Researches on the Effect of Light upon Bacteria and other Organisms

Author(s): Arthur Downes and Thos. P. Blunt

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called a precipice, with a fall of about a thousand feet. A gentle breeze was blowing from the mountains, which were partly snow-covered, and partly of bare rock, towards the precipice. Taking care to cleanse my pliers in the flame of a spirit-lamp, and to keep my body to leeward of the flasks, I snipped off their sealed ends.

The two groups of flasks were then placed in our own little kitchen, where the temperature varied from about 65° to 90° Fahrenheit.

Result:—Twenty-one of the twenty-three flasks opened on the hay-loft are filled with organisms; two of them remain clear.

All the flasks opened on the edge of the precipice remain as clear as distilled water. Not one of them has given way.

This is a striking confirmation of the experiments of Pasteur upon the Mer de Glace.

Ever, my dear Huxley,
Yours faithfully,
JOHN TYNDALL.

III. "Researches on the Effect of Light upon *Bacteria* and other Organisms." By ARTHUR DOWNES, M.D., S.Sc. Cert. Cantab., and THOS. P. BLUNT, M.A. (Oxon.), F.C.S. Communicated by J. MARSHALL, F.R.S., Professor of Anatomy to the Royal Academy of Arts. Received October 18, 1877.

The investigation to which the following communication relates was undertaken by us with the view of ascertaining, first, whether light could be shown to exert any appreciable influence, favourable or the reverse, upon the development of *Bacteria* and other organisms in certain of those solutions which afford a suitable medium for their appearance and increase.

We feel justified in thus presenting our earlier researches and the conclusions drawn from them, by considering that every fact, however small, which tends to throw light upon the life-history of these organisms is of importance as bearing upon questions of the highest moment and most varied interest.

In the experiments about to be recorded the contents of the tubes were in most cases examined under a high power, and the turbidity, when such occurred, was invariably found to be occasioned by swarms of *Bacteria*. The best index of the development of the *Bacteria* we found to be the degree of turbidity and the time of its commencement.

Obs. 1. *April* 24.—Eight ordinary thin test-tubes were cleansed with strong sulphuric acid and thoroughly rinsed with tap-water. They were then partially filled with freshly made unboiled Pasteur's solution, the exact composition of which is given in the Appendix (*A*). Four of the tubes were encased in thin sheet-lead so as entirely to exclude light, and

four were left quite bare. The tops of all were loosely covered with sheet-lead capsules, and the whole were placed in a test-tube-stand outside a window facing south-east, and about thirty feet above the ground. None of the tubes were plugged.

May 4.—Tubes examined as to turbidity. The solution remains perfectly clear in the four bare tubes, while in each of the encased it has become distinctly and uniformly milky. This turbidity was proved microscopically to be caused by innumerable *Bacteria*. The bare tubes remained quite clear till May 28th, when they were unfortunately accidentally lost.

This observation was again and again repeated with similar result.

In the large majority of cases the exposed tubes remained clear for an indefinite time, and in every instance were conserved for a distinct period after their encased companions had become turbid. The most marked differences in the conduct of the two sets of tubes were obtained when the sun shone brightly; when for a period of a day or two at the commencement of the experiment the weather was close and sultry and the sky dull, the conservative effect of light appeared to be less pronounced. Thus, in an observation started on June 12, it was found on the 14th that while there was a thick zooglœa and advanced turbidity at the upper part of the solution in the encased tubes, there had already commenced a much slighter but recognizable cloudiness in two tubes filled with the same Pasteur's solution, but freely exposed to the light. This result we attributed to the fact that throughout the whole of the 12th and 13th there was not one ray of direct sunlight, the sky being completely overcast and the atmosphere remarkably thick and hazy.

Obs. 2. *May* 5.—Two of the bare tubes used in Observation 1 were taken and the contents were found to be equally and perfectly clear. One was then encased, the other left as before, and both again replaced on the window-ledge for exposure.

May 16.—the contents of the encased tube are now distinctly turbid, those of the bare tube being perfectly clear. (The latter remained clear till May 28th, when it was destroyed.)

From this observation it is evident that the fitness of the cultivation-fluid as a nidus for the development of *Bacteria* is not impaired by the action of light; for we find that the contents of a tube, which remain perfectly clear so long as they are freely exposed to the sun's rays, swarm with *Bacteria* after being deprived of the access of light. This being so, it becomes important to determine whether light may exert, either directly or indirectly, a destructive influence on *Bacteria*. The obvious mode of settling this question would be to protect the tube to be insolated from subsequent impregnation, and finally to encase it. If then it remained clear for an indefinite period it might be fairly inferred that the *Bacteria* had been destroyed, either in their rudimentary condition, or successively as they came to maturity.

Obs. 3. *May* 30.—Two tubes were partially filled with Pasteur's solution (Appendix, *B*). Both were plugged with cotton wool. One was encased in paper, the other left bare.

June 5.—The encased tube was quite turbid.

June 12.—The bare tube, which had remained perfectly clear, was now encased, and continued quite free from turbidity up to June 21.

June 28.—Solution quite clear, but small tuft of mycelium growing at bottom.

July 7.—Examined with a high power. Mass of matted mycelium ; no *Bacteria* or other living organisms seen.

July 11.—The solution now teems with *Bacteria*, having doubtless been impregnated by the dipping-rod used on July 7.

Obs. 4. *May* 30.—Two tubes containing unboiled Pasteur solution (Appendix, *A*) were plugged with cotton wool, capsuled, and insolated until June 21, when both were encased in the way previously described, the plug being withdrawn from one, and the lead capsule replaced.

Both tubes remaining clear up to July 2, the unplugged one was impregnated by means of a glass rod dipped in a solution containing abundant *Bacteria*.

July 4.—The impregnated solution is distinctly turbid. The plugged tube has remained perfectly clear up to the present date [October 11th].

From these observations we conclude that so far as *Bacteria* are concerned the solution may be absolutely and perfectly sterilized by sunlight. It is important to note, however, that in Observation 3 the germs of a fungus had apparently survived an amount of insolation which was fatal to the development of *Bacteria*. This interesting point will be further investigated in the sequel.

Obs. 5. *May* 5.—Two of the tubes used in Observation 1 were taken; the contents of the one (encased) were turbid, and the other (bare) perfectly clear. The contents were well mixed, and divided between the two. They were then exposed as before.

May 8.—The contents of the encased tube are much the more turbid, those of the bare tube are but slightly, if at all, more turbid than on May 5th.

This experiment tends to indicate that not only is light inimical to the original development of the individual, but also materially retards the rate of increase, even when the organisms are present in a matured condition. The next observation was designed as a crucial test of this.

Obs. 6. *July* 10.—Seven tubes containing Pasteur's solution (A) were inoculated with a glass rod dipped in a solution teeming with *Bacteria*, care, however, being taken not to impair the translucency of that in the tubes in question. Six of these tubes were then insolated, the seventh being encased. The encased tube became turbid on July 14, the rest remained perfectly clear.

Our next object was to determine what period of exposure to light might be sufficient to sterilize a given solution.

Obs. 7. *July* 10.—Seven full-sized test-tubes were partially filled with solution A and plugged with cotton wool. Six were exposed to the light for periods varying from nine hours to seven days, and were then encased. There was but little direct sunlight upon them during this period, the tube exposed for nine hours having $3\frac{1}{2}$ hours' sun, and the one encased at the end of seven days receiving in the aggregate about 12 hours of direct sunlight.

All, however, were sterilized, while the seventh tube, which was encased from the first, and served as a control, became cloudy with *Bacteria* on July 14th.

Obs. 8.—On July 29th, a very hot day, with much sunshine, six tubes (series *a*) containing solution A were each inoculated with two drops of a similar solution, which was swarming with *Bacteria*. They were all then plugged with cotton wool, exposed to the light for periods varying from thirty minutes to eleven hours, and then encased, the duration of sunlight received in each case being carefully noted.

On July 31st four, which had received half, one, two, and three hours' exposure respectively, were turbid with *Bacteria*. The other two, however, which had been exposed, the one for five and the other for eleven hours, were clear at this date; but some days later the former contained some *Bacteria* and a tuft of mycelium with sporidia, while the latter showed a similar growth of mycelium but no *Bacteria*. These tubes had received four and a half and nine hours' direct and powerful sunlight respectively.

On the same day six tubes (series *b*) containing the same solution, but not inoculated, were similarly plugged, insolated, and encased. These tubes all contained countless *Bacteria* on August 2nd. This somewhat dissimilar result we attribute to the fact that for series *a* we purposely chose very narrow tubes, not exceeding a third of an inch in diameter, while those of series *b* were about two thirds of an inch. The fact, however, that, while in Observation 7 we succeeded in sterilizing a solution by an exposure of nine hours, only three and a half hours of which were direct sunlight, nevertheless in Observation 8 (series *b*) we find the same solution breaking down after eleven hours, nine of which were true insolation, we can only explain by supposing that external conditions—notably temperature—may retard or counteract the preservative quality of the solar rays. This point is one which calls for careful investigation. It must be understood, however, that the putrefactive tendency of warmth does not in our experience, with this solution at least, override what we have termed the preservative quality of light; for, provided that there was a full amount of sunlight, we have preserved tubes exposed *continuously* from day to day as readily in hot weather as in cool.

An important result, we venture to think, in connexion with series *a* of Observation 8, is the appearance of mycelial fungus in the two tubes which were longest insolated.

In the course of our investigation we have found that, when *Bacteria* appear early and in large numbers in the solutions we have used, the mycelium of *Penicillium* or other microscopic fungus is rarely seen, the *Bacteria* apparently preoccupying the ground; when, however, the development of the *Bacteria* is from some cause retarded or prevented, we have frequently found tufts of delicate mycelium submerged in our experimental solutions, after they have been encased or removed into diffused light. An example of this is seen in Observation 3, and other instances might be cited. It is to be observed, however, that no mycelium appeared during the period of exposure of a solution, except under the conditions hereafter stated, nor, indeed, afterwards, if this were sufficiently prolonged.

We infer accordingly that light may retard or altogether prevent the appearance of mycelial fungi, but that its influence in this respect is slower and less powerful than upon the *Schizomycetes*.

May not this explain, in part at least, the sparing distribution of *Bacteria* in ordinary air as compared with the prevalence of the spores of *Penicillium* &c., a fact observed by Burdon Sanderson and others, and which our own experience tends to corroborate?

In the course of our investigation we found that, within certain limits, the rapidity with which *Bacteria* appeared in the solution A was proportionate to its *dilution*. This is illustrated in the following observation, in which we took advantage of the greater resistance to decomposition of the stronger solutions for the purpose of ascertaining whether diffused light exerts any appreciable influence on the processes under consideration. We had already observed that diffused light did not prevent the appearance of *Bacteria* in a solution when made in the strength given in the Appendix (*A*).

Obs. 9. *July* 24.—Four solutions are prepared, so that—

I. is of the ordinary strength.
II. is twice as strong.
III. is $3\frac{1}{2}$ times as strong.
IV. is 5 „ „ „

One tube of each solution respectively is placed—

(1) In the dark;
(2) In the diffused light of a somewhat badly lighted room;
(3) In broad diffused daylight.

The result is seen in the following Table:—

(1) In the dark:

I.	became turbid with *Bacteria*,				July 27
II.	„	„	„	„	„ 28
III.	„	„	„	„	„ 29
IV.	„	„	„	„	Aug. 2

(2) In a dull light:

I. became turbid with *Bacteria*, July 26
II. „ „ „ „ „ 28
III. „ „ „ „ „ 29
IV. contained a tuft of mycelium, Aug. 2, and became turbid with *Bacteria* and mycelium, &c., Aug. 5.

(3) In diffused daylight:

I. contained *Bacteria* ..... July 29
II., III., IV. remained clear.

It is right to state that the tubes of series 3 were inadvertently exposed to about twenty minutes of sunlight on July 24th; but we do not think that this materially interfered with the result, which demonstrates the preservative influence of diffused daylight alone, although in less degree than that of the direct solar rays.

The greater tendency of the more dilute solution to decomposition has been pointed out. We have again and again endeavoured to sterilize a solution one tenth of the strength given in the Appendix (*A*), but without success. Whether this failure was due to the unfavourable state of the weather and clouded skies which have invariably supervened, or whether in a solution of this strength the development of the *Bacteria* proceeds with such rapidity that a warm night may in its favouring tendencies outbalance the retarding influence of the day, we cannot say. We have, however, notwithstanding one or two failures in dull, close weather, repeatedly succeeded in sterilizing *urine*, and have at the present time in our possession tubes containing that liquid which has been preserved perfectly fresh and clear through the summer months.

One example is given.

Obs. 10. *July* 26.—Three test-tubes were partially filled with fresh urine of a golden sherry tint, and the mouth of each was guarded by a pledget of cotton wool. Two were insolated, one encased.

Aug. 1.—The contents of the encased tube were turbid and putrid, but the urine in the tubes which were exposed to the light remained perfectly pellucid. One of these (*a*) was now encased, the other (*b*) was left as before.

In about a week two small tufts of mycelium had appeared at the bottom of the tube marked *a*, the solution in which, however, was otherwise perfectly clear.

Oct. 13.—The urine in tube *b* was as clear as when the experiment was first started, nor could any thing except mycelium with sporidia be discovered in tube *a* on close examination with an immersion $\frac{1}{12}''$. The urine in this tube had a strongly acid reaction. On the other hand the urine in the tube which was encased from the first was so offensive as to render the examination of even a drop a disagreeable task. It contained

rods and dumb-bells in great numbers, and an abundance of the micrococci associated with the ammoniacal fermentation of urea. The reaction was alkaline. Prolonged insolation, it may be noted, had a bleaching effect on the urinary colouring-matter.

Most of our preliminary observations have been made with Pasteur's solution and with urine, but more recently we have experimented on some hay-infusion.

Obs. 11. *Sept.* 22.—Three capillary tubes were filled with infusion made from some very old hay. One end of each was sealed off, the other end having a small plug of cotton wool. The infusion was of a deep yellow-brown colour. Two of the tubes (*a*) were insolated, and one (*b*) encased.

A portion also of the infusion was boiled for five minutes in a test-tube, the mouth of which was closed during ebullition with cotton wool, the tube (which was labelled *c*) being then placed in the dark.

Oct. 7.—Each sample was closely examined under the microscope. In the encased tube *b* large numbers of moving rod-like *Bacteria* were seen; but in the insolated tubes *a* a very few moving particles alone were visible to an immersion $\frac{1}{1\frac{1}{2}}''$. The solution *c*, which had been boiled, contained a large number of rods, of greenish tint, with slightly clubbed refractive ends, for the most part motionless, and usually single. None of these were observed in the capillary tubes.

In some observations with turnip-infusion made in May last we found that while it became extremely rotten and offensive in those tubes which were encased, in corresponding tubes exposed to the light it was comparatively odourless, although the development of the *Bacteria* had not been wholly prevented.

Judging from the following experiment with *zymase*, light would not appear to exercise any retarding influence on the "indirect ferments."

Obs. 12.—Some yeast water, four or five times filtered, was mixed with weak syrup (which previously to the experiment was proved to have scarcely any action on Fehling's solution) and placed in two test-tubes. One was encased in the usual way, the other was exposed for two hours to full daylight, including about three quarters of an hour of direct sunlight.

At the end of this time,

25 grain measures of Fehling's solution were reduced by 110 grain measures of the insolated liquid.

25 grain measures of Fehling's solution were reduced by 112 of the solution from the encased tube.

These results may be regarded as being, within the limit of experimental error, practically the same.

What is the true account of this influence of light which has been shown to act so destructively on organisms by no means deficient in their tenacity of life? We do not profess to enter into this question at pre-

sent any further than to state the results of one or two of the numerous experiments which we have made in this direction. The first question which presents itself is, with what rays of the spectrum is this property of light coincident? Is it localized in any one part, or is the unbroken pencil of rays necessary? The most definite result which we have hitherto obtained is embodied in the following experiment.

Obs. 13.—On Oct. 8th eighteen small test-tubes were partially filled with a solution, which was purposely made of twice the strength given in the Appendix, and were then plugged with cotton wool.

Three of these tubes were placed in each of four boxes, the sides of which were made of blood-red, yellow, deep blue, and ordinary glass respectively. Of the remaining six tubes, three were encased, and three were simply exposed to the light. All were placed as usual on an outside window-ledge facing south-east.

Oct. 11.—The encased tubes became turbid with innumerable *Bacteria*.

Oct. 12.—The tubes in the yellow box were all clouded with *Bacteria*.

Oct. 13.—The tubes in the red box showed signs of commencing turbidity, all the other tubes remaining up to this time quite pellucid.

Oct. 14.—All the red tubes were very turbid; two of the tubes in the box of ordinary colourless glass were also slightly turbid.

Oct. 17.—One blue tube has become slightly clouded with *Bacteria*.

The tubes as they broke down were all carefully examined with a $\frac{1}{12}''$ immersion. No organisms other than the ordinary rod-shaped *Bacteria* were seen in any, nor did these differ as regards their apparent vitality and activity.

The remaining tubes in the blue glass case, those simply exposed to the light (*i. e.* not placed in any glass case), and the surviving tube of the three exposed in the box of ordinary window-glass have remained perfectly clear to the date of writing (Oct. 17th). This experiment points forcibly to the actinic rays of the spectrum as the active agents; we hesitate, however, to affirm this positively for the present.

In the course of our investigation we have repeatedly had occasion to notice the proneness of mycelial fungi to appear (to the exclusion of *Bacteria*) in solutions which were themselves of a yellow colour, or were subjected to yellow light. Thus tubes containing urine exposed to light have several times developed a tuft of mycelium either during insolation or afterwards when encased.

The following observation illustrates the same fact as regards the artificial solution with which most of our experiments have been made.

Obs. 14. *May* 30.—Three small test-tubes containing solution A were suspended in three larger tubes containing solution of picric acid of such strength as to represent three gradations of colour from a barely perceptible yellow to a deep tinge of the same. Three corresponding arrangements were made with pure distilled water in place of the picric acid

solution. A tube containing some of the same Pasteur's solution was encased in the usual manner and all were plugged with cotton wool.

June 5.—The encased tube became turbid.

June 8.—A tuft of mycelium appeared in the *medium* yellow.

June 9.—The *deep* yellow became clouded with *Bacteria*.

June 11.—Tufts of mycelium appeared in the *faintly* yellow.

The tubes suspended in distilled water became sterilized, nor could any thing save mycelial growths be discovered in either of the solutions exposed to yellow light, with the exception of that suspended in the yellow of deepest tint, in which *Bacteria* had appeared at an early period and excluded the mycelium. This result we attribute to the apparent fact that the less deep shades of yellow allow rays to pass which may at least check the development of *Bacteria*, but are less potent in their influence on the germs of the higher fungi, which accordingly develop the more readily since they have not to struggle with the former for the mastery. By this indirect influence, therefore, rather than by any special and direct action, we explain the tendency of mycelium to spring up in yellow light. This explanation, moreover, is in accordance with our deduction that, although the mycelial fungi are injuriously affected by light, they are nevertheless more resistant to its influence than *Bacteria*.

The breaking down in Observation 13 of the tubes in the case of window-glass is remarkable when contrasted with the survival of the solutions in the blue case. We have, however, invariably found it a difficult matter to sterilize an ordinary cultivation-liquid when a second screen of glass was placed between it and the light.

Early in the investigation it occurred to us that oxygen might be found to play a part in the phenomena under observation. Tubes containing solution A were therefore exhausted at the Sprengel pump and sealed off, our intention being, in the first place, to observe the effect of insolation in the absence of an atmosphere, filtered air being afterwards admitted. To our surprise we found in a preliminary experiment that on breaking the sealed points of the tubes under cotton wool, after the vacuum had been maintained for two days, the solutions were perfectly sterilized and could be preserved indefinitely. This fact (which, we have since been informed, was stated by Professor Tyndall in the Transactions of the Royal Society this year) cut off for the present this mode of approaching the problem.

The deductions which we draw from these simple experiments may be summed up as follows:—

1. Light is inimical to the development of *Bacteria* and the microscopic fungi associated with putrefaction and decay, its action on the latter organisms being apparently less rapid than upon the former.

2. Under favourable conditions it wholly prevents that development, but under less favourable it may only retard.

3. The preservative quality of light, as might be expected, is most

powerful in the direct solar ray, but can be demonstrated to exist in ordinary diffused daylight.

4. So far as our investigation has gone it would appear that it is chiefly, but perhaps not entirely, associated with the actinic rays of the spectrum.

5. The fitness of a cultivation-liquid to act as a nidus is not impaired by insolation.

6. The germs originally present in such a liquid may be wholly destroyed, and a putrescible fluid perfectly preserved by the unaided action of light.

Although there are many vital phenomena, both of plant-life and of animal, whether in health or disease, to the elucidation of which may be applied this quality of light (now demonstrated, so far as we are aware, for the first time), we have endeavoured in this paper to confine ourselves to the plain facts of our observations, and have studiously avoided speculation and theory. We cannot, however, refrain from offering one comment on the striking antagonism between these facts and many views that have hitherto prevailed on the relation of light to life. This relation has been principally investigated as regards the chlorophyl-cell; but chlorophyl may be regarded as simply an organ of nutrition adapted to special circumstances, and differing essentially in its vital phenomena from the true cellular tissue of the plant and its protoplasmic contents.

It appears to us that the organisms which have been the subject of our research may be regarded simply as individual "cells" or minute protoplasmic masses specially fitted by their transparency and tenuity for the demonstration of physical and other influences. May we not expect that laws similar to those which here manifest themselves may be in operation throughout the vegetable, and perhaps also the animal kingdom wherever light has direct access to protoplasm? On the one hand we have chlorophyl, owing its very existence to light, and whose functions are deoxidizing; on the other the white protoplasm, or germinal matter, oxidizing in its relations, and to which, in some of its forms at least, the solar rays are not only non-essential, but even devitalizing and injurious.

This suggestion we advance provisionally and with diffidence; nor do we wish to imply that the relations of light to protoplasmic matter are by any means so simple as might be inferred from the above broad statement.

APPENDIX.

The artificial solutions employed were similar to those used by Pasteur. The following are the formulæ employed:—

A.—Water, 1500.
Brown Sugar Candy, 70.
Tartaric Acid, 4.
Ammonium Nitrate, 4.

Potassic Carbonate, 0·6.
Ammonium Phosphate, 1.
Solution neutralized with Ammonia and filtered.

B.—Approximately the same as last, but the ingredients not weighed.

Unless otherwise stated, the tubes were always prepared and exposed as in Observation 1.

It is self-evident that tubes exposed with a south-easterly aspect would receive but a fraction of the total solar rays each day. We have not, however, been able to place them in a position where they would be under the direct influence of the sun during the whole period that it was above the horizon; with such an arrangement we should expect to obtain results proportionately greater.

[Received November 5, 1877.]

Postscript.

We have stated in the preceding paper that, on exhausting tubes containing solution A by means of a Sprengel pump and sealing them, we found that not only, as might be expected, did no development of organisms occur under these conditions, but that if the vacuum was maintained for a sufficient length of time, the solution became absolutely *barren*. Knowing the necessity of oxygen to *Bacteria* (of the ordinary kind at least), and taking into consideration the products of their "respiration," we inferred that this result was attributable to the absence of oxygen, and consequent asphyxia not only of the mature forms of those organisms visible to the higher powers of the microscope, but also, it necessarily followed, of that rudimentary "germinal" material which, eluding even the piercing test of the electric beam, is distributed with extraordinary uniformity in almost every water.

We observed, also, that we have since learned that this mode of sterilization has been recently demonstrated by Professor Tyndall, with a similar interpretation, and on this account we did not consider it worth while to enter upon any details of our own experiments in this direction. Within the last few weeks, however, we have, by employing *urine* as the experimental fluid, obtained results of considerable interest, and have thought it well to append some account of them.

Observations with tubes exhausted at the Sprengel pump.

Obs. 1. *July* 18.—Two tubes containing Pasteur solution were exhausted. The one (*a*) was at once sealed off; the other (*b*) was left attached to the pump (the vacuum being maintained) for three hours, air carefully filtered through cotton wool being then gradually admitted. On July 23 the contents of *b* were found to be turbid.

On July 26 air was admitted with similar precautions into tube *a* by

breaking the sealed capillary end within a ball of cotton wool. This tube is still perfectly pellucid [Nov. 3].

We concluded from this experiment that while three hours' vacuum was insufficient to sterilize the solution employed, one of eight days duration rendered it absolutely barren, the latter fact being confirmed by several repetitions of the experiment, one of which demonstrated the sterilization of the solution by two days' vacuum.

On substituting *urine* for Pasteur solution different results were obtained.

Obs. 2.—A tube containing fresh acid urine was on September 19 exhausted and sealed off. On September 22 the capillary point was broken under cotton wool. On September 29 the contents were found to be turbid with *Bacteria*.

Having thus ascertained that urine resisted the sterilizing effects of a vacuum for a period sufficiently long to enable us to test the effects of insolation on this fluid *in vacuo*, we proceeded to carry out our original plan of investigating the more intimate nature of the processes by which light exerts its germicidal action. The result of this inquiry, so far as it has gone, is shown in the following observation :—

Obs. 3. *Oct.* 26.—Of eight test-tubes containing fresh acid urine, two, which we will call *aa*, were simply plugged with cotton wool and insolated in the ordinary way; two, labelled *a'a'*, were plugged and encased. The four remaining tubes were exhausted at the Sprengel pump and sealed; two, *bb*, were exposed for insolation, the remaining two, *b'b'*, being encased.

Oct. 30.—*a'a'* were both swarming with *Bacteria*.

Oct. 31.—The exhausted tubes, *both insolated and encased* (*bb* and *b'b'*) were distinctly and nearly equally turbid, the degree of cloudiness being, if any thing, more marked in the insolated tubes *bb*.

The urine in the tubes *aa*, which were insolated in the ordinary manner, contained numerous small points of submerged growing mycelium, but with this exception was perfectly bright and clear.

Nov. 2.—On examination with a $\frac{1}{12}''$ immersion objective numerous rods in active movement were seen in each of the four tubes which had been exhausted.

The tubes *a'a'* also swarmed with bacterial life; their contents were putrid and slightly alkaline in reaction. The contents of the exhausted tubes had in each case a disagreeable putrefactive odour, but the reaction was still acid.

The urine in tubes *aa* was acid and fresh in odour. With the exception of numerous mycelial tufts and one or two moving rods in the meshes, nothing was seen in these tubes on microscopical examination. This experiment was a repetition of two previous observations which gave similar results and need not be detailed.

In all cases exhaustion at the Sprengel pump was carried on until ebullition occurred in the liquid operated upon, and the mercury had for

a minute or two fallen with a well-marked "water-hammer" click. No gauge was attached to the pump. We do not, of course, regard such a vacuum as perfect; but it was sufficient for our purpose, and, as regards the Pasteur solution, proved fatal to the contained organisms.

In the experiment of which *Obs.* 3 is here given as an example we observe, on the one hand, the prevention of bacterial development and consequent growth of mycelial forms (the quantity of light being insufficient for the destruction of these) in those tubes which were insolated in the presence of ordinary atmospheric air. On the other hand we see specimens of the same urine insolated to precisely the same degree as the former, but, *in the absence of an atmosphere*, becoming turbid, even *in vacuo*, with *Bacteria* as early as their encased congeners.

This remarkable fact, then, appears to follow as a deduction, that a vacuum (or approximation to such) which of itself is a condition antagonistic to the development of *Bacteria*, nevertheless shields these organisms from the germicidal effect of light*.

It is not our present purpose to speculate on the interpretation of the phenomena here presented, nor should we be justified in so doing until we have further extended our observations, and more fully confirmed the curious results here provisionally detailed.

IV. "Points of Resemblance between the Suprarenal Bodies of the Horse and Dog, and certain occasional Structures in the Ovary." By Charles Creighton, M.B., Demonstrator of Anatomy, Cambridge University. Communicated by Professor Humphry, F.R.S. Received October 12, 1877.

(Abstract.)

The object of this communication is to prove, with the aid of accurate drawings, that there exists an essential resemblance between the constituent parts of the suprarenal bodies of mammals and certain structures in the mammalian ovary that are of occasional but normal occurrence. The appearances on which the comparison is based are best seen in the suprarenals of the dog and horse, and in the ovaries of the bitch. The suprarenals of the horse and dog are known to have, immediately under the fibrous tunic, a zone of follicles of singular though well-defined structure. The first point in the communication is one of criticism, and has reference to the division of parts within the suprarenal. It is held that the outer zone of follicles, as they are seen in the horse and dog, are quite unique among the structures composing the suprarenal, and are broadly contrasted with the rest of the organ lying internal to them. The contrast is unmistakable in these two animals, and it is equally

* We wish, however, to make it clear that we by no means insist on this explanation; the facts, indeed, admit of other explanations.