XII. *Studies on Biogenesis.* *By* William Roberts, *M.D.*, *Manchester.* *Communicated by* Henry E. Roscoe, *F.R.S.*

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Introduction.

The question of the origin of *Bacteria* and *Torulæ* lies so deeply at the root of some of the most important problems, not only of biology, but of pathology and practical therapeutics, that I make no apology for bringing forward the fruits of another investigation on the subject.

The main question in controversy is whether these organisms originate *de novo* in the media where they grow, or whether they spring, like higher beings, from germs or parents like themselves.

On the one hand it is contended that there exist in ordinary air and water (in addition to their proper elements) multitudes of germinal particles, and that the quasi-spontaneous production of *Bacteria* and *Torulæ* in organic media is in reality due to infection by these particles. On the other hand the existence of these supposed germs is doubted or denied; and it is affirmed that these organisms can and do arise where infection by preexisting germs is impossible.

It is important to bear in mind at the outset that these two theories (Panspermism and Abiogenesis) are not necessarily wholly destructive of each other. It is conceivable that, while the ordinary and common origin of *Bacteria* and *Torulæ* is by procession from preexisting germs, there may also be conditions in which they arise *de novo.* At any rate it appears very desirable to establish the fundamental propositions of the panspermic theory, as broadly expressed in the preceding paragraph, on independent grounds, and without prejudice to the question of abiogenesis.

The point of view here indicated is adhered to throughout this paper. It resembles the attitude assumed by pathologists in regard to contagious diseases. No pathologist doubts, for example, the contagiousness of small-pox, nor that the ordinary production and spread of the disease is due to infection. And this belief is not inconsistent with the notion, very commonly held, that in some previous age small-pox did arise *de novo*; nor would it now be shaken, nor the practical deductions therefrom set aside, if it were proved that under certain rare etiological combinations small-pox might still arise *de novo.*

The inquiry is divided into three sections. The first section is devoted to the examination of the conditions under which organic liquids and mixtures are rendered barren by heat. In the second section is investigated the question whether the normal juices and tissues of animals and plants are capable of producing organisms without infection by extraneous germs. In the third section the facts adduced in the two previous sections are considered in their bearing on the origin of *Bacteria* and *Torulæ*, and some of the alleged cases of abiogenesis are tested experimentally.

The experiments were all contrived on a plan which favoured not only the birth but the continuous growth of any organisms which made their appearance. The materials were both maintained at a suitable temperature and furnished with a free supply of air, so that the changes initiated might have an opportunity of going on until their nature became undoubted.

In judging of the absence or presence of organisms, the microscope was, of course, the principal test. The magnifying-power generally employed was 500 diameters, controlled sometimes by a magnifying-power of 1200 diameters. In addition to this, however, note was always taken of the naked-eye appearances, of the presence or absence of turbidity, of a film on the surface, and of a deposit at the bottom, as well as of the reaction and odour. Signs of growth and multiplication were regarded as the only indefeasible evidence of living organisms.

The term *Bacteria* is used in the comprehensive sense adopted by COHN to include the various organisms described as vibrios, micrococci, microzymes, and schistomycetes. The terms *Torulæ* and *fungoid vegetations* are used to designate organisms belonging to the type of the yeast-plant and the *Penicilium glaucum*. The word *germ* is used simply in the general sense of a particle endowed with the power of provoking germination in a suitable medium.

SECTION I.—ON THE STERILIZATION BY HEAT OF ORGANIC LIQUIDS AND MIXTURES.

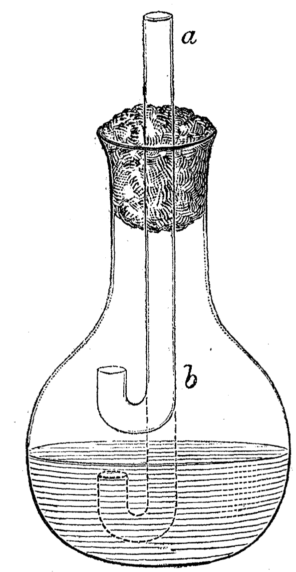
When beef-tea or a decoction of turnip is boiled for a few minutes in a flask, of which the neck is plugged with cotton-wool, the liquid passes into a state of permanent sterility. It can be kept for months and even years exposed to the most favourable conditions of warmth and light, with a constantly renewed supply of air; but so long as the cotton-wool plug remains undisturbed, neither *Bacteria* nor *Torulæ*, nor any other organisms, make their appearance in it. The liquid has not, however, lost its fitness to nourish and promote the growth of these organisms; for if the cotton-wool plug be withdrawn so as to admit unfiltered air into the flask, or if a drop of ordinary water be introduced, *Bacteria* or *Torulæ* speedily make their appearance, and grow and multiply with the utmost luxuriance.

This experiment, which will be referred to as the "plugged-flask" experiment, may be instructively varied in the following manner, which is a simplification of PASTEUR's bent-tube experiment. A glass-tube (*a b*, fig. 1), 4 inches long, is bent at one end into a U-shape. The longer limb of the tube is then tightly wrapped round with cotton-wool so as to form a plug. This plug is inserted into the neck of a flask * half

* The flasks used in this and the "plugged-flask" experiments were the ordinary four-ounce flasks used by chemists.

filled with beef-tea or a decoction of turnip, as represented in fig. 1. A plug of cotton-wool is also inserted into the upper end of the glass tube at *a*. The flask is then boiled over the flame for five minutes. When the flask is quite cold, the plug at *a* is gently withdrawn. The liquid in a flask thus prepared remains permanently barren. In a week or two the condensed steam collected at the bend of the tube dries up, and the tube becomes an open channel into the flask. If the flask be examined at the end of two or three months, the contained liquid will be found still perfectly transparent. The experiment may then be carried a step further. If the little tube (*a b*) be pressed downward through the cotton-wool plug until the U-shaped portion is completely submerged, as represented by the dotted lines in the figure, the liquid in the flask is brought into contact with the particles of air-dust collected in the bend of the tube. The result of this contact is speedily seen; for in a few days patches of mould appear on the surface, or the liquid becomes turbid from *Bacteria* *.

Fig. 1.



Bent-tube experiment simplified.

These experiments admit of an easy explanation on the panspermic theory. The living germs and organisms contained within the flasks are killed by the heat during ebullition; and the fresh supplies of air which enter the flasks on cooling are deprived of their germs—in the first case by filtration through the cotton-wool plug; in the second case they are arrested in the bend of the tube *a b*, the shorter limb of which they are unable to ascend against the force of gravity; and thus the necrosis at first effected by the heat is succeeded by a state of permanent sterility through want of living germs to start the process of germination.

The condition of "permanent sterility" here described is essentially characterized by loss of the power of *originating* organisms with conservation of the power of *nourishing and promoting the growth* of organisms.

The degree of heat required to induce this state of permanent sterility varies greatly according to the nature of the materials operated upon.

Pasteur long ago pointed out that milk required more heat to sterilize it than sweetened yeast-water; more recently Dr. Bastian and others have shown that turnip-infusion with cheese and some other mixtures cannot be sterilized, as an ordinary decoction can, by boiling for five or ten minutes; and experiments to be presently

* The barrenness in these experiments is not so absolutely permanent as in the simple plugged-flask experiment, as may be seen from the following observation:—On June 19th, 1872, I put up a flask of beef-tea in the manner above described, but in the process of boiling some of the beef-tea frothed over into the bend of the tube. On March 27th, 1873, the liquid in the flask was still quite unaltered, but I could see minute specks of mould creeping up the short limb of the tube *a b*, and about to drop into the liquid below. A few days after specks of mould began to appear on the surface of the liquid in the flask; these speedily grew until they covered the entire surface.

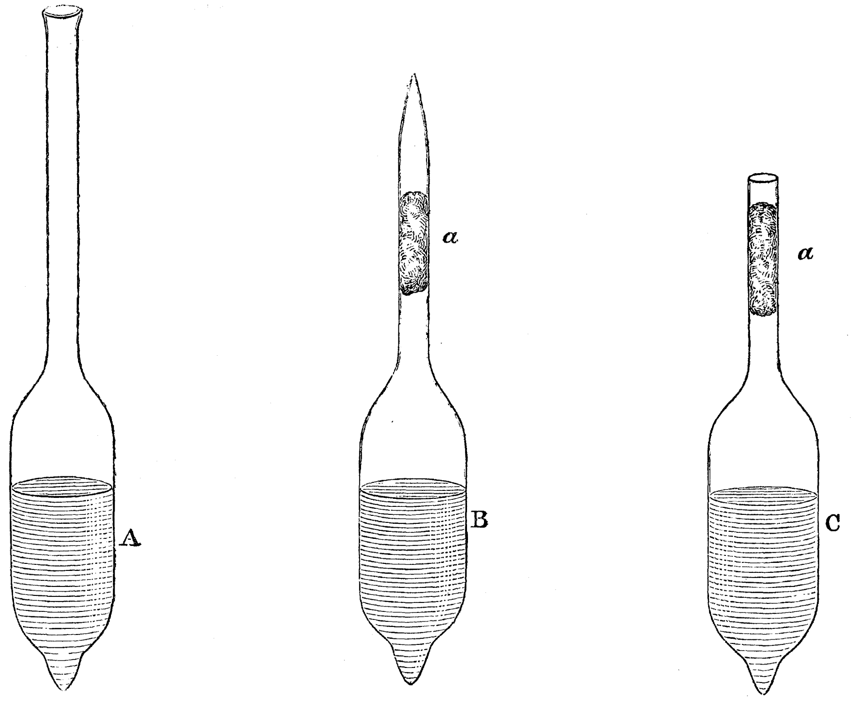
described prove that there are liquids which germinate after exposure to the heat of boiling water for even two and three hours, or after exposure to a heat of 9° Cent. above that of boiling water for more than half an hour.

In pursuing these inquiries it was found that the method of direct boiling over a flame in a plugged flask was unsuited to liquids which required a prolonged application of heat for sterilization. In the first place, the evaporation that ensued on boiling for fifteen or twenty minutes seriously altered the concentration of the materials; and, secondly, this method did not permit an accurate gradation of temperature; for the boiling-point in a plugged flask was observed to rise above the normal temperature of ebullition from 3° to 6° Cent. according to the tightness of the plugging.

To obviate these disadvantages another plan was adopted; and, as most of my experiments were performed by this method, it is necessary to describe it more particularly. It will be referred to as the "plugged-bulb" method.

The "plugged-bulb" Method.—An ordinary delivery-pipette, having on it an oblong bulb capable of containing 30 to 50 cub. centims., was sealed hermetically at one end

Fig. 2. The plugged-bulb experiment.



A. The bulb charged. B. The bulb charged, plugged, and sealed ready for heating. C. The bulb with its neck filed off, and set aside to see if it will germinate. The figures are drawn about half the actual size.

(see fig. 2, A). The materials of the experiment were then introduced into the bulb until it was two thirds full. The inside of the neck of the bulb was next wiped dry, and a plug of cotton-wool (*a*) was inserted about its middle. Lastly, the neck was drawn out above the plug and sealed in the flame, as represented in fig. 2, B.

When the bulb was thus charged and sealed, it was weighted with a leaden collar and submerged in the semi-upright position (so as to prevent the wetting of the cotton-wool plug) in a can full of water. The can was next placed over a source of heat and boiled for the required time. The bulb was then withdrawn; and, when quite cold, its neck was filed off above the cotton-wool plug as represented in fig. 2, C. Finally, it was set aside in the upright position, and maintained at a suitable temperature.

When it was desired to test the effects of a higher temperature than that of boiling water, the can was filled with brine or oil instead of water.

By this method the materials of the experiment could be exposed for any desired time to a desired heat without evaporation, and without the disturbing effects of ebullition; and by subsequently filing off the neck of the bulb above the cotton-wool plug, free access of filtered air was provided, so that the conditions most favourable to germination were maintained for an indefinite time*.

In the last four years I have performed several hundred experiments by the "plugged-flask" and the "plugged-bulb" methods, on a great variety of organic liquids and mixtures. The flasks and bulbs, after heating, were either placed on a marble slab covering a warm-water cistern, which stands in a corner of my room, or they were kept in a warm greenhouse. In the former case their temperature ranged from 15° to 27° Cent., and in the latter from 15° to 32° Cent.

The following summary exhibits the general results of the experiments. The values here given must not, however, be regarded as absolute, but rather as comparative and approximative values, which hold good only for the precise quantities, materials, and methods of experimenting adopted. The experiments are thrown into three groups according to the comparative facility of sterilization exhibited by the various liquids and mixtures employed.

GROUP I. *Substances sterilized by five or ten minutes' boiling in a plugged flask.*—The easiest substances to sterilize were infusions of animal or vegetable tissues raised to the boiling-point for a few seconds and then filtered. These were, strictly speaking, decoctions rather than infusions; they were sterilized, if quite fresh, by three or four minutes' boiling. In the same category stood solutions of organic salts—citrates, acetates, and tartrates, and healthy and diabetic urine.

Infusions made slowly at blood-heat, and not raised to the boiling-point, generally (but not always) required to be boiled for five or ten minutes. Many infusions so made let fall a sediment on boiling, and the non-removal of this by filtration appeared to increase their resistance to sterilization. It appeared also that the longer the infusions were kept before boiling, the greater, as a rule, proved their resistance to sterilization.

* My experience does not agree with that of Dr. BASTIAN, that liquids sealed in ebullition are more favourably circumstanced for germination than the same liquids under the ordinary pressure of the atmosphere. In repeating his experiments I found that the liquids which germinated after being sealed in ebullition, also germinated (and much more abundantly) when air was admitted to them through cotton-wool.

This was attributed to the gradual diminution of the acidity of the infusions when they were long kept, and to the commencing multiplication of organisms in them.

The following were the infusions actually experimented on and found conformable to the above rules:—infusions of beef, mutton, pork, codfish, mussel, carrot, turnip, hay, malt, pear, apple, cucumber, cabbage, lettuce, tomatoes, vegetable marrow, and parsnep.

It was found that pieces of carrot, turnip, apple, cucumber, &c. floating in water were nearly as easily sterilized as filtered infusions of these substances. But chopped *green* vegetables floating in water (such as kidney-beans, asparagus, cabbage and lettuce, and green peas) were very difficult to sterilize by boiling in a plugged flask. Such mixtures could be boiled for fifteen or twenty minutes or longer, and yet they almost invariably germinated a few days after. Chopped boiled white of egg floating in water also behaved in the same way. The singular resistance of green vegetables to sterilization appeared to be due to some peculiarity of surface, perhaps their smooth glistening epidermis, which prevented complete wetting of their surfaces; for I found that if they were previously thoroughly crushed in a mortar they were sterilized much more easily. The difficulty in the case of egg-albumen was presumably due to the alkaline reaction of that substance.

GROUP II. *Substances sterilized by exposure for not less than twenty to forty minutes to the heat of boiling water in a plugged bulb.*—To this group belonged:—mixtures of chopped green vegetables with water (with or without alkali), pieces of flesh-meat, or fish, or boiled egg with water, blood, dropsical fluids, milk, albuminous urine, and turnip-infusion with cheese. Experiments performed in this fashion often yielded beautiful preparations. Owing to the absence of the disintegrating turmoil of ebullition the pieces of vegetable or flesh retained their original appearance in the bulbs, and the supernatant water preserved its transparency. In the case of egg-albumen and dropsical fluids it was found better, instead of mixing the materials at first with water, to proceed as follows:—About two drachms of egg-albumen or dropsical fluid were conveyed to the bottom of the bulb by means of a long-necked funnel. The lower part of the bulb was then immersed in boiling water until the albumen coagulated into a solid cake, then water was introduced and the bulb plugged, sealed, and boiled in the usual way. When the experiment was thus performed, the supernatant water remained brilliantly limpid and colourless.

GROUP III. *Substances sterilized by exposure for not less than one or two hours to the heat of boiling water in a plugged bulb.*—Only one member of this group was encountered, namely, superneutralized hay-infusion. Of all the substances examined by me this proved to be the most difficult to sterilize. As a large number of experiments were made with hay-infusion, and as different specimens of hay differ a good deal from each other, a bundle of good meadow-hay was secured, and all the infusions were made with this hay, so as to insure as great a uniformity as possible in the materials of the experiment. The infusion was always made in the same way. The hay was soaked in a minimum of water for four hours at blood-heat. The infusion was then filtered and diluted with ordinary water until it had a specific gravity of 1006. Prepared in this way, hay-infusion had the colour of sherry; it was slightly acid in reaction, and it boiled without forming any sediment. When neutralized with ammonia or liquor potassæ, it threw down a copious sediment which increased on boiling; this sediment subsided completely on standing, and left a perfectly transparent supernatant liquor. This perfect transparency permitted the detection of the slightest turbidity from the development of *Bacteria*.

Unneutralized hay-infusion, when quite fresh, was easily sterilized by five minutes' boiling in a plugged flask; but when it was slightly alkalized by ammonia or liquor potassæ, its resistance to sterilization by heat was increased to a most marvellous degree. It could no longer be sterilized even by fifteen or twenty minutes' boiling in a plugged flask, which almost reduced the infusion to dryness by evaporation. By means of the plugged-bulb method, however, alkalized hay-infusion could be sterilized without difficulty; but it required more than an hour's exposure to the heat of boiling water to effect this.

It was found that the maximum resistance to sterilization resided in infusions alkalized with about five drops of liquor potassæ per ounce (about one per cent.); two or three drops less than this, or four or five drops more than this, considerably diminished this resistance.

The following experiments will serve to illustrate the high resisting power of alkalized hay-infusion to sterilization by heat. In every case the necks of the bulbs were filed off above the plug after the experiment, and the bulbs were afterwards placed in a warm place.

1. *March* 27, 1873.—Seven plugged bulbs, charged with alkalized hay-infusion, were boiled in a can of water for fifty minutes. On March 31st, four days after, all were turbid and covered with a thick film of *Leptothrix*-filaments.

2. *March* 31, 1873.—Three plugged bulbs, charged with alkalized hay-infusion, were boiled in a can of water for two hours. On April 3rd all three were turbid, and covered with an abundant film.

3. *April* 4, 1873.—Five plugged bulbs (*a*, *b*, *c*, *d*, and *e*) were charged with hay-infusion alkalized with 0·7 per cent. of liquor potassæ. They were boiled in a can of water for various lengths of time: *a* was boiled for an hour and became turbid in four days; *b* was boiled for two hours, and *d* and *e* for seven hours; these three remained permanently barren. An accident happened to *c* just before it was put into the can; it was therefore re-charged with the same infusion, but *alkalized a little more strongly*: this one was boiled for three hours, nevertheless it became turbid and covered with a film in a few days. This was the most extreme example met with of resistance to sterilization with the heat of boiling water.

4. *July* 15, 1873.—Five plugged-bulbs, charged with hay-infusion alkalized with 1·4 per cent. of liquor potassæ, were boiled together in a can of water. One of the bulbs was taken out at the end of every hour. The first bulb, withdrawn after being

boiled an hour, became turbid from *Bacteria* in three days; the other four, boiled for periods varying from two to five hours, remained permanently sterile.

5. *July* 15, 1873.—Six plugged bulbs, charged with hay-infusion alkalized with 1·4 per cent. of liquor potassæ, were boiled together in a can of saturated brine for various periods from fifteen to ninety minutes. One of the bulbs was withdrawn from the boiling brine every quarter of an hour. The boiling-point of the brine was 109° Cent. The first bulb, withdrawn at the end of the first quarter of an hour, became fertile, all the rest remained permanently barren.

6. *July* 30, 1873.—Nine plugged bulbs, divided into sets of three each, were charged with hay-infusion alkalized to three different degrees. The first set was alkalized with one per cent., the second with two per cent., and the third with three per cent. of liquor potassæ. These were all boiled together in a can of saturated brine. One bulb of each set was withdrawn from the boiling brine at the end of fifteen minutes, one of each set at the end of twenty-five minutes, and the remainder at the end of thirty-five minutes. The bulbs of the first set (those which were alkalized with one per cent.) all proved fertile in three days; the other six remained permanently barren.

This last experiment showed that hay-infusion alkalized with one per cent. of liquor potassæ possessed an immensely greater power of resistance to sterilization by heat than infusions alkalized with two and three per cent.

7. *July* 14, 1873.—Six plugged bulbs, charged with hay-infusion alkalized with 1·4 per cent. of liquor potassæ, were slowly heated in a can of oil up to a temperature of 125° Cent., and maintained at that temperature for periods varying from five to thirty minutes. All of these remained permanently barren.

In all the above-described experiments of the three groups, the sterilized liquids and mixtures, whether they were sterilized by a few minutes' boiling in a plugged flask, or by one or more hours exposure in a plugged bulb to the heat of boiling water, or by exposure to the still higher temperature of boiling brine or heated oil, immediately, and without exception, recovered their fertility when exposed to the contamination of unfiltered air or of ordinary water. Their susceptibility in this respect did not differ whether the preparations had been kept for months or even years, or whether they had only been kept a few days.

The following were the general conclusions drawn from the experiments:—

1. Organic liquids and mixtures are capable of being permanently sterilized by the heat of boiling water. Some are sterilized by an exposure to this heat for a few minutes, others require an exposure of twenty to forty minutes, and others an exposure of one, two, or several hours. A slight difference in the aggregation of the materials or a slight difference in their reaction was sufficient to alter very greatly the amount of heat required for their sterilization.

2. There appeared to be two factors of equal importance in the process of sterilization, namely, the *degree* of heat and the *duration* of its application. These two factors were mutually compensatory, in such fashion that a longer exposure to a lower tempe-

rature was equivalent to a shorter exposure to a higher temperature. For example, speaking roughly, an exposure for an hour to a heat of 100° Cent. was equivalent to an exposure for fifteen minutes to a heat of 109° Cent.

3. Slightly alkaline liquids were always more difficult to sterilize than slightly acid liquids. This was probably due to the fact that albuminoid matter is more easily and more completely coagulated by a given heat in acid than in alkaline solutions.

4. No organic liquid or mixture, subjected for however short a time to the heat of boiling water, ever produced (provided there was no fresh infection) any fungoid or torulaceous organisms*. If germination took place, the organisms produced invariably belonged to the great group of *Bacteria*. This appears to indicate that *Torulæ* and their germs are more easily destroyed by heat than *Bacteria* and their germs. No development of *Bacteria* into fungoid vegetations was ever observed.

5. Sterilization generally proved to be permanent if germination did not occur within four days. Sometimes, however, germination took place on the sixth or eighth day, and very rarely even as late as the tenth and twelfth day. Differences in the degree of warmth at which the preparations were maintained (after the heating) accounted for most of these cases of retarded germination, but not for all. Some I explained in this way:—the flasks were often moved about and disturbed, and possibly their contents were thus brought into contact with unsterilized portions which had spurted about the sides of the flask during ebullition. Neither of these explanations, however, appeared applicable to some of the cases of retarded germination. There was certainly no constant relation between retarded germination and the amount of heat used in the experiment. As a rule, germination, if it took place at all, was as prompt in preparations which had been boiled for an hour as in those which had only been boiled for five minutes. Something further will be said of these cases of retarded germination at the end of the third section.

SECTION II.—ON THE CAPACITY OF THE JUICES AND TISSUES OF ANIMALS AND PLANTS TO GENERATE *BACTERIA* AND *TORULÆ* WITHOUT EXTRANEOUS INFECTION.

It cannot be doubted that if the juices and tissues of healthy plants and animals could be placed, without extraneous contamination, in circumstances favourable to germination, their behaviour under such circumstances would furnish important data in the controversy respecting the origin of *Bacteria* and *Torulæ*. If, for example, it was found that the blood, flesh, milk, and urine of an animal, the contents of an egg, and the juices and tissues of a plant remained permanently barren, although adequately supplied with moisture, air, warmth, and light—so long as extraneous infection was prevented—such a result would furnish the strongest argument hitherto adduced in favour of the view that the appearance of *Bacteria* and *Torulæ* in organic infusions was

* Mention will be made subsequently of exceptions to this rule, or rather apparent exceptions, for they evidently belong to a totally different category.

due to the agency of imported germs, and not to any act of so-called spontaneous generation.

The importance of this path of inquiry was distinctly indicated by PASTEUR; but he does not appear to have pursued it, except in regard to urine, which he succeeded in keeping unaltered without heating*.

More recently Dr. BURDON SANDERSON†, in his admirable studies on contagion, incidentally encountered this question, and arrived at some very remarkable results. He satisfied himself that blood, muscle, urine, saliva, milk, egg-albumen, healthy pus, and blister-serum had no power of breeding *Bacteria* except when infected with ordinary water. Mr. LISTER tested this question more directly, and succeeded in showing that blood, milk, and urine remained permanently barren when preserved from the contact of extraneous germs.

Dr. SANDERSON'S experiments were planned on the supposition entertained by him that *Bacteria*-germs, while abundant in water, were almost absent from the air. In repeating some of his experiments, I failed in obtaining similar results. The air of the rooms wherein I worked was evidently highly charged with *Bacteria*-germs. In pursuing the inquiry, therefore, I sought to avoid air-contamination as sedulously as water-contamination ‡.

The experiments which follow were all carried out on a plan which was, in principle, the same in every case. The materials of the experiment were enclosed in sterilized glass bulbs or tubes, which were plugged at one end with cotton-wool and hermetically sealed at the other. They were then set aside in a warm place to see if they would germinate.

The bulbs and tubes were prepared in the following manner :—They were first drawn out at the lower ends into capillary points (fig. 3, *b*, *b*) and sealed in the flame; the upper ends were plugged at *a*, *a* with cotton-wool.

* Annales de Chimie et de Physique, 1862, p. 66.

† Thirteenth Report of the Medical Officer of the Privy Council, p. 65.

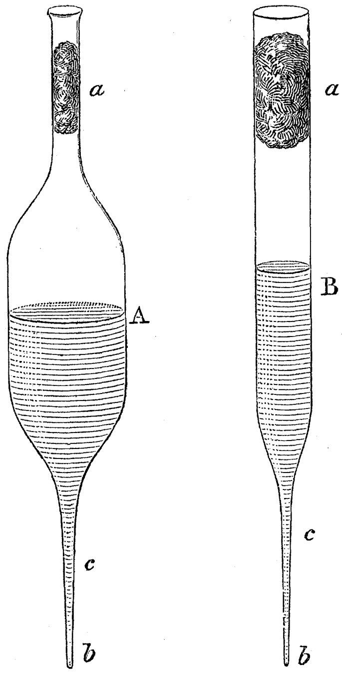
‡ The avoidance of air-contamination is important for another reason. The air is admitted, by most observers, to be highly charged with fungoid germs, and the growth of fungi has appeared to me to be antagonistic to that of *Bacteria*, and *vice versâ*. I have repeatedly observed that liquids in which the *Penicilium glaucum* was growing luxuriantly could with difficulty be artificially infected with *Bacteria*; it seemed, in fact, as if this fungus played the part of the plants in an aquarium, and held in check the growth of *Bacteria*, with their attendant putrefactive changes. On the other hand, the *Penicilium glaucum* seldom grows vigorously, if it grow at all, in liquids which are full of *Bacteria*.

It has further seemed to me that there was an antagonism between the growth of certain races of *Bacteria* and certain other races of *Bacteria*.

On the panspermic theory it may be assumed that what takes place when an organic liquid is exposed to the contamination of air or water is this :—A considerable variety of germinal particles are introduced into it, and it depends on a number of conditions (composition of the liquid, its reaction, precedence and abundance of the several germs) which of these shall grow and take a lead, and which shall partially or wholly lie dormant and unproductive. There is probably in such a case a struggle for existence and a survival of the fittest. And it would be hazardous to conclude because a particular organism was not found growing in a fertile infusion, that the germs of that organism were really absent from the contaminating media.

The next step was to destroy any germs adhering to the interior of the bulbs and tubes or floating in the air within them—in other words, to sterilize them. This was done in the case of the bulbs by introducing water into them before the plugging, and boiling over the flame for ten minutes. The tubes were sterilized by passing and repassing them through the flame of the spirit-lamp until they were quite hot, as shown by commencing charring of the cotton-wool plug. If the conditions of the experiment required that there should be some water in the tubes, their capillary ends were snipped off (after sterilization) and boiling water was sucked into them, and the ends again resealed. Bulbs and tubes thus prepared might be regarded as "sterilized chambers" —that is to say, chambers deprived of germinal particles by the destructive power of heat, and having free access of air, also deprived of germs by filtration through the cotton-wool plug. A number of such sterilized bulbs and tubes (some containing water and some empty) were prepared and laid aside for future use.

Fig. 3.



A. Sterilized bulb. B. Sterilized tube. About half the actual dimensions.

Experiments were made on the following substances:—

1. *Egg-albumen.*—Two sets of experiments were made with egg-albumen.

First set.—At the end of March, 1873, eight sterilized bulbs (fig. 3, A) containing water were charged with egg-albumen, in the following manner:—

A fresh egg was fixed in a convenient support, and a small piece of the shell was chipped off, care being taken to leave the subjacent membrane uninjured. Then a sterilized bulb was taken and the capillary portion (*b c*) immersed for a few seconds in boiling water in order to destroy any adhering germs. The sealed end was then rapidly snipped off, and the capillary portion plunged into the interior of the egg. About two cubic centimetres of the albumen were then sucked up by the mouth into the bulb. When this was accomplished, the bulb was quickly withdrawn, and its capillary end sealed in the flame.

At first the albumen formed a distinct layer below the water in the bulbs; but in a few days the soluble portions diffused into the water above, and formed a transparent colourless solution, while the scanty insoluble portions separated in a hazy cloud or flakes which occupied the lower strata of the fluid.

These eight bulbs were kept through the ensuing summer and autumn, and were finally examined on the 1st of October. Six of them were quite unaltered in appearance, and no trace of any organism could be detected under the microscope. The other two were evidently changed. They had become turbid a few days after being put up, and had become increasingly so for a few weeks, after which they underwent no further change. Under the microscope spherical *Bacteria* were seen abundantly, and a few cells resembling *Torulæ*. There were, however, no staff-shaped *Bacteria* and none of the usual signs of putrefaction.

Second set.—On the 17th of December, 1873, seven sterilized tubes containing water (fig. 3, B) were charged with egg-albumen in the manner above described. Of these, two became turbid with *Bacteria*; the other five continued transparent, and when examined at the end of two months were found quite free from organisms.

2. *Blood*.—The end of the finger was thoroughly cleansed, and blood drawn therefrom by needle-punctures. The blood was sucked into sterilized tubes containing water, and then sealed in the flame with the precautions above described. About two drops of blood were conveyed into each tube. In these experiments there was obviously considerable risk both of air- and of water-contamination. Ten tubes were put up in this way, and examined at the end of four weeks. Four of them had assumed a bright red colour and deposited a sediment, and were more alkaline than the others. These were found to contain *Bacteria*. The other six had an amber tint and no sediment; these were quite free from organisms.

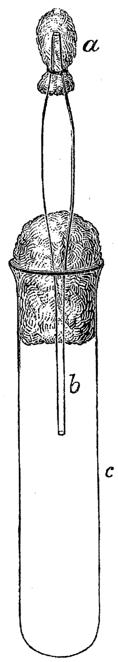
3. *Urine*.—Fresh urine was received into a large superheated test-tube. Six sterilized tubes, not containing any water, were charged with the urine in the usual way, and then sealed at their capillary ends. The urine was alkaline and turbid from precipitated phosphates. On the following day two more tubes were similarly charged with acid urine. Of these eight tubes one became turbid from *Bacteria*, the other seven were found, at the end of eight weeks, perfectly unaltered and free from organisms.

4. *Blister-serum*.—Four sterilized tubes, not containing any water, were charged with blister-serum. The broken capillary ends were thrust directly through the raised epidermis into the sac of the blister, and the serum was sucked into the tubes to the depth of about an inch and a half. The tubes were then quickly withdrawn, and their capillary ends resealed in the flame. At the end of ten weeks all four were found unaltered and perfectly free from organisms.

5. *Milk*.—Milk is very difficult to obtain free from extraneous contamination. In my earlier attempts I used superheated test-tubes plugged with cotton-wool. Test-tubes so prepared were taken into the cowhouse at milking-time, and charged with milk by momentary removal and reinsertion of the plugs. Milk so obtained, however, invariably curdled in a week and swarmed with *Bacteria*.

I afterwards adopted the following plan with somewhat better success:—A glass tube (*a b*, fig. 4) was drawn out at each end to a narrow orifice. The lower portion of this was tightly wrapped round with cotton-wool and inserted as a plug into a large test-tube (*c*) containing water to the depth of about an inch. A cap of cotton-wool was also tied over the narrow orifice at *a*. The water in the test-tube was then briskly boiled, and the boiling was continued almost to dryness. When the apparatus was cold I took it into the cowhouse and, seizing a teat, I pulled off quickly the cotton-wool cap at *a*, and pushed the narrow point (*a*) into the duct of the teat. Holding it firmly in this position, I milked into the test-tube until sufficient milk had been obtained. I then drew the test-tube away from the little tube, pressing in the cotton-wool around it as I did so, until the latter was entirely withdrawn from the test-tube. Operating in this way I trusted to obtain milk, if not entirely free from extraneous germs, at least containing but few of them.

Fig. 4.



About half the actual size.

From a test-tube thus filled I charged ten empty sterilized tubes in the manner already described, and resealed their capillary orifices. Of these ten, three remained unchanged. When examined from three to six weeks afterwards the milk in them was perfectly sweet to the taste, its reaction was neutral or faintly acid, like that of fresh milk, there was no curdling, and no signs of organisms under the microscope. The other seven changed within ten or twelve days. Some of them curdled and others putrefied; all became highly acid, and, under the microscope, *Bacteria*, either staff-shaped or spherical, were found in them.

6. *Grape-juice.*—Eleven sterilized tubes, six empty and five containing water, were charged with grape-juice in the following manner:—A fresh grape was firmly seized with the fingers and thumb, and a spot on its surface was pressed for a few seconds against the flame of a spirit-lamp so as to destroy any adhering germs. The point of the sterilized tube, also heated for a moment in the flame, and quickly snipped off by an assistant, was then thrust into the grape at the heated spot. Compression was now made on the grape until a sufficient quantity of the turbid juice was forced into the tube. The tube was then withdrawn and its point sealed in the flame. The eleven tubes thus charged remained permanently unchanged. When examined at various periods, from five to eight weeks, they were all found free from organisms, and the taste and reaction of their contents were indistinguishable from those of the fresh grape-juice.

7. *Orange-juice.*—Eight sterilized tubes, four empty and four containing water, were charged with orange-juice. The oranges were fresh from the trees*. A portion of the rind was stripped off, and the capillary end of one of the tubes, momentarily heated in the flame, was snipped off and thrust into the orange. The orange was then compressed until a sufficiency of the juice was squeezed into the tube, which was then withdrawn and sealed in the flame. The tubes thus charged were examined at various intervals, from four to eight weeks; all were found perfectly free from organisms, and preserved unaltered the reaction and taste of fresh orange-juice.

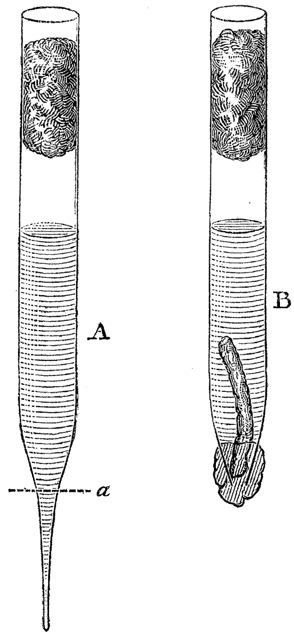
8. *Tomato-juice.*—Three sterilized tubes, containing water, were charged with the

* This and all the other experiments in this section, except the first set with egg-albumen, were performed at San Remo during November, December, and January, 1873–74.

juice of half-ripe tomatoes. The proceeding followed was the same as with oranges. At the end of two months the supernatant liquor in the tubes was perfectly transparent, and no appearance of organisms was found under the microscope.

9. *Turnip.*—I found it much less easy to deal with the solid tissue of the turnip and potato than with the liquid pulp of the grape and orange; but the results obtained, though less uniform, were not less conclusive. The following was the plan adopted:—A sterilized tube (fig. 5, A) containing water was nicked with a file near the base of the capillary part at *a*, where the tube had a diameter of about two millimetres. A fresh oblong turnip was then fractured across, and the tube, snapped off at the nicked point, was quickly thrust into the substance of the turnip. A narrow cylinder of turnip-tissue, about an inch long, was thus forced into the column of water in the tube. The tube was then detached, and its end sealed with melted sealing-wax (fig. 5, B).

Fig. 5.



Fourteen tubes were charged in this way, and examined from time to time in the course of the ensuing two months. Ten remained sterile and four became fertile. The water in the former continued perfectly limpid, and no organisms could be discovered in it when examined microscopically. The water in the latter became turbid, and was found to swarm with *Bacteria*.

10. *Potato.*—Seven sterilized tubes, containing water, were charged with potato-tissue in the way above described for turnip. Of these, four remained barren, and three became fertile with *Bacteria*.

Conclusions.—As already stated, the ideal conditions of the experiments could not in any case be carried out with absolute stringency. Some risk of extraneous infection was always encountered in conveying the materials of the experiments into the sterilized bulbs and tubes. The results obtained are therefore not altogether uniform; but as this was in accordance with the expectation of the experiments, it adds to, rather than detracts from, their validity. Where the conditions of the experiments could be carried out in almost ideal perfection, as with egg-albumen, urine, blister-serum, grape- and orange-juice, the results were nearly uniform; but when, on the contrary, the risk of extraneous infection was obviously considerable, as with blood, milk, turnip, and potato, the results were less uniform, though even in these cases, with the single exception of milk, the sterile tubes were in a majority.

The following Table exhibits a summary of the results:—

Liquid or tissue experimented on.	Number of experiments.	Results.	
		Remained sterile.	Became fertile.
Egg-albumen....	15	11	4
Blood..........	10	6	4
Urine..........	8	7	1
Blister-serum ..	4	4	0
Milk	10	3	7
Grape-juice	11	11	0
Orange-juice....	8	8	0
Tomato-juice....	3	3	0
Turnip-tissue....	14	10	4
Potato-tissue....	7	4	3
Totals.	90	67	23

Thus out of 90 experiments on the juices and tissues of plants and animals, exposed to conditions favourable to germination, absolute sterility was observed 67 times, and development of organisms only 23 times. It was scarcely possible to obtain a more clear demonstration of the general conclusion that the normal tissues and juices have no inherent power to originate organisms, and that when organisms appear therein their development is due to germs imported from without.

SECTION III.—ON THE BEARING OF THE FACTS ADDUCED IN THE PRECEDING SECTIONS ON THE ORIGIN OF *BACTERIA* AND *TORULÆ*, AND ON THE REAL EXPLANATION OF SOME OF THE ALLEGED CASES OF ABIOGENESIS.

We have seen that organic liquids and mixtures sterilized by heat, and the normal fluids and tissues of plants and animals, remain permanently barren, under the most favourable conditions of air, moisture, warmth and light, so long as they are protected from extraneous infection; but if unfiltered air or ordinary water be brought into contact with them, this barrenness is immediately and invariably succeeded by fertility.

Such a sequence of events can only be explained by the supposition that there exist in ordinary air and water, in addition to their proper elements, incredible multitudes of particles capable of provoking germination.

The exact nature of these germinal particles is, however, not a matter of actual knowledge. It is evident that they are organic, because of their easy destructibility by heat. It is further evident that the atmospheric germs are solid particles, for they can be mechanically filtered from the air, and they are incapable of ascending against gravity. It may be assumed that they consist partly of true spores and partly of the organisms themselves, floating amid the dust of the atmosphere or mingled with the molecular matter always present in ordinary water. But it cannot be said that they have ever been actually seen and identified. The ingenious attempts of PASTEUR and others to demonstrate germs in the air are manifestly illusory. Like them I have repeatedly collected air-dust and found abundance of molecules, circles, spheres, and particles of various kinds under the microscope; but these could not be identified as true spores, nor distinguished from particles of inert dust. Indeed the objects sought after are so minute and so wanting in characteristic forms, that such a search, with our present instruments, appears well-nigh hopeless*.

But although an obscurity hangs over the precise nature of these particles, the reality of their existence is not doubtful; nor is it doubtful that the ordinary development of *Bacteria* and *Torulæ* is directly due to their agency. These fundamental propositions of the panspermic theory may be regarded as the expression of acquired facts in science.

I come now to deal with those cases which have hitherto proved a stumbling-block to the general acceptance of the panspermic theory—namely, the development of *Bacteria* (without the possibility of fresh infection) in liquids and mixtures which have been subjected to a boiling heat.

It is truly, as Dr. HUGHES BENNETT has remarked, a "violent assumption" that living particles can survive a boiling heat. Nevertheless it is an assumption that may be justified both on grounds already supplied in the preceding sections and by direct experiment. The *à priori* argument stands thus:—Take milk. It has been shown that pure milk, drawn without extraneous contamination from the teat, has no germinating power. The germination of ordinary milk is therefore due to imported germs; and if the same milk still germinate after boiling in closed vessels, this must be due to the survival of these germs. Or take the case of egg-albumen. Uncontaminated egg-albumen does not germinate when mixed with sterilized water. But if pieces of boiled white of egg be mixed with ordinary water, the mixture invariably germinates after boiling (in a plugged flask) for ten or fifteen minutes. This can only be explained by supposing that the germs contained in the ordinary water have survived the boiling.

But this point was tested by direct experiments with hay-infusion. Two series of experiments were made—A and B.

A. In the first section it has been shown that alkalized hay-infusion will germinate after exposure for more than an hour to the heat of boiling water, but if the heat be continued for two or more hours permanent sterilization is effected. In the course of my experiments a considerable number of bulbs containing alkalized hay-infusion, sterilized in this way, had accumulated on my hands. These bulbs were plugged with

* A striking illustration of the minuteness of the spores of certain organisms is given in a remarkable paper on the life-history of a cercomonad by DALLINGER and DRYSDALE in the Microscopical Journal for August 1873. The spores of this organism were so minute that they could not be individually recognized even with the $\frac{1}{50}$ objective. They could only be recognized with this high power as seen in enormous aggregation and motion in a mass. With a magnifying-power of 2500 diameters they were only just visible as the minutest dots.

cotton-wool, and had been kept in a warm place for several months. The liquids in them were perfectly transparent, and showed no signs of germination. The contents of these bulbs seemed especially favourable for testing the possibility of germs surviving a boiling heat. These bulbs, sixteen in number, were divided into three sets.

The first set (six bulbs) were treated thus:—The plugs were withdrawn and the bulbs were exposed in the vertical position for six hours to the contact of unfiltered air; they were then replugged and boiled briskly over the flame for five minutes. Of these, three germinated within four days, and three remained permanently barren.

The second set (six bulbs) were also unplugged, and a little ordinary water (from one to sixty drops) was introduced into the bulbs; they were then replugged and boiled for five minutes. Four of these germinated and two remained barren.

The third set, consisting of four bulbs, were unplugged, and a few drops of an alkalized hay-infusion turbid from *Bacteria* were added to each; these were then replugged and boiled for five minutes. All of these germinated.

Lastly, the five barren bulbs of the first and second sets, after remaining barren for a month, were unplugged again and infected with alkalized hay-infusion turbid with *Bacteria*. They were then replugged and boiled for five minutes. All germinated in four days.

These experiments prove directly and positively that there exist in ordinary air and water particles which, in certain liquids, are capable of preserving their germinal activity after exposure to a boiling heat for five minutes. They also prove that certain types of *Bacteria*, or their germs, are capable of surviving a similar heat.

If these experiments are regarded in conjunction with those published by Dr. BASTIAN * and F. COHN †, it would seem that the vital resistance to heat of *Bacteria* and their germs varies greatly according to the nature of the liquid in which they subsist, and probably also according to the species or type of *Bacteria*. It does not seem unreasonable to suppose that different races of *Bacteria*, or different phases of their development, are capable of offering very different degrees of resistance to the destructive influences of heat ‡.

B. We have seen that unneutralized hay-infusion is sterilized by exposure to a boiling heat for five minutes; but if the infusion be slightly alkalized by ammonia or potash, it germinates after exposure to the same heat for more than an hour. How does the

* Proc. Roy. Soc., March 20, 1873 (vol. xxi. p. 224).

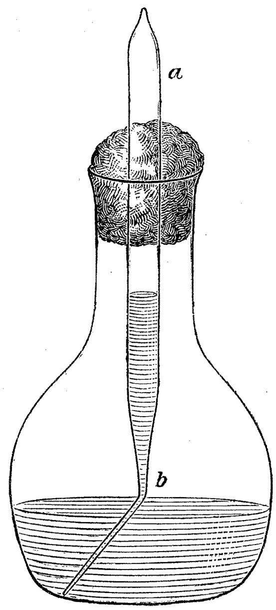
† Beiträge zur Biologie d. Pflanzen, 2tes Heft, p. 217.

‡ I may refer again on this point to the paper of DALLINGER and DRYSDALE already cited. These observers found that while adult cercomonads were killed at a temperature of 60° C. their spores survived a heat of 127° C.

T. PACE (Maandblad v. Natuur-Wettenschappen, May 1873) found that *Bacteria* in turnip-infusion with cheese retained their normal appearances and movements after being heated in closed tubes up to 160° C.; but as their subsequent power of growing and multiplying was not tested, these experiments must be regarded as inconclusive. The same objection lies against the experiments of the late Dr. CRACE-CALVERT (Proc. Roy. Soc. 1871, vol. xix. p. 468).

alkali produce this extraordinary effect? It must be in one of two ways: either it increases the vital resistance to heat of the germs contained in the infusion, or it increases the abiogenic aptitude of the infusion itself. Which of these explanations is the true one? The question was tested in the following manner:—

Fig. 6.



Ten flasks were charged with unneutralized hay-infusion. Five of these were simply plugged with cotton-wool, and boiled over the flame for five minutes. The other five were also plugged with cotton-wool; but through the centre of each plug there passed a hermetically sealed glass tube, of the shape represented in the annexed figure (fig. 6, *a b*), and containing the proper quantity of liquor potassæ to neutralize the infusion in the flask. These tubes had been previously (after being charged with liquor potassæ and sealed) heated in oil up to 121° Cent., in order to destroy any germs they might contain. The flasks thus prepared were then also boiled over the flame for five minutes.

At the end of a fortnight the contents of the ten flasks remained unchanged; they were evidently permanently sterilized. I next proceeded to alkalize the infusions in the flasks without introducing any fresh germs, that is, without removing the plugs.

The flasks of the first set were placed under a bell-jar in company with a beaker containing liquor ammoniæ. In the course of two hours the volatile alkali had diffused through the plugs and had neutralized the infusions, as was evidenced by the formation of abundant deposits in them. These flasks were then removed and set aside in a warm place to see if they would germinate.

The flasks of the second set were each treated as follows:—The little tube *a b* was pressed down against the bottom of the flask until it broke at the bend *b*, then the flame of the blowpipe was cautiously directed against the upper end, *a*. The expansion of the contained air thus induced was sufficient to expel the liquor potassæ into the liquid below; immediate turbidity ensued. The flasks were then placed beside their companions of the first set.

Not one of these ten flasks germinated; at the end of two months they were still barren. Now this result was strictly conformable to the view that the effect of the alkali was to increase the power of survivance of the germs, but it was wholly unconformable to the alternative view. According to the former view, the germs contained in the infusions were destroyed by the preliminary boiling, and no subsequent addition of alkali could, of course, restore their vitality. But on the opposing theory there was no reason why the alkali should not have been equally effective in promoting germination, whether added before or after the short preliminary boiling*.

* In fact this was so. It will be remembered that in preparing the hay-infusions they were first made very

By carrying the experiments a step further it was shown that, although the contents of the flasks had not acquired the power to germinate, they had acquired the property of enabling freshly introduced germs to survive a boiling heat; for when the flasks were unplugged and infected with ordinary air or water and then replugged and boiled five minutes, their contents in every instance germinated in a few days.

These experiments appear to warrant the following conclusions:—

1. That the germinal particles of air and water (or some of them) are capable of surviving the heat of boiling water in certain media.

2. That when we speak of different liquids and mixtures as possessing different degrees of resistance to sterilization by heat, it would be more exact to say that the germinal particles of air and water possess varying degrees of vital resistance to heat according to the nature of the media in which they subsist.

The issue of the foregoing inquiry has been to confirm in the fullest manner the main propositions of the panspermic theory, and to establish the conclusion that *Bacteria* and *Torulæ*, when they do not proceed from visible parents like themselves, originate from invisible germs floating in the surrounding aërial and aqueous media.

Nevertheless I cannot withstand the impression that this general and common mode of origin may, under rare conditions, be supplemented by another and an abiogenic mode of origin.

The facts on which this impression rests are only isolated and quasi-exceptional units among countervailing hundreds; but the record of this inquiry would be incomplete without an account of them.

The facts alluded to consist, first, of cases of retarded germination of *Bacteria*; and, secondly, of two cases in which fungoid vegetation, of a type new to me, appeared in plugged bulbs after being heated in boiling water.

In studying the sterilization of organic liquids by heat, I found, as a rule (as already stated), that if germination did not take place within four days it did not take place at all; that is, the media proved to be permanently sterilized. But occasional exceptions to this rule were encountered, in which the media remained sterile for six, eight, and, very rarely, for ten and twelve days, and then germinated. The most remarkable of these exceptions were, however, the two following:—

1. *November* 21, 1871.—Some dried marrow-fat peas were soaked in water for thirty hours, until they had fully recovered their original plumpness. About a dozen of these were enclosed, with water, in a plugged flask and boiled over the flame for ten minutes. A second flask was charged and boiled in the same way, except that the

strong, and afterwards diluted down with water to the standard density of 1006. But on several occasions I was obliged to boil the infusions before diluting and alkalizing, in order to keep them sweet until my engagements permitted me to go on with the further steps of the experiments. In these instances the resistance to sterilization was just as great as when there was no preliminary boiling.

peas were first finely crushed in a mortar. Both these flasks remained unchanged for a period of seven months; but in the beginning of June 1872 the contents of the first flask became turbid and covered with a film, and, when examined under the microscope, revealed abundance of short staff-shaped *Bacteria*. The reaction, originally neutral, was now faintly alkaline. The second flask remained apparently unchanged for sixteen months; and when the contents were examined at the end of that time no trace of organisms could be found under the microscope, and the reaction was neutral.

2. *May* 4, 1872.—Two plugged flasks, charged with very thin gruel, were gently boiled over the flame for fifteen minutes. These flasks remained apparently unaltered for six weeks, and then one of them began to exhibit rapid changes. Its contents became more diffluent and yellower and covered with a pellicle. On July the 13th this flask was opened for examination. Its contents were faintly acid (originally neutral), and had a smell of sour dough (all the starch had disappeared), and abundance of spherical *Bacteria* were seen with the microscope. The other flask, also opened for examination on the 13th of July, was quite unaltered. Its contents were neutral, odourless, reacted strongly with iodine, and contained no trace of living organisms.

Results of this kind, standing alone, would not have produced much impression on my mind, because, although exceptional in some respects, they were of the same order as those witnessed in defective sterilization, the organisms discovered being of the same kind as those ordinarily produced by extraneous infection. They might therefore have been regarded simply as faulty experiments*.

But the two following results very strongly arrested my attention. Not only did organisms appear under exceptional conditions, but they were of a species distinctly different from the organisms with which I was so familiar as the result of extraneous infection.

1. *July* 17, 1872.—A plugged bulb, charged with a highly albuminous urine, was boiled in a can of water for twenty minutes. When cold the neck of the bulb was filed off above the cotton-wool, and it was set aside in a warm place. An abundant precipitation of albumen had occurred, and the supernatant fluid was perfectly transparent.

This remained unaltered until the beginning of March 1873, a period of nearly eight months. At this time I noticed the appearance of numerous whity-brown specks, like small pins' heads, scattered on the sides of the glass at and near the surface of the fluid. To the naked eye they looked like specks of solidified fat, but with the aid of a lens they were seen to have a radiated structure. These specks steadily, though slowly, grew and multiplied, until at the end of six weeks some of them were as large as hempseeds. The supernatant fluid still continued perfectly transparent. On the 28th of April the bulb was opened for examination. The urine was, as at first, acid. Under the

* It seemed possible that in some of these cases minute insects might have crept into the flasks or bulbs through the cotton-wool plugs, and thus communicated infection to their contents.

microscope the little masses were found to consist of an interlacement of fibres, exactly resembling the mycelium of the *Penicilium glaucum* or of the sugar-fungus, except that the fibres had only about half the diameter of the former. In structure the fibres more resembled those of the sugar-fungus than those of the *Penicilium glaucum*, for they had globular enlargements on them here and there. Amid these fibres were innumerable very minute spores, much smaller than the *Torulæ* usually found in urine. I could not clearly make out any aërial fructification. No *Bacteria* were found.

Several other specimens of albuminous urine treated in the same way were found wholly barren at the end of eighteen months.

2. *February* 25, 1873.—A plugged bulb, containing pieces of Swedish turnip, floating in water alkalized with 0·2 per cent. of sod. bicarb., was boiled in a can of water for thirty minutes. When cold the neck was filed off above the cotton-wool, and the bulb was placed in a warm greenhouse. On the 7th of March the supernatant liquid had become turbid, but on the 21st of March it had again become perfectly transparent, and an immense woolly growth had rapidly sprung from the surface of the liquid and filled up the entire upper part of the bulb, even extending into its neck. Under the microscope this woolly mass was found to consist of an interlacement of fibres exactly resembling the mycelium of the *Penicilium glaucum.* Amid the fibres were numerous minute dots like very young *Torulæ.* There was no proper aërial fructification. No *Bacteria* were found.

In both these instances the organisms evidently belonged to the class of fungoid vegetations represented by the *Penicilium glaucum* and the sugar-fungus. But their mode of growth was so entirely different, that it was equally evident that they were not identical with either of those species, and, further, that they were totally different from each other. These were the only two instances, out of many hundred experiments, in which I witnessed the development of a fungoid vegetation in a liquid which had been exposed, for however short a time, to the heat of boiling water.

The facts just enumerated are far too few, and of too exceptional a character, to permit a deduction in favour of abiogenesis; but they certainly impose a reserve which is highly significant. If future investigations should establish the occurrence of abiogenesis, this would not overturn the panspermic theory, it would only limit its universality; and it may be predicted with some confidence that if abiogenesis exist the conditions of its occurrence can only be determined by an inquirer who is fully alive to the truth and penetrating consequences of the panspermic theory.