some days previously, however, the dressing had been left unchanged, and about the tenth day the discharge, though still remaining in the main serous, increased in amount. This state of matters continued till one by one the ligatures came away. The ligatures, when examined, were found to have a distinctly sourish 'micrococcal' smell and an acid reaction, and under the microscope their interstices were seen to be filled with micrococci. Here we have a perfectly aseptic course for the first few days till the time came when the dressings were left unchanged for several days—till, in fact, as we have seen, micrococci got in. When these organisms appeared they grew in the interstices of the thread, produced their acrid products, and the thread was no longer a substance which might become encapsuled or even disappear by absorption, but it became an irritating foreign body, which had to be thrown off before healing could occur.

In the same manner, if micrococci entered a healthy joint where we have a cavity containing nutritious fluid, rather in the conditions of fluid in a flask than in a cavity in the living body, I would hardly regard their presence with satisfaction; for I should fear that their products would not be altogether neutral to the sensitive synovial membrane, and might, at the very least, cause increased secretion and delay in healing.

These facts are of themselves sufficient to indicate the advisability of taking all possible precautions to exclude these organisms. Dr. Ogston has recently assigned much more serious consequences to them, but the subject is at present too debateable to be suitable for discussion here.

IV. Are organisms present or do fermentations occur in fluids or tissues in the living body which have never been exposed to atmospheric dust? If they do so occur, how is their presence to be explained?

This question especially deals with the occurrence or not of organisms in the healthy living body. I need not enter at length into this question again; I have twice already touched on the subject. I have described the experiments by which it has been shown that the blood, urine, and milk of healthy living animals contain no organisms, and possess no inherent tendency to undergo fermentative changes. Then I narrated,

both at p. 45 and at p. 196, my experiments on the *tissues* of healthy living animals. I need not dilate further on this question, but the conclusions to which we were forced were, that the blood and tissues of healthy living animals do not contain organisms or their spores, and have no inherent tendency to undergo fermentation. But it may be objected that these organs were removed from the living body and placed in flasks, and that the conditions were not the same as if these tissues had been retained in relation with the living body. Such an objection is of no value, because any difference in the conditions is in favour of organisms in the flask experiments, for the walls of the flask are neutral to their development, while healthy living tissues are powerful destroyers of bacteria. This we shall see presently. In the meantime, I will now bring forward a piece of evidence which completely sets any such objection at rest. I refer to what is known as the 'expérience du bistournage.'[1]

This 'bistournage' consists in rupturing the spermatic cord subcutaneously by torsion. The testicle is thus separated from its nutritious vessels, and lies loose in its tunics which protect it from the access of the air. It adheres to the tunica vaginalis, but the new circulation is insufficient; the testicle atrophies and disappears. There are no accidents, because, as we have previously shown and shall immediately see, the air with its dust—*i.e.* bacteria—does not get access to the dead part.

M. Chauveau who is the author of these experiments, and whose name is sufficient guarantee for their scientific accuracy, proved this in the following way:—If the harmlessness of the operation depends on the absence of the putrefactive organisms which would be carried to the testicle by the air, then these accidents ought to occur when the germs are carried to it by the blood. Chauveau accordingly injected organisms into the vascular system of rams. After the fever, sometimes fatal, caused by this injection had subsided, he performed the operation of 'bistournage.' Putrefaction of the testicle occurred in those animals which had been injected with bacterial liquid.

[1] See Jeannel's book, *De l'Infection Purulente*, from which the facts are taken.

Several objections could be urged against the experiments, and are answered by Chauveau :—[1]

'1. Nothing proves that the bacteria in the fluid injected were the active agents. But the experiment repeated after careful filtration of the fluid by special filters—*i.e.* after removal of bacteria or their germs, remained without effect.

'2. Nothing proves that it was not the infective fever itself which caused the putrefaction of the organ. But of two rams injected with the same fluid and the same dose, putrefaction only occurred in the case where 'bistournage' had been practised. Still further, if in the same animal 'bistournage' is practised on the *left* testicle *before injection*, and *on the right after injection*, the *right* testicle is the only one which putrefies: a proof, evident and very ingenious, that it is indeed the penetration of the putrefactive germs into the organ which determines putrefaction; since the testicle which was separated from the general circulation before the injection remained indifferent, and did not undergo putrefaction, in spite of the infection of the whole body.'

I have also referred to several similar well-known facts at p. 50 *et seq.*, and Dr. Ogston has tested such fluids as extravasations of blood, hæmatoma, contents of pathological cysts, the fluids of the natural cavities of the body, &c., by means of Koch's method of staining, and he has failed to find any organisms. I have also referred to Meissner's recent facts.

From all these considerations we may, I think, conclude that organisms are not present in the fluids or tissues of the *healthy* living body.

Are organisms present in the body in states of disease? (I leave out of account here the infective diseases.) If so, how is their presence to be explained?

If we investigate animals suffering from disease, we shall find that in certain cases organisms are present. What these exact conditions are I am not yet able to say, but I will indicate some of the cases.

I have found that if an acute inflammatory process be induced in an animal—say by the injection of ammonia—subcutaneously, as pointed out some years ago by Dr. Burdon-

[1] Quoted from Jeannel.

Sanderson, the organs examined by the method I have described may be found to contain organisms.

I have in three instances produced abscesses in rabbits by the injection of croton oil, and in one case I have found that the pus of the abscess contained micrococci and bacteria, though none were introduced at the time. In one of the others I found only a very few bacilli and in the third I found no organisms. I shall detail the experiments.

Experiment 1, March 24th, 1881.—Pure croton oil was introduced into a tube. This was then sealed at both ends, placed in a sand bath, which was raised to a high temperature (about 270° F.), and maintained at that temperature for about 2 hours. When it had cooled, one end of the tube was carefully opened in a spray of carbolic acid, and a pure syringe being rapidly introduced, some of the oil was sucked up, and at once, in the same spray, half a minim was injected into the dorsal muscles of a mouse. The skin of the mouse was very carefully purified beforehand, and care was taken not to allow any of the oil to escape along the needle track either in introducing or withdrawing the needle. The puncture was dried with a carbolised rag and then touched with collodion.

A flask of meat infusion was inoculated from the oil at the same time.

On March 25th (26 hours later) the animal was found dying, and was at once killed. Great care was taken, in removing the skin from the back, to avoid contamination of the deeper parts. The muscles at the seat of injection were found to be infiltrated with pus, but no trace of the needle track through the skin could be seen. On examining this pus after straining, it was found to contain large numbers of organisms, chiefly micrococci, but a few bacteria and bacilli could be seen.

The meat infusion remained permanently pure.

Experiment 2, April 12th, 1881.—A mixture of equal parts of croton oil and olive oil was purified and 3 minims injected into the dorsal muscles of a large rabbit with the same precautions as in Experiment 1. A flask of meat infusion was inoculated with this mixture at the same time but nothing developed in it.

On April 18th the animal was observed to be ill, and it was

therefore killed. A considerable quantity of pus was found at the seat of injection. On examination of this pus I found numerous pus cells, but I have not been able to satisfy myself that there are any micrococci. A very few bacilli were present.

Experiment 3, April 20th, 1881.—2 minims of the croton oil mixture used in Experiment 2 were injected into the dorsal muscles of a large rabbit, with the usual precautions.

On April 25th, though the animal was perfectly well, I killed it. On examining the seat of injection I found one or two very small cheesy spots (small abscesses). These consisted of pus cells and granular matter, but I have not been able to detect organisms of any kind.

In each of the experiments the pus was stained with methyl violet, according to Koch's plan, and examined with Zeis's $\frac{1}{18}$th oil or $\frac{1}{25}$th water immersion objectives.

Again, I find that if the nutrition of an animal be profoundly interfered with, as in slow poisoning by phosphorus, organisms may after some time be found in the blood and tissues. Here, it may be said, inflammation of the intestinal tract has been caused, and the epithelial barrier against the entrance of organisms has been removed. But *the blood and tissues, when in a healthy state, have the power of themselves of destroying organisms* when these are introduced into the body. Thus, if into the veins of four medium-sized rabbits I introduce $\frac{1}{4}$, $\frac{1}{2}$, $\frac{3}{4}$, and 1 c.cm. respectively, of fluid containing bacteria, keep the animals alive for at least twenty-four hours, then kill them and preserve their organs in the manner described, I shall probably find no organisms in the first three, while in the last they may or may not be present. Where larger quantities of the fluid have been introduced, they will certainly be found. Thus, even though the organisms could gain access to the blood, yet so long as the nutrition of the animal is fairly well carried out, they would be destroyed.[1] The reason that they are found where large quantities of bacterial fluid are injected seems to me to be that, along with the bacteria, their products

[1] Similar facts have been made out by Traube and Gscheidlen. See a paper by these authors, 'Ueber Fäulniss und den Widerstand der lebenden Organismen gegen dieselbe,' *Berlin. Klin. Wochenschrift*, No. 37.

are introduced, that these act in the same way as phosphorus, as poisons, and that thus the resisting power of the animal is diminished.

The conclusion which I would draw from these facts is, that in severe inflammatory processes, or in great interference with the nutrition of an animal, organisms may pass into the blood without losing their vitality. The organisms usually found are micrococci.

I have examined a large number of abscesses, in man, when opened. At first I only used cultivating experiments, and this paragraph gives the result of these. In *chronic* abscesses I have not as yet found any organisms, and in this my results agree with those obtained by Billroth by microscopical examination alone. Of *acute* abscesses I had up till May 1879 inoculated from thirty-two cases. In twenty-five of these no growth of organisms occurred, while from six micrococci were obtained. In no case did I get bacteria (I omit here one abscess in the ischiorectal fossa, where I found both bacteria and micrococci; and one with fæcal odour in the lumbar region, from which I did not inoculate when opened, but in which undoubted bacteria were seen on microscopical examination).

Professor Billroth has paid special attention to this subject, and he has found organisms in a larger proportion of acute abscesses than I have done. Professor Billroth likewise only mentions the occurrence of micrococci. In acute osteomyelitis, where a communication had not yet been established with the external air, organisms have been found in the medullary canal or in the pus surrounding the bone by Von Recklinghausen,[1] Klebs,[2] Eberth,[3] and Godlee.[4] These organisms were as a rule *micrococci*; bacteria have but very rarely been present.

In 1880 Dr. Ogston,[5] of Aberdeen, published an elaborate and careful research on the relation of micrococci to abscesses. In examining pus and discharges for organisms he made use of Koch's method, before described. In the first place, he found, as I had done, that micrococci were not present in chronic

[1] *Deutsche Zeitschrift*, Bd. iv. p. 239.
[2] *Ueber Schusswunden*, 1871.
[3] Virchow's *Archiv*, Bd. lxvi p. 341.
[4] *Lancet*, November 21, 1874.
[5] Published in Langenbeck's *Archiv*, Bd. xxv. Heft 3.

abscesses. In acute abscesses, however, he states that these organisms are always present. This fact, obtained by staining the pus, differs, as will be seen, from my cultivation results, and agrees more with Billroth's results from simple microscopical examination.

Since this statement was made I have examined a number of abscesses by staining the pus obtained when they were opened, and I now quite agree with Dr. Ogston. Organisms are always absent from chronic abscesses. Micrococci are always present in acute abscesses.

Fig. 29 (Plate IV.) is a specimen taken from a chronic abscess over the sternum. It contains no organisms.

Figs. 30 and 31 are taken from acute abscesses—one of the mamma, and the other of the finger. These contain micrococci.

In one case of abscess in the neck, which had been forming for about three weeks, I could find no organisms. The skin was red over it, and I have entered it in my notes as an acute abscess. In another case, which I have also in my notes as an acute abscess, no organisms were found. This was a case of small abscess in the thigh, in a situation where diseased bone had formerly existed. The patient positively asserted that the abscess had only been forming for fourteen days, but it is of course quite possible that it had existed longer, and had only begun to point for fourteen days. Nevertheless the man's positive assertion—for I questioned him again after I saw that no organisms were present—the redness of the skin, the well-formed pus cells, and the absence of fatty débris make it difficult to assert that this was a chronic abscess.

Notwithstanding those two doubtful cases, I am ready to accept Ogston's statement and to receive it as a law, that if the pus taken from an acute abscess when opened be examined after staining, micrococci will always be found.

How, then, are my former results to be explained? By cultivation, from thirty-two cases of acute abscesses I only got micrococci in seven instances, and yet we now know that had I stained the pus I should in all probability have found micrococci in all.

Well, in my recent investigation I not only stained the pus, but tried cultivation experiments, and I got much the same

results as formerly: in some cases micrococci grew, in others I got nothing. It seems, as far as I can judge from my facts, that if the abscess is opened soon after its commencement micrococci almost always develop. If, however, ten days or a fortnight or more elapse, these organisms do not as a rule grow. Why is this? Simply, I believe, because the organisms have died.

I made out early in this investigation, and have often confirmed it since—sometimes, indeed, to my great inconvenience—that micrococci very soon exhaust the nutritive material in a fluid, and that they then fall to the bottom and die, for they will not grow in any fluid nor increase on the addition of fresh nutriment. In a flask containing say an ounce of cucumber or meat infusion, the micrococci grow rapidly, but they do not live more than three days. Then they fall to the bottom, and the fluid at the top becomes clear and remains so permanently. Take a drop from the flask during the first three days and put it into another specimen of pure cultivating liquid, and micrococci develop readily. After the third day or later, in proportion to the amount of the fluid, no development occurs whatever one does. The organisms are dead. And so in an abscess they live as long as they find nutriment, and then they die and cannot be obtained on attempting cultivation, though they may still be seen on microscopical examination.

Ogston relates similar facts, though he apparently does not attach this meaning to them. He remarks that when micrococci are cultivated in flasks containing such fluids as urine, ascitic, ovarian or hydrocele fluid, blood, &c., the fluid at the surface remains for months clear, while a slight deposit, which consists of micrococci, is present at the bottom. He concludes that the micrococci are growing away from the air, and that therefore they are anaerobes. This is not the case, for micrococci grow with greater rapidity and luxuriance in pure oxygen than under any other circumstances which I have yet tried. The truth, I believe, is, that these organisms were at first not quite dead, but had very little vigour and soon ceased to grow and died. They were not growing away from the air.

His results in cultivation from abscesses confirm mine, for he says: 'Often the micrococci grew luxuriantly, sometimes in

chains and sometimes in groups, but *oftener* the experiments were unsatisfactory, and it indeed happened that the micrococci which were introduced died altogether.' Now had Ogston put these two facts together, and had he worked for long with cultivation, he would, I venture to think, conclude as I do, that the micrococci which are found in abscesses, but which will not develop in the cultivating fluid, *are already dead in the abscess* and do not die only on removal from it.

The following fact may be mentioned as bearing out this view:—

Fig. 32 (Plate IV.) was taken from a case of acute abscess in the groin, which was opened on April 29th. A number of streptococci will be seen. Nevertheless, a flask of meat and one of cucumber inoculated at the same time remained quite barren. A specimen was taken from the same case at the next dressing, April 30th. There was plenty of discharge in the drainage tube, but this contained almost no organisms. Had the organisms been alive on the 29th, they would probably have been as numerous, if not more so, in the interior of the drainage tube on the 30th.

So far, then, I conclude, that though micrococci are always present in acute abscesses, yet if the abscesses be not opened for some time, these organisms will be found to have died. I shall not enter here into the question of the relation between these organisms and the abscesses in which they are found. The facts are sufficient for my present purpose.

It thus seems, that in certain states of low vitality and in acute inflammations, organisms may be present in the blood and tissues of animals. These organisms are generally micrococci.

V. How do organisms get into wounds treated aseptically? There are three possible explanations which might be offered:—1. They come from the blood. 2. They arise spontaneously in the wound. 3. They come from without through some insufficiency in the aseptic method.

1. They come from the blood. We have already seen that organisms are not present in the blood or tissues of the healthy living animal, but that they may occur in low or inflammatory states. But a person on whom an operation has been performed

aseptically is, after the effects of the chloroform have passed off, and provided that there has been no great loss of blood, practically as well as before the operation. There is no inflammation and no febrile disturbance—the patient, as far as one can judge, ought to be as able to resist the entrance of organisms into his blood as before the operation.

If, however, these micrococci did enter the wound from within, they would do so during the first few days after the operation; but if we look at the cases described, and the same thing can always be found, we see that these organisms do not, as a rule, enter for several days after an operation—not till the dressings are but infrequently changed.

In the case of acute abscesses opened early, and in which the micrococci are still alive, they of course exist from the first; and so they may occur if for any reason acute inflammation attacks a wound. But that this is their mode of entrance in ordinary cases is against all the evidence.

2. As to spontaneous generation. We have discussed that at such length at various places that I need not enter into it again. The facts with regard to the absence of organisms under certain circumstances and the constant presence of a particular form when they do occur, together with the points to be presently mentioned, sufficiently do away with any necessity for considering a view which can only be thought of where other and more natural modes of origin cannot be traced.

3. We must then search for some mode in which they might enter through the antiseptic arrangements. These we may divide into three parts: a lotion in which the various substances are soaked before being brought into contact with the wounds; a spray to purify the atmosphere; and a dressing so constituted as to give off carbolic acid to the discharge as it passes under it.

That the lotion is sufficiently potent to destroy organisms which come in contact with it, will be very evident to any one who chooses to examine the subject. If one places a preparation of actively moving bacteria under the microscope, and allows a little carbolic acid lotion, 1 in 20, to flow under the cover-glass, the movements will be seen instantly to cease; this, in fact, is the method which I formerly employed when I

wished to draw moving bacteria with the aid of the camera lucida.[1]

That the spray is sufficient has already been shown by a number of experiments described at p. 26 *et seq.* (see also the case of empyema, p. 237).

An attempt has been lately made by Dr. Lewis Stimson[2] to show that the spray does not act as a germicide, and as his results have been extensively quoted, I must briefly notice them here. Three tubes were filled with urine, boiled, and allowed to cool in the spray and then exposed in it for an hour and a half, the floor being swept to raise the dust. The tubes were then closed with plugs of cotton-wool wet with alcohol. Particles of dust were seen to be caught on the edge of the tube, and a purified glass rod was used to push them into the fluid. In one tube in which this was done bacteria developed. In another case the tube was tilted so as to bring the fluid in contact with the neck, and here also development occurred. The third tube was undisturbed and remained pure. In another set of experiments wide-mouthed beakers were used, and were exposed for three-quarters of an hour. In all of these organisms developed. Test experiments showed that these organisms had entered during the exposure.

These experiments do not, however, seem to be satisfactory. We are told in the first case that the spray was placed a foot above and $3\frac{1}{2}$ feet distant from the tubes, and that fifteen ounces of carbolic lotion, 1–20, were used. During half the time a board was placed so as to throw the spray back over the flasks. Now if the flasks were only *beneath* the spray, a foot distant from it, during half the period of exposure—the result cannot be wondered at, for eddies would be produced by the spray which would drive unpurified dust into the vessels. If, however, the spray passed directly over the mouths of the flasks —if these were enveloped by it—it is difficult to conceive that the spray could have played for so long at such a close distance, and that so much lotion could be used without the entrance into the urine of a quantity of carbolic acid more than sufficient to render it sterile. But granting that the method is correct in these respects, Stimson sweeps the floor, raises *large masses of dust which he can see*, and expects that as these fall through the spray they will be soaked through and through, and any organisms in their interior be destroyed! It never has been asserted that a mass of filth falling through a spray can be soaked completely in a moment by the acid. We do not sweep the floors while performing a surgical operation.

[1] Dr. Koch has found that the *spores* of Bacillus Anthracis can resist 1–20 watery solution of carbolic acid for a considerable time, though the fully developed organisms are at once destroyed by it. *Micrococci*, however, are very susceptible to the action of this antiseptic.

[2] Shown to the New York Surgical Society, November 25, 1879.

All that I assert is, that under ordinary circumstances the spray is thoroughly efficient. From this point of view it is in the main a question of size of the particles which the spray meets with. If these are minute and but little compact, they will be disinfected. If they are large and dense, as will be the case if the floor be swept during or immediately before the experiment, one could not expect the spray to soak through them sufficiently during their transit. There is another way, however, in which the spray may act on these larger particles—viz. by bedewing the surface of the wound, and thus keeping up the action on the dust which began during its transit through the spray. In fact, the particle of dust already moistened while passing through the spray, falls into a thin layer of carbolic lotion, and thus disinfection is completed. As a rule, however, particles of dust which are small enough and light enough to float about in the atmosphere, such particles as are present in ordinary rooms or wards, will, as far as I can judge, be acted on directly in a sufficient manner by the spray, for they will not fall straight through it, but will be carried along with it after being moistened with carbolic acid, and thus time is afforded for the thorough action of the antiseptic before they reach the wound. If these organisms got in through fault of the spray, we can hardly imagine that they would always be micrococci. We must, therefore, examine the action of the dressing in order to see if any explanation can be obtained there. We shall see that it can, and that these organisms do enter on account of failure of the dressing to fulfil all requirements for a lengthened period.