Forms of Pellicles obtained in Hay Infusion exposed to the following Gases—

1 Carbonic Acid
2. Oxygen.
3 Atmospheric Air. (Cells with Nuclei)
4 Nitrogen. (many monads moving about.)
5 Hydrogen.

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PAPERS, ETC.

On the Influence of Artificially-formed Atmospheres in Modifying the Development of the Lower Forms of Living Organisms.

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THE increasing interest which continues to attach to those researches which are being made into what is now commonly designated "the Evolution of Life," induces me to offer a few observations as a slight contribution to the general fund of information on the subject. My object is to direct attention to the modifying influence exerted by artificially-formed, or factitious atmospheres, on the development of the lower forms of life—a branch of inquiry which, it will at once be perceived, differs in some respects from those more commonly undertaken, the special object of which has been rather to determine whether the lower forms of living organisms do not occasionally spring into existence *de novo* from their elements, or, as it has been termed, "spontaneously"—a doctrine which, previous to the recent admirably conducted experiments of Professor Tyndall, seemed to be gaining such general acceptance as threatened to prove

altogether subversive of the celebrated aphorism, "*omne vivum ex ovo.*"

Having been led many years since to bestow some attention on the subject in question, I commenced what I intended should form the first of a series of experiments in reference to it. The claims, however, of my more strictly professional engagements interfered at that time to prevent me from carrying my intention into effect, and circumstances, which it is unnecessary that I should here detail, occurred subsequently to abate and almost to extinguish the interest I had originally felt in my first design, and it was consequently suffered to fall into abeyance.

In the endeavour of late to resume it, new difficulties have presented themselves, of which the want of the accuracy of vision, so requisite in the prosecution of microscopical researches, is the chief, and must accordingly serve as my apology for making known in its present imperfect form what little I have to communicate on this interesting department of biological inquiry.

It is now so long since as the year 1851, when I was residing at Weston-super-Mare, that, having collected some new-made hay from a hay field in front of my house, I made a decoction of it, boiling it for a considerable time, and then filling five bell-glasses with the decoction, as hot as I could bear my hand in it. This done, I immediately proceeded to fill each bell-glass about three-fourths full of the following gases, viz., oxygen, carbonic acid, hydrogen, nitrogen, and atmospheric air—the latter being intended to serve rather as a standard of comparison. In the course of a few weeks I found the surface of the decoction in each case covered by a pellicle, apparently a low type of vegetable growth. A portion of each of these pellicles I carefully transferred, for examination, to an equal number of separate slips of glass, and on these it was left, and allowed to dry.

Subjected to a high magnifying power, I could clearly perceive that *the appearance of the pellicle varied in each instance with the gas employed.* Being at the time in correspondence with

the late Mr. Quekett upon other subjects, I mentioned the matter incidentally to him, and he expressed a wish that I should forward the glass slips to him, which I did, and he returned them to me with drawings, which he very obligingly had made of them, and stated that the specimens were certainly all plants. These drawings and glass slips are still in my possession, and I have now the pleasure of exhibiting the former, and trust that they will be deemed of sufficient interest and value to merit engraving in illustration of this paper.

The drawings, as will be seen, sufficiently explain themselves, the specimens being magnified in each case 500 diameters, whilst the difference of form observable in each instance is due to no other cause than the variety of gas to which the surface of the same fluid was respectively exposed. (See plate.)

Submitting the specimens some years afterwards to the inspection of Dr. Phipson, of Putney, with whom I happened then to be in correspondence, and who had given considerable attention to these low forms of organised beings, he gave it as his opinion that the different specimens were all the same plant, but in different degrees of development. Being much more conversant than myself with the use of the microscope, and with such researches, I have never ventured to question the correctness of his views on this point; but previously to the opinion thus expressed, I was led, with the magnifying power at my command, to regard the pellicles as differing essentially from each other in each case—that under the carbonic acid appearing to have a higher form of organization than the rest. It would appear that Mr. Quekett must have regarded them also in this light, referring to them, as he did, as being "certainly *all plants*." But however this may be, the marked difference obtained in this experiment, varying as we have seen with the gas employed, is a fact that must be regarded as important, having more than a mere collateral bearing on questions at present under discussion, and being apparently somewhat in conflict with the conclusions deduced from the admirable experiments recently laid before the

Royal Institution by Professor Tyndall. In the very delicate and patiently conducted researches in question, the learned Professor has demonstrated the extreme difficulty of freeing the fluids to be experimented on from living germs imported into them, either through the air, or from some other source; and it appears that neither lengthened boiling of the liquid, nor calcining at the same time the superincumbent air, is found sufficient to insure the destruction of the vitality of these minute organic germs, especially at certain stages of their existence, though it is stated that their sterilization may be infallibly effected by short and repeated exposures to a temperature of even less than 212 degs. of Fahrenheit. One great object, indeed, of these experiments of Professor Tyndall seems to have been to prove that the sterilization of the fluids experimented on may certainly be accomplished, first, by taking every precaution to prevent the admission of germs into them; and secondly, by repeatedly subjecting to the boiling temperature such germs as, notwithstanding the utmost precaution, may have become accidentally admitted. In the case, however, of the experiments to which I have now the honour of directing attention, there was no occasion for calcining or filtrating the superincumbent air, or for removing the apparatus employed from one locality to another, in order to avoid an "infective atmosphere," laden with living germs. Care was taken in each instance to ascertain that pure gas was coming over, before the beak of the retort was introduced beneath the fluid to be experimented on, and thus all access of atmospheric air, in which germs were floating, was more perfectly excluded than could have been effected by removing the infusions which were the subject of the experiment from the germ-laden air of the Royal Institution to the comparatively purer atmosphere of Kew Gardens; and yet in this case there was in due time a manifest development of organic life, the character of this development varying also in this instance with the particular kind of gas to which the surface of the liquid happened to be exposed. Before quitting this part of the subject, there remains yet to be

noticed another point in which these experiments with the gases present us with a result apparently antagonistic to some of the conclusions of Professor Tyndall. Towards the close of his memoir, a résumé of which occurs in the number of *Nature* for the 14th June, 1877, we learn that he regards the use of the Sprengel air-pump, in conjunction with boiling, as the most certain and efficient means of sterilizing the fluids under experiment, observing in conclusion, that "the inertness of the germs in liquids deprived of air is not due to a mere *suspension* of their powers. They are *killed* by being deprived of oxygen. For when the air which has been removed by the Sprengel pump is, after some time, carefully restored to the infusion, unaccompanied by germs from without, there is no revival of life. By removing the air we stifle the life, which the returning air is incompetent to restore," (p. 129). If the conclusions thus arrived at are admitted to be correct, how, it may be asked, are they to be reconciled with the free development of germs in those gases—hydrogen, nitrogen, and carbonic acid—which are wholly devoid of oxygen? and how comes it that carbonic acid affords an atmosphere favourable to the development of germs presenting a highly organised structure?

Previously to the recent investigations of Professor Tyndall, I was under the impression that these experiments, where only gases were employed, and in which the possibility of the introduction of germs from without, was thus apparently excluded, tended to favour the view that "the low organisms which form a pellicle on the surface of infusions or other liquids are produced *de novo* in such infusions." (*Nature*, vol. vi, p. 300.) The precise and very delicate experiments of Prof. Tyndall, however, have now suggested that germs may possibly have been adherent in this instance to the interior surface of the retorts and bell-glasses employed, and may thus, (if not otherwise), have found their way into the fluid forming the subject of experiment, so that these particular experiments must thus be held to lose a portion of the interest and value with which they

seemed formerly invested. The fact, however, of the modifying influence exerted in each separate instance by the special gas employed, still remains; and by those who, like Dr. Bastian and his followers, maintain the view that "all the lower forms of life are being continually produced *de novo*, under the influence of unknown laws of development," (p. 303), it is possible that the fact thus established may be regarded as tending to supply at least one link in the chain of "the unknown laws of development," under the influence of which these lower forms of life originate.

At all events, a field of research would seem to be thus opened, the fuller cultivation of which may possibly exercise important influences on some of those biological questions of the day, which must be regarded as being still *sub judice*. The substances, of which so long a list is now frequently employed in similar investigations, might be tried in a great variety of gases, and these, again, may be mixed in various proportions, whilst the exposure to, or the exclusion from, the powerful influence exercised by light, is a point that may be deserving of further attention. Then again, the subjection, under the foregoing conditions, of the fluid forming the subject of experiment to the action of a long continued low galvanic current, would promise to lend further interest and variety to the group of experiments thus indicated, since it was in the course of experiments in which the electric agency was thus employed by the late Mr. Crosse, of Broomfield, near Taunton, that the *Acarus electricus*, or *Crossii*, an insect which may perhaps be regarded as furnishing the most decided and evident instance of spontaneous generation hitherto recorded, is said to have been formed. And here, in furtherance of this part of the subject, I would venture also to suggest that, when it is considered how the spread of zymotic disease is often associated with the diffusion of various mephitic gases, the modification effected in the development of germs by their contact with particular gases, and possibly their greater aptitude for development in some gases rather than in others, are

points, the further investigation of which may prove interesting to those engaged in the important study of the laws of infection.

It may perhaps be expected that some fuller and more precise observations should here be offered as regards the bearing which these experiments are calculated to exercise on the much debated question of spontaneous generation. Beyond the incidental comments, however, already made, I purposely refrain from indulging in any remarks on this particular point, preferring to content myself with placing a statement of the bare facts on record, and merely observing that they certainly tend to show that life may be developed and maintained under conditions which have usually been regarded as adverse to, if not actually incompatible with, its existence; and that, assuming germs to have been present, their modification by contact with specially prepared atmospheres, as here shown, has yet to be accounted for.

In conclusion, I would remark that, as from what has been already advanced it may be inferred that these observations rest only on a single group of experiments, made long ago, it is well to state that the results then obtained were very clear and decisive, as may at once be seen by reference to the drawings; and it is right here to add that these first experiments have been fully and carefully verified by comparatively recent repetitions.

They have been applied also in a slight measure to turnip and other infusions, in which, so far as they were carried, similar differences were presented with different gases. For the reasons already assigned, however, the observations have been chiefly confined to effects obtained with the infusion of hay, with which the results are sufficiently distinct and constant to establish the fact here insisted on, that *the development of the lower organisms from solutions of organic matter is sensibly and specially modified by the particular kind of superincumbent gas or atmosphere to which the surface of such solution is exposed.*