# II. *The Optical Deportment of the Atmosphere in relation to the Phenomena of Putrefaction and Infection.* By John Tyndall, *F.R.S.*

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## § 1. *Introduction.*

An inquiry into the decomposition of vapours by light, begun in 1868 and continued in 1869*, in which it was necessary to employ optically pure air, led me to experiment on the floating matter of the atmosphere. A brief section of a paper published in the Philosophical Transactions for 1870† is devoted to this subject.

I at that time found that London air, which is always thick with motes, and also with matter too fine to be described as motes, after it had been filtered by passing it through densely packed cotton-wool, or calcined by passing it through a red-hot platinum-tube containing a bundle of red-hot platinum wires, or by carefully leading it over the top of a spirit-lamp flame, showed, when examined by a concentrated luminous beam, no trace of mechanically suspended matter. The particular portion of space occupied by such a beam was not to be distinguished from adjacent space.

The purely gaseous portion of our atmosphere was thus shown to be incompetent to scatter light.

I subsequently found that, to render the air thus optically pure, it was only necessary to leave it to itself for a sufficient time in a closed chamber, or in a suitably closed vessel. The floating matter gradually attached itself to the top and sides, or sank to the bottom, leaving behind it air possessing no scattering power. Sent through such air, the most concentrated beam failed to render its track visible.

I mention 'top' and 'sides,' as well as 'bottom,' because gravity is not the only agent, probably not the principal agent, concerned in the removal of the floating matter. It is practically impossible to surround a closed vessel by an absolutely uniform temperature; and where differences of temperature, however small, exist, air-currents will be established. By such gentle currents the floating particles are gradually brought into contact with all the surrounding surfaces. To these they adhere, and, no new supply being admitted, the suspended matter finally disappears from the air altogether.

The parallelism of these results with those obtained in the excellent researches of Schwann‡, Schroeder and Dusch§, Schroeder himself||, and Pasteur¶ in regard to the question of "spontaneous generation," caused me to conclude that the power of scattering light and the power of producing life by the air would be found to go hand in hand.

* Proc. Roy. Soc. vol. xvii.

† Vol. clx. p. 337.

‡ Pogg. Ann. 1837, vol. xli. p. 184.

§ Ann. der Pharmacie, vol. lxxxix. p. 232.

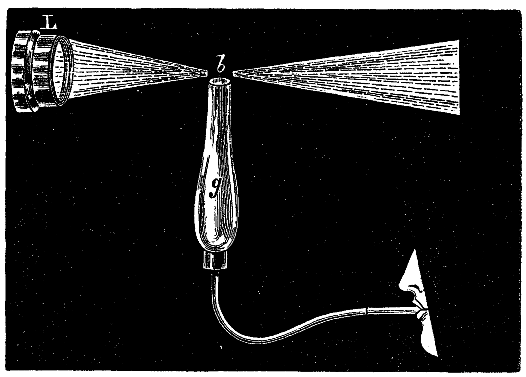
|| Ann. der Pharmacie, vol. cix. p. 35.

¶ Ann. de Chim. et de Phys. 3rd series, vol. lxiv. p. 83.

This conclusion was strengthened by an experiment easily made and of high significance in relation to this question. It had been pointed out by Professor LISTER, of Edinburgh *, that air which has passed through the lungs is known to have lost its power of causing putrefaction. Such air may mix freely with the blood without risk of mischief; and that truly great scientific Surgeon had the penetration to ascribe this immunity from danger to the filtering power of the lungs. Prior to my becoming acquainted with this hypothesis in 1869, I had demonstrated its accuracy in the following manner†.

Condensing in a dark room, and in dusty air, a powerful beam of light, and breathing through a glass tube (the tube actually employed was a lamp-glass, rendered warm in a flame to prevent precipitation) across the focus, a diminution of the scattered light was first observed. But towards the end of the expiration the white track of the beam was broken by a perfectly black gap, the blackness being due to the total absence from the expired air of any matter competent to scatter light. The experimental arrangement is represented in fig. 1, where *g* represents the heated lamp-glass, and *b* the gap cut out of the beam issuing from the lamp L. The deeper portions of the lungs were thus proved to be filled with optically pure air, which, as such, had no power to generate the organisms essential to the process of putrefaction‡.

Fig. 1.




It seemed that this simple method of examination could not fail to be of use to workers in this field. They had hitherto proceeded less by sight than by insight, being in general unable to see the physical character of the medium in which their experiments were conducted. But the method has not been much turned to account; and this year I thought it worth while to devote some time myself to the more complete demonstration of its utility.

I also wished to free my mind, and if possible the minds of others, from the uncertainty and confusion which now beset the doctrine of "spontaneous generation." PASTEUR has pronounced it "a chimera," and expressed the undoubting conviction that this being so it is possible to remove parasitic diseases from the earth. To the medical profession, therefore, and through them to humanity at large, this question, if the

* Introductory Lecture before the University. † Proc. Roy. Inst. vol. vi. p. 9.

‡ "No putrefaction," says COHN, "can occur in a nitrogenous substance if it be kept free from the entrance of new *Bacteria* after those which it may contain have been destroyed. Putrefaction begins as soon as *Bacteria*, even in the smallest numbers, are accidentally or purposely introduced. It progresses in direct proportion to the multiplication of the *Bacteria*; it is retarded when the *Bacteria* (for example, by a low temperature) develop a small amount of vitality, and is brought to an end by all influences which either stop the development of the *Bacteria*, or kill them. All bactericidal media are therefore antiseptic and disinfecting."—*Beiträge zur Biologie der Pflanzen*, zweites Heft, 1872, p. 203.

illustrious French philosopher be correct, is one of the last importance. But PASTEUR'S labours, which have so long been considered models by most of us, have been subjected to rough handling of late. His reasoning has been criticised, and experiments counter to his have been adduced in such number and variety, and with such an appearance of circumstantial accuracy, as to render the evidence against him overwhelming to many minds. This, I have reason to know, has been the effect wrought, not only upon persons untrained in science, but also upon biologists of eminence both in this country and America. The state of medical opinion in England is correctly described in a recent Number of the 'British Medical Journal,' where, in answer to the question, "In what way is contagium generated and communicated?" we have the reply that, notwithstanding "an almost incalculable amount of patient labour, the actual results obtained, especially as regards the manner of generation of contagium, have been most disappointing. Observers are even yet at variance whether these minute particles, whose discovery we have just noticed, and other disease-germs, are always produced from like bodies previously existing, or whether they do not, under certain favourable conditions, spring into existence *de novo*."

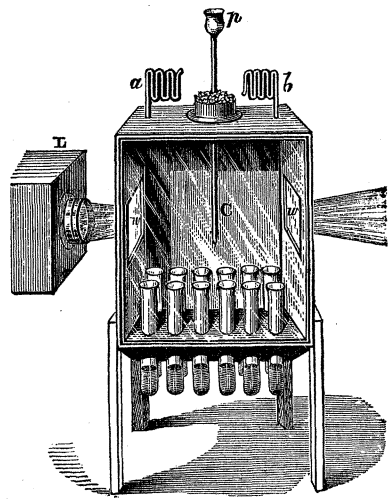
With a view to the possible diminution of the uncertainty thus described, I beg without further preface to submit to the Royal Society, and especially to those who study the etiology of disease, the following description of the mode of procedure followed in this inquiry, and of the results to which it has led.

## § 2. *Method of Experiment.*

A chamber, or case, was constructed, with a glass front, its top, bottom, back, and sides being of wood. At the back is a little door which opens and closes on hinges, while into the sides are inserted two panes of glass, facing each other. The top is perforated in the middle by a hole 2 inches in diameter, closed air-tight by a sheet of india-rubber. This sheet is pierced in the middle by a pin, and through the pin-hole is passed the shank of a long pipette ending above in a small funnel. A circular tin collar, 2 inches in diameter and $1\frac{1}{2}$ inch deep, surrounds the pipette, the space between both being packed with cotton-wool moistened by glycerine. Thus the pipette, in moving up and down, is not only firmly clasped by the india-rubber, but it also passes through a stuffing-box of sticky cotton-wool. The width of the aperture closed by the india-rubber secures the free lateral play of the lower end of the pipette. Into two other smaller apertures in the top of the cupboard are inserted, air-tight, the open ends of two narrow tubes, intended to connect the interior space with the atmosphere. The tubes are 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.

The bottom of the box is pierced with two rows of holes, six in a row, in which are fixed, air-tight, twelve test-tubes, intended to contain the liquid to be exposed to the action of the moteless air.

Fig. 2.



The arrangement is represented in fig. 2, where *w w* are the side windows through which the searching beam passes from the lamp L across the case C; *p* is the pipette, and *a b* are the bent tubes connecting the inner and outer air. The test-tubes passing through the bottom of the case are seen below.

On the 10th of September this case was closed. The passage of a concentrated beam across it through its two side windows then 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 of quiet sufficed to cause all the floating matter to be deposited on the interior surfaces, where it was retained by a coating of glycerine, with which these surfaces had been purposely varnished.

### § 3. *Deportment of Urine.*

The pipette being dipped into the tubes, fresh urine was poured into eight of them in succession on the 13th of last September. Each tube was about half-filled with the liquid. The tubes were then immersed in a bath of brine, raised to ebullition, and permitted to boil for five minutes. Aqueous vapour rose from the liquid into the chamber, where it was for the most part condensed, the uncondensed portion escaping, at a low temperature, through the bent tubes at the top. Before the brine was removed little stoppers of cotton-wool were inserted in the bent tubes, lest the entrance of the air into the cooling-chamber should at first be forcible enough to carry motes along with it. As soon, however, as the ambient temperature was assumed by the air within the case the cotton-wool stoppers were removed.

The front and back of this chamber were squares of 14 inches the side, the depth of the chamber being 8·5 inches. It contained, therefore, 1666 cubic inches of air, which had unimpeded access to the liquid in the tubes. No stoppers were employed. The air was unaffected by calcination, or even by filtering. Neither cotton-wool nor hermetic sealing was resorted to. Self-subsidence was the only means employed to rid the "untortured" air of its floating matter.

A second series of eight tubes were filled at the same time with the same liquid, and subjected to the same boiling process. The only difference between the two series was, that these latter tubes were placed in a stand beside the former and exposed to the common air of the laboratory.

For the sake of distinction I will call the tubes opening into the case the *protected* tubes, and those opening into the common air the *exposed* tubes.

On the 17th of September all the protected tubes were bright and clear, while all the exposed tubes were distinctly turbid. Specks of mould, moreover, were in every case seen on the surface of the exposed liquid. These waxed daily larger, and finally formed a thick layer on the top of every column. The liquid changed from a pale sherry to a reddish-brown colour, some of the tubes being more deeply tinged than others.

On the 27th of September I provided myself with a microscope having a magnifying-power of 1200 diameters. Under its scrutiny the turbidity of the liquid immediately resolved itself into swarms of *Bacteria* in active motion. COHN correctly explains the turbidity. The index of refraction of the *Bacterium* being slightly different from that of the surrounding medium, a scattering of light is the consequence. This scattering, however, and the opalescence it produces, are practically independent of the motions of the *Bacteria*.

Since the date here referred to the exposed liquid has been frequently examined, both with the eye and with the microscope. To the former it is thickly turbid, to the latter it is swarming with life. Its smell is putrid. *All this time the protected tubes exhibit a liquid perfectly unchanged in appearance.* For four months it has remained as transparent and of as rich a colour as the brightest Amontillado sherry.

On the 1st of October another experiment similar in principle to that just described was begun. Fresh urine was employed, and a much smaller case. The capacity of the latter was 451 cubic inches; and three test-tubes, instead of twelve, were passed air-tight through its bottom. Like those in the larger chamber they were filled by a pipette, and boiled for five minutes in a bath of brine. Beside them were placed three other tubes containing the same liquid treated in exactly the same way, but exposed to the common air. On the 5th all the exposed tubes were turbid, and found by microscopic examination to be swarming with *Bacteria.* The colour of the exposed liquid changed from a pale sherry colour to a brown orange. On the 25th the tubes were again examined, and found full of *Bacteria.* Two months subsequent to this latter date the infusion, diminished by evaporation, was found well charged with Bacterial life.

*While this process of putrefaction was going on outside, the tubes opening into the moteless air of the case remained perfectly clear.*

The chamber represented in fig. 2, and above described, was the first operated on, and the liquid is shown by the draughtsman as filling only a small portion of the test-tubes. This smallness of volume is in part due to evaporation. Test-tubes 1·2 inch wide and 9 inches long were, in all subsequent experiments, nearly filled with the infusions. Strong in the first instance, these were sometimes kept until slow evaporation through the open tubes at the top of the case had reduced them to one third or one fourth of their original volume. Each experiment, therefore, was, in reality, a series of experiments, extending over months, on infusions of different strengths, the concluding ones of the series attaining a very high degree of concentration. In fig. 2 the portion of the tubes within the case ought to be less than one half of what it is there shown to be.

### § 4. *Mutton-Infusion.*

A case was constructed to contain six test-tubes. It, like the others, had a front of glass, side windows, and a back door. Its capacity was 857 cubic inches. It was sealed up on the 21st of September, and found free from floating matter on the 24th. Lean mutton, cut into small pieces, was digested for four hours in water of a temperature of 120° F. The infusion was then carefully filtered, and introduced into the six test-tubes by a pipette which was never removed from the case.

The mutton-juice was of a fine ruby colour; but on boiling, its albumen was precipitated, subsequently sank, and carried the colouring-matter with it. The supernatant liquid was perfectly clear. The frothing was considerable when the boiling began. Beside this case was placed a stand containing six test-tubes filled with the same infusion, but exposed to the common air.

On the 27th all the outside flasks were perceptibly turbid; on the 28th they were found well filled with *Bacteria*, which on the 30th had increased to astonishing swarms. On the 15th of October the tubes were again examined, and found charged with undiminished life. They remained thus "putrid" until the 14th of November.

*During the whole of this time the infusion in contact with the moteless air of the chamber remained as clear as distilled water.*

On the 14th of November I infected one of the clear tubes by introducing into it through the pipette a few drops of mutton-infusion which had been prepared and exposed upon the 12th of November, and which two days had sufficed to render turbid. On the 15th the inoculated infusion showed signs of turbidity, and on the 16th putrefaction had actively set in, the liquid being thickly muddy and full of life.

With a moteless chamber and three tubes, experiments were subsequently made on a second infusion of mutton. In this case, however, the infusion was boiled, its albumen was precipitated, and removed by filtration prior to its introduction into the chamber. The pellucid liquid was introduced on the 1st of October, boiled for five minutes in the brine-bath, and abandoned to the air of the case. A series of exposed tubes containing the same infusion, similarly treated, was placed beside the protected ones. On the 4th all the outside tubes were muddy and swarming with *Bacteria.* SCHROETER and COHN have shown that different colours are produced by different kinds of *Bacteria.* In the three exposed tubes here referred to a yellow-green pigment was developed.

*Up to the present date, or for more than three months after its preparation, the infusion, considerably diminished by evaporation, remains in all the protected tubes as clear as at first.*

### § 5. *Beef-Infusion.*

A beef-steak, after having its fat removed, was cut up into small pieces, and digested for three hours at a temperature of 120° F. The liquid was then poured off, boiled, and filtered. It was as clear and colourless as pure water. On the 4th of October it was introduced into three tubes protected by a chamber of 451 cubic inches capacity. It was boiled for five minutes in a brine-bath. Three exposed tubes, containing the same infusion, were placed beside the protected ones. On the 5th the exposed tubes showed signs of haziness, on the 6th they were turbid, of a green colour, and filled with *Bacteria*. They have maintained their muddiness, colour, and swarming life up to the present time.

*While the exposed beef-infusion putrified in this way, all the protected infusions remained perfectly sweet and clear.*

### § 6. *Haddock-Infusion.*

The haddock was cut up and digested on the 24th of September; it was afterwards introduced into six tubes, protected by a chamber. On boiling, its albumen, like that of the mutton first referred to, coagulated and sank to the bottom, leaving a perfectly clear liquid behind. Six exposed tubes filled with the same infusion were placed beside the six protected ones.

On the 27th the exposed tubes were all turbid and swarming with *Bacteria*. On the 29th one of the tubes showed a fine green colour; three other tubes showed the same colour afterwards. The vivacity of the organisms was extraordinary, and their shapes various. They darted rapidly to and fro across the field, clashing, recoiling, and pirouetting—rendering it, indeed, difficult to believe in the vegetable nature which the best microscopists assign to them.

*For nearly three weeks the protected tubes remained perfectly clear.* To gain room, the case was subsequently shifted, and soon afterwards one of the six tubes became turbid. Something, doubtless, had been shaken into it from the top of the chamber.

For more than a month this single infected flask remained in company with the five healthy ones. The air containing the products of putrefaction had free access to the whole of them, but there was no spread of the infection. As long as the organisms themselves were kept out of the flasks, the "sewer-gas" developed by the putrefaction had no infective power. On the 14th of November I infected two perfectly pellucid tubes with haddock-infusion which, after boiling, had been exposed for two days to the air. On the 15th the two tubes had obviously yielded to the infection. On the 16th disease, if I may use the term, had completely taken possession of them. Into one of them only one or two drops of the turbid infusion had fallen, while ten times this amount was introduced into the other. Nevertheless on the 16th both appeared equally turbid. The infection acted exactly like the virus of smallpox, a small quantity of which will in the long run produce the same effect as a large one.

### § 7. *Turnip-Infusion.*

Turnip-juice had a special interest for me in consequence of the important part it plays in the experiments of Dr. BASTIAN. I turned to it with the anxious desire to learn whether the statements made concerning it were correct.

The conditions laid down as to the strength of the solution, the temperature to be maintained during the time of digestion, and the time it was to be maintained* were scrupulously adhered to. Thus the turnip was cut into thin slices, and digested for four hours in a beaker of water immersed in a water-bath kept at a temperature close to 120° Fahr. The infusion was then carefully filtered, introduced through a pipette into its case, and boiled there for five minutes. Six protected test-tubes were charged with the infusion on the 24th of September, while six other tubes were placed on a stand outside, and exposed to the common air of the laboratory.

On the 27th the exposed tubes were distinctly turbid, and on microscopic examination were found peopled with *Bacteria*. The protected tubes, on the contrary, were perfectly clear. A little distilled water had been added to one of the outer tubes. The germinal matter, whatever it may be, must have been copious in the water; for the tube to which it was added far exceeded the other two in the rapidity of life-development. On the 30th this tube contained *Bacteria* in swarms, of small size, but of astonishing activity. The other tubes also were fairly charged with organisms, larger and more languid, but not at all so numerous as in the watered tube. On the 5th of October some of the exposed tubes began to clear; as if the *Bacteria* had died through lack of nutriment, and were falling as a thick sediment to the bottom.

*During these changes the protected tubes were visibly unaltered, the liquid within every one of them remaining as clear as it had been on the day of its introduction.*

In this instance I was specially anxious to verify the result by repetition. Two other cases were therefore fitted up to contain three tubes each, and instead of a door a movable panel was placed at the back. After two or three days' rest both cases were found free from floating matter, and on the 1st of October the turnip-infusion was introduced, and boiled for five minutes in a bath of brine.

In the former experiment the temperature of digestion was maintained by keeping the beaker containing the turnip in a bath of warm water. In the present instance the turnip was sliced in a dish and placed before a fire. An occult but efficient power like that already ascribed to the actinic rays†, might, I thought, be ascribed to radiant heat, and I therefore copied to the letter the mode of digesting pursued by Dr. BASTIAN.

Adjacent to the closed cases was placed a series of three exposed tubes, containing a liquid prepared in precisely the same way. On the 4th of October the exposed tubes were all turbid, and swarmed with *Bacteria*. In two of the tubes they were distinctly more numerous and lively than in the third. Such differences between sensibly conterminous tubes, containing the same infusion, are frequent. On the 9th, moreover, the two most actively charged tubes were in part crowned by beautiful tufts of mould. This expanded gradually until it covered the entire surface with a thick tough layer, which must have seriously intercepted the oxygen said to be necessary to Bacterial life. The *Bacteria* lost their translatory power, fell to the bottom, and left the liquid between them and the superficial layer clear.

* Beginnings of Life, vol. i. p. 357, note.

† Nature, vol. iii. p. 247.

Another difference, pointing to differences in the life of the air, was shown by these tubes. The turbidity of the two mould-crowned ones was colourless, exhibiting a grey hue. The third tube, the middle one of the three, contained a bright yellow-green pigment, and on its surface no trace of mould was to be seen. It never cleared, but maintained its turbidity and its Bacterial life for months after the other tubes had ceased to show either. It cannot be doubted that the mould-spores fell into this tube also, but in the fight for existence the colour-producing *Bacteria* had the upper hand. Six other tubes, similarly exposed, showed the grey muddiness: all of them became thickly covered with mould, under which the *Bacteria* died or passed into a quiescent state, fell to the bottom, and left the liquid clear.

*Up to the 13th of October the purity of the six protected tubes remained unimpaired.*

Here a complementary experiment was made. It remained to be proved that those long-dormant clear infusions had undergone no change which interfered with their ability to develop and maintain life. On the 13th, therefore, the small panel was removed from the back of one of the cases, and with three new pipettes specimens were taken from the three tubes within it. The closest search revealed no living thing. The air of the laboratory being permitted to diffuse freely into the case, on the day after the removal of the panel the test-beam showed the case to be charged with floating matter.

*The access of this matter was the only condition necessary to the production of life; for on the 17th all the tubes were muddy and swarming with* Bacteria.

A similar experiment, subsequently made, revealed to me some of the snares and pitfalls which await an incautious worker on this question. The chamber already referred to as containing six tubes, filled with turnip-juice, preserved the infusion clear for a month. On the 21st of October the back door of the chamber was opened, and specimens of the clear infusion were taken out for examination by the microscope. The first tube examined showed no signs of life. This result was expected, but I was by no means prepared for the deportment of the second tube. Here the exhibition of life was monstrously copious. There were numerous globular organisms, which revolved, rotated, and quivered in the most extraordinary manner. There were also numbers of lively *Bacteria* darting to and fro. An experimenter who ponders his work and reaches his conclusions slowly, cannot immediately relinquish them; and in the present instance some time was required to convince me that no mistake had been made. I could find none, and was prepared to accept the conclusion that in the boiled infusion, despite its clearness, life had appeared.

But why, in the protected turnip-infusion, which had been examined on the 13th of October, could no trace of life be found? In this case perfect transparency was accompanied by an utter absence of life. The selfsame action upon light that enabled the *Bacteria* to show themselves in the microscope must, one would think, infallibly produce turbidity. Why, moreover, should life be absent from the first member of the present group of tubes? I searched this again, and found in it scanty but certain signs of life. This augmented my perplexity. A third tube also showed scanty traces of life. I reverted to the second tube, where life had been so copious, and found that in it the organisms had become as scanty as in the others. I confined myself for a time to the three tubes of the first row of the six, going over them again and again; sometimes finding a *Bacterium* here and there, but sometimes finding nothing. The first extraordinary exhibition of life it was found impossible to restore. Doubtful of my skill as a microscopist I took specimens from the three tubes and sent them to Prof. HUXLEY, with a request that he would be good enough to examine them.

On the 22nd the search was extended to the whole of the tubes. Early in the day lively *Bacteria* were found in one of them; later on, not one of the six yielded to my closest scrutiny any trace of life. On the evening of the 22nd a note was received from Prof. HUXLEY stating that a careful examination of the specimens sent to him revealed no living thing.

Pipettes had been employed to remove the infusion from the test-tubes. They were short pieces of narrow glass tubing, drawn out to a point, with a few inches of india-rubber tubing attached to them. This was found convenient for bending so as to reach the bottom of the test-tubes. Suspicion fell upon this india-rubber. It was washed, the washing-water was examined, but no life was found. Distilled water had been used to cleanse the pipettes, and on the morning of the 23rd I entered the laboratory intending to examine it. Before dipping the pipette into the water I inspected its point. The tiniest drop had remained in it by capillary attraction from the preceding day. This was blown on to a slide, covered, and placed under the microscope. An astonishing exhibition of life was my reward. Thus on the scent, I looked through my pipettes, and found two more with the smallest residual drops at the ends; both of them yielded a field rampant with life. The *Bacteria* darted in straight lines to and fro, bending right and left along the line of motion, wriggling, rotating longitudinally, and spinning round a vertical transverse axis. Monads also galloped and quivered through the field. From one of these tiny specks of liquid was obtained an exhibition of life not to be distinguished from that which had astonished me on the 21st.

Obviously the phenomenon then observed was due to the employment of an unclean pipette. Equally obvious is it that in inquiries of this nature the experimenter is beset with danger, the grossest errors being possible when there is the least lack of care.

The door of this case had been opened with a view to testing the capacity of the infusions within it to develop and maintain life. *For four weeks they had remained perfectly clear.* Two days after the door was opened and the common laboratory air admitted all six tubes were turbid, and swarming with *Bacteria*. Some of them were very long, and their wriggling and darting hither and thither very impressive.

The chamber here referred to was again thoroughly cleaned, sealed, and permitted to remain quiet until its floating matter had subsided. On the 17th of November a fresh infusion of turnip was introduced into it through the pipette, boiled in an oil-bath, and again abandoned to the air of the case.

*Up to the present time the infusion in every tube of the six remains as clear as it was on the day of its introduction.*

Six other tubes charged with the same infusion, boiled in the same way, became turbid in a few days, and subsequently covered with thick layers of *Penicillium*.

### § 8. *Hay-Infusion.*

This infusion has been credited with a power of spontaneous generation similar to that ascribed to turnip-juice. The hay being chopped into short lengths was digested for four hours in water kept at a temperature of 120° Fahr. On the 24th of September the filtered infusion was introduced into its chamber, and boiled there for five minutes. Six tubes were charged with the protected liquid, while six other tubes, filled with the same infusion, were placed on a stand outside the case.

On the 27th the inside flasks were clear, the outside ones faintly turbid. On the 28th spots of mould appeared upon all the exposed surfaces. The infusion in one of the tubes had been diluted with distilled water, and in it the development of life was far more rapid than in the five others; all of them, however, on the 28th contained *Bacteria.*

On the 29th I noticed a larger organism than the *Bacteria* moving rapidly to and fro across the field, the drop containing it being taken from the dilute infusion. Several of them were seen upon the 30th gambolling among the smaller *Bacteria*, appearing bright or dark as they sank or rose in the liquid, a film of which, large as they looked, was to them an ocean. Swarms of *Bacteria* were seen on the 2nd of October, their translatory motions being so rapid and varied, and guided by so apparent a purpose, as to render it difficult to believe that they could be any thing else than animals. On the 15th there was a marvellous exhibition of the larger Infusoria, which appeared to have driven the *Bacteria* from their habitat, as few of them were to be seen. My inability to find the larger creatures a second time in such numbers perplexed me, causing me to conclude that I had accidentally alighted upon a colony of them. Subsequent experience with the pipettes already described pointed, however, to another source.

While three days sufficed to break down the purity, and to fill with Bacterial life, the six exposed tubes, *the six protected ones remained for more than three months as clear and healthy as they were on the day the infusion was poured into them.* Neither a trace of mould upon the surface of any one of them, nor a trace of turbidity in its mass, was to be seen.

Into another case containing three test-tubes a very strong infusion of hay was introduced on the 1st of October. It was boiled for five minutes, and then abandoned to the air of the case. Three other tubes exposed to the laboratory air were placed on a stand beside the case. The colour of the infusion was very deep, but it was quite transparent. One of the outer tubes was diluted with distilled water. On the 3rd the infusion in this tube was turbid, the others remaining clear. The germinal matter had in some way or other invaded the distilled water, and made its action rapid. The dilute infusion contained multitudes of *Bacteria*, many motionless, but many moving rapidly about. On the 4th of October all the tubes swarmed with *Bacteria*. They continued muddy till the middle of November, when they were employed for experiments on infection.

*Throughout the whole of this time the protected tubes remained unchanged.*

With regard to infection, it may be stated here that the merest speck of a vegetable infusion containing *Bacteria* infects all animal infusions, and *vice versâ*. The bursting of a bubble infects an infusion reached by the spray. It is the envelope, and not the gas of the bubble, which produces this result.

Other experiments on hay-infusions, acid, neutral, and alkaline, placed in contact with air purified in various ways, yielded the same negative result.

### § 9. *Infusion of Sole.*

The fish was cut up and digested for three hours in water kept at 120° Fahr. On the 17th of November it was introduced into a case containing three test-tubes, and boiled there for five minutes. Three other tubes hung outside the case were exposed to the ordinary laboratory air.

The three exposed tubes were feebly but distinctly cloudy on the 19th. On the 22nd they were all thickly turbid. Scattered spots of *Penicillium* then appeared on two of them, while the third tube, which stood between these two, kept the *Penicillium* down. This central tube contained the pigment-forming *Bacteria*, which have frequently shown a singular power in preventing the development of mould. For nearly two months the central tube has successfully withstood this development, while its two neighbours are covered by a matted layer of *Penicillium*.

*During the whole of this time the protected infusion continued as clear and colourless as distilled water.*

### § 10. *Liver-Infusion.*

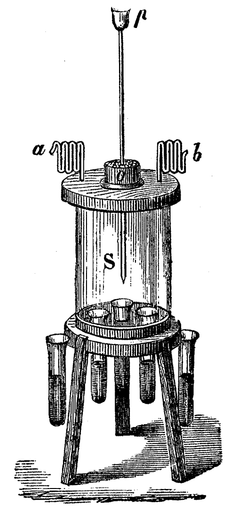
On the 10th of November the infusion was prepared by the process of digesting already so often described. It was introduced into a case containing three protected tubes, and boiled there for five minutes in the brine-bath. Hung on to the chamber at the same time were three tubes containing the same infusion, but exposed to the common air. On the 13th *Bacteria* were numerous in the exposed tubes, and soon afterwards all three of them became thickly muddy and putrescent. They continued so for months.

*The protected tubes, on the contrary, showed throughout a bright yellow liquid, as transparent as it was on the day of its introduction into the case.*

### § 11. *Infusions of Hare, Rabbit, Pheasant, and Grouse.*

For the sake of economy, as so many of them were employed, the shape of the cases was subsequently varied. The rounded end of a tall glass shade was cut off, so as to convert the shade into a hollow cylinder (S, fig. 3), open at both ends. This was set

Fig. 3.




upright on a wooden stand, and cemented to it air-tight. Through the stand passed three large test-tubes (shown in the figure) also air-tight. To the top of the cylinder was cemented a circular piece of wood, the middle of which was occupied by a pipette, *p*, passing first through india-rubber and then through a stuffing-box, *o*, of cotton-wool moistened by glycerine*. The air within the case was connected with the air without by means of the open bent tubes *a* and *b*.

In the first experiments made with these cases defects of construction were revealed during the boiling of the infusions. But increased experience enabled my assistant to render them secure. The floating matter within the cases having been permitted to subside, into four of them, on the 30th of November, infusions of hare, rabbit, pheasant, and grouse were introduced. They were boiled in the usual way, and abandoned to the air of the case. Outside each case, and hung on to it, were three test-tubes of the same size and containing the same infusion as that within.

Examined on Christmas-day, the following were the observed results:—

*Pheasant.*—The three interior tubes perfectly limpid; the three exposed ones turbid and covered with *Penicillium.*

*Grouse.*—The same as pheasant.

*Hare.*—The same as grouse and pheasant.

*Rabbit.*—The three interior tubes covered with tufts of particularly beautiful *Penicillium*, some of the tufts striking deep into the liquid. In two out of the three tubes, moreover, mycelium was flourishing below. All the outer tubes were, as usual, turbid and covered with *Penicillium.*

Is this, then, a case of spontaneous generation? Without further evidence no cautious worker would draw such a conclusion. Opposed to this isolated instance stand all the others mentioned in these pages, and their proper action on the mind is to compel the closest scrutiny before accepting this apparent exception as a real one. Subjected to such scrutiny, it appeared that of the four shades the one containing the rabbit-infusion, and that only, had yielded to the heat of boiling. The shade had been fastened upon its slab with plaster and cement, which became so loose during the boiling that the steam issued from the chinks. But crannies which could permit steam to escape could permit air to enter, and to the presence of such air the appearance of the *Penicillium* was doubtless due.

I did not, however, rest content with mere inference, but tested the rabbit-infusion by placing three fresh tubes of it in one of the firmer cases first described. It was introduced and boiled on the 5th of January, three other tubes filled with the same boiled

* In the earlier experiments the india-rubber formed the bottom of the stuffing-box, where particles were sometimes detached from it by the motion of the pipette. To prevent this the positions of wool and rubber were afterwards reversed.

infusion being exposed on the same day to the ordinary air. The three protected tubes remained clear for three months, while in three days the three exposed ones were charged with *Bacteria*.

*Salmon*.—The colouring-matter of this fish did not at all affect the infusion; indeed no better example of original freedom from colour or opalescence, and of persistent purity in contact with the moteless air, has occurred to me than salmon-infusion. It was introduced into a cylindrical case on the 13th of December, where it continued for months to show the brilliant transparency exhibited at first. Three unprotected tubes, on the other hand, became turbid and covered with mould in a few days.

*Hops*.—One tube of this infusion was protected simply by a lamp-glass, corked and cemented above and below. Through the lower cork passed the single test-tube, air-tight; while through the upper one passed the pipette and the bent tubes intended to connect the outer and the inner air. The infusion was prepared and introduced on the 28th of October. In a few days the exposed tube was found turbid and covered with mould; the protected tube, on the contrary, remained clear for several months.

*Tea and Coffee*.—One tube of each was protected by a lamp-glass similar to that employed in the infusion of hops. Both were prepared on the 28th of October, exposed tubes being hung up at the same time. The protected tea has remained clear, while the exposed tea is turbid and covered with mould. Both the exposed coffee and the protected coffee are turbid and covered with mould.

The remarks already made with regard to the rabbit-infusion apply here. The case is one, not for the hasty admission of spontaneous generation, but for further scrutiny. I examined the apparatus as it stood. The pipette used to introduce the coffee (and this one only of the three employed in these experiments) rested against the outer edge of the tube containing the infusion. This had in part evaporated, had been in part condensed, and had trickled down the pipette so as to form a small drop at the point where pipette and tube touched each other. The drop had virtually washed the outer surface of the pipette, carrying with it, in part, such matter as might have attached itself to that surface. A portion of this washing-water reaching the infusion was doubtless the origin of the life observed. The sure test, however, was the repetition of the experiment under conditions which should exclude this source of error. On the 27th of December accordingly two tubes protected by lamp-glasses were prepared, two other tubes of the infusion being exposed to the air. The former remained clear for months, the latter in the same number of days became turbid and covered with *Penicillium*.

### § 12. *Infusions of Codfish, Turbot, Herring, and Mullet.*

With a view of causing these experiments on moteless and mote-laden air to run parallel with others made with hermetically-sealed tubes, to be described further on, I added the fish named in the heading of this section to the other substances examined. The mullet was introduced into its case on the 3rd of January. The warm air had, however, so acted on the wood of the case, which had been employed in former experiments, that the water of condensation trickled from a chink in the bottom. The other cases were mended as far as possible, and into them the infusions were introduced on the 4th of January. Each case, as before, was provided with three exposed tubes for comparison with the three protected tubes within. On the morning of the 6th the exposed turbot-infusion was clear in all the tubes; a few hours subsequently two out of the three became cloudy; while on the 7th *Bacteria* had taken possession of all of them. All the unprotected tubes of codfish were cloudy on the 6th, more cloudy on the 7th, and covered with a soapy layer upon the 8th. The three exposed herring-tubes were also cloudy on the 6th, the cloudiness advancing afterwards to thicker turbidity. The mullet gave way in the same manner. For more than three months the protected tubes, including even the imperfect chamber which protected the mullet-infusion, have remained as clear as they were upon the day of their introduction.

To these fish-infusions may be added others of eel and oyster. Two tubes of each, protected by lamp-glasses, were charged on the 27th of December. They remain unchanged. Two other pairs of tubes, prepared in the same way and exposed to the laboratory air, are turbid and covered with *Penicillium*.

### 13. *Infusions of Fowl and Kidney.*

Three tubes of the fowl-infusion were introduced into a case, and boiled there for five minutes, on the 4th of January. Three similar tubes were at the same time exposed to the air. On the 6th all the outer tubes were cloudy, the cloudiness becoming denser on the following days, while disks of *Penicillium* began to form on the exposed surfaces. It was found exceedingly difficult to obtain a clear infusion of kidney. The liquid, after it had passed through a dozen filters, was still quite muddy. With considerable labour and care, and by the employment of 200 filters, the mechanically suspended matter was at length removed, and a hyaline infusion obtained. It was introduced into its case, to which three exposed tubes were attached, on the 4th of January. On the 7th the latter were perceptibly cloudy, on the 8th distinctly so, while specks of mould rested upon them all. The protected tubes, on the contrary, have for months maintained their transparency undimmed*.

The entire number of experiments made to illustrate the association of scattered light and Bacterial and fungoid life are not here recounted. Whiting, for example, may be added to the fish, and pork to the flesh examined, while many of the other substances have been tested oftener than I have thought it necessary to record. The method of boiling was also varied in a manner which may claim a passing reference here.

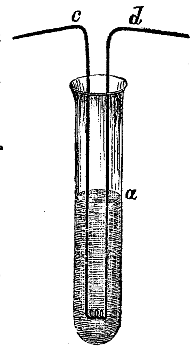
### § 14. *Boiling by an Internal Source of Heat.*

Two large test-tubes were fixed air-tight in the same case. On the 8th of November,

* Kidney has been mentioned by Dr. BASTIAN as a substance with which he demonstrates the occurrence of spontaneous generation. He does not mention the extraordinary turbidity of the infusion, which proved so troublesome to me.

after the floating matter had subsided, infusions of hay and turnip were introduced. Dipping into each test-tube were two tinned copper wires, connected below by a spiral of platinum wire. The arrangement is represented in fig. 4. The copper wires (*c d*) passed through the case, and were connected with a voltaic battery outside. The spiral was heated by the current. After a few minutes ebullition set in, and was continued for five minutes in each tube. Two other tubes containing the same infusions were boiled in the same way, and afterwards hung on outside the case containing the two protected tubes.

Fig. 4.




In a separate case were placed two tubes containing infusions of beef and mutton. The arrangement and the treatment were precisely the same as those just described in the case of hay and turnip.

Examined some months subsequently, the exposed tubes of all four infusions were found turbid and covered with *Penicillium*, while all the four protected tubes remained unchanged. During the boiling process some flocculi detached themselves from the tinned surfaces of the copper wires; but in the protected tubes these have fallen to the bottom, and left the supernatant liquid clear. Platinum wires would have been better than tinned copper ones.

### § 15. *Partial Discussion of the Results.*

Thus by experiments, reiterated in many cases, with urine, mutton, beef, pork, hay, turnip, tea, coffee, hops, haddock, sole, salmon, codfish, turbot, mullet, herring, eel oyster, whiting, liver, kidney, hare, rabbit, fowl, pheasant, grouse, has the induction been established that the power of developing Bacterial life by atmospheric air, and its power of scattering light, go hand in hand. We shall immediately examine more closely what this means.

In his published works, Dr. BASTIAN has frequently dwelt upon the necessity of employing strong infusions when investigating the phenomena of spontaneous generation. I would therefore recall to mind what has been stated on a previous page, that in most of the experiments here described the infusion at starting was strong, and that it was permitted to evaporate with extreme slowness until its concentration became three or four fold what it had been at starting. Every experiment was thus converted into an indefinite number of experiments on infusions of different strengths. Never, in my opinion, was the requirement as to concentration more completely fulfilled, and never was the reply of Nature to experiment more definite and satisfactory. The temperatures, moreover, to which the infusions have been subjected embrace those hitherto found effectual, extending indeed beyond them in both directions*. They reached from a lower limit of 50° to a higher limit of more than 100° Fahr. Still higher temperatures were applied in other experiments to be described subsequently. With regard to the number of the infusions, more than fifty moteless chambers, each with its system of tubes, have been tested. There is no shade of uncertainty in any of the results. In every instance we have, within the chamber, perfect limpidity and sweetness—without the chamber, putridity and its

* See Proc. of Roy. Soc. vol. xxi. p. 130, where a temperature of 70° is described as effectual.

characteristic smells. In no instance is the least countenance lent to the notion that an infusion deprived by heat of its inherent life, and placed in contact with air cleansed of its visibly suspended matter, has any power whatever to generate life anew.

If it should be asked how I have assured myself that the protected liquids do not contain *Bacteria*, I would, in the first place, reply that with the most careful microscopic search I have been unable to find them. But much more than this may be affirmed. The electric or the solar beam is a far more powerful and searching test in this matter than the microscope. In the foregoing pages I have more than once described the clearness of my protected infusions, after months of exposure, as equal to that of distilled water. So far is this from being an exaggeration, that it falls short of the truth; for I have never seen distilled water so free from suspended particles as the protected infusions prove themselves to be. When for months a transparent liquid thus defies the scrutiny of the searching beam, maintaining itself free from every speck which could scatter light as a *Bacterium* scatters it—when, moreover, an adjacent infusion, prepared in precisely the same way, but exposed to the ordinary air, becomes first hazy, then turbid, and ends by wholly shattering the concentrated beam into irregularly scattered light, I think we are entitled to conclude that *Bacteria* are as certainly absent from the one as they are present in the other. (See Note I. at the end.)

For the right interpretation of scientific evidence something more than mere sharpness of observation is requisite, very keen sight being perfectly compatible with very weak insight. I was therefore careful to have my infusions inspected by biologists, not only trained in the niceties of the microscope, but versed in all the processes of scientific reasoning. Their conclusion is that it would simply weaken the demonstrative force of the experiments to appeal to the microscope at all.

### § 16. *Suspended Particles in Air and Water; their relation to* Bacteria.

Examined by the concentrated solar rays, or by the condensed electric beam, the floating matter of the air is seen to consist:—first, of particles so coarse that their individual motions can be followed by the eye; secondly, of a finer matter which is not to be distinguished as motes, but which emits a uniform and changeless light. In this finer matter the coarser motes move as in a medium.

As regards the production of colour, the action of small particles has been examined by Brücke in a paper "On the Colours of Turbid Media"*. In relation to the question of polarization, Professor Stokes has made some remarks in his memoir "On the Change of the Refrangibility of Light"†. I may also be permitted to refer to my own papers "On New Chemical Reactions by Light" and "On the Blue Colour of the Sky," in the Proceedings of the Royal Society for 1868–69, and to a paper "On the Action of Rays of High Refrangibility on Gaseous Matter," in the Philosophical Transactions for 1870. M. Soret, Lord Rayleigh, and Mr. Bosanquet have also worked at this subject, which, as far as it now concerns us, a few words will render clear.

* Pogg. Ann. lxxxviii. p. 363.

† Philosophical Transactions, vol. 142, pp. 529–530

When the track of a parallel beam in dusty air is looked at horizontally through a NICOL'S prism, in a direction perpendicular to the beam, the longer diagonal of the prism being vertical, a portion of the light from the finer matter, being polarized, is extinguished. The coarser motes, on the other hand, which do not polarize the light, flash out with greater force, because of the increased darkness of the space around them.

The individual particles of the finest floating matter of the air lie probably far beyond the reach of the microscope. At all events it is experimentally demonstrable that there are particles which act similarly upon light, and which are entirely ultra-microscopic. A few days ago, for example, an inverted bell-jar was filled with distilled water, into which, while it was briskly beaten by a glass rod, was dropped a solution of mastic in alcohol. The proportion was less than that employed by BRÜCKE, being about 10 grains of the gum to 1000 grains of the alcohol. The jar was placed under a skylight, at the height of the eye above the floor. It was of a beautiful cerulean hue, this colour arising wholly from the light scattered by the mastic particles. Looked at horizontally through a NICOL'S prism, with its shorter diagonal vertical, the blue light passed freely to the eye. Turning the long diagonal vertical, the scattered light was wholly quenched, and the jar appeared as if filled with ordinary pure water.

I tried the effect of a powerful filter upon those particles, and found that they passed sensibly unimpeded through forty layers of the best filtering-paper*.

The liquid containing them was examined by a microscope magnifying 1200 diameters. The suspended mastic particles entirely eluded this power, the medium in which they swam being as uniform as distilled water in which no mastic whatever had been precipitated.

The optical deportment of the floating matter of the air proves it to be composed, in part, of particles of this excessively minute character. The concentrated beam reveals them collectively, long after the microscope has ceased to distinguish them individually. They are, moreover, organic particles, which may be removed from the air by combustion. In presence of such facts, any argument against atmospheric germs, based upon their being beyond the reach of the microscope, loses all validity.

We are here brought face to face with a question of extreme importance, which it will be useful to clear up. "Potential germs" and "hypothetical germs" have been spoken of with scorn, because the evidence of the microscope as to their existence was not forthcoming. Sagacious writers had drawn from their experiments the perfectly legitimate inference that in many cases the germs exist, though the microscope fails to reveal them. Such inferences, however, have been treated as the pure work of the imagination, resting, it was alleged, on no real basis of fact. But in the concentrated beam we possess what is virtually a new instrument, exceeding the microscope indefinitely in power. Directing it upon media which refuse to give the coarser instrument any information as to what they hold in suspension, these media declare themselves to

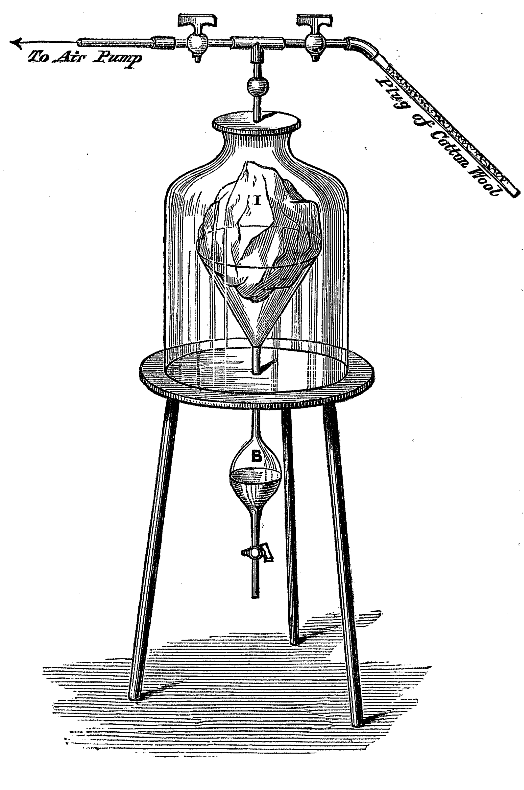
* There are filters, however, which stop them; but of this immediately.

be crowded with particles—not hypothetical, not potential, but actual and myriadfold in number—showing the microscopist that there is a world beyond even his range.

In §§ 6 and 8 experiments on the infection of clear infusions by others containing visible *Bacteria* are referred to. But for the infection to be sure it is not necessary that the *Bacteria* should be visible. Over and over again I have repeated the experiments of Dr. BURDON SANDERSON on the infective power of ordinary distilled water, in which the microscope fails to reveal a *Bacterium*. The water, for example, furnished to the Royal-Institution laboratory by Messrs. HOPKIN and WILLIAMS is sensibly as infectious as an infusion swarming with *Bacteria*.

Perhaps the severest experiment of this kind ever made was one executed by Dr. SANDERSON with water prepared by myself. In 1871 I sought anxiously and assiduously for water free from suspended particles. The liquid was obtained in various degrees of purity, but never entirely pure. Knowing the wonderful power of extrusion, as regards foreign matter, brought into play by water in crystallizing, the thought occurred to me of examining the liquid derived from the fusion of the most transparent ice. Mr. COTTRELL, at my request, arranged the following apparatus for me:—Through the plate of an air-pump (fig. 5) passed air-tight the shank of a large funnel. A small glass bulb, B, furnished with a glass stopcock, was attached to the shank of the funnel below. Prior to being put together all parts of the apparatus had been scrupulously cleansed. In the funnel was placed a block of ice, I, selected for its transparency, having a volume of 1000 cubic inches or thereabouts, and over the ice was placed an air-tight receiver. Several times in succession the air was removed from this receiver, its place on each occasion being taken by other air carefully filtered through cotton-wool. The transparent ice was thus surrounded by moteless air.

Fig. 5.




The ice was now permitted to melt; its water trickled into the small glass bulb below, which was filled and emptied a great number of times. From the very heart of the block of ice the water was finally taken and subjected to the scrutiny of the concentrated beam. It proved to be the purest liquid I had ever seen—probably the purest human eye had ever seen; but still it contained myriads of ultra-microscopic particles. The track of the beam through it was of the most delicate blue, the blue light being perfectly polarized. It could be wholly quenched by a NICOL'S prism, the beam then passing through the liquid as through a vacuum. A comparison of the light with that scattered by such mastic particles as those above referred to, proved the suspended particles of the ice-water to be far smaller than those of the mastic. No microscope, therefore, could come near them*. Such water, however, was proved by Dr. SANDERSON to be as infectious as the water from any ordinary tap.

Infinitesimal as these particles are, however, they may be separated by mechanical means from the liquid in which they are held in suspension. Filters of porous earthenware, such as the porous cells of BUNSEN'S battery, have been turned to important account in the researches of Dr. ZAHN, Professor KLEBS, and Dr. BURDON SANDERSON. In various instances it has been proved that, as regards the infection of living animals, the porous earthenware intercepts the contagium. For the living animal, organic infusions or PASTEUR'S solution may be substituted. Not only are ice-water, distilled water, and tap-water thus deprived of their powers of infection, but, by plunging the porous cell into an infusion swarming with Bacterial life, exhausting the cell, and permitting the liquid to be slowly driven through it by atmospheric pressure, the filtrate is not only deprived of its *Bacteria*, but also of those ultra-microscopic particles which appear to be as potent for infection as the *Bacteria* themselves. The precipitated mastic particles before described, which pass unimpeded through an indefinite number of paper filters, are wholly intercepted by the porous cell.

These germinal particles abound in every pool, stream, and river. All parts of the moist earth are crowded with them. Every wetted surface which has been dried by the sun or air contains upon it the particles which the unevaporated liquid held in suspension. From such surfaces they are detached and wafted away, their universal prevalence in the atmosphere being thus accounted for. Doubtless they sometimes attach themselves to the coarser particles, organic and inorganic, which are left behind along with them; but they need no such rafts to carry them through the air, being themselves endowed with a power of flotation commensurate with their extreme smallness and the specific lightness of the matter of which they are composed.

I by no means affirm that the developed *Bacterium*, which requires for its maintenance nutriment beyond that which ordinary water can always supply, is never wafted through the air. Cases doubtless will arise favourable for the growth and dispersion of the full-grown organism. Whether, after desiccation, it retains the power of reproduction is another question. But it ought, I think, to be steadily borne in mind that the *Bacteria* and the atmospheric matter from which they are developed are, in general, different things. I have carefully sought for atmospheric *Bacteria*, but have never found them. They have never, to my knowledge, been found by others; and that they arise from matter which has not yet assumed the Bacterial form is, as just shown, capable of demonstration. An organic infusion, boiled and shielded from atmospheric particles, will remain clear for an indefinite period, while a fragment of glass which

* I have endeavoured to convey some notion of the smallness of these scattering particles in 'Fragments of Science,' 1876, pp. 441–443. See note on Mr. DALLINGER'S observations at the end of this Memoir.

has been exposed to the air, but on which no trace of a *Bacterium* is to be found, will in two or three days develop in it a multitudinous crop of life.

We have now to look a little more closely at these particles, foreign to the atmosphere but floating in it, and proved beyond doubt to be the origin of all the Bacterial life which our experiments have thus far revealed. We must also look at them as they exist in water, in countless multitudes, being as foreign to this medium as the floating atmospheric dust is to the air in which it swims. The existence of the particles is quite as certain as if they could be felt between the fingers, or seen by the naked eye. Supposing them to augment in magnitude until they come, not only within range of the microscope, but within range of the unaided senses. Let it be assumed that our knowledge of them under these circumstances remains as defective as it is now—that we do not know whether they are germs, particles of dead organic dust, or particles of mineral matter. Suppose a vessel (say a flower-pot) to be at hand filled with nutritious earth, with which we mix our unknown particles, and that in forty-eight hours subsequently buds and blades of well-defined cresses and grasses appear above the soil. Suppose the experiment when repeated a hundred times to yield the same unvarying result. What would be our conclusion? Should we regard those living plants as the product of dead dust, of mineral particles? or should we regard them as the offspring of living seeds? The reply is unavoidable. We should undoubtedly consider the experiment with the flower-pot as clearing up our preexisting ignorance; we should regard the fact of their producing cresses and grasses as proof positive that the particles sown in the earth of the pot were the seeds of the plants which have grown from them. It would be simply monstrous to conclude that they had been "spontaneously generated."

This reasoning applies word for word to the development of *Bacteria* from that floating matter which the electric beam reveals in the air, and in the absence of which no Bacterial life has been generated. I cannot see a flaw in the reasoning; and it is so simple as to render it unlikely that the notion of Bacterial life developed from dead dust can ever gain currency among the members of the medical profession.

It has been said of those whom the evidence adduced in favour of spontaneous generation fails to convince, that they seem willing to believe in almost any infringement of natural uniformity rather than admit the doctrine*. This surely is an inversion of the true order of the facts. Natural uniformity is the record of experience; and, apart from the phenomena to be accounted for, there is not a vestige of experience, possessed either by the man of science or the human race, which warrants the notion that dead dust, and not living seed, is the source of the crops which spring from our infusions after their impregnation by the floating particles of the atmosphere.

* Transactions of the Pathological Society, vol. xxvi. p. 273.

### § 17. *Dr.* BASTIAN'S *Experiments.*

The uniform sterility of the boiled infusions described in the foregoing pages, when protected from the floating matter of the air, proves that they do not contain germs capable of generating life. Dr. BASTIAN, indeed, affirms that a temperature of 140° FAHR. reduces, in all cases, such germs to a state of actual or potential death. But even in flasks which have been raised to a temperature of 212°, and hermetically sealed, putrefaction, and its associated Bacterial life, do, he alleges, most certainly arise; from which he infers that *Bacteria* are spontaneously generated. "We know," he says, "that boiled turnip- or hay-infusions, exposed to ordinary air, exposed to filtered air, to calcined air, or shut off altogether from contact with air, are more or less prone to swarm with *Bacteria* and *Vibriones* in the course of from two to six days" *.

We are here met by a difficulty at the outset. Dr. BASTIAN'S proof of Bacterial death at 140° FAHR. consists solely in the observed fact, that when a certain liquid is heated to that temperature no life appears in it afterwards. In another liquid, however, he finds that life appears two days after it has been heated to 212°. Instead of concluding logically that in the one liquid life is destroyed and in the other not, he chooses to assume arbitrarily that 140° FAHR. is the death-temperature for both; and this being so, the life observed in the second liquid figures, in his inference, as a case of spontaneous generation. A great deal of Dr. BASTIAN'S most cogent reasoning rests upon this extraordinary foundation. Assumptions of this kind guide him in his most serious experiments. He finds, for example, that a mineral solution does not develop *Bacteria* when exposed to the air; and he concludes from this that an organic infusion also may be thus exposed without danger of infection. He exposes turnip-juice accordingly, obtains a crop of *Bacteria*, which, in the light of his assumption, are spontaneously generated. Such are the warp and woof of some of the weightiest arguments on this question which have been addressed by him to the Royal Society†.

Granting, then, all that Dr. BASTIAN alleges regarding his experiments to be correct, the logical inference would be very different from his inference. But are his statements correct? This is the really important point; and to its examination I now address myself.

### § 18. *Experiments with Filtered Air.*

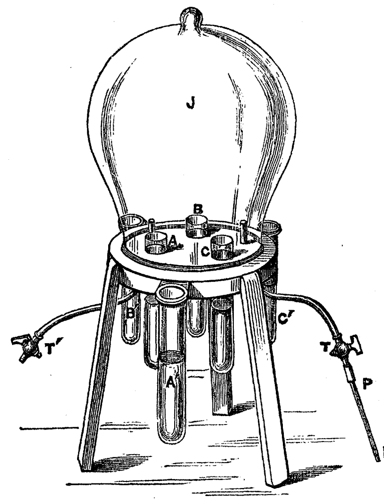
A bell-jar containing about 700 cubic inches of air was firmly cemented to a slab of wood supported on three legs‡. Through the slab passed, air-tight, three large test-

* Evolution and the Origin of Life, p. 94.

† Proceedings, vol. xxi. p. 130.

‡ Two hoops of sheet iron, with an annular space about an inch wide, were fastened on to the slab of wood. The annular space was filled with hot cement, into which the hot bell-jar was pressed. The circular space within the smaller hoop was also covered by a layer of cement.

Fig. 6.




tubes (A, B, C, fig. 6). Prior to cementing, the tubes had been three fourths filled, one with an infusion of hay, another with an infusion of turnip, and a third with an infusion of mutton. On the 2nd of November the mote-laden air was pumped out, air slowly filtered through a long tight plug of cotton-wool being allowed to take its place. The jar was emptied and refilled until the closest scrutiny by a concentrated beam revealed no floating matter within it. The infusions were then boiled for five minutes, and abandoned to the air of the jar. During ebullition a small quantity of the liquid in one of the tubes boiled over, and rested upon the interior resinous surface at a little distance from the mouths of two of the tubes. The germinal matter, it may be remarked, is not readily blown away from such a surface, and it certainly was not removed by our feeble current of filtered air. Three similar tubes containing the same infusion were placed at the same time beside the protected ones.

In three days these exposed tubes became turbid and charged with life; *but for three weeks the infusions in contact with the filtered air remained perfectly clear.*

At the end of three weeks, that is on the 23rd of November, I desired my assistant to renew the air in the bell-jar. He pumped it out, and while permitting fresh air to enter through the cotton-wool filter, my attention was directed to a couple of small round patches of *Penicillium* resting on the liquid that had boiled over. I at once made the remark that the experiment was a dangerous one, as the entering air would probably detach some of the spores of the *Penicillium* and diffuse them in the bell-jar. This was, therefore, filled very slowly, so as to render the disturbance a minimum. Next day, however, a tuft of mycelium was observed at the bottom of one of the three tubes, namely that containing the hay-infusion. It has by this time grown so as to fill a large portion of the tube. For nearly a month longer the two tubes containing the turnip- and mutton-infusions maintained their transparency unimpaired. Late in December the mutton-infusion, which was in dangerous proximity to the outer mould, showed a tuft of *Penicillium* upon its surface. The beef-infusion continued bright and clear for nearly a fortnight longer. The cold winter weather caused me to add a third gas-stove to the two which had previously warmed the room where the experiments are conducted. The warmth of this stove played upon one side of the bell-jar; and on the day after the lighting of the stove, the beef-infusion gave birth to a tuft of mycelium. In this case the small spots of *Penicillium* might have readily escaped attention; and had they done so we should have had here three cases of "spontaneous generation" far more striking than many that have been adduced.

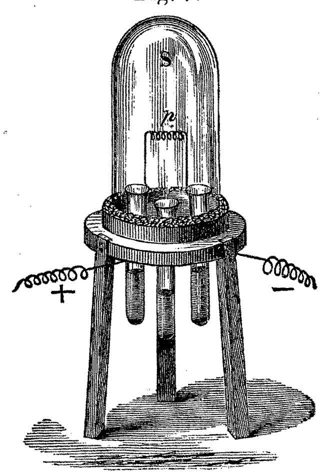
The experiment was subsequently made upon a larger scale. Twelve very large test-tubes were caused to pass air-tight through a slab of wood; the wood was thickly coated with cement, in which, while it was hot and soft, a heated "propagating-glass," resembling a huge bell-jar, was imbedded. The air within the glass was pumped out several times, air filtered carefully through a plug of cotton-wool being permitted to supply its place. The test-tubes contained infusions of hay, turnip, beef, and mutton, three of each, twelve in all. For two months they remained as clear and cloudless as they were upon the day of their introduction, while twelve similar tubes, prepared at the same time, in precisely the same way, and hung on to the slab of wood outside the propagating-glass, were, in less than a week, clogged with mycelium, mould, and *Bacteria*.

One of the protected tubes was accidentally broken, and though its aperture was rapidly plugged with cotton-wool, some common air must, at the time, have entered the propagating-glass. Evaporation from the infusions went on; the vapour was condensed by the glass above, trickled down its interior surface, carrying with it, in part, such matter as had attached itself to that surface. A kind of pool was thus formed upon the cement below. This, after an interval of three months, is now spotted with disks of *Penicillium*, by the spores of which one or two of the infusions have been recently invaded, the production of very beautiful mycelium-tufts being the consequence.

### § 19. *Experiments with Calcined Air.*

Six years ago* I showed that the floating matter of London air could be completely removed by permitting a platinum wire heated to whiteness to act upon it for a sufficient time. I availed myself of this mode of calcining the air on the present occasion. The apparatus employed is shown in fig. 7. A glass shade, S, is placed upon a slab of wood mounted on a tripod, and through which passes three large test-tubes nearly filled with the infusion to be examined. A platinum spiral, $p$, unites the ends of two upright copper wires, which pass through the stand and are seen coiled outside it. The shade is surrounded by a tin collar, with a space of about half an inch all round between it and the shade. This space is filled with cotton-wool firmly packed. Connecting the wires with a battery of fifteen cells, the spiral $p$ was raised to whiteness, and was permitted to continue so for five minutes. Experiments previously executed had shown that this sufficed for the entire removal of the floating matter. When the spiral was heated, a portion of the expanded air was driven through the cotton-wool packing below; and when the current was interrupted, this air, returning into the shade, was prevented by the cotton-wool from carrying any floating matter with it.

Fig. 7.




The first three substances brought into contact with air calcined in this way were damson-juice, pear-juice, and infusion of yeast. They were boiled for five minutes, and

* Proc. Roy. Inst. vol. vi. pp. 4 & 5.

*for five months they have remained without speck or turbidity.* Other tubes similarly boiled, and placed underneath shades containing the floating matter of the air, have long since fallen into mould and rottenness.

Turnip- and hay-infusions rendered slightly alkaline have been mentioned as particularly prone to spontaneous generation. I wished to test this. On the 26th of November, therefore, four shades were prepared, two containing strong turnip-infusion and hay-infusion unneutralized, two containing infusions which had been rendered slightly superneutralized by caustic potash. The alleged spontaneous development of life was not observed. The tubes exhibit to this hour the clearness and colour which they showed on the day they were boiled. Hermetically sealed tubes, containing the same infusions, prepared on the same day, remain equally clear; while the specimens exposed to the laboratory air have fallen into rottenness.

The experiments with calcined air were also executed in another form and on a larger scale. A "propagating-glass," similar to that already described, was cemented in the same way to a slab of wood through which passed twelve large test-tubes. The infusions, as before, were hay, turnip, beef, and mutton. The air being removed from the propagating-glass by a good air-pump, its place was supplied by other air which had passed slowly through a red-hot platinum tube containing a roll of platinum gauze, also heated to redness. Tested by a searching beam, this calcined air was found quite free from floating matter. For two months no speck invaded the limpidity of the infusions exposed to it, while a week's exposure to the ordinary air sufficed to reduce twelve similar infusions, hung on to the slab of wood outside the glass, to rottenness.

## § 20. *Infusions withdrawn from Air.*

The arrangement was the same as that in the first experiment with filtered air, the only difference being that the bell-jar, with a view to its more perfect exhaustion, was smaller. It was cemented air-tight to a slab of wood through which passed three large test-tubes, filled to about two thirds of their capacity with infusions of beef, mutton, and turnip respectively. The air was pumped out six times in succession, and filled after each exhaustion with air carefully filtered through cotton-wool. While this air was in contact with the infusions they were boiled in a brine-bath. The receiver was afterwards exhausted as perfectly as a good air-pump could exhaust it; while outside the receiver were hung three tubes to compare with those within.

Here the protected infusions remained as clear as they were on the day of their introduction, not only after the exposed infusions had charged themselves with life, but for many weeks after they had evaporated away.

Such, then, are the tests to which I have subjected the statement that "boiled turnip- and hay-infusions exposed to filtered air, to calcined air, or shut off altogether from contact with air, are more or less prone to swarm with *Bacteria* and *Vibriones* in the course of from two to six days." In no single instance has the statement borne the stress of accurate experiment. These results, and others that might be adduced, leave no doubt upon my mind that the deportment of air from which the floating matter has been removed by filtration or calcination is precisely the same as that of air from which the particles have disappeared by self-subsidence.

### § 21. *The Germ-theory of Contagious Disease.*

It is in connexion with the so-called germ-theory of contagious disease that the doctrine of spontaneous generation assumes its gravest aspect. My interest in the general question was first excited by the imperishable investigations of PASTEUR, while the medical bearings of the doctrine were subsequently made clear to me, mainly, I ought to say, by the writings and conversation of Dr. WILLIAM BUDD, who was the first of our countrymen to grasp definitely the doctrine of "the vitality of contagia," which is now every day gaining ground.

At the present moment, indeed, no other medical principle occupies so much thought, or is the subject of so much discussion. "How does it happen," says Dr. BURDON SANDERSON*, "that these *Bacteria*, which we suppose must have existed half a dozen years ago in as great numbers as at present, were then scarcely heard of, and that they now occupy so large a place in the medical literature of this country and of Germany, and have lately afforded material for lively discussion in the French Academy?" Dr. SANDERSON points out the relation of LISTER in England, and of HALLIER in Germany, to the movement regarding *Bacteria* which is now working like a ferment through the medical world. But to no other workers in this field are we more indebted than to Dr. SANDERSON himself, and to his colleagues, for the continued and successful prosecution of researches bearing upon the pathology of contagion.

"In 1870," writes Mr. JOHN SIMON, in one of his excellent reports to the Privy Council, "I had the honour of presenting Dr. SANDERSON's first report of researches made in this matter. At that time general conclusions seemed justified, first, that the characteristic-shaped elements which the microscope had shown abounding in various infective products are self-multiplying organic forms, not congeneric with the animal body in which they are found, but apparently of the lowest vegetable kind; and secondly, that such living organisms are probably the essence, or an inseparable part of the essence, of all contagia of disease. . . . . This view of the matter has since then become greatly more distinct, in consequence of the investigations made by Dr. SANDERSON, particularly in 1871 and 1872, with reference to the common septic contagium or ferment. For in that ferment there seems now to be identified a force which, acting disintegratively upon organic matter, whether dead or living, can, on the one hand, initiate putrefaction of what is dead, and, on the other hand, initiate febrile and inflammatory processes in what is living."

* British Medical Journal, January 16, 1875.

The latest investigation of Dr. KLEIN has reference to the intimate anatomy of enteric fever. Its distinctive feature is set forth in the following extract from the report upon it by Mr. SIMON:—"The paper has its distinctive and very great interest in the fact that it purports to describe for the first time the contagium of enteric fever as something cognizable to the eye; in respect of certain multiplying microscopical forms, apparently of the lowest vegetable life, which are found in innumerable swarms in the bowel-textures and bowel-discharges of the sick; penetrating from the former to diffuse throughout the patient's general system*, and teeming in the latter to represent, as this view supposes, the possible germs of epidemic infection."

As regards the medical profession, results like the foregoing and the interpretations affixed to them, are simply revolutionary. They are, therefore, not likely to be accepted without opposition. At a Meeting of the Pathological Society, held on the 6th of last April, the germ-theory of disease was formally introduced as a subject for discussion, the debate being continued with great ability and earnestness at subsequent meetings. The Conference was attended by many distinguished medical men, some of whom were profoundly influenced by the arguments, and none of whom disputed the facts brought forward against the theory on that occasion. The leader of the debate, and the most prominent speaker, was Dr. BASTIAN, to whom also fell the task of replying on all the questions raised. The coexistence of *Bacteria* and contagious disease was admitted; but instead of considering these organisms as "probably the essence, or an inseparable part of the essence" of the contagium, Dr. BASTIAN contended that they were "pathological products," spontaneously generated in the body after it had been rendered diseased by the real contagium. The grouping of the ultimate particles of matter to form living organisms, Dr. BASTIAN considers to be an operation as little requiring the action of antecedent life as their grouping to form any of the "other less complex chemical compounds." Such a position must, of course, stand or fall by the evidence which its supporter is able to produce; and accordingly Dr. BASTIAN appeals to the law and testimony of experiment as demonstrating the soundness of his view. He seems quite aware of the gravity of the matter in hand: this is his deliberate and almost solemn appeal:—"With the view of settling these questions, therefore, we may carefully prepare an infusion from some animal tissue, be it muscle, kidney, or liver; we may place it in a flask whose neck is drawn out and narrowed in the blowpipe-flame, we may boil the fluid, seal the vessel during ebullition, and keeping it in a warm place, may await the result, as I have often done. After a variable time the previously heated fluid within the hermetically sealed flask swarms more or less plentifully with *Bacteria* and allied organisms—even though

* In the silkworm epidemic called *pébrine*, which was extirpated by PASTEUR, the parasitic contagium first took possession of the intestinal canal, and spread thence "throughout the patient's general system." He would be a hardy man who would deny the identity of parasite and contagium in this case, which appears to be precisely analogous to that of typhoid fever. See 'Fragments of Science,' 1876, pp. 135–139.

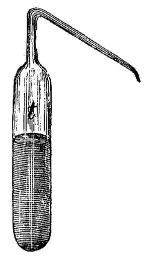
the fluids have been so much degraded in quality by exposure to the temperature of 212° FAHR., and have thereby, in all probability, been rendered far less prone to engender independent living units than the unheated fluids in the tissues would be" *.

We have here, to use the words of Dr. BASTIAN, "a question lying at the root of the pathology of the most important and most fatal class of diseases to which the human race is liable." Let us now examine his settlement of the question, as described in the foregoing extract by himself.

### § 22. *Experiments with Hermetically sealed Vessels.*

Experiments with hermetically sealed tubes were begun on the 5th of October. The shape of the tubes after sealing is represented in fig. 8. Each of them contained about an ounce of liquid. They were boiled for only three minutes in an oil-bath, and were sealed, during ebullition, not by a blowpipe, but by the far more effectual spirit-lamp flame.

Fig. 8.



*Hay.*—Four tubes were charged on the date mentioned with a strong infusion, four with a weak infusion. All eight flasks remain to the present hour clear.

*Turnip.*—Two kinds of turnip were tried in these first experiments. Two tubes were charged with a strong infusion, and two with a weak infusion of a sound hard turnip; while two other pairs of tubes were filled with strong and weak infusions from a soft woolly turnip. All the tubes remain transparent to the present time. Two or three days' exposure to the air of the laboratory sufficed to cloud all these infusions and fill them with life.

On the 8th of October twenty-one tubes were charged with infusions of the following substances:—Mackerel, beef, eel, oyster, oatmeal, malt, potato. There were three tubes of each infusion. All of them remain to the present hour unchanged.

I had not previously seen a more beautiful illustration of the dichroitic action which produces the colours of the sky than in the case of the oyster-infusion. With reflected light it presented a beautiful cerulean hue, while it was yellow by transmitted light. This was due to the action of suspended particles which defied the power alike of ordinary filtration and of the microscope. At right angles to a transmitted beam the infusion copiously discharged perfectly polarized light. Suspended particles in the potato-infusion produced a somewhat similar effect, but it was by no means so fine as that of the oyster-infusion. By ordinary filtration it was not possible completely to rid the malt and oatmeal of suspended matter; but both remain exactly as they were when the flasks containing them were sealed.

These experiments had been made before the volume of the Transactions of the Pathological Society containing the discussion referred to above came into my hands †.

* Transactions of the Pathological Society of London, 1875, p. 272.

† To the courtesy of Dr. BASTIAN I am indebted for a separate copy of the report of the discussion here referred to.

It caused me to turn again to my tubes, seeking further evidence. On the 12th of November thirty-six of them were charged, boiled, and hermetically sealed; on the 13th fifty-seven, on the 16th thirty-one, and on the 17th six tubes were similarly treated. The entire group of tubes, therefore, numbered one hundred and thirty. I tried moreover to multiply the chances of spontaneous generation by making the infusions of the most diverse materials. The following Table gives the names of the substances operated on, the number of tubes sealed, and the date of sealing:—

| | | |
|---|---|---|
| Fowl | 6 tubes. | November 12th. |
| Mutton | 6 „ | „ |
| Wild Duck | 6 „ | „ |
| Beef | 6 „ | „ |
| Herring | 6 „ | „ |
| Haddock | 6 „ | „ |
| Mullet | 6 „ | November 13th. |
| Codfish | 6 „ | „ |
| Pheasant | 6 „ | „ |
| Heart | 6 „ | „ |
| Rabbit | 6 „ | „ |
| Hare | 6 „ | „ |
| Snipe | 6 „ | „ |
| Partridge | 6 „ | „ |
| Plover | 5 „ | „ |
| Liver | 4 „ | „ |
| Tongue of Sheep | 6 „ | November 16th. |
| Brains of Sheep | 3 „ | „ |
| Sweetbread | 6 „ | „ |
| Humour of Ox-eye (undiluted) | 2 „ | „ |
| Lens of Ox-eye | 3 „ | „ |
| Lungs of Sheep | 5 „ | „ |
| Tripe | 6 „ | „ |
| Sole | 6 „ | November 17th. |

The tubes were immersed in groups of six at a time in an oil-bath, boiled for three minutes, and then sealed.

More than one hundred of these flasks were sensibly transparent and free from turbidity at the outset, and they remain so to the present hour. In some cases, however, it was not possible to wholly remove turbidity by filtration. I have already referred to the opalescence of oyster-infusion, which has invariably appeared whenever oyster has been digested. A still more pronounced case of the kind is furnished by an infusion of the crystalline lens of the ox. Nothing hitherto encountered imitates the flush of the true opal so closely as this infusion. Filtration through 100 layers of paper was quite incompetent to remove the suspended particles to which this opalescence is due. Some of the other infusions remained turbid under filtration, without exhibiting what I should call opalescence. The sheep's lungs furnish an example of this. In some cases, moreover, where repeated filtering failed to remove the suspended particles, a few weeks' quiet caused them to sink, and leave the supernatant liquid clear. It may be worth remarking that some rabbit-infusions have shown a decided opalescence, while others have been perfectly clear. The same remark applies to turnip-infusions, some of which have been found as clear as distilled water, while in general a slight opalescence is not to be got rid of by filtering.

These later experiments are quite in harmony with the earlier ones. Not one of this cloud of witnesses testifies in favour of Dr. BASTIAN. Not a single flask of the multitude manifests the deportment alleged by him to be a matter of common observation. If the power of spontaneous generation be a scientific verity, surely amid opportunities so multiplied and various it must have asserted itself. That the infusions employed were not "degraded" by the boiling so as to be incapable of supporting life, was proved by the fact that exposed tubes containing the same infusions, treated in precisely the same way, resolved themselves with the usual speed into Bacterial swarms. The conclusion to which these results point is, that here, as elsewhere, Dr. BASTIAN has allowed the gravest errors to invade his experimental work.

### § 23. *Conditions as to the Temperature and Strength of Infusions.*

In connexion with these experiments, I have sought, to the best of my ability, to meet every condition and requirement laid down by others as essential to success. With regard to warmth, a temperature of 90° was generally attainable in our laboratory, while on certain days of mild weather without, and in favourable positions within, the temperature to which the infusions were subjected reached over 100° FAHR. As Dr. BASTIAN, however, has recently laid considerable stress on warmth, though most of his results were obtained with temperatures from 15° to 30° lower than mine*, I thought it desirable to meet this new requirement also. The sealed tubes, which had proved barren in the Royal Institution, were suspended in boxes copiously perforated, so as to permit of the free circulation of warm air, and placed under the supervision of an intelligent assistant in the Turkish Bath in Jermyn Street. The washing-room of the establishment was found to be particularly suitable for our purpose; and here, accordingly, the boxes were suspended. From two to six days are allowed by Dr. BASTIAN for the generation of organisms in hermetically sealed tubes. Mine remained in the washing-room for nine days. Thermometers placed in the boxes, and read off twice or three times a day, showed the temperature to vary from a minimum of 101° to a maximum of 112° FAHR. At the end of nine days the infusions were as clear as at the beginning.

* Proc. Roy. Soc. vol. xxi. p. 130. Also 'Beginnings of Life,' vol. i. p. 354.

They were then removed to another position where the temperature was a few degrees higher. Dr. BASTIAN mentions 115° as favourable to spontaneous generation. For fourteen days the temperature hovered about this point, falling once as low as 106°, reaching 116° on three occasions, 118° on one, and 119° on two. The result was quite the same as that recorded a moment ago. The higher temperatures proved perfectly incompetent to develop life*.

Fifty-six observations, including both the maximum and minimum thermometers, were taken while the tubes occupied their first position in the washing-room, and seventy-four while they occupied the second position. The whole record, carefully drawn out, is before me, but I trust the statement of the major and minor limits of temperature will suffice.

Dr. BASTIAN'S demand for these high temperatures is, as already remarked, quite recent. Prior to my communication to the Royal Society on January 13, he had successfully worked with temperatures lower than those within my reach in Albemarle Street. There I followed his directions, adhered strictly to his prescriptions; but, taking care to boil and seal the liquids aright, his results refused to appear in my experiments. On learning this he raised an objection as to temperature, and made a new demand. With this I have complied; but his position is unimproved.

With regard to the question of concentration, I have already referred, in sections 3 and 16 of this memoir, to the great diversity in this particular presented by all my infusions, through their slow evaporation. But more than a general conformity to prescribed conditions was observed here also. The strength of an infusion is regarded as fixed by its specific gravity; and I have worked with infusions of precisely the same specific gravity as those employed by Dr. BASTIAN. This I was specially careful to do in relation to the experiments described and vouched for, I fear incautiously, by Dr. BURDON SANDERSON in vol. vii. p. 180 of 'Nature.' It will there be seen that though failure attended some of his efforts, Dr. BASTIAN did satisfy Dr. SANDERSON that in boiled and hermetically sealed flasks *Bacteria* sometimes appear in swarms. With purely liquid infusions I have vainly sought to reproduce the evidence which convinced Dr. SANDERSON. Hay- and turnip-infusions, of accurately the same character and strength as those employed on the occasion referred to, were prepared, boiled in an oil-bath, carefully sealed up, and subjected to the proper temperatures. In multiplied experiments they remained uniformly sterile. I am therefore compelled to conclude that Dr. SANDERSON has lent the authority of his name to results whose antecedents he had not sufficiently examined, and that the life to which he testifies, in the case of the purely liquid infusions, arose from errors of manipulation.

* My thanks are due to the managers of the bath for their obliging kindness in this matter.

### § 24. *Developmental Power of Infusions and Solutions: Air-germs contrasted with Water-germs.*

Wishing to make no experiment, whether with self-cleansed, filtered, or calcined air, or with infusions withdrawn from air by the air-pump, or contained in hermetically sealed vessels, without exposing the same infusions to ordinary air, this comparison was instituted on the present occasion. One hundred test-tubes, an inch wide and 3 inches deep, were divided into groups, each being filled with the same infusion. The groups were sufficiently numerous to embrace all the substances mentioned in the last Table. Exposed to the uncleansed air, they were attacked with different degrees of rapidity and vigour; but in a few days all of them without exception became muddy and crowded with life. On the whole, the hare- and pheasant-infusions presented the greatest contrast. The tubes containing the former were far gone before those containing the latter were sensibly invaded. The putrescibility of the pheasant, moreover, was exceeded by that of the snipe, partridge, and plover. The sheep's heart examined was also slow to putrefy. A single illustration of this difference of developmental power may be given here.

On the 13th of November thirty tubes, containing infusions of partridge, pheasant, snipe, hare, sheep's heart, and codfish, five tubes being devoted to each, together with four tubes of plover, three of mullet, and three of liver, were exposed to the laboratory air. On the 15th, 16th, and 22nd the numbers taken possession of by *Bacteria* were as follows:—

| | 15th. | 16th. | 22nd. |
|---|---|---|---|
| Partridge | 0 | 3 | all |
| Pheasant | 0 | 1 | „ |
| Snipe | 2 | 3 | „ |
| Hare | 2 | 4 | „ |
| Heart | 0 | 1 | „ |
| Codfish | 2 | 4 | „ |
| Plover | 1 | 2 | „ |
| Mullet | 1 | 2 | „ |
| Liver | 1 | 3 | „ |

They had probably all given way some days before the 22nd, but I had not taken the precaution to look at them.

Thus, then, the first two days produced no visible change in the pheasant-infusion, while in two of the hare-tubes putrefaction had vigorously set in. Three days' exposure caused only one of the pheasant-tubes to yield; four of the hare-infusion had yielded in the same time. The difference between them was also illustrated by the mould upon their surfaces. Some days after their exposure four of the five pheasant-tubes were thickly covered with *Penicillium*, while the five hare-tubes, with one exception, which could hardly be considered such, had repelled the enemy, maintaining their *Bacteria* undisturbed.

Still the deportment of the hare-infusion may have been due, not to any specific difference between hare and pheasant, but to the circumstances preceding death. The researches of Dr. BROWN-SÉQUARD show that even the same animal tissue exhibits, under different circumstances, very different tendencies to putrefaction. In guinea-pigs subjected immediately after death to the action of the magneto-electric current, he found the rapidity of putrefaction to correspond with the violence of the tetanization. He also draws attention to the influence of muscular exercise on cadaveric rigidity and putrefaction, showing how quickly they appear in "overdriven cattle and in animals hunted to death." It is known, indeed, to sportsmen that a shot hare will remain soft and limp for a day, while a hunted one becomes rigid in an hour or two. In September 1851 two sheep which had been overdriven to reach a fair were killed by the section of the carotid arteries. "Putrefaction," says Dr. BROWN-SÉQUARD, "was manifest before the end of the day, or in less than eight hours after death"*. The deportment of the hare operated upon by me may therefore depend upon the circumstance of its being brought down by the greyhound instead of the gun. It will be interesting to inquire how far the peculiarity of the animal tissue is transferred to the infusion. This is a subject for further investigation†.

Such observations inculcate caution in drawing inferences from the deportment of any infusion as to the distribution of germs in the air. The germs may be demonstrably present while the infusion may not favour their development. As to the quantity of atmospheric germs, the hare and the pheasant might lead to different conclusions. A passing reference to an important practical inference may be fitly introduced here. In one of the earliest of the able series of researches with which he has enriched medical science, Dr. BURDON SANDERSON exposed to the air "PASTEUR'S solution," which is capable of vigorously developing and nourishing *Bacteria* when they are communicated to it by inoculation; he also permitted air to bubble through the liquid, and finding no development in either case he inferred the entire absence of *Bacteria* and their germs from the air, considering water to be their exclusive habitation. Other distinguished men have come to the same conclusion; while in his books and papers, and in the discussion before the Pathological Society already referred to, Dr. BASTIAN has forcibly dwelt upon the result as justifying the interpretation which he has affixed to his experiments. If, he rightly urges, the air be "entirely free" from matter which could produce *Bacteria*, then their appearance in boiled infusions exposed to the air must be due, not to any thing contained in the air, but to the inherent power of the infusions. Spontaneous generation is undoubtedly the logical outcome of the position that "the germinal manner from which

* Croonian Lecture, Proc. Roy. Soc. 1862, vol. xi. p. 210.

† Five and twenty flasks containing pheasant-infusion were compared during the month of December with five and twenty containing infusion of hare. Neither in the rapidity of Bacterial development, nor in the readiness to support the growth of *Penicillium*, did the considerable differences between hare and pheasant first observed repeat themselves.

*Bacteria* spring does not exist in ordinary air." The experiments, however, recorded in this memoir constitute an ocular demonstration of the respective parts played by the infusion and the air. A pinch of fungus-spores, taken between the fingers, sown in a suitable medium, and producing their appropriate crop, could not more clearly indicate the origin of that crop than experiments with the luminous beam indicate the origin of our harvests of *Bacteria*. Dr. SANDERSON is, I doubt not, now well aware that his first statement was founded on an error of interpretation. In a lecture delivered at Owens College, Manchester, and published in the 'British Medical Journal' for January 16, 1875, he to a great extent qualifies and corrects his first inference. He there says that the *Bacteria* "attach themselves without doubt to these minute particles, which, scarcely visible in ordinary light, appear as motes in the sunbeam, or in the beam of an electric lamp." In fact the experiments on which he based his first inference owed their barrenness, not to the absence of *Bacteria*-germs from the air, but to the inability or, rather, slowness of his mineral solution to develop them.

With regard to the part played by the visible motes, I may repeat here what has been previously stated, namely, that while the coarser particles could hardly exist in their midst without loading themselves to some extent with the minute germs of *Bacteria*, there is no reason to think the motes indispensable for the diffusion of the germs. Whether they are attached to each other or not, the dryness and the moisture of the air are shared equally by both. The germs, moreover, float in the air more readily than the larger particles; and they, I doubt not, when properly illuminated, shed forth a portion of that changeless light to which reference has been already made, and the perfect polarization of which declares the smallness of the masses which scatter it.

The prevalence of the germinal matter of *Bacteria* in water has been demonstrated by the experiments of Dr. BURDON SANDERSON. But the germs in water, it ought to be remembered, are in a very different condition, as regards readiness for development, from those in air. In water they are already wetted, and ready, under the proper conditions, to pass rapidly into the finished organism. In air they are more or less desiccated, and require a period of preparation more or less long to bring them up to the starting-point of the water-germs *. The rapidity of development in an infusion infected by either a speck of liquid containing *Bacteria* or a drop of distilled water is extraordinary. On the 4th of January I dipped a thread of glass almost as fine as a hair into a cloudy turnip-infusion, and introduced the tip only of the glass fibre into a large test-tube containing an infusion of red mullet: twelve hours subsequently the perfectly pellucid liquid was cloudy throughout. A second test-tube containing the same

* The process by which an atmospheric germ is wetted would be an interesting subject of investigation. A dry microscope covering-glass may be caused to float on water for a year. A sewing-needle may be similarly kept floating, though its specific gravity is nearly eight times that of water. Were it not for some specific relation between the matter of the germ and that of the liquid into which it falls, wetting would be simply impossible. Antecedent to all development there must be an interchange of matter between the germ and its environment; and this interchange must obviously depend upon the character of the encompassing liquid.

infusion was infected with a single drop of the distilled water furnished by Messrs. Hopkin and Williams; twelve hours also sufficed to cloud the infusion thus treated. Precisely the same experiments were made with herring with the same result. In the winter season several days' exposure to warmed air are needed to produce this effect. On the 31st of December a strong turnip-infusion was prepared by digesting in distilled water at a temperature of 120° Fahr. It was divided between four large test-tubes, in one of which the infusion was left unboiled, in another boiled for five minutes, and in the two remaining ones boiled, and after cooling infected with one drop of beef-infusion containing *Bacteria*. In twenty-four hours the unboiled tube and the two infected ones were cloudy, the unboiled tube being the most turbid of the three. The infusion in the unboiled tube was peculiarly limpid after digestion; for turnip it was quite exceptional, and no amount of searching with the microscope could reveal in it at first the trace of a living *Bacterium*; still germs were there which, suitably nourished, passed in a single day into Bacterial swarms without number. Five days failed to produce an effect approximately equal to this in the uninfected boiled tube, which was exposed to the common laboratory air.

There cannot, I think, be a doubt that the germs in the air differ widely among themselves as regards *preparedness* for development. Some are fresh, others old; some are dry, others moist. Infected by such germs the same infusion would require different lengths of time to develop Bacterial life. And this remark, I doubt not, applies to the different degrees of rapidity with which epidemic disease affects different people. In some the hatching-period, if I may call it such, is long, in some short, the differences depending upon the different degrees of preparedness of the contagium *.

## § 25. *Diffusion of Germs in the Air.*

During the earlier observations recorded in this paper, and others not here mentioned, about 100 exposed tubes or flasks had been distributed irregularly in the rooms where the inquiry is conducted. They expanded to nearly 1000 in the end: not one of them escaped infection. A few days always sufficed to cloud the exposed infusions, and fill them with Bacterial life. I placed tubes at various points in the Royal Institution—on the roof of the house outside, in my bed-room, in an upper kitchen, in my study, in the upper and lower libraries, in the theatre, model-room, reading-room, manager's room, and in a kitchen at the bottom of the house below the level of Albemarle Street. All were smitten with putrefaction, and with its invariable associate, *Bacteria*. In the rooms without fires the action was slower than in the warmer rooms; but all the infusions gave way in the end.

Considering the assertions which had been made regarding the scantiness of *Bacteria*-

* The medical student of the future will probably connect these remarks with the following statement of Dr. Murchison:—"In that protean disease typhoid fever, I have repeatedly had occasion to observe a remarkable similarity in the course, and even in the complications, according to the source of the poison."—Trans. Path. Soc. vol. xxvi. p. 315.

germs in the air, observations outside of London would, I thought, be interesting. Accordingly, on the 27th of October, a tube containing an infusion of beef was placed in the hands of Mr. DARWIN, who had the kindness to set it in his study at Down and observe its changes. In three days it became cloudy and peopled with *Bacteria*. The same result was obtained in the open air. Mr. FRANCIS DARWIN was good enough to expose an infusion for me in his father's orchard: the weather was cold, and the progress, therefore, slow; but the tube which had been exposed on the 2nd of November was cloudy and full of *Bacteria* on the 9th. In Sir JOHN LUBBOCK's study a similar result was obtained. From Sherwood, near Tunbridge Wells, infusions of fowl and wild duck were returned to me by Mr. SIEMENS thickly turbid and crowded with *Bacteria*. From Pembroke Lodge, Richmond Park, Mr. ROLLO RUSSELL returned tubes of turnip, beef, and mutton swarming with life. An infusion of beef exposed at Heathfield Park, Sussex, for a week was returned to me by Miss HAMILTON muddy and filled with *Bacteria*. From Greenwich Hospital Mr. HIRST sent me tubes of beef-, mutton-, and turnip-infusion filled with vigorous *Bacteria*. Dr. HOOKER was good enough to take charge of three sets of tubes at Kew. One set was placed in the conservatory, with a temperature of 45° to 50°; one in his own study, with a temperature of 54° to 60°; a third set was placed in the orchid-house (the hottest in the gardens), with a temperature of 62° to 75°.

The tubes were opened on the 4th of December, all of them being then clear. In the orchid-house the turnip became cloudy on the 7th, the two others on the 8th, after which the opacity rapidly increased. In the study all remained clear until the 9th, when the turnip began to cloud. On the 11th the beef was still clear, while the mutton had given way. On the 13th all of them had yielded. In the conservatory the turnip began to cloud on the 10th; the others followed much in the same order as in the other cases.

The influence of temperature seems well shown by these observations. Three days sufficed to cloud the turnip in the orchid-house, five days in the study, and six days in the conservatory. The mutton in the study gathered over it a thick blanket of *Penicillium*. On the 13th it had assumed a light brown colour, "as if by a faint admixture of clay;" but the infusion became transparent. The "clay" here was the slime of dormant or dead *Bacteria*, the cause of their quiescence being the blanket of *Penicillium*. I found no active life in this tube, while all the others swarmed with *Bacteria*. From the Crystal Palace at Sydenham Mr. PRICE sent me tubes of mutton, beef, and turnip charged with *Bacteria*. The temperature was low at night, the development of life being thereby considerably retarded.

Thus everywhere it has been tested the atmosphere has been found charged with the germs of *Bacteria*.

I wished, however, to obtain clearer and more definite insight as to the diffusion of atmospheric germs. Supposing a large tray to be filled with a suitable organic infusion and exposed to the air. Into it the germs would drop; and could the resulting organisms be confined to the locality where the germs fell, we should have the floating life of the atmosphere mapped, so to speak, in the infusion. But in such a tray the organisms would intermingle and thus mar the revelation of their distribution. Valuable information I thought might be gained by breaking up the infusion into isolated conterminous patches, and exposing them to the air.

A square wooden tray was accordingly pierced with one hundred circular apertures; into each of which was dropped a test-tube 3 inches long and 1 inch wide, with its rim resting in each case upon the rim of the aperture. There were ten rows of tubes, with ten tubes in each row. On the 23rd of October, 1875, thirty of these tubes were filled with an infusion of hay, thirty-five with an infusion of turnip, and thirty-five with an infusion of beef. The tubes with their infusions had been previously boiled ten at a time in an oil-bath.

One hundred circles were marked upon paper so as to form a plan of the tray, and every day the state of each tube was registered upon the corresponding circle. Seven such maps or records were executed.

I will use the term "cloudy" to denote the early stage of turbidity, distinct but not strong. The term "muddy" will be used to denote thick turbidity.

### § 26. *Tray of one hundred tubes.*

On the 25th of October one or two of the tubes exposed on the 23rd showed signs of yielding; but the progress of putrefaction was first registered on the 26th. Map I., embracing the first record, is annexed (p. 64); it may be thus described.

*Hay.*—Of the thirty specimens exposed, one had become 'muddy'—the seventh in the middle row reckoning from the side of the tray nearest a stove. Six tubes remained perfectly clear between this muddy one and the stove, proving that differences of warmth may be overridden by other causes. Every one of the other tubes containing the hay-infusion showed spots of mould upon the clear liquid.

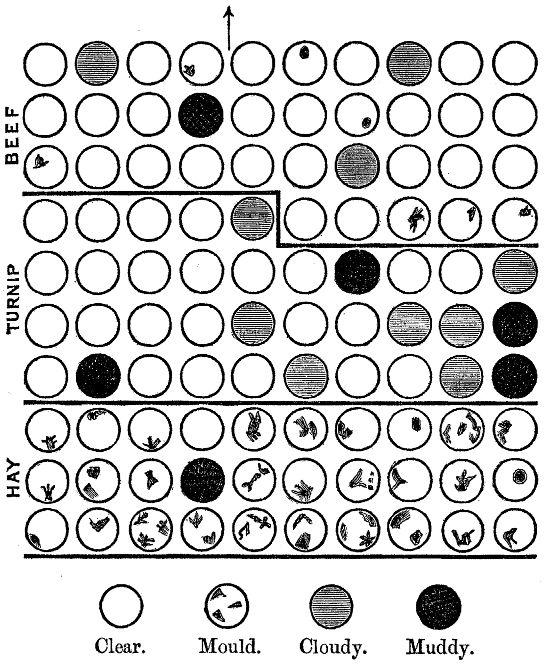
*Turnip.*—Four of the thirty-five tubes were very muddy, two of them being in the row next the stove, one four rows distant, and the remaining one nine rows away. Besides these, seven tubes had become clouded. There was no mould on any of the tubes.

*Beef.*—One tube of the thirty-five was quite muddy, in the seventh row from the stove. There were three cloudy tubes, while seven of them bore spots of mould.

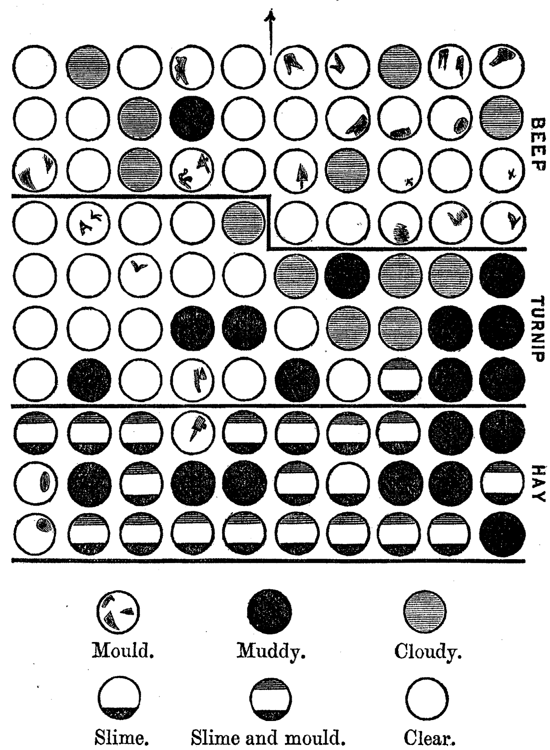
As a general rule organic infusions exposed to the air during the autumn remained for two days or more perfectly clear. Doubtless from the first germs fell into them, but they required time to be hatched. This period of clearness may be called the "period of latency;" and, indeed, it exactly corresponds with what is understood by this term in medicine. Towards the end of the period of latency the fall into a state of disease, if I may use the term, is comparatively sudden; the infusion passing from perfect clearness to cloudiness more or less dense in a few hours.

Thus the tube placed in Mr. DARWIN'S possession was clear at 8.33 A.M. on the 19th of October, and cloudy at 4.30 P.M. Seven hours, moreover, after the first record of our tray of tubes, a marked change had occurred. For the purpose of comparison the second record (Map II.) is placed beside the first. The change may be thus described:—Instead of one, eight of the tubes containing hay-infusion had fallen into uniform muddiness. Nineteen of these had produced Bacterial slime, which had fallen to the bottom,

Map I. 26th Oct., noon.



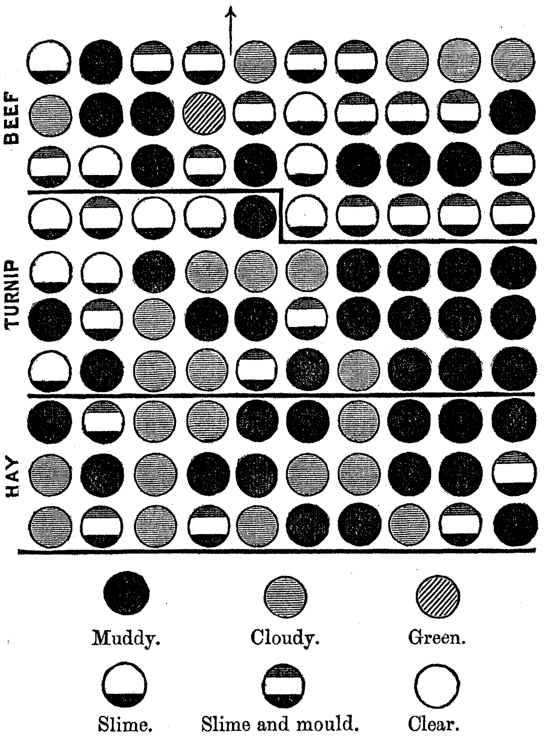

Map II. 26th Oct., 7 p.m.



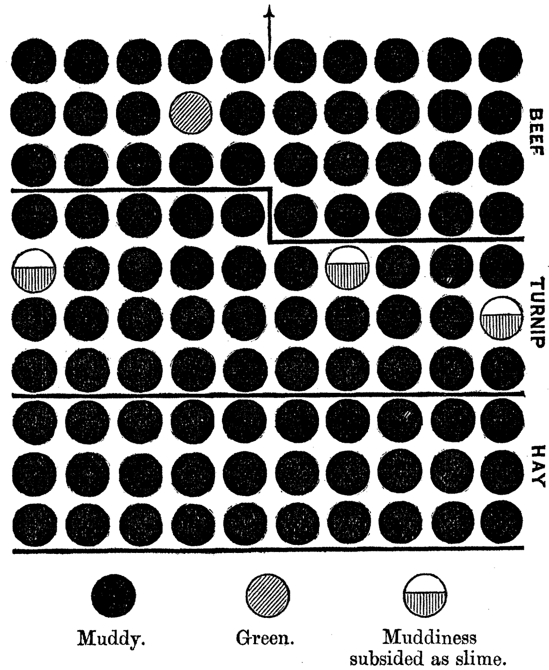

every tube containing the slime being covered by mould. Three tubes only remained clear, but with mould upon their surfaces. The muddy turnip-tubes had increased from four to ten; seven tubes were clouded, while eighteen of them remained clear, with here and there a speck of mould on the surface. Of the beef, six were cloudy and one thickly muddy, while spots of mould had formed on the majority of the remaining tubes. Fifteen hours subsequent to this observation, viz. on the morning of the 27th of October, all the tubes containing hay-infusion were smitten, though in different degrees, some of them being much more turbid than others. Of the turnip-tubes, three only remained unsmitten, and two of these had mould upon their surfaces. Only one of the thirty-five beef-infusions remained intact. A change of occupancy, moreover, had occurred in the tube which first gave way. Its muddiness remained grey for a day and a half, then it changed to bright yellow-green, and it maintained this colour to the end. On the evening of the 27th every tube of the hundred was smitten, the majority with uniform turbidity, some, however, with mould above and slime below, the intermediate liquid being clear. The whole process bore a striking resemblance to the propagation of a plague among a population, the attacks being successive and of different degrees of virulence. I annex copies of the fourth and seventh maps with their respective dates.

On the 31st of October I finally inspected the tray of tubes. All those containing the hay-infusion were turbid, some thicker and much more deeply coloured than others. They were all at first alike in colour. Out of the thirty tubes four only were free from

Map IV. 27th Oct., 6.30 P.M.




Map VII. 29th Oct., 10.30 A.M.




mould. Three of these were adjacent to each other, the fourth at a distant portion of the tray.

The *Penicillium* was exquisitely beautiful. Its prevalent form was a circular patch made up of alternate zones of light and deep green. In some cases the liquid was covered by a single large patch; in others there were three or four patches, each made up of its differently coloured zones. Reticulated patterns also occurred. Three kinds of *Penicillium* seemed struggling for existence, namely :—that just described; a second kind, of the same consistency and colour, but forming little rounded heaps instead of circles; thirdly, a woolly, voluminous, white mould, in the middle of which a zoned circle of the other mould sometimes formed a little islet.

All the tubes containing the turnip-infusion were also turbid on the 31st. Nine of them were free from mould. This, where it occurred, exactly resembled small cocoons in shape. The beef-tubes were also all turbid on the 31st, and seventeen of them were free from mould. The mould upon the beef, moreover, was much less luxuriant than that on the hay- and turnip-infusions. The mould-developing power is obviously greatest in the hay-, less in the turnip-, and least of all in the beef-infusion. In every case where the mould was thick and coherent the *Bacteria* died, or became dormant, and fell to the bottom as a sediment. The growth of mould and its effect on the *Bacteria* are very capricious. The turnip-infusion, after developing in the first instance its myriadfold Bacterial life, frequently rapidly contracts mould, which stifles the *Bacteria* and clears the liquid all the way between the sediment and the scum. Of two tubes placed beside each other, one will be taken possession of by *Bacteria*, which successfully fight the mould and keep the surface perfectly clean; while another will allow the mould a footing, the apparent destruction of the *Bacteria* being the consequence. This I have proved to be the case with all infusions, fish, flesh, fowl, and vegetable. At the present moment, for example, of three tubes containing an infusion of sole, placed close together in a row, the two outside ones are covered by a thick tough blanket of mould, while the central one has not a single speck upon its surface. The *Bacteria* which manufacture a green pigment appear to be uniformly victorious in their fight with the *Penicillium*.

These observations enable us, I think, to draw some interesting conclusions. From the irregular manner in which the tubes are attacked we may infer that, as regards *quantity*, the distribution of the germs in the air is not uniform. A single tube will sometimes be a day or more in advance of its neighbours. The singling out, moreover, of one tube of the hundred by the particular *Bacteria* that develop a green pigment, and other cases just adverted to, shows that, as regards *quality*, the distribution is not uniform. This has been further illustrated by the following observations:—Of five and twenty tubes of different animal infusions exposed in groups of five, in the middle of November, and all swarming with Bacterial life, five were green. They were distributed as follows:—Beef 2, herring 1, haddock 1, fowl 1, wild duck 0. The same absence of uniformity was manifested in the struggle for existence between the *Bacteria* and the *Penicillium*. In some tubes the former were triumphant; in other tubes of the same infusion the latter was triumphant. It would also seem that a want of uniformity as regards *vital vigour* prevailed. With the selfsame infusion the motions of the *Bacteria* in some tubes were exceedingly languid; while in other tubes the motions resembled a rain of projectiles, being so rapid and violent as to be followed with difficulty by the eye. Reflecting on the whole of this, I conclude that the germs float through the atmosphere in groups or clouds, and that now and then a cloud specifically different from the prevalent ones is wafted through the air. The touching of a nutritive fluid by a Bacterial cloud would naturally have a different effect from the touching of it by the interspace between two clouds. But, as in the case of a mottled sky, the various portions of the landscape are successively visited by shade, so, in the long run, are the various tubes of our tray touched by the Bacterial clouds, the final fertilization or infection of them all being the consequence*.

* In hospital practice the opening of a wound during the passage of a Bacterial cloud would have an effect very different from the opening of it in the interspace between two clouds. Certain caprices in the behaviour of dressed wounds may possibly be accounted for in this way.

Under the heading "Nothing New under the Sun," Prof. HUXLEY has lately sent me the following remarkable

The tray of tubes proved so helpful in enabling me to realize mentally the distribution of germs in the air, that on the 9th of November I exposed a second tray containing one hundred tubes filled with an infusion of mutton. On the morning of the 11th six of the ten nearest the stove had given way to putrefaction. Three of the row most distant from the stove had yielded, while here and there over the tray particular tubes were singled out and smitten by the infection. Of the whole tray of one hundred tubes, twenty-seven were either muddy or cloudy on the 11th. Thus, doubtless, in a contagious atmosphere, are individuals successively struck down. On the 12th all the tubes had given way, but the differences in their contents were extraordinary. All of them contained *Bacteria*, some few, others in swarms. In some tubes they were slow and sickly in their motions, in some apparently dead, while in others they darted about with rampant vigour. These differences are to be referred to differences in the germinal matter, for the same infusion was presented everywhere to the air. Here also I imagine we have a picture of what occurs during an epidemic, the difference in number and energy of the Bacterial swarms resembling the varying intensity of the disease. It becomes obvious from these experiments that of two individuals of the same population exposed to a contagious atmosphere, the one may be severely, the other lightly attacked, though the two individuals may be as identical as regards susceptibility as two samples of one and the same mutton-infusion. What I have already said regarding the "preparedness" of contagium has its application here.

The parallelism of these actions with the progress of infectious disease may be traced still further. The 'Times,' for example, of January 17 contained a letter on typhoid fever, signed "M.D.," in which occurs the following remarkable statement:—"In one part of it [Edinburgh], congregated together and inhabited by the lowest of the population, there are, according to the Corporation return for 1874, no less than 14,319 houses or dwellings—many under one roof, on the 'flat' system—in which there are no house connexions whatever with the street-sewers, and, consequently, no water-closets. To this day, therefore, all the excrementitious and other refuse of the inhabitants is collected in pails or pans, and remains in their midst, generally in a partitioned-off corner of the living-room, until the next day, when it is taken down to the streets and emptied into the Corporation carts. Drunken and vicious though the population be, herded together like sheep, and with the filth collected and kept for 24 hours in their very midst, it is a remarkable fact that typhoid fever and diphtheria are simply unknown in these wretched hovels."

The analogy of this result with the behaviour of our infusions is perfect. On the 30th of November, for example, a quantity of animal refuse, embracing beef, fish, rabbit, hare, was placed in two large test-tubes opening into a protecting-chamber con-

extract:—'Uebrigens kann man sich die in der Atmosphäre schwimmenden Thierchen wie Wolken denken, mit denen ganz leere Luftmassen, ja ganze Tage völlig reinen Luftverhältnisse wechseln.' (EHRENBERG, 'Infusionsthierchen,' 1838, p. 525.) The coincidence of phraseology is surprising, for I knew nothing of EHRENBERG's conception. My 'clouds,' however, are but small miniatures of his.

taining six tubes. On December 13, when the refuse was in a state of noisome putrefaction, infusions of whiting, turnip, beef, and mutton were placed in the other four tubes. They were boiled and abandoned to the action of the foul "sewer-gas" emitted by their two putrid companions. On Christmas-day these four infusions were limpid. The end of the pipette was then dipped into one of the putrid tubes, and a quantity of matter, comparable in smallness to the pock-lymph held on the point of a lancet, was transferred to the turnip. Its clearness was not sensibly affected at the time; but on the 26th it was turbid throughout. On the 27th a speck from the infected turnip was transferred to the whiting; on the 28th disease had taken entire possession of the whiting. To the present hour the beef- and mutton-tubes remain as limpid as distilled water. Just as in the case of the living men and women in Edinburgh, no amount of fetid gas had the power of propagating the plague, as long as the organisms which constitute the true contagium did not gain access to the infusions.

In the foregoing observations the tubes were arranged in the same horizontal plane; but I also sought to obtain some notion of the vertical distribution of the germs in the air of the room. Two trays, each containing 100 tubes, were supported the one above the other in the same frame. The upper tray had all the air between it and the ceiling, a height of about 12 feet, from which the germs might descend upon it; the lower tray was shaded by the upper, a space of only 6 inches existing between them. If the number of germs deposited in the tubes were determined by the air-space above, the upper tray would be the one most rapidly and thoroughly taken possession of. The reverse was the case. As regards the development of Bacterial life, the lower tray was from first to last in advance of its neighbour. It is not air-space, then, so much as stillness, that determines the deposition of the germs. The air between the two trays being less disturbed than the general air of the room, the germs were less wafted about, and therefore fell in greater numbers into the tubes of the lower tray. We have here data which will enable us to form a rough notion of the lower limit of the number of germs contained in the room where the experiments were made.

The floor of the room measured 20 feet by 15 feet; its area was therefore 43,200 square inches, and every square inch would afford room for the section of one of our test-tubes. The height of the room is 180 inches; hence 30 layers of tubes 6 inches apart might be placed one above the other between the floor and ceiling. This would make 1,296,000 tubes. If only a single germ a day fell into each tube this would be the number of the germs. If the number deposited were one an hour, we should have thirty millions a day sown in the tubes. Probably the average time necessary for infection is very much less than an hour. At all events 30,000,000 of germs daily would be an exceedingly moderate estimate of the number falling into our thirty layers of tubes. This, moreover, would only be a fraction—probably a small fraction—of the germs really present in the air. In his Presidential Address to the British Association at Liverpool, Prof. HUXLEY ventured the statement that myriads of germs are floating in our atmosphere. Untrained experimenters and rash reasoners have ridiculed this statement. In view of the foregoing calculation it, however, expresses the soberest fact Indeed, taking the word myriad in its literal sense of ten thousand, it would be simple bathos to apply it to the multitudinous germs of our air.

§ 27. *Some Experiments of* PASTEUR *and their Relation to Bacterial Clouds.*

Quite recently I had occasion to refresh my memory of PASTEUR's paper published in the 'Annales de Chimie' for 1862. The pleasure I experienced on first reading it was revived by its reperusal. Clearness, strength, and caution, with consummate experimental skill for their minister, were rarely more strikingly displayed than in this imperishable essay. Hence it is that during recent discussions, in which this and other labours of the highest rank met with such scant respect, those in England most competent to judge of the value of scientific work never lost faith in the substantial accuracy of PASTEUR. One striking example of his penetration has an immediate bearing on the conclusion regarding Bacterial clouds, independently drawn by me from the deportment of the tray of one hundred tubes. On the 28th of May, 1860, PASTEUR opened, on an uncovered terrace a few metres above the ground, four flasks containing the water of yeast. Nothing appeared in any of them until the 5th of June, when a small tuft of mycelium was observed in one of them. On the 6th a second tuft appeared in another flask; the two remaining flasks remained intact and without organisms. On the 20th of July he opened, in his own laboratory, six flasks containing water of yeast. Four of them remained perfectly intact, while two of them became promptly charged with organisms. PASTEUR infers from these observations the non-continuity of the cause to which so-called spontaneous generation is due. This inference is quite in accord with the notion of Bacterial clouds suggested by my observations. PASTEUR, in fact, sometimes opened his flask in the midst of a Bacterial cloud and obtained life, sometimes in the interspace between two clouds, and obtained no life.

Not with a view of repeating this observation, which had been forgotten, but for another reason, I opened on the 6th of January last a number of hermetically sealed tubes in one and the same room of the Royal Institution. The names of the infusions contained in the tubes, the date of sealing them up, their condition before opening on the 6th, and their appearance six days subsequently on the 12th are given in the accompanying statement. I chose for these observations tubes which contained a little liquid in their drawn-out portions. In every case the motion of this liquid, when the tube was broken, indicated a violent inrush of air.

| Infusion. | Date of sealing. | Appearance, Jan. 6. | Appearance, Jan. 12. |
|---|---|---|---|
| Grouse | Nov. 27th | Clear | Clear. |
| Sole | ,, 17th | ,, | Turbid. |
| Turnip No. 1 | Oct. 5th | ,, | Penicillium on surface. |
| Turnip No. 2 | ,, ,, | ,, | Clear. |
| Hay | ,, ,, | ,, | Mycelium at bottom. |
| Wild Duck | Nov. 12th | ,, | Turbid. |

| Infusion. | Date of sealing. | Appearance, Jan. 6. | Appearance, Jan. 12. |
|---|---|---|---|
| Mutton | Nov. 12th | Clear | Cloudy. |
| Fowl | ,, ,, | ,, | Clear. |
| Beef | ,, ,, | ,, | Mycelium at bottom. |
| Haddock | ,, ,, | ,, | Clear. |
| Sweetbread | ,, 16th | ,, | Mycelium at bottom. |
| Rabbit | ,, 13th | ,, | Clear. |
| Heart | ,, ,, | ,, | Curdy layer at top. |
| Pheasant | ,, ,, | ,, | Clear. |
| Mullet | ,, ,, | ,, | ,, |
| Hare | ,, ,, | ,, | ,, |
| Snipe | ,, ,, | ,, | ,, |
| Partridge | ,, ,, | ,, | ,, |
| Plover | ,, ,, | ,, | Mycelium below. |
| Codfish | ,, ,, | ,, | Clear. |
| Kidney | Jan. 5th | ,, | Mycelium at bottom. |
| Salmon | Dec. 13th | ,, | Clear. |
| Whiting | ,, ,, | ,, | ,, |
| Turnip | ,, 29th | ,, | ,, |
| Hay 4 drops of caustic potash | Nov. 22nd | Clear with sediment | Mycelium at bottom. |
| Hay 2 drops of caustic potash | ,, ,, | Clear | Mycelium at bottom. |
| Hay 5 drops of caustic potash | ,, ,, | Clear with sediment | Clear. |
| Hay 6 drops of caustic potash | ,, ,, | Clear with sediment | ,, |
| Liver | Nov. 30th | Clear. | |
| Hay | ,, 18th | ,, | Clear. |
| Hay | ,, ,, | ,, | ,, |
| Turnip | ,, ,, | ,, | Muddy. |

Thus, out of 31 flasks opened in the same air, 18 remained intact, while 13 were taken possession of by organisms—a fact obviously the same in character as that described by Pasteur. Such experiments demonstrate, if demonstration were needed, that it is not the air itself, or any gaseous or vaporous substance uniformly diffused through it, but some discontinuous substance floating in it, that is the cause of the infection. Instead of our tubes let us suppose thirty-one wounds to be opened in the same ward of a hospital; plainly what has occurred with the tubes may occur with these wounds—some may receive the germs and putrefy, others may escape. Helped by the conception not only of germs, but of germ-clouds, the different behaviour of wounds subjected apparently to precisely the same conditions will cease to be an inscrutable mystery to the surgeon*.

During the course of this inquiry some eminent biologists have been good enough, from time to time, to look in upon my work, and to give me their views regarding the evidential force of the experiments. To Professor HUXLEY, moreover, I am indebted for undertaking the examination of a number of the hermetically sealed tubes. Thirty of them were placed in his hands, none of them being regarded as defective. A close examination, however, disclosed in one of them a mycelium. No faultiness could for a time be discovered in the tube; the sealing appeared to be quite as perfect as that of its sterile fellows. Once, however, on shaking it a minute drop of liquid struck my friend's face; and he soon discovered that an orifice of microscopic minuteness had been left open in the nozzle of the tube. Through this the common air had been sucked in as the liquid cooled, and hence the contamination. It was the only defective tube of the group of thirty, and it alone showed signs of life.

The statement of this fact before the Royal Society, by Prof. HUXLEY, brought to my mind a somewhat similar experience of my own. One morning in November I lifted one of the hermetically sealed tubes from the wire on which it was suspended, and, holding it up against the light, discovered, to my astonishment, a beautiful mycelium flourishing at the bottom. Before restoring the tube to its place I touched its fused end and found it sharp. Close inspection showed that the nozzle had been broken off; the common air had entered, and the seed of the mycelium had been sown. Two other instances, one like that observed by Prof. HUXLEY, have since come to light. In one of them a minute orifice remained after the supposed sealing of the tube. The other case was noticed when the tubes were returned from the Turkish Bath. One of them contained a luxuriant mycelium. It was noticed that the liquid in this tube had singularly diminished in quantity, and on turning the tube up it was found cracked at the bottom.

*No case of pseudo-spontaneous generation ever occurred under my hands that was not to be accounted for in an equally satisfactory manner.*

In this inquiry, thus far, I have confined my observations to purely liquid infusions, purposely excluding milk, mixtures of turnip-juice and cheese, and, indeed, mixtures of

* "We have ample facts of experiment in our hands," said Mr. KNOWSLEY THORNTON (Trans. of the Pathological Society, vol. xxvi. p. 313), "to show that it is not the gases of the air, or any soluble material in water, but something 'particulate' which sets up all the train of changes in an open wound, which may, after the patient has passed through a period of more or less constitutional disturbance, end in the healing of the wound, or may end in septicæmia and death. This particulate material, then, I believe we have evidence enough to prove consists of germs of *Bacteria* and other low organisms." All the evidence points to this conclusion. I may say that I entirely agree with Mr. THORNTON in the distinction he draws between *germs* and developed *Bacteria* floating in the air. It is, in my opinion, of the very last importance to seize this distinction with clearness. When it is fully realized we shall probably hear less of the arguments against Bacterial contagia founded on the fact that a virus diminishes in strength as the *Bacteria* multiply. A portion of the energy of the virus consists in its passage from the germ state to that of the finished organism.

solids and liquids of all kinds. The next section of the investigation will be devoted to these and kindred subjects; and to it I also postpone the complete examination of *pepton*, and of the remarkable experiments described by Dr. WILLIAM ROBERTS, a small residue of which only I have failed to corroborate.

Throughout the whole of this investigation I have had to congratulate myself on the zealous and efficient aid of my excellent assistant, Mr. JOHN COTTRELL. His intelligence in seizing my ideas, and his mechanical skill in realizing them, have rendered me admirable service. Without such aid, indeed, so much ground could not have been covered in the time. Mr. COTTRELL'S junior colleague, Mr. FRANK VALTER, has also acquitted himself to my entire satisfaction.

Royal Institution, 5th April, 1876.

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It gives me special pleasure to direct attention here to a paper by the Rev. W. H. DALLINGER, for an advanced proof of which I am indebted to the courtesy of Dr. LAWSON, editor of the 'Popular Science Review.' Mr. DALLINGER and his colleague Dr. DRYSDALE are known to have pushed the microscope to its utmost power of performance at the present time. Their 'Researches into the Life-History of the Monads' are models of scientific thoroughness and concentration. Mr. DALLINGER'S review of the present position of the doctrine of spontaneous generation, his remarks on Bacterial germs in relation to the limits of the powers of the microscope, his demonstration that the germs of monads survive in a medium raised to a temperature which destroys the adult, and that precipitated mastic particles like those mentioned in § 16 of this paper are not to be discerned by a magnifying-power of 15,000 diameters, constitute a most interesting and important communication.

### NOTE I. *Action of* Bacteria *upon a Beam of Light.*

To trace the gradual growth and multiplication of the *Bacteria* by their action on a beam of light an infusion of beef was prepared on the 5th of October, placed in a globular flask of about 50 cubic inches capacity, and put aside with its mouth open to the laboratory air. On the 8th, 9th, 10th, 11th, and 12th similar flasks were prepared and put aside in succession. On the 12th all the flasks were examined by the concentrated electric light. The freshest one showed the track of the beam as a richly coloured green cone. The green light was unaffected by a NICOL'S prism, which, however, quenched the ordinary scattered light and augmented the purity and vividness of the green. It was a case of fluorescence. In the second flask, one day old, the fluorescent beam was in great part masked by the scattered light; the latter, however, could be partially quenched by a NICOL'S prism, the purity of the fluorescence being thus in part restored. Through the third flask, two days, and the fourth flask, three days old, the track of the beam was still discernible; through the fifth flask, four days old, it was all but obliterated, while in the sixth flask, seven days old, it was entirely shattered, the turbid medium being filled uniformly with the laterally scattered light.

Two of these flasks were of a bright yellow-green colour, two were milky or white, and two of a dull brownish hue.

COHN mentions the bluish tinge of the infusion by reflected and its yellow tinge by transmitted light when the *Bacteria* are incipient. This is due to a dichroitic action, similar to that which produces the blue of the sky and the morning and evening red. The blue, however, though discernible, is not pronounced, for the *Bacteria* are too large to scatter the colour in any high degree of purity; but with a "muddy" infusion a very fair red may be obtained from transmitted light. I have used the Bacterial turbidity for photometric purposes. On the 9th of October, for example, I accompanied Sir RICHARD COLLINSON and a Committee of the Elder Brethren of the Trinity House to Charlton, with the view of comparing together two lights mounted at the Trinity Wharf at Blackwall. To imitate a foggy atmosphere, I employed an infusion cloudy with *Bacteria* and placed in a glass cell. With it the beams could be toned gradually down to complete extinction.

NOTE II. *Fluorescence of Infusions.*

All the animal infusions, both flesh and fish, showed the same fluorescence. It was the same green hue throughout, though of varying degrees of intensity. In wild duck, grouse, snipe, hare, partridge, and pheasant the fluorescence was fine—sometimes exceedingly fine. In rabbit it was less fine than in hare, and in a tame rabbit less fine than in a warren rabbit. Fishes also differed from each other. Mullet, for example, was finer than cod, herring, or haddock. Beef, mutton, heart, liver all showed the same green fluorescence.

Led up to it by a series of remarkable experiments on the rapidity of the passage of crystallized substances into the vascular and non-vascular textures of the body*, Dr. BENCE JONES and Dr. DUPRÉ communicated to the Royal Society in 1867 a highly interesting paper "On a Fluorescent Substance, resembling Quinine, in Animals"†. They then showed that "from every texture of man and of some animals a fluorescent substance can be extracted, which, when extracted, has a very close optical and chemical resemblance to quinine." They therefore called it animal quinoidine. In dilute solutions they found that the fluorescence of the animal substance was not to be distinguished from that produced by quinine. When the solution was concentrated, the colour of the light was of a decidedly greenish hue. This latter observation is most in agreement with mine. In all the infusions examined by me the fluorescent light was a decided green, and not to be mistaken for the blue light of quinine.

The green colour is similar to that emitted by the crystalline lens when a beam of

* Proceedings of the Royal Society, vol. xiv. 1865.

† Ibid. vol. xv. p. 73.

violet light impinges on it *; sending such a beam through any of the infusions, the "degradation" of the violet to green is strikingly illustrated.

The foregoing statement refers to the deportment of the infusions after boiling and filtering. Prior to boiling some of them were of a brilliant ruby colour; but even here, when the layer of liquid between the eye and the beam was not too thick, the green fluorescence could be seen through the red liquid.

* On plunging the eye into the beam of the electric lamp, transmitted through violet glass, the moment the crystalline lens is seen to fluoresce by a second observer, a blue shimmer is seen by the eye on which the beam falls. In the case of my own eye, I can always readily tell when the fluorescence has set in.