beam passing among these particles in the fluid "they discharge perfectly polarised light." "The optical deportment of the floating matter of the air proves it to be

* *Vide* "Monthly Micros. Journ." April 1876.

composed, in part, of particles of this excessively minute character," and it is among the finest of these ultra-microscopical particles that Professor Tyndall finds the sources of bacterial life. It is almost impossible to conceive a nearer approach to certainty concerning the nature of these minute particles than this. Their minuteness, their capability of being physically demonstrated, the absolute necessity of their presence to the origination of bacteria in sterilised infusions of any and every kind, taken in connection with what we *know* concerning the germs of the *heteromita* whose life-histories have been studied, render it simply inevitable that we have at length reached, what we are justified in believing to be, a genetic product of the bacteria through which their continuation as organisms is preserved. When first I saw the simplicity and beauty of this method, it struck me that its applicability as a test in reference to germs—*known to be such*—would have considerable collateral weight; and a method of employing it was suggested by a fact in past experience.* I had in my possession a maceration of cod's head, which I had kept in use for eleven months. It had become a pulpy mass, and in the middle of January last it was comparatively free from bacteria, but swarmed with two monads—the fourth and sixth of the series described by my colleague and myself. To ascertain their exact condition, I watched them on the "continuous stage" for three consecutive days, and found that both forms were to be seen plentifully emitting spore. The maceration had become very short of moisture, which served my purpose. I subjected it to a dryer air with a higher temperature, and it was not very long in becoming a moist pulpy mass, with sufficient cohesiveness to be removed from the vessel; and in this condition it was placed in a heating chamber, which was slowly raised to a temperature of 150° Fahr., and kept at this for an hour. This was 10° Fahr. higher than Dr. Drysdale and myself had proved necessary to destroy absolutely every adult form. The baked mass now appeared cracked, porous, and flaky. In parts it was extremely friable, and with little pressure crumbled into almost impalpable powder; while by friction a very large proportion was reduced to the finest dust. To avoid all possibility of error this powder was again exposed in the heating chamber, spread over a plate of glass, to a temperature of 140° Fahr. for ten minutes—thus rendering the plea of mere desiccation impossible.

A chamber or box was now prepared precisely like Professor Tyndall's, except that there were no tubes to communicate with the outer air.

* *Vide* "Monthly Micros. Journ." vol. xii. pp. 262-3.

In the "Researches" on the life-history of monads we had proved that they could live, thrive, and multiply almost as well in Cohn's "nutritive fluid" as in the normal animal infusion. This fluid is composed of phosphate of potash, sulphate of magnesia, triple basic phosphate of lime, tartarate of ammonia, and distilled water. If these ingredients are all mingled the fluid becomes speedily charged with bacteria, unless hermetically sealed, and sometimes even then. We therefore keep the ammonia in a separate solution, mixing them when required.

A portion of the fine dust of the maceration was now taken and thoroughly scattered through the air of the prepared chamber. The condensed beam from an oxyhydrogen lime-light* was then sent through it. Its line of passage was far more brilliantly marked inside the chamber than in the outer air. It was deemed inexpedient to insert the fluids while such brilliant points were visible in the air, and four hours were suffered to elapse. The lime-light beam was still visible with perfect distinctness, but its path within the chamber was much less brilliant and more homogeneous than it was without. The fluids were then carefully mixed, and five small glass basins of the mixture were inserted. The whole was undisturbed for five days. At the expiration of that time the beam of the lime-light sent through the chamber was absolutely invisible, although perfectly clear in the open air on both sides of it.

The fluids were now withdrawn. Ten "dips" were taken out of each basin for microscopical examination. *In every "dip"* —that is, fifty in all—*one or other of the monads appeared, and were in a state of active fission*; and in twenty-seven of the "dips" both monads were found. Bacteria swarmed the field, which of course I fully expected.

I now took five *other* glass vessels, and inserted them with great care into the now moteless air of the chamber, and poured in, as before, fresh Cohn's fluid. They were exposed for another five days. On careful microscopical examination of seventy-five "dips" *not a single monad of either form appeared*; bacteria were feebly present, but of course no steps were taken to guard against these, and, as before, they were anticipated.

The air of the chamber was again impregnated with dust, as before suffered for a time to settle, and *these same vessels of fluid*, which had yielded negative results, were again placed in the chamber. At the expiration of five days they were again examined, *and one or other of the monads was found in every successive "dip."*

Now let it be observed that there can be no possible error as

* This was of course very much less capable of "searching" than the electric beam; but it served for the rougher end I had in view.

to the forms. They were the identical species of the maceration, with which I am as familiar as with a barn-door fowl. What, then, is the logic of these facts? Dr. Tyndall proves that bacteria only develope in sterilized infusions when the air around them is laden with motes of incalculable multitude and exquisite minuteness. Given the presence of these, and the development of bacteria is inevitable. The inference is that the motes are *germs*. The above experiments show, that in closely allied septic organisms, the germs of which have been demonstrated and their developments watched, if the dry *débris* of a maceration in which these forms are found be scattered in the air around a prepared fluid, and demonstrated by similar optical means, that the said organisms develope; but if the minute dust from the *débris* be optically proved to be *absent*, none of the monad forms appear. Here we do not hypothecate a germ, but we *know* that it exists; and its deportment in similar conditions is identical with that of the assumed bacterial germ. Do we need more irresistible evidence that the bacteria develope, not *de novo*, but from genetic products?

Evidently Dr. Bastian thinks we do. He tells us in effect that if Dr. Tyndall has not succeeded, others have, in seeing bacteria reappear in infusions that have been exposed to a boiling heat for five minutes. This is true; but not to the extent nor with the meaning Dr. Bastian claims. He furnishes a list in "Nature"* for example, of those who are supposed to have secured the results he insists on. But this list is, perhaps hastily, but in effect, most unjustly framed. It is not surprising to see strong protests from the investigators concerned.† The citing of Roberts, for example, or Lankester and Pode, or Pasteur or Schwann, is simply a meaningless exercitation to all but the ignorant. Stripped of all disguise, the number of cases of the appearance of bacteria in sealed infusion after five or ten minutes boiling is few and doubtful indeed. But still there *are* cases, and in one instance at least admirably attested; but they are confessedly exceptional in a high degree. Dr. Bastian, however, prefers to interpret nature from the exceptional flasks, and infer "spontaneous generation" rather than be guided by the cumulative and overwhelming evidence of the existence of bacterial germs, as the medium of their normal reproduction. This must mean either that he believes that these organisms originate *de novo as well as* by germs, which is a direct *petitio principii*; or else that he is incapable of seeing the force of the facts which render the existence of germs inevitable. From the conflicting evidence of his own writing it would almost appear that he endeavoured to maintain

* Feb. 10, 1876. † E. G., "Nature," Feb. 24, 1876, p. 324.

both these views. He has recently said, "Professor Tyndall's results, admirable as they may be in themselves, *are altogether collateral, and do not bear upon the main point at issue.*" * Surely the "main point at issue" is the mode of origin of bacteria, and we cannot get much nearer the origin of an organic form than by tracing it to a genetic product—a spore! This *was* originally Dr. Bastian's question—did bacteria originate *de novo*, or from parents? It is not so now. He says, "*The* question is, not what air does or does not contain, since I have long ago shown .... that boiled fluids can be made to putrefy and swarm with bacteria in closed flasks, from which air and whatever it may contain has been expelled." † The same reasoning also obtains in his communication to the "Lancet" ‡ and to "Nature." § The result is clear. The doctrine of "spontaneous generation" rests upon *exceptions* for its truth. In rare instances, and in special infusions, bacteria have appeared after prolonged boiling. After a careful sifting of the evidence, the meagreness of the testimony is striking. All that can be fairly taken at all, when justly weighed, if taken altogether, is not equal to the evidence given by Dr. Burdon Sanderson. ‖ But it is well known that, while admitting and publishing the facts, he ignores absolutely Dr. Bastian's inference. And surely this is the truer philosophy. Let it be granted that by means not now explicable, the germs of bacteria, destructible in filtered infusions at a boiling temperature, are feebly, and at times, able to survive a slight continuation of the boiling point in infusions containing solid particles without apparent injury, is not that a ground for enquiring the reason why, rather than for inferring "spontaneous generation?" If we can prove that in 99 cases out of 100 *actual germs* are destroyed at 212° F., but that, in exceptional circumstances, the remaining one case yields bacteria *after* exposure to 212° F. for some minutes, is not that a reason for inferring, and looking for, some protective influence upon the germ, rather than launching into an hypothesis of an new mode of origin?

That the medium in which minute organic forms are subjected to heat exerts an influence on their subsequent deportment I can abundantly prove. I am equally convinced that the death-point of bacteria germs hovers very near the boiling point of water—a conviction amply sustained by fact. This being so, the survival, as germs, of some few, amidst incalculable myriads, by some accidental protection, is surely possible. So that, indeed, all true work now should be a study of the *germ* and its

* "Times," Jan. 29, 1876. † Ibid.
‡ Feb. 5, 1876. § Feb. 10, 1876.
‖ "Nature," Jan. 9, 1873, vol. vii. and vol. viii.

properties, and a discovery by patient research of the life-history of the organism.

The valueless nature of mere temperature experiments on such organisms, as tests of their ability to survive, without a knowledge of their life-history, Dr. Bastian, *without knowing it*, has made sufficiently plain. He gives a brilliant illustration —styled by himself "typical"—of the futility of his own method. Consider the facts.

In our "Researches" on the monads, my colleague and myself made it a special point to institute a series of investigations on the points of temperature which the adults, and the spore, of each form studied could resist. The results were as unexpected as they were remarkable. Only the results can here be stated. Taking the spore-sacs of the several forms in the order in which our illustration gives them, the data are as follow—viz. fig. 1 survived after exposure to 250° F.; figs. 2 and 4, 300° F.: fig. 3 (which produced living young), 180° F.; figs. 5 and 6, 250° F. That is to say, the spore, *after the heating to the above-named temperatures*, were followed step by step until they reached the parent condition. The adults of each form were absolutely destroyed at from 130° to 140° F. Thus, if all the examples be taken together, it will be seen that on the average the spore have a capacity to resist heat better than the adult in the proportion of 11 to 6. This is surely important.

Now, until Dr. Bastian's promised "new results"* have appeared, I believe I am justified in affirming that the strongest cases on which even he relies for "spontaneous generation" are recorded on pp. 175–180 of his "Evolution and the Origin of Life." They are thus introduced:—"After this I may, perhaps, be deemed fully justified in quoting two *very typical* experiments for the further consideration of those who stave off the belief in spontaneous generation—either by relying on insufficient reasons for doubting the influence of boiling water, or because of their following Pasteur, Cohn, and others in supposing that certain peculiar bacteria germs are not killed except by a brief exposure to a heat of 227° or 230° F. For even if we could grant them these limits, of what avail would the concession be . . . . in the face of the following experiments?" The details of the experiments follow. They are alike in method, and we will concern ourselves only with the second. A strong infusion of common cress, with a few of the leaves and stalks of the plants, were inclosed in a flask, which was hermetically sealed while the fluid within was boiling. It was then introduced into a digester and gradually heated, and afterwards kept at a temperature of 270–275° F. for twenty minutes, and was

* *Vide* "Times," Jan. 29, 1876.

retained at a temperature, if the time of heating and cooling be considered, over 230° F. for one hour. This flask was opened after nine weeks. The reaction was acid; the odour was not striking. On microscopical examination with a $\frac{1}{12}$ inch objective "*there appeared more than a dozen very active monads.*"

Now, fortunately, Dr. Bastian has not only carefully measured and described these organisms, but he has drawn them, and they are reproduced on the frontispiece of the book. He describes them as the 1-4000th of an inch in diameter; they were provided with a long, rapidly moving lash (flagellum), by which granules were freely moved about. But, besides this, "*there were many smaller, motionless, tailless spherules, of different sizes, whose body-substances presented a similar appearance to that of the monads*—and of which they were in all probability earlier developmental forms." *

Now, by careful comparison, I find that this monad is no other than the "uniflagellate monad," which is the fourth in the series whose life-histories were studied by Dr. Drysdale and myself.† Figs. 7 and 8, Pl. CXXXIII., will help to make this clear, where fig. 7 is an exact rendering of Dr. Bastian's monad magnified 800 diameters; and fig. 8 is a drawing of the "uniflagellate monad" described by my colleague and myself, magnified 2,500 diameters. We describe it thus:—"Its exterior form is extremely simple, being ovoid, with a single flagellum. Its long diameter never exceeds the 1-4000th part of an inch" in length.‡ Now, from a very prolonged and careful study of these organisms, I am convinced that Dr. Bastian's form and ours are absolutely identical. But to make the thing simply irresistible we have further and final evidence. One of the metamorphoses of this monad on its passage to multiple fission is that it loses its flagellum, and becomes precisely what Dr. Bastian saw all around—a motionless spherule." § These little bodies are less in diameter than the active monad, and of precisely the same structure. The identity is thus complete. The evidence is as full as may be; the monad Dr. Bastian saw was the one whose life-history was fully worked out. As usual, it multiplies by fission, but the fission is multiple. It then passes to a sac-like condition, resulting from the uniting together or fusion of two individuals. This sac becomes still and bursts, as seen in fig. 4, pouring out spore that taxed our highest powers and closest watching. The spore of only two of the monads studied survived after exposure to a temperature of 300° F. *This is one of them.*

Now, Dr. Bastian says, "A drop of the fluid containing several

* "Evolution," p. 178. † "Monthly Micros. Journ." vol. xi. p. 69, *et seq.*
‡ P. 69, ibid. § P. 69, ibid.

of these active monads was placed for about five minutes on a glass slip in a water oven, maintained at a temperature of 140° F. All the movements of the monads ceased from that time, and they never afterwards showed any signs of life."* *This is precisely our experience.* But now mark the reasoning. This monad was killed at 140° F., *but it was found in an infusion that had been heated up to* 275° F.; THEREFORE *it must have originated de novo.*

But it has been shown that the monad has germs, and that these have a power of resisting heat up to 300° F.—that is to say, 25° F. higher than that to which Dr. Bastian's infusion *was* exposed—and *therefore*, by the logic of facts, the monads found were not a result of "spontaneous generation," *but were the natural outcome of a genetic product contained in the infusion, and which the heat employed could not destroy.*

We need no stronger proof of the futility of reasoning concerning the thermal death-point of a minute organism where developmental history is wholly unknown. Yet so confident is our experimenter of his result that he says: "Nothing that has yet been alleged, by way of objection to the admission of spontaneous generation as an everyday fact, at all effects such experiments as these. The shortest way out of the difficulty would, therefore, be to doubt the facts." But I think I have shown a still shorter way "out of the difficulty," and that, without the discourtesy of doubting Dr. Bastian's experimental "facts."

The truth, then, is that Dr. Bastian had no real knowledge of the monad; but he argued as if he had. Hence assumed premises led to a false and fatal conclusion.

He is simply repeating this in his latest attitude in reference to the question of the mode of origin of bacteria. Compelled to yield all else, he throws up a rampart round his exceptional flasks, and declares "spontaneous generation" to be impregnable—an inviolable law of nature. Dr. Tyndall is plainly told that his knowledge is insufficient, that he has mistaken the meaning of the question, and that his mode of treating it is "laughable;" † and all this arises from the fact that Professor Tyndall dealt with the question of *the mode of origin* of bacteria generally; whereas, to have pleased Dr. Bastian, he ought to have explained some exceptional conditions to which he now points—the exceptions being more important than the rule!

What are the facts?

I. Dr. Tyndall has proved, in connection with a host of others, but in a more definite and precise manner, that in *filtered infusions* five minutes' boiling does kill every form of bacteria.

* "Evolution and the Origin of Life," p. 179.

† "Lancet," Feb. 5, 1876; and "Brit. Med. Journ." Feb. 5, 1876.

II. He has further shown that they are propagated by demonstrable germs *only*, in such infusions ; and

III. This fact removes the probability of their spontaneous generation to an almost infinite distance.

As to the development of bacteria in infusions charged with solid matter, precise experiment of a sufficiently comprehensive character has yet to be made on them, in relation to the demonstrated germs. Meantime, shall we accept "spontaneous generation" on such ground as its strongest advocate has now to offer, and ignore the vast chain of facts copiously attested and controlled, which are in perfect harmony with the known laws of the entire organic world ? This, and nothing less than this, is what Dr. Bastian inculcates and demands.

EXPLANATION OF PLATE CXXXIII.

FIG. 1. Spore-sac of the cercomonad—the first in the series of Messrs. Dallinger and Drysdale's "Researches"—emitting spore.

FIGS. 2, 3, 4, 5, and 6. The same states of the five other monads of the series.

FIG. 7. A monad found by Dr. Bastian in an infusion, after heating up to 275° F., said to be spontaneously generated.

FIG. 8. The same monad as seen by Dallinger and Drysdale, and the spore of which (fig. 4) survives 300° F.