no corrosive sublimate can fly off. It can be manipulated as you please, and, as applied dry to the skin, it is absolutely unirritating.[1] Then, in the next place, it is unirritating as acted on by the perspiration. If water is made to act on sublimated serum dried, it does not redissolve it as serum does, but it renders the mass opaque, the water being only partly absorbed into it; and the water which remains unabsorbed contains exceedingly little of the corrosive sublimate, which is almost all retained by the albumen. Hence, when perspiration soaks into such a dressing, though it moistens it, it does not produce irritation. I made some gauze with serum so strong with corrosive sublimate as to have 1 part to 30, which implies more than 6 per cent. of corrosive sublimate in the dried gauze. I moistened a piece of this with distilled water, and fixed it on my arm for six hours in the manner above described; and, when I removed it, I found the skin free from irritation. Thus, you will observe that, by associating albumen with the corrosive sublimate, we seem to be able to get rid of its irritating properties.

But the important question arises, Does corrosive sublimate, when thus associated with albumen, retain sufficient antiseptic virtue for surgical purposes? The method of experimenting which I have described is adapted for testing the efficacy of any antiseptic dressing, and I have used it for various others besides sublimated ones. I have employed it for salicylic cotton-wool, for iodoform cotton-wool, for eucalyptus gauze, and for carbolic gauze. I have mentioned that the test is an exceedingly severe one, and I find that, after the lapse of a few weeks, salicylic wool soaked with serum and inoculated as above described, stinks; and the same is the case with iodoform wool. The eucalyptus gauze, however, if freshly prepared, remains pure; as also does carbolic gauze. We have seen that the 1 per cent. sublimate-wool resisted, still more the 5 per cent. and the 10 per cent. In accordance, therefore, with our previous experiments with iodoform and salicylic acid, they did not stand the test as well as carbolic acid, or eucalyptus, or the corrosive sublimate. But we get a different result if, instead of using serum of blood, we use serum mixed with blood-corpuscles, such as we readily get from the cow, in which the corpuscles do not aggregate so closely as in the horse, but remain suspended in the serum. I need not, of course, tell any members of this Society, that the corpuscles are enormously richer in protein substances than the serum is, so that serum and corpuscles contain about $2\frac{1}{2}$ times as much of proteid material as the serum does; and as albuminous materials mitigate the action of the corrosive sublimate, they cannot fail to interfere more or less, also, with its

[1] If such a gauze be torn, it gives off a dust which irritates the nostrils. It is, therefore, better to cut it with scissors.

antiseptic action ; and when we use serum and corpuscles, instead of serum only, we find that the sublimated wood-wool (which I should have said did very well with the serum so far as the sense of smell indicates) fails completely. The 1 per cent. sublimate-wool failed also ; the 10 per cent., however, stood the test perfectly, even with blood in substance. Now, as to our gauze made with albumen associated with the sublimate, the sero-sublimate gauze. Such a gauze, prepared with 1 part of sublimate to 100 of serum, stood the test absolutely when tried with serum. It therefore proved itself superior to salicylic and to iodoform wool. But with the blood in substance, how does it behave ? The tube in this bottle contains a portion of the gauze treated with the cow's blood, serum and corpuscles, and inoculated nearly a month ago in the same potent manner to which I have referred, and you will observe that it has no putrid odour. Really, then, this sero-sublimate gauze seems to stand the test completely. I may say that, when tried with corpuscles and serum, our best eucalyptus gauze failed utterly, so that everything that I have tried failed with serum and corpuscles, except the stronger sublimate preparations and carbolic-acid gauze.

Then the question comes, how far may we go in the strength of our sublimate combined with the albumen without causing irritation ? During the last three weeks, my cases at the hospital have been dressed with this material. We have used the kind of gauze which was tested in the above experiments, made with 1 of sublimate to 100 of serum, and also one made with 1 of sublimate to 50 of serum. We find that the 1 to 50, in the majority of cases, has caused no irritation whatsoever, but in a very few it has caused some irritation, which, however, has disappeared, and the sores caused by the 1 to 50 have healed when the 1 to 100 has been substituted for it. Therefore it looks as if we were very near our limits, as if that prepared with 1 of sublimate to 100 of serum was trustworthy and unirritating, even to all skins, and that prepared with 1 to 50 was unirritating to most skins. Now there is this to be observed for our comfort, that the discharge both from wounds and from abscesses antiseptically treated is a serous discharge, not a bloody one, except in the case of wounds during the first twenty-four hours ; and even in the first twenty-four hours, except in cases in which the dressing has to be taken down for reactionary haemorrhage, the blood is always more or less diluted with serum. If, therefore, we have a dressing which has stood our severe test with serum mixed with the full amount of corpuscles, we are surely right in regarding it as trustworthy.

Last Friday, I amputated at the hip-joint in a boy twelve years of age, on account of a sarcoma of the lower half of the femur. The wound was exceedingly vascular, and there was a great deal of bloody oozing in the first twenty-four

hours, but very little blood escaped externally, because this gauze absorbs very much better than our carbolic-acid or eucalyptus gauzes do, containing as they do resin and paraffin; and I would strongly recommend that, during the first twenty-four hours, this gauze should be used in at least sixteen layers. It is now three days since the operation was performed, and, from the perfectly normal temperature and returning appetite and strength, I think we may be satisfied that the boy is already out of risk of septic complications. And this you will observe, is a very testing case.

I may mention one other case, that of a boy six years old, from whom, nearly three weeks ago, I removed a portion of a rib, for the purpose of allowing free drainage to empyema. We let out thirty ounces of thick, odourless pus, and a great quantity came out afterwards. He has been dressed with sero-sublimate gauze; and he is one of the instances in which gauze prepared with the 1 to 50 serum caused irritation, which disappeared under the 1 to 100.

In him we have witnessed the beautiful course which, I believe, we can only see under antiseptic treatment efficiently managed, of no more pus formed after the first pus has been evacuated—nothing but a serous oozing rapidly diminishing; and I was delighted to see, on coming back after a fortnight's absence, how plump the emaciated little fellow had become. And the serous discharge is now so slight, that I believe it would be already safe to remove the drainage-tube. This case, I think, proves that our dressing is aseptic, that the germicidal properties of the corrosive sublimate have come into play in the preparation of the dressing, so that any injurious organisms which existed in the blood or in the gauze before they were brought into preparation, have been destroyed: because, if there had been merely the inhibitory influence of the sublimate upon the organisms, the serum pouring out from the pleura, washing away all the antiseptic in the vicinity of the wound, but leaving the organisms lodging among the fibres, then we should have had putrefaction, or other disturbing causes, showing themselves. No such thing having occurred, this case seems of itself sufficient evidence that our dressing is really a safe one in so far that it contains no living organisms of importance to start with.

I have here a sample of a very cheap fibre, sent me from the south of France, prepared with this sublimate-serum, and then teased out, showing that we may use this material for charging various fabrics. This fibre is highly absorbent; and I may remark that, if we have a very highly absorbent dressing, we may, and must, use a larger proportion of the sublimate. A gauze will absorb only about three times its weight of liquid; cotton-wool will absorb ten times its weight; and therefore you observe, when the one dressing is saturated, it has three times as much of the liquid in it, and thus has the sublimate three

times as much diluted. Being so much diluted, it will be in proportion less irritating, but a stronger proportion is required to make it safe unless you use it in a very large mass. I believe the French charpie, made of old rags, or even old rags themselves, might be quite well prepared with sublimated serum. I have here some rags which have been so treated, and which are quite absorbent, and therefore the dressing promises to be a very cheap one. If the serum is treated with a certain proportion of sublimate, not sufficient to make it solid, it may be kept for any length of time. For aught I know, this sublimated serum may come to be an article of commerce, which may be used in hospitals, or even in private practice. I also think it possible that a material of this kind, dried and reduced to powder, may come to be used for the purpose of mixing with vaseline for an antiseptic ointment, or even for dusting in, under certain circumstances, among our dressings.

I regret that time has not allowed me to bring this matter more completely to an issue as regards its practical applications. At the same time, though the subject is, to a certain extent, immature, I ventured to hope that the interest of some of the points to which I have referred might justify me in bringing it before you.

AN ADDRESS ON A NEW ANTISEPTIC DRESSING

Delivered before the Medical Society of London, November 4, 1889.

[*Lancet*, 1889, vol. ii, p. 943.]

Mr. President and Gentlemen.—When I last had the honour, five years ago, of addressing this Society at the request of its President, I brought before you an attempt I had made to utilize the powerful antiseptic properties of corrosive sublimate without the great disadvantages attendant upon its highly irritating qualities.[1] I had ascertained that when corrosive sublimate precipitates albumen the precipitate is not, as had been generally supposed, an albuminate of mercury—that is to say, a combination of albumen as an acid with mercury as a base; in other words, that the albumen does not displace the chloride from its combination, but that the bichloride of mercury retains its properties intact, the albumen being loosely associated with it, in a species of solid solution, if I may so speak.

Further, I had found that this precipitate, even after drying, is capable of being dissolved in the serum of the blood, and that the solution in blood-serum is powerfully antiseptic while not irritating. I proposed and brought before you a new dressing in the shape of what was termed the sero-sublimate gauze, charged with a solution of corrosive sublimate in the serum of the blood. This gauze gave very satisfactory results, both in my own hands and in those of surgeons in places so far distant as Poland and Spain. Nevertheless, it was not all that could be desired by any means; it was somewhat harsh mechanically; it was not very absorbent (a serious defect), and one of the materials of which it was made (the serum of horse's blood) was not always easily obtainable. I was, therefore, well disposed to look for something superior.

A few weeks after that communication was made to the Society, a firm of manufacturing chemists, Messrs. Gibbs, Cuxson & Co., wrote to me saying that they had found that if chloride of ammonium or sal ammoniac in quantity equal to one-fifth part of the weight of the bichloride of mercury was added to the mixture of bichloride of mercury and blood-serum, the result was a much more fluid preparation than I had obtained. If I used a preparation of one part of bichloride of mercury to 100 of blood-serum, I got a thick liquid somewhat difficult to diffuse in gauze. They therefore suggested that by adding

[1] See p. 293 of this volume.

sal ammoniac in that proportion I should get a much more workable arrangement. On consulting chemical works I found that one-fifth of sal ammoniac was exactly sufficient to produce with bichloride of mercury the salt long known to chemists as sal alembroth, a double salt of bichloride of mercury and chloride of ammonium. I naturally wished to ascertain whether this addition of sal ammoniac would impair or even destroy the antiseptic properties of the bichloride of mercury. I therefore made experiments on the point, and I found that the sal ammoniac associated in the form of a double salt with the bichloride of mercury did not by any means impair its antiseptic properties ; on the contrary, it improved them, so far at least as concerned that which we have to deal with as surgeons, an albuminous fluid like the serum of the blood. Sal alembroth and bichloride of mercury proved to be exactly equivalent weight for weight as antiseptics in such a fluid. Each had to be used in normal serum of specific gravity of about 1,025 in the proportion of about 1–1,000th in order to prevent altogether the development of micro-organisms. Those who are acquainted with Koch's researches will consider this a very high proportion. Koch has shown that in a solution destitute of albumen 1–100,000th part of bichloride of mercury is sufficient to prevent the development of organisms ; but when we have albumen present in the solution the case becomes altogether altered. Albumen interferes with the antiseptic action of corrosive sublimate, and thus in serum of the blood of specific gravity 1,025 we require, as I have already said, as large a proportion as about 1–1,000th instead of 1–100,000th.

When the albumen is small in amount, less corrosive sublimate proves efficient. Thus, in a case of spermatocele, where the specific gravity of the fluid was exceedingly low—only 1,007—I found that 1–1,000th, just intermediate between the proportions required for blood-serum and water, was efficacious. On the other hand, when blood-corpuscles are mixed with the serum in the same amount as in the circulating blood, making the albuminoid substances much more abundant than in serum, we require proportionately more of the corrosive sublimate ; at least 1 to 500 is required for the purpose of preventing development. This is a most important consideration after an operation. In the first twenty-four hours the discharge contains a large amount of blood-corpuscles as well as the serum ; whilst, at the same time, it is the most copious—more copious than it will be in any subsequent day, provided all goes well aseptically. It is therefore, an exceedingly serious consideration that in the first twenty-four hours we have a discharge which in both these respects tests our antiseptic more severely than it will ever be tested again, both from its abundance and from its quality. Still, the sal alembroth, whether used with blood-serum or with a normal mixture of serum and corpuscles such as that obtained by

whipping the blood of the ox so as to get rid of the fibrine, proved equivalent antiseptically to bichloride of mercury.

I may perhaps say in a few words how the experiments were conducted. For the purpose of ascertaining whether a given antiseptic can or cannot prevent development of organisms, a very simple mode of experimentation suffices. What we have to do is to ascertain whether it is *inhibitory*, not whether it is *germicidal*. The only special apparatus required for such experiments is a warm chamber which can be kept pretty constantly about the temperature of the human blood. Beyond this a few stoppered bottles, with well-fitting stoppers—perhaps half-ounce stoppered bottles—are really all that is required. Into a series of such bottles serum of the blood, to the amount, say, of 150 grains, containing solutions of the antiseptic of different strengths, is introduced. These are all inoculated by means of a small syringe pipette with the same quantity, say, 1–10th of a minim, of some potently septic liquid, such as blood-serum in a state of advancing putrefaction. The bottles are placed in the incubator, and then, if development takes place, that is evidence that in the bottle in which it occurs such proportion of the antiseptic as that bottle contains is inadequate ; if no development occurs, we have proof of antiseptic efficacy. The transparency of the serum permits a recognition of the development of the organisms, which invariably causes opacity. If we have no change in this respect, if the serum maintains its transparency, at the same time keeping its odour unimpaired, and, further, if microscopic examination of any little sediment there may be shows that it contains no organisms, we have clear evidence that the antiseptic in the proportion concerned has proved efficacious.

Now if the sal alembroth was equivalent to the bichloride of mercury, weight for weight, that shows that the sal alembroth was really more efficacious as regards the quantity of bichloride of mercury it contained. The bichloride of mercury having the sal alembroth added to it, and also water in the double salt that is formed, is increased in atomic weight very considerably ; and therefore if an equal weight of alembroth is equivalent in antiseptic action to bichloride of mercury, that shows that the bichloride of mercury is made more efficacious in the blood-serum by the addition of the chloride of ammonium. The chloride of ammonium in combination protects the bichloride of mercury, so to speak, to a certain extent, from the interfering influence of the albumen.

At the same time, the sal alembroth proved much less irritating than bichloride of mercury. Experimenting on my own skin, I ascertained that it has certainly not half the irritating property of corrosive sublimate. The chloride of ammonium attached to the bichloride of mercury, while it protects the bichloride in some degree from the influence of albumen interfering with

its antiseptic operation, prevents it also from acting so powerfully on the human skin. And thus the union of sal ammoniac with corrosive sublimate had the double advantage of rendering it both more efficacious antiseptically and much less irritating.

Hence I was at first much pleased with sal alembroth. But it soon appeared that there were certain disadvantages attending it. These depend very much upon its excessive solubility. It is essential for a satisfactory antiseptic dressing that the antiseptic should not be readily washed out of it by the discharge. Sal alembroth is so exceedingly soluble that it is washed out with the greatest ease ; thus we were always, even if we used large masses of sal-alembroth gauze, in fear that when the discharge was copious the antiseptic would disappear, say, within the first twenty-four hours, and then the septic mischief would have an opportunity to enter. There was another disadvantage from this great solubility. When the discharge entered a mass of the sal-alembroth dressing it dissolved out the alembroth from it, passed into another part of the dressing, and there took up another portion of the sal alembroth, and so went on from part to part of the dressing, until, if the discharge was copious and the dressing large, as it must be when the discharge is copious, before the discharge got to the edge of such a dressing it became so concentrated a solution of the sal alembroth as to be highly irritating. We have seen, for instance, after the removal of the mamma, when the first dressing was changed on the following day, that there has been over the scapula and the neighbourhood a huge blister. No doubt that was only a temporary inconvenience. We never had the discharge again so great as in the first twenty-four hours, but still it was a great inconvenience.

Such being the disadvantages of sal-alembroth dressing, I was disposed to seek for something better. I may say that I myself have never published anything in favour of sal-alembroth dressings ; I have never been satisfied with them. It has leaked out that I have used them, and they have come into extensive employment, but never with my published sanction.

In the course of the following year I made various experiments in the hope of rendering sal alembroth more useful in different ways, with which I need not trouble you, but without much success. In February 1886 my attention was drawn by Mr. Martindale of New Cavendish Street, to cyanide of mercury as possibly a valuable antiseptic, and, if so, having this advantage, that it did not coagulate albumen. I therefore proceeded to make experiments with cyanide of mercury, and I found, indeed, that in inhibitory power it was remarkably efficacious. I have said that with blood-serum sal alembroth or bichloride of mercury is required in about 1–1,000th part. I found that the cyanide of

mercury kept blood-serum with only 1–10,000th part perfectly free from organic development, in spite of inoculation with potent septic fluid, for a month, when the experiment ended. I may remark that we have in the cyanide of mercury a striking instance of the discordance that there may be between inhibitory power and germicidal power in an antiseptic. In inhibitory power the cyanide of mercury is, as we have seen, exceedingly high; but in germicidal power it turns out to be very low. Mr. Cheyne has made experiments for me, which have shown that even 1–1,000th part in water is incapable of destroying the germs of bacteria. Still, the inhibitory property of cyanide of mercury was a most important point if in other respects the salt were not disadvantageous; but, unfortunately, it proved to be so highly irritating that the greater irritating property of the cyanide of mercury more than counterbalanced its superior inhibitory power. It naturally occurred to me that the cyanide of mercury might perhaps combine with some other cyanide and form a double salt, having advantages corresponding with those presented by sal alembroth as compared with bichloride of mercury. I tried the soluble double cyanide of mercury and potassium, but found it quite too irritating. I here again consulted Mr. Martindale, and he mentioned to me that in Watts's *Dictionary of Chemistry* it is stated that a double cyanide of mercury and zinc of very slight solubility may be formed by mixing together a solution of the double cyanide of mercury and potassium with a soluble salt of zinc, the zinc taking the place of potassium. I therefore obtained some of this material, and proceeded to make experiments on it. I found, in the first place, that it was quite insoluble in water. This seemed at first extremely unpromising. It was soluble in about 150 parts of glycerine, but insoluble in water. I found, however, that it was soluble in about 3,000 parts of blood-serum, and therefore it was possible that it might work antiseptically. I made experiments to ascertain whether such was the case or not, and I found that this zinco-cyanide of mercury, as we may call it, had really most important antiseptic properties; that in the proportion of 1–5,000th part it kept blood-serum perfectly free from the development of organisms for eighteen days, in spite of potent septic inoculation.

I then tried experiments with serum and blood-corpuscles as presented in the whipped blood of the ox. On instituting comparative experiments between sal alembroth, cyanide of mercury, and this double cyanide, I found that with the alembroth all proportions lower than 1–400th putrefied within twenty-four hours after septic inoculation, while with the cyanide of mercury 1–800th part sufficed to preserve the serum and corpuscles from putrefaction. At the same time, with the double cyanide I was much surprised and much pleased to find that 1–1,200th, half as little again, was sufficient to keep the mixture of serum

and corpuscles permanently free from putrefaction. It may be said that the absence of putrefactive odour is but a rude test. There may be organisms developed without any putrefactive odour being present; that is perfectly true. At the same time, if in a given experiment we find that with one agent putrefaction occurs within twenty-four hours with 1–400th part, while with another salt there is no putrefactive odour after the lapse of weeks with 1–1,200th part, we have pretty conclusive evidence that, so far as the mixture of corpuscles and serum is concerned, you have a more efficacious antiseptic in the latter. I therefore proceeded to prepare dressings of this new substance, diffusing it—for it is an exceedingly fine powder—in water with a little glycerine added to fix it, to prevent it from dusting out. If you simply diffuse it in water, and pass gauze through it, with nothing more than the water, the result is that you have gauze which, with the slightest touch, gives out the double cyanide in a cloud of dust, which produces not only inefficacy of your dressing by loss of the proper proportion of the substance, but becomes in the highest degree irritating to the nostrils of those who are near. A little glycerine, however, prevented the double cyanide from thus dusting out. I proceeded to try it in practice. I confess I did not dare to use it—considering its very slight solubility in serum—unmixed, and I associated with it some of the very soluble cyanide of mercury; and with this cyanide gauze we tried various experiments in the way of dressing, and got some admirable results. But then, on the other hand, there were disappointments. We found, for one thing, now and then very troublesome pustules as the result of a peculiar kind of irritation. Another disadvantage was that occasionally we got suppurations, coming on at a late period in the case, such as we had never been accustomed to with our carbolic dressings. A case might go on perfectly well for, say, ten days, and then suppuration might occur about a stitch track and spread perhaps further; and sometimes the healing of cases was greatly protracted by this late suppuration. In consequence of these two circumstances I gave up the use for the time being of this material.

I then directed my attention to biniodide of mercury, which has been highly spoken of for its antiseptic powers, and which has the advantage over alembroth of being comparatively little soluble either in water or in serum. I found that the iodide-of-mercury gauze answered our purpose very well so far as its antiseptic properties were concerned, but that it had the great disadvantage of producing irritation, which it was extremely difficult to control. In order to control it, we interposed between the iodide-of-mercury gauze and the skin unprepared gauze, except in so far as it was steeped in a weak solution of bichloride of mercury (1 to 4,000). But we found that different skins differed

greatly in liability to this irritation. The iodide of mercury, which is so very slightly soluble, as you know, in water, is much more soluble in serum, and the solution in blood-serum becomes irritating. In consequence of this, we had the same disadvantages from irritation as we had with the alembroth; and although, as in the case of the alembroth, this was only a transient inconvenience, I became dissatisfied with the iodide-of-mercury gauze. There was, however, one point of considerable interest, both theoretically and (as it turned out afterwards) practically, which we found in this investigation with the iodide of mercury. If we simply charged with iodide of mercury, say, from a solution in spirit of wine, and then applied the gauze so prepared, we found that the particles of iodide of mercury tended to gravitate down towards the skin, and there produce the most fearful irritation. It was absolutely necessary that it should be fixed. I tried various means, and among the rest a solution of starch, and then there came out this remarkable fact, that if a solution of starch is used with one of the ingredients employed for forming the biniodide of mercury by mixing a solution of iodide of potassium with solution of bichloride of mercury—if you dissolve the iodide of potassium in a weak solution of starch—the iodide of mercury thus formed in the nascent state associates itself with the starch particles in the most intimate manner, and the starch becomes entirely precipitated along with the iodide of mercury. If you take a drop of the red fluid formed by mixing these two solutions, and place it on a piece of calico, the watery material is absorbed by the calico and passes into the surrounding parts without colouring them, while the red spot with its insoluble iodide remains. If now you take a little iodine water, and apply it to the part of the calico moistened by the fluid that has exuded, you find that the iodine water produces no blue colour of iodide of starch, showing that there is no starch in the fluid that thus oozes out and leaves the iodide of mercury behind; whereas, if you take a solution of starch and apply it as such to a piece of fabric, as far as the fabric becomes moistened so far do you get a blue colour with the iodine. Here we have, as it seems to me, a somewhat analogous case to the so-called albuminate of mercury. We have the starch particles associated with the particles of iodide of mercury; the starch remains as such, the iodide of mercury remains as such, still they are attached to each other. And so the result was that when such a red solution produced by these two liquids was used for charging a gauze, the iodide of mercury was stuck to the gauze by means of the starch, and we had a most satisfactory arrangement in that respect. The iodide of mercury could not be washed out by water, nor did it in the least dust out. The value of this observation with reference to our present subject will appear shortly.

Being dissatisfied with the iodide of mercury, I turned my attention again

to the double cyanide of mercury and zinc. Looking back to my notes, I found such evidence of its superior antiseptic properties that I felt that we had deserted this material too readily. In the interval we had had other experience of importance with the alembroth gauze. Occasional late suppurations had at first occurred under its use, just as was the case with the double-cyanide gauze. These, however, had ceased to trouble us after my attention had been directed to the expediency of always using mercurial dressings in a moist condition. If they are used dry, the mercurial salt having no volatility, and having no power, therefore, of destroying any micro-organism in contact with it, whether derived from the manufactory or elsewhere, there could be no security that the dressing when applied was free from living organisms. This important object could, however, be infallibly attained if the dressing were used moist with an efficient germicidal solution. I put this idea into practice, and during the two years that have since elapsed we have never on any single occasion had to complain of these late suppurations. Might not the same immunity attend our cyanide gauze if we adopted with it the same expedient ?

The other objection to this double-cyanide gauze had been the irritation which it occasioned. Might not this have been due to the simple cyanide, which, as I have said, I used along with the double cyanide ? The simple cyanide is highly irritating, and just as with the sal alembroth, being freely soluble, it can be taken up by successive portions of discharge, and, when the discharge is free, may come to be in so strong a solution as to irritate. On the other hand, experiments on my own person had shown that the powder of the double cyanide might be kept applied to the skin for an indefinite time, whether moistened with water or with blood, without occasioning any irritation whatever. If this was really the explanation, and if, as our experiments seemed to indicate, the double cyanide could be trusted of itself, we might easily get rid of all irritation by using a double-cyanide dressing moistened with a weak solution of bichloride, say, 1 to 4,000, which, while it is securely germicidal, can never irritate. But here arose a new difficulty. I have told you that when we tried this double cyanide in a gauze at first, in order to prevent the dusting out, with its great inconveniences, we used glycerine ; but if we were to moisten the gauze with 1 to 4,000 solution of corrosive sublimate, we should run great risk of washing out the glycerine, and then the double cyanide would be free to dust out on drying. And, besides that, it must be admitted that the glycerine arrangement was not a good one, independently of that consideration, inasmuch as when the discharges flowed into the gauze they would wash away the glycerine, and then the double cyanide might be washed out also, and so fail in one of the most important requisites of an efficient antiseptic dressing—the storage of the

antiseptic in the dressing in spite of the discharge. How was this difficulty to be overcome ?

Now came to our aid our experience with the iodide of mercury and the starch. Might it be that the particles of the double cyanide would attract starch as those of the iodide had done ? It did not seem very likely, seeing that cyanogen is not known to have the special affinity for starch that iodine has. Still, I thought I would try the experiment. I prepared the double cyanide by mixing a solution of the double cyanide of mercury and potassium with a solution of sulphate of zinc. I tried this with one of the ingredients dissolved in a starchy solution, and, to my great satisfaction, I found that the precipitated double cyanide left a supernatant liquor almost absolutely free from starch, and that the particles which thus fell, the double cyanide with the starch associated, fixed themselves to a gauze in such a way that it did not in the least dust when dry. Not only so, but immediately after being charged with the precipitate diffused in water it might be washed in the wet state without the double cyanide being washed out of it, so closely did the starchy particles stick the double cyanide to the fabric. It is of great importance that in some way or other the double cyanide should be washed, because at the same time that an insoluble double cyanide is formed there are produced other double cyanides which are soluble, and which are in the highest degree irritating ; they must be washed out.

Well, I thought I had thus attained my object, and that by mixing starch with one of the two solutions necessary for forming this double cyanide and allowing the precipitate to deposit itself, then pouring in more water, and, after precipitation, decanting and repeating the process another time, so as to get rid of all the irritating soluble salts, and then diffusing the precipitate through a gauze, I should have all that I desired. But when I tried to get this done by a manufacturer I found that I got blundering after blundering in such a way as to make the thing practically hopeless. There was nothing for it but in some way or other to get the double cyanide from the chemist as a definite article, and then in some way devise a means of fixing that powder of the double cyanide to the fabric. I therefore naturally tried whether a solution of starch would answer this purpose, whether the starchy particles would associate themselves with the double-cyanide particles, not only in the nascent state, which we had before tried, but also when the already formed double cyanide was mixed with the starchy solution. I found that it did so ; that when a starchy solution was stirred up with the double cyanide in the proportion of one part of starch to two of cyanide, the starch was almost all precipitated, and the precipitate so formed adhered to the gauze in the most satisfactory manner.

But, though its adhesion was satisfactory enough, it turned out that the precipitate thus formed aggregated into a tenacious mass, which could not be diffused uniformly through the gauze, and here I was again at fault. This difficulty was overcome by first charging the gauze with the double-cyanide powder diffused in water, and then transferring it to a starchy bath. This at once fixed the cyanide in the gauze ; and, whereas, before it was placed in the starchy bath, the slightest squeeze made a milky fluid exude, no sooner had it been well penetrated by the starchy liquid than you might squeeze it as you pleased, and nothing came out but a clear fluid. I was much pleased with this, and it is in this way that I have prepared the gauze that I have used for the last twelve months, both in the hospital and in private practice. Still, this method had its disadvantages. When the gauze had been passed through the fluid in which the double cyanide had been diffused without any starch, it required very tender handling. If you gave it a squeeze, out came a quantity of double cyanide ; and it was plain that, although one might do it oneself satisfactorily, if we trusted to the manufacturers there would be an utter uncertainty as to what quantity of material might ultimately remain in the gauze.

Only lately has this difficulty been surmounted. It occurred to me that perhaps if the starch were first blended with the double cyanide and then dried and reduced to powder, if water were afterwards added to this dried dissolved starch associated with the cyanide, there might not be the same tendency to lumpiness and difficulty of diffusion. I found that the process did not answer quite as I hoped in the first instance, in this respect ; that the dried starch and double cyanide were extremely difficult to scrape off from any plate on which they were put to dry, and also very difficult to pound up and to diffuse for charging the gauze. But I got rid of these inconveniences by means of sulphate of potash, used for the same reason as in the preparation of Dover's powder—viz. that it is an inert substance, but with sharp, gritty particles. Mixing a pretty strong solution of starch with the double-cyanide powder, and adding to this a quantity of pounded sulphate of potash, the result is that you get a material which, after drying, is easily scraped off by the manufacturer, and easily reduced by him to an impalpable powder, which is then readily diffused in water, and makes a perfectly uniform gauze ; being mixed in large quantity with water in order to charge the gauze, the sulphate of potash is practically got rid of, and if any of it remains it does no harm, because it is inert. Thus we have the means of easily charging fabrics with this double cyanide.

I have spoken of diffusing this preparation in water, but in reality we employ for this purpose the 1 to 4,000 solution of bichloride of mercury, which fortunately does not in any way interfere with the process. I may remark

that the double cyanide, like the simple cyanide of mercury, though very efficient as an inhibitor, cannot be trusted as a germicide. There are different ways in which absorbent gauze such as this may be charged. One is to pass it folded in about sixteen layers through a trough, such as the one before me, which I have myself used, having a bar near the bottom to ensure the gauze being kept well under the liquid. It is then, as soon as you please, squeezed to press out superfluous liquid, and then, if wanted for immediate use, a simple way is to place the masses of gauze—say, six-yard pieces—in a folded sheet, turn the folded sheet over them, and roll it up. The folded sheet then absorbs the still redundant liquid, and you have moist gauze ready for use in five minutes. For the use of the ordinary surgeon it will probably be best to have the gauze dried, on the understanding that it is again moistened with 1 to 4,000 sublimate solution before being used. Here is a sample of the gauze in the dry state, which, you see, does not give off dust even when freely handled.

Other articles may be charged as well as gauze with this substance. The double cyanide being perfectly unirritating in its own substance, there is no objection to having an excess of it. If you take, therefore, some of the preparation and stir it up with 1 to 4,000 sublimate lotion, so as to produce an opaque liquid, and put linen rags into it, and then place them in a folded towel to take out the excess of liquid, you have your dressing ready prepared then and there. It can thus be very easily worked on an emergency.

We have seen that the double cyanide requires about 3,000 parts of blood serum to dissolve it. If, therefore, it is present in a gauze in the proportion of about 3 per cent., you will easily understand that blood-serum may soak through such a gauze time after time without washing the ingredient all out; so that it is a material which is admirably stored up in the dressing. That is one of its three great advantages, the others being that, while trustworthy as an antiseptic, it is completely unirritating. In actual practice the few layers placed next to the wound are washed in a solution of carbolic acid 1 to 20; this washes out the corrosive sublimate, which, though present in small amount, might irritate the wound to some extent. The carbolic acid soon flies off, and there is left in the application next the wound merely the unirritating double cyanide, and under this we find that not only do wounds, the edges of which are brought accurately together, unite beautifully by first intention, but even granulating sores heal by the gradual process of cicatrization from the edges—heal by scabbing in a way that we have never seen so satisfactory under any other dressing.

Having satisfied myself that this was really a useful material, I proceeded to request a manufacturing chemist to provide it for me on a large scale. Messrs.

Morson & Son, of Southampton Row, kindly undertook to do this, and I have to thank them for the great pains they have taken in carrying out experiments on this subject at my suggestion. Their manager, Mr. Taubman, soon informed me that in his opinion there was exceedingly little mercury in this so-called double cyanide of mercury and zinc. Very little mercury could be got from it on testing in comparison with what would be obtained if it were a true double salt. He asked if I was sure that the cyanide of zinc was not, after all, the thing that was efficacious! Was the idea of the double cyanide altogether a delusion? I need not say how much pleased I should have been if such had been the case—if we could have had the cyanide of zinc without any poisonous mercury in it as an antiseptic. The cyanide of zinc was a perfectly definite compound, there could be no mistake about it. I proceeded to make experiments, and I found, indeed, that cyanide of zinc had antiseptic properties. I made, for instance, experiments of this kind: I took a piece of glass tube like that which I hold in my hand, and packed it in two inches of its length with a piece of gauze charged with cyanide of zinc only, and then, holding it vertically, poured serum of horse's blood into it till the gauze was fully moistened; and then poured more in, till a quantity dropped out from the lower end equal to that which had produced saturation, the upper part of the gauze being thus thoroughly washed with the serum.

I then inoculated the top of the gauze with a potent septic drop. I had another such tube packed with gauze that had no cyanide of zinc in it, and I inoculated that in the same way after pouring serum upon it. I then put each into a well-fitting stoppered bottle, so as to prevent any evaporation, and placed them in the incubator. At the end of four days I opened the two bottles. That which contained the gauze without the cyanide of zinc stank, and, on taking portions of the gauze from either one end or the other, squeezing them and examining under the microscope the fluid that escaped, there were seen teeming multitudes of bacteria of various sorts. The bottle with the cyanide-of-zinc gauze, on the other hand, had a pure odour of hydrocyanic acid, which this gauze always has when moist. I then examined drops squeezed from both ends, and I found no bacteria in the clear serum that was pressed out, not only from the lower end, but even from the upper end, in the immediate vicinity of the inoculating drop, and where the gauze had been drenched repeatedly with the serum.

Now that, so far, is a result that no other antiseptic had ever given me. Take iodide of mercury, for example. Comparatively insoluble as it is, if you pour blood-serum upon it in such profusion you wash the iodide of mercury out, and if you inoculate septically the part so washed you induce bacteric

development. The test applied was, of course, an extremely severe one. In actual surgical practice the discharge which pours into the dressing is pure to begin with, supposing the wound to be aseptic at the outset. The septic agency only acts from without where the dressing has not been washed by the discharge, and in a far milder form than here, where a potent septic drop was used. Thus we had clear evidence that the cyanide of zinc really is an antiseptic. On the other hand, it turned out to be not so powerful antiseptically as our double cyanide so called. In order to compare the two salts I made another experiment similar to that last described. I packed three pieces of glass tube with gauze in two inches of their length, one of the gauzes being charged with cyanide of zinc and another with the so-called double cyanide (neither of these gauzes having been treated with solution of bichloride of mercury), while the third gauze was unprepared. Serum of horse's blood was poured into the upper end of each vertically held tube till it thoroughly soaked the mass of gauze, after which each gauze was inoculated septically at the centre of its upper end. The tubes were then placed vertically in stoppered bottles in the incubator. It happened that in the septic liquid used for the inoculation that I used there was, among other organisms, a species of streptococcus which had a remarkable power of producing an acid fermentation in blood-serum. After four days I proceeded to examine the contents of the three tubes. In the unprepared gauze there was utter putrefaction. In the gauze prepared with cyanide of zinc only, no putrefaction had taken place, but acid fermentation had occurred: both at the upper and lower end of the gauze litmus paper was reddened on application to the serum. In the putrid gauze turmeric paper was most intensely reddened, much more so than by normal blood-serum, an alkaline fermentation having occurred there. On the other hand, with the gauze that contained the double cyanide, with mercury as well as zinc, both at the upper and lower end the turmeric paper was reddened exactly as it was by the normal blood-serum. This state of things continued the next day; but on the following day, six days after the commencement of the experiment, I found that at the upper end, in the vicinity of the inoculated spot, this double-cyanide gauze purpled litmus, while at the lower end it still reddened turmeric. At the end of seven days the same condition persisted. After eight days, however, both the upper and lower end of the gauze purpled litmus. This peculiar septic organism, with the power of producing acid fermentation in serum, had gradually worked its way, in spite even of the cyanide of zinc and mercury; but the cyanide of zinc and mercury, you observe, had been much more efficacious than the cyanide of zinc alone. The cyanide of zinc had prevented the development of organisms that produced putrefaction, and only permitted the

development of the coccus that produced acid fermentation. Cyanide of zinc and mercury had for several days prevented all development. This was proof, therefore, that the mercurial element in our compound was valuable, and that we could not dispense with it.

It may be thought an unsatisfactory thing that there should have been any organism able to work its way thus through a gauze charged with our antiseptic. But I may remark, in the first place, that, as above stated, we tested the material exceedingly severely ; in the second place, that it was a long while before the organism penetrated the gauze even for a short distance ; and, in the third place, that penetration of micro-organisms through such a dressing into wounds does not seem to occur in practice, seeing that in the year during which I have used this antiseptic in my surgical work at King's College Hospital we have had no single instance in which we have had any reason to suspect septic change in the deeper parts of our dressing ; we have had no instance in which deep-seated suppuration has occurred in an operation-wound made through unbroken integument. If we have had any pus at all in such cases, it has been from the surfaces exposed between stitches or at situations where drainage-tubes have been inserted, where what I have termed antiseptic suppuration has occasionally shown itself, and even this in very slight degree. Such being the case, I feel not only permitted, but bound to bring this material under the notice of my professional brethren.

As to the composition of this so-called double salt, it is for the present uncertain. This much is already established : that the cyanide of mercury is in very much smaller proportion to the cyanide of zinc than Watts's *Dictionary* would lead us to expect from a true double salt. But what the precise composition of the salt is we do not yet know. I am having it investigated by the Pharmaceutical Society, who have kindly undertaken the work.

There is another use for this material besides the charging of dressings. The powder moistened with a weak solution of corrosive sublimate may be rubbed into hairy parts, when it will convert the hairs into an antiseptic dressing. Not long ago a medical friend of mine brought his wife to me with no less than seven sebaceous cysts in the scalp, requesting me to remove them. Having washed the hair with 1 to 20 carbolic-acid solution, I simply passed a comb over each tumour in the line where I was about to transfix without shaving at all ; and, after taking out the cysts, rubbed in some of the moistened powder into the hair in the vicinity. I then applied a dressing of cyanide gauze, and I was glad to learn that all the seven wounds had healed without disturbance.

We have now in the hospital a case of psoas abscess, shown to be of spinal origin, not only by the history of the case and the symptoms, but by the discharge

of a portion of bone with the pus. That case is pursuing a course which, allow me to say, psoas abscesses will pursue in the great majority of cases, if the surgeon uses a trustworthy antiseptic, and takes the same pains with dressing to the last as at the outset; that is, he will find his trouble rewarded by the complete cure of these formerly incurable cases. I say this because I grieve to think that psoas and lumbar abscesses still seem to be regarded as hopeless affairs by many surgeons. In this man's case the temperature has never been affected in the least; he has put on flesh rapidly; the discharge, after the purulent and curdy matter that existed originally in the abscess was got rid of, has been of a serous character, and is in small and diminishing quantity. But the opening made for the discharge is in the vicinity of the pubes, and the pubic hairs used, under such circumstances, to be a constant source of anxiety to us unless frequently shaved away. Here we rub in at each dressing a little of the moistened cyanide, and convert the hairs into an antiseptic application.

I will not at present enter into the details of the preparation of this material; these will be supplied in a note on a future occasion.

The sketch which I have given you of this investigation, though it has, I fear, wearied you, conveys but a small idea of the toil it has involved. There are those who still believe that the use of antiseptic substances in surgical practice is always useless, if not injurious. The germ theory of septic diseases is indeed now happily established incontrovertibly. All now admit that septic mischief in our wounds depends upon the development of micro-organisms in them derived from without. But the gentlemen to whom I refer are, more or less logically, disposed to trust everything to the antiseptic powers of the human tissues.

I believe I happened to be the first to direct attention to the antiseptic agency of living structures, and there is, perhaps, no one who attaches greater importance to it than I do. Without it, surgery in former days would have been absolutely impossible. Still I know too well from experience that it cannot always be trusted, and that the use of antiseptic adjuvants is in the highest degree important; and I have the satisfaction of knowing that there is among you a constantly increasing number who, when they have operated on an unbroken skin, with a fair field around for the application of their dressings, if they see septic inflammation occurring in the wound with its attendant dangers, know that it is their fault or the fault of the antiseptic appliances at their disposal. To those among you who are impressed with this conviction I offer the dressing which I have described as the most satisfactory that I have hitherto met with; and I venture to hope that you will regard it as a not unwelcome addition to your resources.

FURTHER OBSERVATIONS ON THE CYANIDE OF ZINC AND MERCURY

Read before the Hunterian Society, November 27, 1889.

[*Lancet*, 1890, vol. i, p. 1.]

[On the 27th of November, 1889, Sir Joseph Lister described to the Medical Society the operations he had done on two cases of long-standing dislocation of both shoulders, and in concluding made the following observations on the double cyanide of zinc and mercury.]

Mr. President, I have hitherto felt considerable hesitation in publishing cases in which the safety and success of an operation are essentially dependent upon strict antiseptic management; and my principal efforts for some years past have been directed to an endeavour to procure, if possible, greater simplicity and at the same time greater efficacy in our antiseptic methods. At a recent meeting of the Medical Society [1] I brought forward a kind of dressing which I believe will prove more satisfactory than any which has been hitherto employed. For the successful antiseptic treatment of a wound two essential points are of course necessary. In the first place, we should proceed so as to leave nothing septic in the wound before we apply the dressing, and in the second place we should put on such a dressing as we can thoroughly trust to keep out septic mischief until that dressing shall be changed. I had intended to bring before you this evening some points with regard to the former of these objects—the means by which the wound can be kept aseptic till the conclusion of the operation; but since the communication that I made to the Medical Society, I have been led to make further investigation into some matters regarding the use of the materials I then described, which seem to me of sufficient importance and interest to warrant me in taking this opportunity of bringing them before you. The material, I may remind you, is a sort of double salt, an amorphous powder, insoluble in water, composed of cyanide of mercury in combination with cyanide of zinc. It does not seem to be a true double cyanide, inasmuch as the proportion of the mercurial element is considerably less than that which should be in a true double salt; nevertheless, the mercurial element, as I have found, is of essential importance to the full antiseptic efficacy of the material. It was necessary that this powder, if introduced into a gauze or other fabric, should be fixed so as to prevent it from dusting out; for it is highly

[1] See *Lancet*, November 9, 1889 (page 309 of this volume).

irritating to the nostrils, and besides, if it dusted out, the dressing charged with it would lose more and more of its virtues. I described at the Medical Society a means by which this was prevented; how by the use of starch the powder might be fixed in any fabric which was charged with it. But I have long felt that it would be an exceedingly desirable thing if this material could in some way be coloured, because, being perfectly colourless, if a gauze is charged with it, we have to trust entirely to the manufacturer as to whether the antiseptic element is present in due proportion or is not. It would be very advantageous if it could be coloured, so that we might see by the tint where the antiseptic substance was, and whether it was uniformly distributed or otherwise. Therefore, before publishing the note which I had promised as to the preparation of the substance, I made attempts to stain this material. I tried various forms of dye, and I found that some of the aniline dyes are precipitated by this zinco-mercuric cyanide and some are not. For instance, magenta is not precipitated in the least, but methyl-aniline violet, and gentian violet, which seems to be a mere variety of the same thing—these are precipitated, and an exceedingly small amount of the dye is sufficient to give adequate colour to the double cyanide. I proceeded to charge a piece of gauze with some of this dyed cyanide, to see how it would tint it; and when it was dry I was much surprised to find that the gauze charged with the tinted cyanide did not dust in anything like the same degree as a gauze would have done which had received the untinted salt; so much so that a gauze charged with the tinted cyanide was very much on a par as to dusting with the gauze charged by means of starch.

Of course, if this were so, it would be a very satisfactory arrangement: we should dispense with the starch and also with a quantity of sulphate of potash which was used for purposes that I need not here refer to;[1] we should greatly simplify the method of manufacture, and also, by getting rid of the starch, we should make our gauze softer and more comfortable to the patient. It seems a remarkable thing that the dye should thus be able to fix the powder. Of course, we understand how the starch does it. The starch particles, becoming attached to the particles of the cyanide, glue them, as it were, to the fibres of the fabric. But how can we explain this dye, in the minute quantity in which we use it, answering the same purpose? I have here some gentian violet dissolved in 50,000 parts of water, and you see the great colouring power that this dye possesses. If I take a piece of gauze and dip it into the solution up to a certain point, you will see the gauze coloured up to that point, but the part that is moistened above by capillary attraction is colourless, showing the avidity with which the fabric seizes the dye. The dye has a remarkable fondness for

[1] See p. 318 of this volume.

the fabric ; at the same time, it is attached to the cyanide, for it is precipitated by it. We can thus understand that the dye may act as a go-between, attaching the cyanide to the fabric by virtue of its affinity for the fabric on the one hand and for the particles of the cyanide on the other. The mode of attachment is altogether different from that by starch, but the thing is done nevertheless. It seems to me astonishing that the dye should have this power. The quantity of gentian violet used is exceedingly small. We take, say, twenty grains of the salt, and diffuse it in sixteen ounces of a liquid containing only 1–50,000th part of the dye, draw a piece of the fabric through it, and so charge it with the requisite amount of the cyanide. If now we consider what proportion the gentian violet bears to the cyanide which it fixes, we find that there is only about one grain of the dye to 140 grains of the salt. But more than that, the molecule, the atom of the dye, is an exceedingly complex and heavy one ; so that if we consider how many there are in comparison with the atoms of the cyanide which it fixes, we find that there is only one molecule of the dye to nearly 600 molecules of the cyanide salt. It is simply wonderful that each molecule of the dye should have the power of fixing such a multitude of other molecules. It seems another instance of what I have ventured to call solid solution. It is not a chemical combination ; it is not a combination of one atom with one atom, but it is an attachment of one molecule with a multitude of other molecules. I have often contemplated with amazement the familiar fact of the solution of a soluble salt in water. Put a bit of common salt into a tumbler of water, and, as everybody knows, it will be quite uniformly distributed in a second or two. This marvellous fact implies that every molecule of the chloride of sodium has an area of a multitude of molecules of water in relation to it. If there were not the arrangement of a definite number of molecules of water round every molecule of chloride of sodium, there would not be an equable solution. So, I conceive, on the same sort of principle, without chemical combination, this dye influences a multitude of particles of cyanide in its vicinity. Here is a piece of gauze charged in the way I have described, and you notice its delicate violet tint ; and we have the satisfaction of knowing that, wherever we see the dye, there is the antiseptic salt. You also observe that, when freely handled, it does not dust materially. Thus we have the two advantages combined, one of which I had not hoped for—that while we have the material dyed so as to show its presence by its tint, it is also prevented from dusting.

Note.—After the above paper had been read, I was mortified to find that some gauze charged by aid of gentian violet dusted to a very inconvenient degree. This appeared to be due to the influence of the bichloride of mercury, which was

used in weak solution (1 part to 4,000) along with the gentian violet in the water in which the cyanide salt was diffused. Bichloride of mercury interferes, to a certain extent, with the precipitation of the gentian violet, and, leaving some of the dye in solution, causes tinting of the gauze independently of the presence of the cyanide salt, and at the same time it impairs the efficacy of the dye in fixing the salt to the fabric. Yet the use of the bichloride of mercury is a matter of great importance, for reasons which I have given elsewhere,[1] and it became necessary to look for some other dye on which the bichloride might not exert this prejudicial influence. I have found that there are several colouring matters which answer the purpose fairly well. Thus both carmine and prussian blue attach the cyanide salt to a cotton fabric perfectly so long as it is moist, but when it is thoroughly dry they are not very good as regards the question of dusting. The dye which I have found to comply best with all the requisite conditions is logwood, or rather the essential ingredient of logwood—haematoxylin, which is a definite crystalline substance, and not unduly expensive.

The manner in which I have found it best to use this substance is the following. It is incomparably better to apply it to the freshly precipitated and wet cyanide than to mix it with the salt after its particles have been aggregated in the process of drying. It may be well to mention here the manner in which the cyanide is prepared. Cyanide of potassium, cyanide of mercury, and sulphate of zinc are mixed together in solution in quantities proportioned to the atomic weights of $2KCy$, $HgCy_2$, and $ZnSO_4 + 7H_2O$; the cyanide of potassium and cyanide of mercury being dissolved together in $1\frac{1}{2}$ oz. of water for every 100 grs. of potassium cyanide, and added to the sulphate of zinc dissolved in three times that amount of water. The precipitate is collected on a strainer, and when well drained is washed with two successive portions of water, equal in quantity to that used for the solutions—viz. 6 oz. for every 100 grs. of potassium cyanide; at least this amount of washing being essential in order to free the precipitate sufficiently from the highly irritating soluble salts which are associated with it in its formation. The precipitate having been thus washed and drained, but not dried, it is thoroughly diffused with pestle and mortar in distilled water (6 oz. for every 100 grs. of potassium cyanide), containing in solution 1 part of haematoxylin for every 100 parts of the cyanide salt, the amount of which is known from the circumstance that the dry product of cyanide salt is almost exactly equal in weight to the potassium cyanide employed. Haematoxylin is readily soluble in a small quantity of hot water and remains in solution when added to a large quantity of cold water. The cyanide salt, while it precipitates the haematoxylin, changes its colour to a pale-bluish tint. This is advanta-

[1] Vide *Lancet*, loc. cit. (p. 300 of this volume).

geously enhanced by the addition of a little ammonia to the mixture, in the proportion of 1 atom of ammonia ($NH_3 = 17$) to each atom of haematoxylin ($C_{16}H_{14}O_6 3H_2O = 356$). More than this proves prejudicial. The ammonia is added in a dilute form, and it is convenient to have the dilution such that one fluid drachm of the ammoniacal liquid shall correspond to one grain of haematoxylin. The dye is further economized by allowing the ammoniated mixture to stand for three or four hours and stirring it occasionally, so that the ingredients may react thoroughly upon each other. If the mixture is filtered immediately, there is considerable loss of colouring matter. The dyed salt having been drained and dried at a moderate heat, is levigated, and may then be kept for any length of time fit for use. When employed for charging a dressing, it is diffused by means of pestle and mortar in solution of bichloride of mercury (1 to 4,000) in sufficient abundance to drench the fabric thoroughly, for which 4 imperial pints to 100 grs. of the salt will be found adequate. This will give a percentage of between 2 and 3 of the cyanide to the dry gauze. For reasons which I have stated elsewhere,[1] the gauze should always be used moist ; and if it be prepared for immediate use, as by the dispenser of a hospital, the process of drying may be omitted, the gauze, after being hung up for a while to drain, being deprived further of superfluous moisture by placing it for a while in a folded sheet. It may afterwards be conveniently kept moist by wrapping it in a piece of macintosh cloth. When obtained dry from the manufacturer, it should be moistened again with the weak corrosive sublimate solution before it is used.

[1] Vide *Lancet*, loc. cit. (p. 319 of this volume).

NOTE ON THE DOUBLE CYANIDE OF MERCURY AND ZINC AS AN ANTISEPTIC DRESSING

Contributed by Lord Lister to Sir Hector Cameron's Dr. James Watson Lectures, Glasgow, 1907.

[*British Medical Journal*, 1907, vol. i, p. 705. Together with a later Note.]

I HAVE often regretted that the double cyanide of mercury and zinc is not more generally employed, especially in foreign countries. This is, I feel sure, due to want of acquaintance with it, and I avail myself of the opportunity kindly afforded me by Sir Hector Cameron of saying a few words here regarding its nature, mode of preparation, and use.

Professor Dunstan, of the Imperial Institute, who most kindly undertook to investigate its composition, found it to be a double salt of very unusual type, being a tetrazincic monomercuric decacyanide, $Zn_4Hg(CN)_{10}$. Its insolubility in water appears to be also a very unusual feature in a double salt.[1]

Messrs. T. Morson and Son (of Elm Street, Gray's Inn Road, London, W.C.), to whom I am much indebted for the great pains they have taken in the preparation of the salt, have given me for publication the following formula :—

Pot. cyanid. 98 per cent.	46 parts.
Hydrarg. cyanid.	88 ,,
Dissolve in water	240 ,,
Zinc. sulphat.	102 ,,
Dissolve in water	120 ,,

When the solutions are cooled to about 60° Fahr., mix, collect the precipitate, and wash until no precipitate occurs with ammon. sulphid.

The white powder so obtained is dyed with rosalane, $\frac{1}{4}$ oz. being used to colour 4 lb. of the powder.

I tried various aniline and other dyes, and found none that answered its purpose in all respects so perfectly as purified rosalane (as supplied by Messrs. Meister, Lucius, and Brüning, of Hoechst-on-Main). Its principal object is to attach the cyanide to a fabric charged with it, and this it does with absolute security. At the same time the colour which it imparts to the white powder has the important effect of indicating the presence and distribution of the salt in the fabric.

[1] See *Journal of the Chemical Society* for 1892.

Gauze may be charged by drawing it in several thicknesses through a 5 per cent. solution of carbolic acid in which the dyed cyanide is diffused in sufficient quantity to be about 3 per cent. of the weight of the dry gauze, the liquid being constantly stirred to prevent deposition of the heavy salt.

Old rags or other absorbent fabrics can be readily charged by dipping several layers of them in the 5 per cent. solution of phenol, and dusting one surface with an excess of the powder, which is then diffused by folding the mass, and pressing it till a pretty uniform tint is produced. The absolutely unirritating character of the double cyanide makes a little excess of it in any parts a matter of indifference.

The solution of carbolic acid is used because the cyanide powder is much more readily diffused in it than it is in water, while it destroys any microbes present in the gauze as it comes from the manufacturer. The solution of phenol has the further advantage that it does not receive the slightest colour from the dyed cyanide, so that the depth of tint of the fabric charged with it is in exact proportion to the amount of salt it contains.

The gauze, as supplied by the chemist, is dry, and having lost the carbolic acid used in charging it, may have been subsequently contaminated with septic material. The double cyanide, though very remarkable for its inhibitory power over bacteric development, is without efficacy as a germicide ; and the microbes in the contaminating material would be free to develop in the deep parts of the gauze as soon as the cyanide in them had been exhausted. In case of moderate discharge this would probably never occur, thanks to the slight solubility of the salt and its secure fixation by the dye. But in case of copious effusion of blood and serum, the salt would in time be exhausted, and the microbes in the infective material would be free to develop. In order to guard against this risk, the dressing may either be damped throughout with the carbolic lotion, or, as ample experience has proved to be sufficient, a portion of the gauze in several layers, soaked with the lotion, may be applied over and around the wound and the rest of the dressing used dry.

Bichloride of mercury must not be employed for moistening the gauze, because it forms with the double cyanide a triple compound which is both feebly germicidal and highly irritating.

The double cyanide might, I believe, be very satisfactorily used in military practice as a first dressing, by dusting it over the wound with a pepper box, and covering with any absorbent material that might be at hand. The salt might be used with the utmost freedom, as experience has shown that there is no risk of its producing poisonous effects. Some surgeons who undertook to use the cyanide in this way in the late South African war, had unfortunately no opportunity

of doing so at the front.[1] But Mr. Cheatle informed me that granulating wounds behaved more satisfactorily with the cyanide than with iodoform, while the unpleasant odour of the latter was of course avoided. For further particulars regarding the use of the double cyanide I would refer to an address on the antiseptic treatment of wounds published in the *British Medical Journal* for the 28th of January, and the 11th and 18th of February, 1893.[2] The part in the number for the 18th of February contains the reference to the double cyanide.[3]

[1] Since the above was published I have learned from Mr. Cheatle that he had considerable experience with the use of the double cyanide as a first dressing on the battle-field in South Africa. Having found that the prevailing strong winds made dusting with the powder impracticable, he used a paste made by mixing the salt with a 1 to 20 solution of carbolic acid. This was readily improvised in his tent and was taken to the field in a bottle. With it he smeared the surface of the wound and the surrounding skin, and also his own fingers; thus combining the germicidal action of carbolic acid with the inhibitory effect of the cyanide. Cyanide gauze was then bandaged on. The cases were afterwards under the care of others: but he not unfrequently had the opportunity of seeing them again, and was well pleased with the results obtained. See 'A First Field Dressing', by G. Lenthal Cheatle, *British Medical Journal*, September 8, 1900.

[2] This address is printed at p. 349 of this volume.

[3] See p. 358 of this volume.

AN ADDRESS ON THE PRESENT POSITION OF ANTISEPTIC SURGERY

Delivered before the International Medical Congress, Berlin, 1890.

[*British Medical Journal*, 1890, vol ii, p. 377.]

Mr. President and Gentlemen.—At the International Congress in London, in 1881, Robert Koch demonstrated in King's College his then new method of cultivating microbes upon solid media. The illustrious veteran Pasteur was present at the demonstration ; and at its conclusion exclaimed, ' C'est un grand progrès, Monsieur.' How vast have been the extensions of our knowledge which have resulted from that great step in advance ! Of these none perhaps have been more striking than Koch's own brilliant discovery of the cholera microbe—picked out with unerring precision by his beautiful method from among the multitude of bacteric forms that people the intestinal contents, and grown and studied with as much definiteness as if it were a cabbage or a rose.

But while we have during the last nine years learned so much more of the nature and habits of the micro-organisms which invade our bodies, a new and surprising light has been thrown within the same period upon the means by which the living animal defends itself against their assaults. This we owe to the eminent naturalist Metchnikoff, who, having long carefully studied intracellular digestion in the amoeboid cells which form the main mass of the bodies of sponges and other humble organisms, was prepared to observe and rate at its true value an analogous process in the wandering leucocytes of vertebrata. He found that these migratory cells, with whose amoeboid movement we have been long familiar, feed also like amoebae, and while almost omnivorous in their appetites, have a special fondness for bacteria ; taking them into their protoplasmic substance and digesting them, thus preventing their indefinite propagation among the tissues. The cells which exercise this devouring function he termed phagocytes.

Various objections have been urged against Metchnikoff's views ; but so far as I am able to judge, he has met these effectively by his masterly series of researches ; and his observations have been confirmed and extended by several independent investigators.[1] For the sake of those among my audience who may chance not to be familiar with Metchnikoff's work, I am tempted to relate briefly

[1] See for example Dr. Tchistovitch, *Annales de l'Institut Pasteur*, 25 juillet, 1889, and Dr. Armand Ruffer, *British Medical Journal*, May 24, 1890.

some of his experiments. The green frog, below the temperature of 20° C. (68° Fahr.) is incapable of taking anthrax: the bacilli of that disease cannot grow when introduced under the skin of that animal. To what was this immunity of the frog to anthrax due? Were its juices an unfit pabulum for the microbe, or was the phagocytic action of its leucocytes the explanation? In the hope of solving this question, Metchnikoff formed a tiny bag out of the pith of the reed, and having placed in it some spores of anthrax, closed the bag and inserted it beneath a frog's skin. The pith wall of the bag allowed the animal's lymph to penetrate by diffusion, but excluded the leucocytes: and the result was that the spores sprouted and grew into luxuriant threads of anthrax in the lymph, which was thus proved to be a suitable medium for the growth of the bacillus. Meanwhile under another part of the skin of the same frog had been placed a small piece of the spleen of an animal that had just died of anthrax and contained the microbe in its most virulent form; but there, the leucocytes having free access, no growth occurred.

Another experiment on the same principle was still more instructive. It consisted in introducing the spores of anthrax into the anterior chamber of the eye of a frog, which, as we have seen, is naturally insusceptible of the disease; and also into that of a sheep and of a rabbit rendered insusceptible artificially by 'vaccination' with Pasteur's attenuated virus. The aqueous humour of the healthy eye contains few if any leucocytes to interfere with the perfect transparency essential to vision. Accordingly, the spores sprouted and grew for a while freely in the anterior chamber. Meanwhile, the growth of the bacillus occasioned irritation to the eye, resulting in the immigration of a constantly increasing number of leucocytes, producing turbidity and, in time, hypopion. If a drop of the aqueous humour was withdrawn at an early period after the commencement of the experiment, and examined with the microscope, it was found to contain anthrax bacilli, some of them free in the liquid, but others enclosed in the bodies of leucocytes. But a drop taken after a longer period had elapsed showed no free bacilli, all being now within the leucocytes, and exhibiting signs of degeneration in various degrees as the result of their advancing digestion. Finally the anthrax disappeared entirely and the eye cleared up, the animal in all cases remaining healthy, although inoculation into the aqueous humour proved a peculiarly deadly mode of infecting a susceptible animal.[1]

Here we see that the inflammation excited by the microbe becomes, through the medium of the leucocytes, the cause of its destruction. How little can the lamented Cohnheim have dreamed that his observation of the emigration of

[1] See *Annales de l'Institut Pasteur*, 25 juillet, 1887, pp. 320, 327.

leucocytes in inflammation would prove to have so far-reaching a bearing upon the pathology of infective diseases !

I have brought before you two samples of the kind of evidence upon which the phagocyte theory rests, and if we accept it, as I believe we must, it serves at once to explain much that has hitherto been mysterious in the relations of micro-organisms to wounds. Take, for example, that which the surgeon makes for the cure of hare-lip. Its posterior edge is perpetually bathed with the saliva, which contains many kinds of septic bacteria. But these do not enter and people the fibrine that glues together the cut surfaces, as they infallibly would do if those surfaces were composed of glass or any other chemically inert material destitute of life. It has long been very evident that the living tissues exerted a potent influence in checking bacteric development in such a wound ; but what was the nature of that influence ? This used to be an enigma, but now receives its natural explanation in the phagocytic action of the cells that crowd the lymph soon after its effusion.

At the London Congress I brought forward an experiment which proved that a blood-clot within the body may exert a powerful anti-bacteric agency. I will not repeat the details of that experiment further than to say that a very small piece of linen cloth soaked with putrid blood was mounted by means of silver wire in the interior of a short glass tube open at both ends, which was slipped into the jugular vein of a donkey, and kept in position between two ligatures. After two days the venous compartment was removed, and the coagulum within it investigated. In and near the glass tube it was in a state of advanced putrefaction, as was indicated by its foul odour and greatly altered appearance ; and microscopic examination showed that it abounded with bacteria. But near the wall of the vein it looked to the naked eye like a recent clot ; I could not detect in it any putrid odour, nor could I discover bacteria with the microscope.[1] Stained sections of these outer parts of the coagulum, made after hardening in alcohol, showed great multitudes of cells differing from one another in size and other characters, just as is often the case with Metchnikoff's phagocytes. I supposed that these cells must have been in some way or other the anti-bacteric agents, but how, I could not imagine. The phagocyte theory clears up the mystery.

By means of this same theory we can account for what would otherwise have seemed to me incomprehensible—the use, without evil consequences, of silk ligatures which have not been subjected to any antiseptic preparation. We learn from the experiments of Ziegler and others that leucocytes soon penetrate very thin spaces between plates of glass or other chemically inert foreign bodies inserted among the tissues. And we can understand that they may creep into

[1] See *Transactions of the London International Medical Congress* (p. 275 of this volume).

the intervals between the fibres of a silk thread and destroy any microbes that may have lodged there before they have had time to develop serious septic mischief. But there must surely be a limit to the thickness of the threads. No one, I imagine, would feel justified in leaving in the peritoneal cavity an unsterilized cord as thick as a finger. Dr. Bantock, whose remarkable series of successful ovariotomies may seem to justify his practice, does not, I believe, prepare his ligatures antiseptically; and I understand that he uses, for tying the pedicle of a tumour, silk twist of so strong a nature that it can be trusted to bear the needful strain, with a diameter of only about 1–30th of an inch. But it would surely be wiser to sterilize even so slender a cord. Who can say that septic mischief may not occasionally lurk in the ligature in a form which may baffle the phagocytes?

The success in abdominal surgery achieved by Bantock and Lawson Tait, without, it is said, the use of antiseptic means, proves a stumbling-block to some minds. But in truth the practice of these surgeons is by no means conducted without antiseptic precautions, nor would they, I am persuaded, desire that such an impression should prevail. Both are scrupulously careful in the purification of their sponges, and if there is one thing more important than another in the antiseptic management of wounds of the peritoneum it is the avoidance of impure sponges. Both observe the strictest cleanliness—which is surely an antiseptic precaution—for it owes its virtue to the fact that it presents the septic organisms in the smallest possible numbers and thus reduces their power for evil to the utmost that can be done by any measures that are not germicidal. Both these surgeons also wash out the peritoneum with water so as to get rid of coagula without injuring the peritoneal surface by rubbing it with sponges, and this is done in order to avoid the risk of sepsis in residual clots. The drainage of the peritoneum is another antiseptic measure, and Dr. Bantock, I am informed, has the sponges which absorb the serum wrung out of sulphurous acid, and changes them very frequently.

This is a department of surgery in which I have had but little personal experience. But I can see that while the measures to which I have referred are, so far as they go, highly valuable, it must be in itself a very desirable thing to avoid the direct application to the peritoneum of strong and irritating antiseptic solutions. But now that we are all agreed that microbes are the evil with which we have to contend, it is surely wiser to ensure by germicidal means their entire absence from our hands and instruments rather than trust to the most perfect cleanliness in the ordinary sense of the term. And if water is used for washing out the peritoneum, prudence seems to me to dictate that it ought to be freed entirely from living organisms, if this can be done without making it irritating.

This object is, I believe, aimed at by Dr. Bantock by boiling the water before using it, but I would advise as more effectual an extremely weak solution of corrosive sublimate, such as 1 in 10,000, which, as Koch has taught us, may be implicitly trusted as antiseptic, while it is not appreciably irritating and involves no risk of mercurial poisoning.

In general surgery, the direct application of strong antiseptic solutions is not attended with the same disadvantages as in operations in the peritoneal cavity. My practice for some time past has been to wash the wound, after securing the bleeding-points, with a pretty strong solution of corrosive sublimate (1 to 500) and irrigate with a weaker solution (1 to 4,000) during the stitching, and I have had no reason to complain of the results. To this, however, I must make one marked exception. When applied to the healthy synovial membrane of a joint, the 1 to 500 sublimate lotion produces inconvenient irritation, and therefore, when opening an articulation—as for suturing a transverse fracture of the patella—I abstain from the washing, and, as a substitute, have hitherto irrigated during the whole operation with the weak solution (1 to 4,000).

And yet I must confess that I have for a long time doubted whether either the washing or the irrigation was really necessary. These doubts have been raised partly by experiments—some of which I mentioned at the London Congress—which had proved to me that normal blood and serum, and even pus, were by no means favourable soils for the growth of microbes in the form in which they are present in the air—and partly by reflection upon the experience we had when we used the carbolic spray.

As regards the spray, I feel ashamed that I should have ever recommended it for the purpose of destroying the microbes of the air. If we watch the formation of the spray and observe how its narrow initial cone expands as it advances, with fresh portions of air continually drawn into its vortex, we see that many of the microbes in it, having only just come under its influence, cannot possibly have been deprived of their vitality. Yet there was a time when I assumed that such was the case, and, trusting the spray implicitly as an atmosphere free from living organisms, omitted various precautions which I had before supposed to be essential. Thus, in opening the pleura in empyema for the purpose of evacuating the pus and introducing a drainage-tube and afterwards in changing the dressings, I had previously applied over the opening a piece of cloth steeped in an antiseptic lotion to act as a valve and prevent the entrance of air during inspiration. But under the spray I omitted the valve and allowed the air to pass freely in and out of the pleural cavity, although I used the spray at such a distance from the producing apparatus that it was dry and transparent, with the particles of carbolic solution necessarily widely separated from each other.

And these particles cannot have been in more than instantaneous contact with much of the dust before it was drawn within the chest, and securely protected by the pus or serum there from any further action of the antiseptic. It is physically impossible that the microbes in such dust can have been in any way whatever affected by their momentary presence in the spray.

Yet we did not find our results in the treatment of empyema rendered worse by this false confidence in the spray. There are few more beautiful things in antiseptic surgery, as contrasted with the results of former practice, than to see the abundant purulent contents of the pleural cavity give place at once to a serous effusion, rapidly diminishing from day to day till, the opening being allowed to close, the pleura, restored to its healthy condition, resumes its normal function of absorbing gases; and, as the natural vacuum within it becomes re-established, the atmospheric pressure blows up the contracted lung, and brings it again into contact with the chest wall unimpaired in its dimensions. Such a case we had witnessed before the days of the spray, and such we continued to see during its use.

If, then, no harm resulted from the admission day after day of abundant atmospheric organisms to mingle unaltered with the serum in the pleural cavity, it seems to follow logically that the floating particles of the air may be disregarded in our surgical work; and, if so, we may dispense with antiseptic washing and irrigation, provided always that we can trust ourselves and our assistants to avoid the introduction into the wound of septic defilement from other than atmospheric sources.

Since we abandoned the spray, three years ago, we have been careful to compensate for its absence, not only by antiseptic washing and irrigation, but by surrounding the seat of operation with widespread towels wrung out of an antiseptic solution. For the spray, though useless for the object for which it was originally designed, had its value as a diffuse and perpetual irrigator, maintaining purity of the surgeon's hands and their vicinity as an unconscious caretaker. But if besides the spray we give up all washing and irrigation of the wound, our vigilance must be redoubled. Yet I believe that, with assistants duly impressed with the importance of their duties, the task would prove by no means difficult.

I have not yet ventured to make the experiment on any large scale, although I have long had it in contemplation. It is a serious thing to experiment upon the lives of our fellow men, but I believe the time has now arrived when it may be tried. And if it should succeed, then perhaps may be fulfilled my early dream. Judging from the analogy of subcutaneous injuries, I hoped that a wound made under antiseptic precautions might be forthwith closed com-

pletely, with the line of union perhaps sealed hermetically with some antiseptic varnish, and bitter was my disappointment at finding that the carbolic acid used as our antiseptic agent induced by its irritation such a copious effusion of bloody serum as to necessitate an opening for its exit ; hence came the drainage of wounds. But if we can discard the application of an antiseptic to the cut surfaces, using sponges wrung out of a liquid that is aseptic but unirritating, such as the 1 to 10,000 solution of corrosive sublimate, we may fairly hope that the original ideal may be more or less nearly attained.

We have already made of late considerable approaches towards it. Our wounds being no longer subjected to the constant irrigation of the spray, and carbolic acid having given place to the less irritating, though more efficient, solutions of corrosive sublimate, serous discharge is much less than formerly, and less drainage is required. In many small wounds where we used to find drainage imperative we omit it altogether, and in those of larger extent we have greatly reduced it. Thus, after removing the mamma and clearing out the axilla, I now use one short tube of very moderate calibre, where I used to employ four of various dimensions. But it would be a grand thing if we could dispense with drainage altogether ; without applying the very firm elastic compression adopted by some surgeons, which, besides involving the risk of sloughing of parts of low vital power, with the chance that it may after all fail in its object, proves often extremely irksome to the patient.

It remains for me to say a few words regarding the best form of external dressing. Some surgeons have thought that simplicity and efficiency may be combined in the maximum degree by the use of cotton-wool sterilized by heat. But though it may be a simple thing to heat the wool appropriately by means of suitable apparatus in a public institution, for the ordinary practitioner it would be impracticable. And as regards efficiency, I need hardly remark that cotton-wool, merely aseptic, can only exclude septic mischief when it is in the dry state. When it is soaked to its external surface with a copious discharge, it must be liable to become septic *en masse*. And however well we may succeed in the future in diminishing or abolishing discharge from wounds made by the surgeon, there must always remain cases in which it will occur in greater or less amount.

Contused wounds, for example, into which dirty material of one kind or another has been introduced before they are seen by the surgeon, must be purified by the use of powerful antiseptic means, and must, for a while, discharge freely. The same is to be said of cases in which we make the attempt, often with signal success, to restore an aseptic condition in a part affected with septic sinuses. Again, there are abscesses in which, in the present state of our knowledge, we cannot avoid the occurrence of considerable serous oozing, and in which a

perfectly trustworthy antiseptic dressing is a matter of life and death. And whenever discharge is considerable, it is essential that the dressing be of a kind which will not permit the development of septic organisms in it, although it be saturated throughout ; and this can, I believe, only be attained by the use of chemical antiseptic substances.

I have for some time past employed for this purpose a combination of the two cyanides of zinc and mercury, which appears to fulfil the requisite conditions of antiseptic efficacy and due storage of the agent in spite of free discharge, together with absence of irritating properties. Having already published on this subject, I will not detain the members of the Congress with details regarding it, further than to say that since the date of that publication Professor Dunstan, of the London Pharmaceutical Society, has devised means by which the substance can be prepared in a perfectly definite manner, and containing twice as great a percentage of the cyanide of mercury as that which we have hitherto used ; and, as I have ascertained that the cyanide of mercury is the more important ingredient antiseptically, and also that its larger amount in Dunstan's material does not make the salt irritating, we may fairly regard the new preparation as an improvement. And yet we have had no need to complain of this substance in the form in which we have used it hitherto. Those who have followed my practice at King's College Hospital during the year and a half in which this dressing has been employed will agree with me that we have secured a constancy of aseptic results which has more than ever justified the performance of operations once quite unwarrantable.

In thus referring to my own work, I do so, believe me, in no boastful spirit ; but in the hope of stimulating some of those whom I address on this memorable occasion to more thorough earnestness in pursuit of the great objects of antiseptic surgery.

ON THE PRINCIPLES OF ANTISEPTIC SURGERY

[*Virchow-Festschrift.* Bd. iii (1891).]

The fundamental truth on which Antiseptic Treatment in surgery is based is now universally recognised. All are now agreed that the once formidable complications of wounds are caused by living organisms derived from the external world and incapable of originating *de novo* within the animal body. But the practice which has resulted from a recognition of this truth varies greatly in the hands of different surgeons ; and it is of great importance to endeavour to ascertain, in accordance with the present state of our knowledge, what are the points which it is essential to attend to, so that on the one hand we may be freed from the encumbrance of needless precautions, and on the other hand may not omit anything which is conducive to such constancy of aseptic results as can alone justify many operations which are in themselves desirable but fraught with grave dangers if septic complications arise.

The original idea of the antiseptic system of treatment was the exclusion of all microbes from wounds. It had long been obvious that the putrefaction which at that period attended all wounds except the very small proportion which united entirely by the first intention, was a grievous cause of mischief. Various antiseptic substances were used to mitigate the evil, but entirely to prevent its occurrence appeared hopeless so long as it was believed, in accordance with the teaching of Gay Lussac, backed by the high authority of Liebig, that the access of a minute quantity of free oxygen could start progressive fermentative changes in organic substances. Where discharge escaped from a wound, oxygen must be able to enter. But when Pasteur had shown that putrefaction and other fermentative changes were caused by the growth of micro-organisms, and had at the same time demolished the idea of spontaneous generation, the problem of the prevention of putrefaction in wounds seemed no longer hopeless. The fermentative microbes could not arise *de novo* in the blood or tissues, and the experience of the absence of all danger in simple fracture seemed to indicate that they could not gain access by any other channel than an open wound. It therefore seemed possible that putrefaction might be entirely prevented in wounds by treating them with some substance which might destroy the life of the microbes, though not excluding the atmospheric gases.

The first attempt to put this idea in practice was made with compound fractures, in which the evils caused by putrefaction were especially manifest

and disastrous; and the substance which I employed for the purpose was undiluted carbolic acid, a most potent germicide. The experiment answered my most sanguine expectations; the compound fractures following the same safe and tranquil course as simple ones.

The powerful caustic property of the agent employed was of trivial moment in comparison with the greatness of the danger to be averted in compound fracture, but made it quite unfit for application to incised wounds. But we soon found that carbolic acid could be used with equally good effect under various forms of dilution, so that the application of the principle could be extended to wounds in general. The result was a complete revolution in the practice of surgery. Hospitals which had previously been little short of pest-houses became more healthy than private dwellings had been before; and operations which had been from time immemorial prohibited on account of their danger were freely and successfully performed.

Meanwhile it soon became apparent that putrefaction was by no means the only evil that was avoided by treatment conducted on these lines. Hospital gangrene, though in itself entirely free from unpleasant smell, disappeared as if by magic, and the same was the case with erysipelas and odourless forms of suppuration. This naturally suggested the idea that various diseases to which wounds were liable, though not septic in the original sense of the word, were, like putrefaction, caused by microbes, each disorder having, probably, its own specific organism; a view the truth of which has been amply demonstrated by the study of bacteriology, to which the success of antiseptic treatment in surgery gave a powerful impetus.

Thus the attempt to exclude microbes entirely from wounds was followed by results which more than fulfilled the highest hopes entertained of it. Yet the advance of knowledge has shown that to carry out such an idea in its entirety is on the one hand impossible, and on the other hand unnecessary.

It has been ascertained that many common bacteric forms produce spores which resist for a long time the germicidal power of all known agents which could be used in operations. Hence to exclude living microbes entirely from wounds is an impossibility.

It is, on the other hand, happily unnecessary; and that for more reasons than one. In the first place, it appears that none of the bacteria which can cause mischief in wounds are of the spore-bearing kinds,[1] while the sporeless bacteria, such as the various streptococci and staphylococci and the *Bacillus pyocyaneus*,

[1] An exception was once met with by von Volkmann who observed anthrax result from the use of the catgut ligature, prepared, no doubt, from the intestine of a sheep that had died of that disease. But this risk having been pointed out, care is now taken to treat the catgut in such a way as to make such an occurrence impossible.

have been shown by the most careful recent investigations to be deprived of life within a minute by a 1 to 20 watery solution of carbolic acid,[1] the agent which we have always trusted for the purification of sponges and instruments, the hands of the operator, and the integument of the patient at the seat of operation.

These are the points of greatest importance to attend to during the performance of an operation, the once dreaded atmospheric dust being, as it would seem, a matter that may be disregarded. We learn from various independent inquiries that the effects of micro-organisms upon the living body are greatly influenced by the dose, that is to say by the numbers in which they are present at the point of introduction.[2] And this seems to provide a clue to understanding how bacteria in the attenuated and minutely subdivided form in which they are present in the atmosphere may be effectually disposed of by the natural antiseptic action of the blood and the tissues. In pre-antiseptic days this natural antisepsis often triumphed over enormous obstacles, preventing the layer of lymph and coagulum between cut surfaces from putrefying, in spite of the use of unclean sponges, instruments, and hands, and the presence, over the outlet of the wound, of water dressing which, though cleanly when applied, was within a few hours a stinking, putrid mass. But under the converse conditions in which we now operate, this beneficent natural agency may, it seems, be implicly trusted, if the microbes which enter the wound are only such as are deposited from the atmosphere. That such is really the case has become apparent from the uniform attainment of aseptic results by the use of means which could not, as we now see, completely exclude living atmospheric organisms, whether spore-producing or otherwise, during the performance of operations. The carbolic spray, which was introduced for the purpose of destroying the microbes of the air, could not, from its physical constitution, really effect that object,[3] and owed whatever good it did to its properties as an irrigator. But no system of irrigation that can be devised can prevent, during the application of the sutures, the occasional entrance of air into deep parts of the wound from which blood is oozing on which the liquid of irrigation cannot act Yet under the use of the spray or other forms of irrigation the results obtained may be fairly described as uniformly aseptic, when opportunity for efficient antiseptic work was afforded by unbroken skin of sufficient extent for the needful dressings. The complete exclusion of living atmospheric organisms during operations is impossible; but no harm appears to arise from their introduction.

[1] Vide Behring, 'Ueber Desinfection,' &c., *Zeitschrift für Hygiene*, Neunter Band, 1890, p. 417.
[2] Vide W. Watson Cheyne, *Suppuration and Septic Diseases*, Pentland, Edinburgh, 1889, pp. 73 ff.
[3] Vide *Transactions of the Tenth International Medical Congress*, vol. i, p. 32 (p. 336 of this volume).

Confirmation of this opinion has lately come from an unexpected quarter. The glowing accounts published by Koch ten years ago[1] of the antiseptic properties of corrosive sublimate, led us to adopt solutions of that substance in place of the 1 to 40 carbolic lotion for washing and irrigating our wounds. But, beautifully conclusive as Koch's experiments appeared, it turns out that the effects of the bichloride supposed to be due to germicidal action were in reality caused by the inhibitory power which, as was shown by Koch, that agent possesses even when present in extremely minute proportions; and that if, instead of being merely washed away, however carefully, from the objects on which it has been made to act, it is got rid of entirely by converting it into inert sulphide, the original reports have to be toned down to an extraordinary degree. Instead of the resisting spores of anthrax being killed, as we were at first led to believe, by being dissolved in 20,000 parts of bouillon acting for ten minutes, we now learn that a solution of twenty times that strength fails to deprive them of vitality by an action of some hours' duration.[2] And even some sporeless micrococci resist the germicidal action of the bichloride in a most unexpected manner. Thus Behring found that the *Staphylococcus pyogenes aureus* was not destroyed completely by a 1 to 1,000 solution of sublimate in bouillon acting for twenty-five minutes at about the ordinary temperature of wounds, 22° C.[3] Such being the case, we cannot suppose that corrosive sublimate as I have used it can have acted with germicidal effect upon that microbe. My practice has been to abstain from irrigation during the operation, and at its conclusion wash the wound with 1 to 500 solution and irrigate during the application of the sutures with a 1 to 4,000 lotion. As regards the washing, considering its very brief duration and also that the germicidal action of sublimate is greatly interfered with by albuminoid substances, such as the coagula in which the microbes are entangled, I cannot conceive that the process can have acted destructively on any of the *Staphylococcus pyogenes aureus* which might have been deposited on the wound from the atmosphere. And as to the irrigation, it was obviously simply nugatory with respect to that species of microbe.

Nevertheless entire success attended this use of the sublimate; and we are therefore forced to conclude either that the *Staphylococcus pyogenes aureus*, which seems to be the most frequent cause of suppuration in man, never fell upon our wounds during the space of about seven years from the air of our operating theatre, or else that, although present, unharmed by our sublimate lotions, it failed to develop. It has, indeed, been shown by experimental

[1] Vide 'Ueber Desinfection' by Dr. Robert Koch, *Mittheilungen des Kaiserlichen Gesundheitsamtes*, Band I, Berlin, 1881.

[2] Vide Behring, op. cit., pp. 441, 443.

[3] Vide Behring, op. cit., p. 404.

research [1] that the pyogenic organisms are by no means abundant constituents of the dust of hospitals; but their rarity can hardly explain the entire absence of suppuration in our wounds for so long a period, and the fact can, I think, only be explained by the co-operation of the natural antisepsis.

It would, however, be a mistake to suppose that no good can ever be done by corrosive sublimate used in the manner which I have described. Resisting as the staphylococci have shown themselves to that agent, there are other microbes very mischievous to wounds, such as the *Streptococcus pyogenes*, the streptococcus of erysipelas and the sporeless *Bacillus pyocyaneus*, which are destroyed by very much weaker solutions.[2] And it may be well that if, as once occurred in my experience, a careless nurse were to come fresh from fomenting a bad case of erysipelas and, without changing her dress, to hand sponges at an operation, the washing with 1 to 500 sublimate lotion might avert a calamitous attack of that disease.

But if, for the sake of guarding against carelessness on the part of our assistants we think it prudent to wash our wounds before stitching them, it will, I believe, be wise for us, in the present state of our knowledge, to revert to that which we trusted in former years, the 1 to 40 solution of carbolic acid. This agent has been shown to be far more uniform in its action upon micrococci than corrosive sublimate. Behring found that even the staphylococci are killed in a minute by a solution of about the strength mentioned,[3] while, at the same time carbolic acid is not hindered in its action by albuminoid substances in at all the same degree as sublimate is. The 1 to 40 solution, while it appears adequate for the purpose, is far less irritating than the 1 to 20 lotion, and therefore induces less discharge and involves less necessity for drainage. But here, as in other cases, prevention is better than cure; and it must ever be borne in mind that nothing that the surgeon can do can make up for want of care in his assistants. If, for example, a pair of forceps is handed to the operator with the intervals between its teeth occupied by dry septic pus, and a portion of this dirt becomes detached and left in the wound, the evil cannot be corrected by any antiseptic wash that is now at our disposal or any that the world is likely ever to see. Hence I must repeat that our chief attention must be devoted to enforcing scrupulous care on the part of all concerned in the operation in guarding against the grosser forms of septic impurity. Towels dipped in an

[1] Vide Cheyne, op. cit., p. 88.

[2] My colleague Professor Crookshank has ascertained that a cultivation of the streptococcus of erysipelas in bouillon is killed by a solution of sublimate in 4,000 parts of water acting for one minute.

[3] Vide Behring, op. cit., p. 417. Crookshank finds that *Staphylococcus pyogenes aureus* is killed in one minute by 1 to 50 watery solution of carbolic acid.

antiseptic lotion and spread widely round the field of operation are an important aid in this respect.

The foregoing considerations indicate that the troublesome complication of irrigating during stitching may be safely omitted.

The operation being concluded, an external dressing such as shall effectually prevent the access of septic mischief till healing is accomplished is, of course, a matter of essential importance. For this purpose some surgeons have of late years employed materials merely aseptic, such as cotton wadding sterilized by heat. But such a dressing having nothing in it to counteract any accidental defilement, must demand an almost impossible degree of care in its manipulation in order to ensure that it is truly aseptic as left upon the patient. The mere aseptic dressing has also the fatal defect that it is liable to be occasionally soaked to the surface with discharge, in which septic development will then be free to spread inwards to the wound. I believe, therefore, that a dressing, in order to be trustworthy, must be charged with some chemical antiseptic substance. Ideally this substance ought to possess three qualifications; it should be thoroughly reliable in its antiseptic action, it should be capable of being stored up in the material charged with it so that it cannot be washed away by the discharge before the dressing is renewed, and it should be free from irritating properties, so as not to interfere with healing. The nearest approach to this ideal which I have yet met with is presented by a combination of cyanide of mercury with cyanide of zinc. Chemists are not agreed as to whether the two constituents are united in true chemical combination. But however this may be, their association is so intimate that, whereas the cyanide of mercury alone is freely soluble in water and serum and highly irritating to the skin, the combination is almost absolutely insoluble in water and requires about 3,000 parts of serum to dissolve it at the temperature of the human body. Hence, if diffused in a dressing, it remains most efficiently stored in spite of very free discharge; while it is so slightly irritating as not to interfere materially with healing, requiring no protective layer to be interposed between it and the wound. As regards its antiseptic virtues, it is very remarkable for inhibitory efficacy, i.e. for the power of preventing the development of microbes in its vicinity, even in the liquid which tests more severely than any other the antiseptic properties of mercurial compounds, viz. the mixture of serum and blood corpuscles which constitutes the first and most copious discharge from a wound. It is, however, very feeble as a germicide: and in order to make sure that a dressing containing it shall have no hurtful organism alive in it when it is applied, it is well to damp the dressing with a germicidal lotion before applying it. For this purpose a 1 to 20 carbolic solution seems the best that can be em-

ployed.[1] The carbolic acid soon flies off and leaves nothing in contact with the wound but the unirritating cyanide and the fabric charged with it.

In changing the dressing, the skin around the wound is purified on each occasion with carbolic lotion, the wound itself having been previously covered with some trustworthy antiseptic material to avoid the chance of its contamination. These may seem minute details to refer to here ; but in truth they all illustrate principle.

In wounds already septic, attempts are made with more or less success to restore the aseptic state ; but this is a matter on which it is not now needful to enter.

Abscesses, whether acute or chronic, are a field for antiseptic surgery which yields very beautiful results, in striking contrast with those of former practice and at the same time of great pathological interest.

As an example of the former class let us take a case of extensive suppuration of the mammary gland during lactation. Here, under the old system of poulticing, protracted suppuration followed the evacuation of the cavity ; and in spite of free incision, sinuses often remained which could only be cured by laying them open throughout their extent. Under antiseptic management, the abscess being emptied by a puncture sufficient to admit the introduction of a drainage-tube, nothing but bloody serum is found next day upon the dressing, the serous discharge diminishes rapidly, and healing is complete in a very few days, sinus of the mamma being a thing unknown.

To illustrate the chronic class may be taken a psoas abscess consequent on tubercular caries of the spine. Under free incision and poulticing, such cases were almost invariably fatal. If the patient survived the acute fever of the first few days, he perished after a longer or shorter period of hectic caused by protracted free suppuration. But if under antiseptic precautions a drainage-tube is inserted and, without the introduction of any medication into the abscess, a trustworthy dressing is applied, no fever whatever occurs, and the discharge, as in the acute case, is as a rule sero-sanguineous at the outset and afterwards merely serous and soon trifling in amount ; and if scrupulous antiseptic care is maintained a cure is almost always at last effected.[2]

[1] A solution of bichloride of mercury is of little value for this object, inasmuch as it forms with the two cyanides a soluble salt of very feeble germicidal power.

[2] Acting on a hint derived from the Vienna practice of washing out these abscesses with a weak antiseptic lotion and then introducing iodoform and closing the incision, I have of late years washed the cavity with 1 to 10,000 solution of corrosive sublimate and stitched the wound ; dispensing with the iodoform which, I believe, cannot effect what has been expected of it, while it involves a certain risk of iodoform poisoning. The results have been much on a par with those of the Vienna practice. Quite recently, however, we have derived very great advantage from adopting the use of the ' flushing gouge ' suggested by Mr. Arthur Barker, by which the pyogenic membrane and all cheesy matter, with sequestra,

When first I witnessed the remarkable fact of the entire cessation of suppuration as a result of relieving abscesses of their contents and at the same time preventing the access of micro-organisms from without, I inferred that microbes could have nothing to do with the production of the pus, but that it was caused by inflammation which, however it had originated, was kept up by the tension of the pent-up liquid operating through the nervous system. This view has, however, been disproved for both acute and chronic abscesses; for the acute by Ogston's observation that they invariably contain micrococci, which experiment has since proved to be truly pyogenic, and for the chronic by Koch's discovery of the tubercle bacillus, which we now know to abound in the pyogenic membrane and caseous material in such cases. Some other explanation is therefore called for. As regards acute abscesses, if we consider what is the primary difference made by a poultice, as compared with an antiseptic dressing, we see that putrefaction is admitted by the former, while it is excluded by the latter. And I conceive that the acrid products of putrefaction act injuriously upon the pyogenic membrane and prevent destruction of the micrococci by the natural antisepsis which is always disposed to operate, but, so long as the abscess is unopened, is hindered by the disturbing influence of tension caused by the rapidly accumulating pus.

In chronic abscesses the slowly increasing contents cause but little tension But we know that a very slight degree of tension on the wall of a cavity containing fluid is sufficient to keep up chronic inflammation in the sac and surrounding tissues. This is well illustrated by the obstinacy of chronic bursitis patellae so long as the bland serous contents remain in the sac; and conversely the rapid cure that takes place when provision is made antiseptically for the escape of the fluid. Not only does the tendency to abnormal effusion of fluid cease, but the inflammatory induration around the sac speedily disappears. And as inflammation, in whatever degree, is always a cause of weakness of the part affected by it, we can understand that, so long as a psoas abscess remains unopened, the enfeeblement of the surrounding tissues, caused in the way referred to, may place the tubercular vertebrae at a disadvantage in their combat with the tubercle bacilli and prevent them from throwing off the disease as they would have done before abscess had occurred, if the spine had been placed at rest in the recumbent posture. If tension is relieved by antiseptic drainage, the tissues are allowed to recover vigour and assert their supremacy. But if such an abscess is poulticed after incision, though tension is removed, far worse causes of disturbance come into operation. The pyogenic organisms, previously absent,

are simultaneously scraped away and washed out. An antiseptic dressing is of course applied to the sutured wound, which may either heal at once throughout or furnish a temporary leakage of serum.

are admitted, and along with them the microbes of putrefaction, the products of which are at first absorbed by the sac and cause the primary toxic fever, but soon by their irritation convert the pyogenic membrane into a huge granulating surface which suppurates like an ulcer under water dressing. The tubercle bacilli meanwhile are allowed to develop at will in the tissues enfeebled by this fresh cause of disturbance.

Even in abscesses with fetid contents antiseptic treatment is often rewarded by brilliant success. I once opened an abscess in the lumbar region, giving exit to a brown liquid, closely resembling thin faeces and with a smell like that of putrid intestines in the dead-house. Being provided with an antiseptic dressing, I applied it, and on changing it next day I was, I confess, surprised as well as delighted to see nothing issue from the opening but a few drops of transparent and odourless serum. Microscopic examination showed the original contents to consist almost entirely of closely packed very slender bacilli in active writhing movement ; of what species I know not. Healing took place rapidly with a typically aseptic course.

In that case I picture to myself the following series of events. The colon was at some spot affected with inflammation not severe enough to cause death of its tissues, but sufficiently intense to prostrate for the time the agency by which, in a healthy state of the bowel, bacteria in the faeces are prevented from passing through its walls. One or more of this particular species of bacillus, having traversed the inflamed intestine, developed in the tissues outside the bowel, and, by a peculiar fermentative action, transformed the effused liquor sanguinis into the offensive material of the abscess contents. This species of bacterium, however, while it throve on this foul pabulum, was unable to grow in pure blood, and when the cavity of the abscess was flushed with liquor sanguinis effused from its wall after it was opened, the microbe ceased at once to develop ; and the abscess followed the same course as if no unusual organism had been present.

Whatever may be thought of this explanation, I venture to urge that all abdominal abscesses with foul contents (excepting those which obviously contain faecal matter) should be afforded the chance of following an aseptic course under antiseptic management.

Submucous abscesses of the rectum pointing beside the anus have always fetid contents, but if carefully treated antiseptically will, as a rule, heal without the occurrence of 'fistula in ano', i.e. without the formation of a communication with the interior of the intestine.[1]

[1] See p. 215 of this volume.

AN ADDRESS ON THE ANTISEPTIC MANAGEMENT OF WOUNDS

Delivered at King's College Hospital in the London Post-Graduate Course, January 18, 1893.

[*British Medical Journal*, 1893, vol. i, pp. 161, 277. 337, with subsequent Corrections.]

This day five weeks the patient before you was operated on for a badly united fracture of the patella. He had been kicked on the knee by a horse twelve months previously. In spite of the care of excellent surgeons his limb was in a very useless condition. He could not raise it at all in the extended position, and, in short, he was a complete cripple. When we operated both the upper and lower fragments were firmly adherent to the bone beneath, and separated by a considerable interval. The operation was difficult and protracted, but at length we succeeded in getting the fragments together and fixing them by means of two stout silver sutures. The patient, as you see, can now walk without a stick; he can raise the limb freely in the extended position, and bend it through a limited angle. A useful limb is already assured to him, and he will no doubt acquire in course of time a much greater degree of movement than there is at present. I bring this case before you as an illustration of what may be done by antiseptic measures. In my opinion, such an operation would be unjustifiable unless the surgeon could say to himself with a good conscience that he was practically sure of avoiding septic contamination of the wound. If you consider how terrible the disaster would probably be if septic suppuration occurred after such an operation, I think you will see that I am warranted in this view.

I propose, therefore, now to offer a few remarks as to the principles on which we proceed and the means we employ in order to attain constancy of aseptic results in our wounds. The matter divides itself into two heads: first, during the operation to avoid the introduction into the wound of material capable of inducing septic changes in it; and, secondly, to dress the wound in such a manner as to prevent the subsequent entrance of septic mischief.

As regards the former of these heads, advancing knowledge has enabled us greatly to simplify our procedure. When I first entered upon this subject, knowing as we did that our wounds, with rare exceptions, underwent putrid suppuration, it was natural to suppose that they were very favourable soils for the development of septic organisms. We knew from the experiments of Pasteur that the air of every inhabited place teemed with microbes of various kinds. We

were in almost entire ignorance of the various species of bacteria, and there was no reason then to doubt that any of them getting into a wound would produce serious mischief. Happily, however, we now know the case to be really extremely different. It is but a small proportion of these organisms which are capable of doing mischief in surgery; and even such species as do produce injurious effects, when they develop in wounds, are by no means always sure of gaining a footing when introduced into them. This depends upon two circumstances. In the first place, we have learned that although putrid blood teems with bacteria of various kinds, some of them in the highest degree pathogenic, normal blood-serum is by no means a very favourable soil for the growth of bacteria, provided that they are in an attenuated condition—not in too strong a dose. I may illustrate what I mean by a simple experiment. If we draw blood, with antiseptic precautions, say from a horse or from an ox, into purified stoppered bottles, and simply place them in a stove at the temperature of the human body, the blood remains permanently unaltered. If we dip the point of the finest needle into already putrefied blood and touch the blood in one of those bottles with the needle so contaminated, and replace the bottle in the stove, to an absolute certainty within twenty-four hours the blood is foul and putrid throughout. But if, instead of applying the putrid blood in substance, I mixed it with an abundance of sterilized water, so as to diffuse the bacteria widely, at the same time washing them of their products, I found that a small drop of this diluted putrid blood, though it contained abundance of bacteria, failed for days together to induce putrefaction. The grossly putrid material—if I may so speak—inevitably causes putrefaction in the blood, but the washed and widely diffused bacteria are unable to do so.[1]

Then there is another even more important point, and that is that the living animal body has the power of defending itself against microbes introduced into it, chiefly, as it appears, by the process of phagocytosis, which Metchnikoff has revealed; so that if the micro-organisms are not introduced in too large a dose, they are consumed by the wandering cells. These two great truths, then, have been taught us by advancing science: that normal serum is not a good soil for the development of attenuated microbes, and that bacteria introduced among the tissues, if in not too concentrated a form, are disposed of by phagocytosis. The result is that microbes in the form in which they are present in the air are unable to develop in our wounds; and thus we are able to disregard in our operations the once dreaded atmospheric dust.

Hence we may dispense entirely with irrigation, whether in the form of the spray, which was a kind of irrigation, or in any other; in fact, our operations

[1] See *Transactions of the International Medical Congress*, 1881, vol. ii, p. 372 (p. 281 of this volume).

may be performed with just the same simplicity as in former years. What we have to attend to is to prevent the entrance into our wounds during operations of the grosser forms of septic mischief, such, for instance, as exist in impure sponges, on dirty instruments, or in any unclean material upon our hands or on the skin of the patient. Then, again, the *entourage* of the seat of the operation must be considered. To speak first of this last point, we cover the region round about the field of operation with towels soaked in a trustworthy antiseptic solution, and then we are quite sure that if we touch any neighbouring object there can be no chance of our contaminating the wound as the result of this contact.

As to the best means of purifying the sponges, &c., it appears that there is, after all, nothing better than the agent which I happened to employ first—carbolic acid. There was a time when, in consequence of Koch's publications on the subject of corrosive sublimate, it to a large extent displaced carbolic acid in the practice of surgeons. It turns out, however, that Koch, able as he is, was misled on a certain point which led him greatly to exaggerate the germicidal power of corrosive sublimate, and that in truth it is for surgical purposes very inferior to a solution of carbolic acid in water. It is a happy thing for us as surgeons that those organisms which have the most resisting spores do not trouble us in surgical work. For instance, the hay bacillus, which is sure to grow in an infusion of hay left exposed for a while, has spores of an exceedingly resisting kind; but supposing the hay bacillus to get into a wound it would do no harm whatever. Again, the anthrax bacillus has very resisting spores, but if we take good care that the catgut which we use for tying bleeding vessels has been treated with an antiseptic that will certainly kill any spores of anthrax with which the sheep might have been affected that furnished the intestines for the catgut, we shall never have any chance of anthrax getting into our wounds. What we have to deal with as our surgical enemies in the shape of microbes are almost exclusively sporeless micrococci. Some of these, however, are much more resisting than others. The *Staphylococcus pyogenes aureus*—a very common cause of suppuration—is very resisting. Now it has been shown that in such solutions as would be used in surgery carbolic acid destroys this organism much more rapidly than bichloride of mercury does.[1]

There is, however, one spore-bearing bacillus with which we have to deal but too often as surgeons, namely the tubercle bacillus. Some experiments were made a few years ago by M. Yersin, at the Institut Pasteur, on the germicidal action of various agents upon tubercle bacilli grown in pure culture on glycerine jelly. I will not enter into the details of his experiments, but if you

[1] See Behring, 'Ueber Desinfection,' &c., *Zeitschrift für Hygiene*, Neunter Band, 1892, p. 417.

refer to his paper [1] you will see that they are very beautiful and very trustworthy. He found that a watery solution of carbolic acid (1 in 20) killed the bacilli in thirty seconds ; carbolic acid (1 in 100) killed them in a minute ; while corrosive sublimate (1 in 1,000), which we had been led to regard as a most potent germicide, required ten minutes for their complete destruction.

But though the bacilli of tubercle, as grown on glycerine jelly by Yersin, seem really to have had spores, yet those spores were in a less resisting form than they assume in the living body. In sputum, for example, they are much more resisting. Accordingly, I lately asked my colleague, Professor Crookshank, to make some experiments for me with reference to the tubercle bacilli as they exist in phthisical sputum, and he has been good enough to do so. I may refer in detail to the method of procedure. On the 13th of December, 1892, three guinea-pigs were inoculated under the skin of the thigh with a little of the sputum which had been subjected for different periods to the action of a solution of carbolic acid in 20 parts of water. Some of the liquid sputum was introduced into a test-tube; to this was added the carbolic solution, in volume about five times that of the sputum. This was shaken up freely and then allowed to stand at rest, and after a certain time the supernatant liquid was poured off from the precipitate. Sterilized water was then poured in in abundance, and shaken up with the precipitate to wash out the carbolic acid ; and of the precipitate which again formed a little was introduced by means of a sterilized pipette under the skin of the animal's thigh. If the bacilli were destroyed, no harm would result to the animal ; if, on the other hand, they remained alive, the fact would declare itself in due time by enlargement of the inguinal glands affected by the tubercle. One portion of the sputum was subjected to the action of the carbolic lotion for one minute ; another portion for an hour ; and a third portion for four hours. Three control experiments were performed ; that is to say, three guinea-pigs were inoculated with sputum which had not been acted on by carbolic acid at all, but treated in a similar manner with sterilized water. I saw those guinea-pigs yesterday. The three which were inoculated with the sputum on which carbolic acid had not acted all had enlargement of the inguinal glands of that side, showing that tubercle had developed there. The one that had received sputum acted upon by the 1 in 20 carbolic solution for one minute had indeed enlargement, but exceedingly trifling compared with that in the other three. The two inoculated with sputum on which the carbolic acid had acted longer, in one case for one hour and in the other for four hours, appeared to have absolutely sound groins ; showing that the tubercle bacilli, in this most resisting form in which we can find them, had been perfectly destroyed by the carbolic-

[1] See *Annales de l'Institut Pasteur*, tome deuxième, 1888, p. 60.

acid solution, 1 in 20, acting upon them for those periods of time; while even one minute had been sufficient very materially to affect them.

Now this is to me a very satisfactory matter, because it gives experimental demonstration of the truth, of which I have long been convinced by experience, that we need not fear tubercle bacilli in our sponges if we keep them for a considerable time in 1 in 20 carbolic lotion. The way in which our sponges are treated is this: they are washed well with soap and water, and afterwards with soda; then thoroughly washed again with water, and finally, after drying, put to steep in 1 in 20 carbolic solution till they are again required for use. For my own part, I purify my sponges for private operations in a somewhat rough and ready way. I put the sponges after an operation into a tank of water, and let them putrefy there. The fibrine, which clings among the pores of the sponges, becomes liquefied by putrefaction. They can then be washed thoroughly clean of their fibrine, and the washing is continued until they no longer give a red colour to water. They are then put into 1 in 20 carbolic solution and kept there. In my Edinburgh practice I used to proceed in a bolder way. Taking the sponges out of the putrid tank, I washed them in water, and sometimes, if I was in a hurry, even before the water which came from them was completely freed from red colour, I dipped them into the 1 in 20 carbolic solution, and took them at once to my operations. I have before now applied a sponge so treated immediately to a wound for the purpose of exercising elastic pressure and absorbing blood and serum from it, and then put on my external antiseptic dressing over it without any bad result. These facts taken together will, I think, be enough to convince you that it is not necessary, as is sometimes done, to discard these most valuable articles and substitute for them sterilized cotton-wool or tissue of one kind or another, incomparably inferior to sponges for the purpose of absorbing blood.

This same 1 in 20 carbolic solution is what we use for purifying our instruments, our hands, and the skin of the patient. For the instruments, it is very much more convenient to be able to purify them by a solution like this than to boil them, as is sometimes the fashion at present. For private practice it would be a most troublesome thing to have to boil your instruments; and even when you had boiled them and brought them sterilized to your operation, it might often happen that an instrument might fall upon the floor or otherwise come in contact with some source of contamination. You could not boil it again before going on with the operation; but the bath of carbolic lotion at once puts it right.

As to the length of time for which the instruments should be kept in the solution, a good deal depends upon the care with which you wash your instruments before putting them away. Any which have teeth, such as forceps,

require special attention. They should always be brushed with a nail-brush before they are dried, so that there may be no crusts of dried blood upon them which the carbolic lotion might require a considerable time to penetrate. If this has been done, a very short period is sufficient for sterilizing. In private practice I put the instruments into 1 in 20 carbolic lotion just before the patient is brought into the room. They continue to be kept in it during the administration of the anaesthetic and during our other preparations, and this is quite adequate for the purpose. It is of great importance that we should not make things unnecessarily complicated.

So also with the purifying of the skin of the patient. It is not needful to apply an antiseptic lotion for hours together, as is sometimes done; a few minutes' action of the 1 in 20 carbolic solution is really sufficient; while its long-continued operation sometimes produces troublesome irritation. For purifying the eyelids before ophthalmic operations the carbolic lotion would excite conjunctivitis. In this special case a weak solution of corrosive sublimate, applied in compresses, is probably the best. It must, however, be continued for a lengthened period.

While carbolic acid is more trustworthy as a germicide for surgical purposes than corrosive sublimate, it is in other respects also greatly to be preferred. Carbolic acid has a powerful affinity for the epidermis, penetrating deeply into its substance; and it mingles with fatty materials in any proportion. Corrosive sublimate solution, on the other hand, cannot be expected to penetrate in the slightest degree into anything greasy; and therefore, as the skin is greasy, those who use corrosive sublimate require elaborate precautions in the way of cleansing the skin—treating it with oil of turpentine or ether, not to mention soap and water, to remove the grease which they feel it essential to get rid of for the efficient action of the corrosive sublimate. Now all this is unnecessary care if you use carbolic lotion. I can testify to this from very ample experience. For my part, I do not even use soap and water. I trust to the carbolic acid, which, by its penetrating power and great affinity for organic substances, purifies the integument as corrosive sublimate cannot.

Our sponges during the operation are washed with 1 in 40 carbolic lotion. You will see how important it must be to have your nurses and assistants careful. In truth, it needs no small pains to teach them to take the care, simple as it is, yet all-important, that is requisite for avoiding the contamination of a wound with gross septic material. Finally, because we cannot be always quite certain of our assistants being as careful as we wish, before we close the wound we wash it with 1 in 40 carbolic lotion. This irritates very much less than the spray, which applied a stronger solution during the whole operation; and in proportion

to the diminished irritation there is less serous effusion, and therefore less necessity for drainage.[1]

Before proceeding to consider the second division of our subject, the best form of external dressing for the wound, I have a few words to say regarding the course you might adopt in case you were called upon to operate under circumstances where you had no chemical antiseptic at your disposal. First, you should have your sponges well boiled, and also the fine silk threads which you will use for securing bleeding-points (the ends being cut short). Such instruments as will not be injured by the process may also be purified in the same way; and for washing the sponges during the operation it will be well to use boiled water, although, from the facts before brought under your notice, you may infer that unboiled water, if free from visible floating particles, would not be likely to cause mischief. Towels dipped in the boiled water and spread about the seat of operation will diminish the chance of contamination of the wound from surrounding objects. Then thorough cleanliness in the ordinary sense, by the free use of soap and water, must be practised for the hands of the surgeon and his assistants and for the skin of the part operated on. For sutures under these imperfect antiseptic arrangements, materials incapable of absorbing putrescible liquids, silver wire, silkworm gut, or horsehair, should be used rather than sterilized silk, in order to avoid suppuration in the stitch tracks.

For dressing the wound in the absence of chemical antiseptics, dry substances such as absorbent cotton-wool or old linen (preferably boiled before use) are far better than anything kept permanently moist, like water dressing. It was shown several years ago by Naegeli of Munich that the more concentrated an organic solution is, the less easily do bacteria develop in it, much in the same sort of way as a cook who makes her jam has to boil it down until the syrup has a sufficient proportion of sugar in it, or else fungi will develop in the preserve. And so the blood and serum oozing into a dry dressing, becoming more or less inspissated by evaporation, are in proportion a less favourable soil for microbic development. If we look back to our old experiences with water dressing, we can only wonder that wounds ever united by first intention at all under such treatment. The water dressing, clean at the moment of application, was invariably stinking when it was taken off in the course of twenty-four hours, and it seems astonishing that septic mischief ever failed to develop in a wound with this putrid mass lying over its outlet. It only serves to illustrate how powerful are the means by which Nature defends herself against the microbes.

But with dry dressing, in conjunction with the care in other respects which

[1] Note by Lord Lister, 1907: In my later practice, when I could feel secure against contamination of the wound by assistants, I omitted the final washing.

I have referred to, you would find that complete primary union, instead of being a rarity as formerly, would be a matter of very frequent occurrence; although you would not be at all able to reckon upon the constancy of aseptic results which may be obtained by the right use of chemical antiseptics.

Iodoform is an agent very much trusted by some surgeons. It is a very peculiar antiseptic, having extremely little influence over the growth of bacteria outside the body. That was illustrated by a very simple experiment I performed a good many years ago. I took two purified stoppered bottles, and put into one of them cotton-wool strongly impregnated with iodoform—10 per cent. iodoform wool; and into the other ordinary absorbent wool. I poured milk from a dairy into each, just sufficient to soak the mass of cotton, and left them at the temperature of the air. In one of these bottles the milk was thus most intimately associated with iodoform, yet it soured like that in the other bottle, though somewhat later, and when I examined a little of the iodoform wool under the microscope, I found the milk which it contained teeming with bacteria of different species. That simple experiment was enough to show how little power iodoform exerts over the growth of microbes outside the body. This conclusion has since been amply confirmed by the observations of others. It has been even ascertained, as a matter of experiment, that if iodoform is dusted over sterilized cultivating jelly in a test-tube, growth will take place from organisms that were contained in the iodoform itself.

But though such is the case, it is nevertheless unquestionably true that iodoform exercises a powerful antiseptic influence upon wounds. The most probable explanation of this apparent anomaly is that suggested by Behring, namely, that iodoform produces its beneficial effects, not by acting directly upon the bacteria, but by inducing chemical changes in their toxic products. Behring has ascertained as a matter of fact that some of these toxines are altered chemically by iodoform and at the same time rendered harmless. Two of his experiments, performed in conjunction with De Ruyter, may be quoted in illustration. A ptomaine obtained from a culture of pyogenic micrococci killed a mouse in twelve hours when injected pure into the peritoneal cavity, but proved entirely harmless under similar circumstances when mixed with a little iodoform. Again, a sample of decomposing pus, which had fatal effects when introduced unmixed into the peritoneum of the mouse, had no influence whatever upon the health of the animal if treated with iodoform, which meanwhile left intact the pyogenic microbes.[1] In the absence of their toxic products, the bacteria could do little harm, and would probably soon be disposed of by phagocytosis.

[1] See De Ruyter, 'Zur Iodoformfrage,' *Langenbeck's Archiv*, 1887, p. 984. Some bacteria are more affected than others by the direct action of iodoform. In the special case of the cholera microbe it seems to act as a poison. See Neisser, *Centralblatt für Bacteriologie*, 1888, p. 387.

We seem thus able to understand how iodoform dusted over the cut surfaces of a wound may have great antiseptic efficacy, more especially as it remains for a long time unconsumed among the tissues, and is remarkably free from irritating properties. In circumstances where it is impossible to exclude septic agencies, as in operations upon the mouth or the rectum, or when putrid sinuses are present, iodoform is of very high value. Before applying the iodoform in such cases we mop the cut surface with a solution of chloride of zinc, 40 grs. to the ounce of water, which has a remarkable power of retarding septic changes in wounds in the presence of contaminating materials. On the field of battle iodoform is probably the best means at present at our disposal. Again, in compound fractures, while we endeavour to purify the wound with strong carbolic lotion, we cannot be certain of entire success in this respect, and I should be sorry to dispense with iodoform.

But if you operate when the integument is unbroken, with a sufficient space around you for the application of a dressing, I would not recommend you to use it. To apply it to the interior of the wound would be then entirely superfluous, provided that you have taken care to avoid its contamination while operating, and have at your disposal some trustworthy material for preventing the subsequent access of septic mischief. This, as we have seen, iodoform cannot be expected to do. A porous material impregnated with it, when soaked through and through with blood or serum, will allow the microbes of external defilement to propagate in its substance, though doubtless more slowly than if the iodoform were absent. It is essentially in the interior of the wound that the virtues of iodoform are displayed; and the original Vienna practice of dusting the cut surface with the powder, and applying simple absorbent cotton externally, gave results which were much extolled at the time, and were probably not far inferior to those obtained by the use of iodoform wool or iodoform gauze. An iodoform dressing affords no security against the penetration of septic microbes to the outlet of the wound. At the same time, it is easy to see that circumstances may often arise in which iodoform dusted over the cut surfaces may fail to act effectually; as, for example, when those surfaces are separated by extravasated blood.

Any material that is merely aseptic, such as cotton-wool or gauze sterilized by heat, having nothing in its substance to check in any degree the development of microbes, will allow the septic evil to spread freely to the wound from the external world, if blood or serum happens to penetrate at any point to the exterior. In addition to this fatal objection such a dressing has other disadvantages. The necessary sterilizing apparatus, though it may be provided at a public institution, cannot well be at the disposal of the private practitioner. And, further, the merely aseptic material, having no power to correct any accidental

defilement, must require an almost impossible degree of care in its manipulation. I have seen this system in operation in very able hands with results by no means satisfactory.

An external antiseptic dressing, to be ideally perfect, should have four essential qualities. It should contain some thoroughly trustworthy antiseptic ingredient ; it should have that substance so stored up that it cannot be dissipated to a dangerous degree before the dressing is changed ; it should be entirely unirritating ; and it should be capable of freely absorbing any blood and serum that may ooze from the wound.

The carbolic gauze which we formerly used did, indeed, contain a very efficient antiseptic ; but this, being volatile, was perpetually flying off in spite of our endeavours to fix it, and it was a matter of uncertainty in how many days it might have so far disappeared from the dressing as to leave it untrustworthy. Carbolic acid had also this disadvantage as an element of an external dressing that, acting, as we have seen, with peculiar energy on the epidermis, it interfered seriously with cicatrization, and we were obliged to interpose what we termed a ' protective ' to shield the healing wound from its action. And this gauze, containing resin for the purpose of fixing the carbolic acid, was not a very good absorber of blood and serum. Carbolic gauze, then, was not an ideally perfect dressing.

Corrosive sublimate had the advantage over carbolic acid of not being volatile. But it was readily washed out of gauze or wool charged with it, and under some circumstances it proved very irritating. The discharge, passing from one part of the dressing to another, took up more and more of the bichloride in its passage, and sometimes became so strong a solution of the salt as to cause vesication. I endeavoured to remedy these defects by combining the bichloride with the albumen of the serum of horse's blood.[1] But though the sero-sublimate gauze answered its purpose, in so far that it contained the bichloride better stored up and in a less irritating form, it had inconveniences, especially as regards its preparation, which induced me to abandon it.

The agent which we have found the most satisfactory as the antiseptic ingredient of the dressing is the double cyanide of mercury and zinc.[2] Cyanide of mercury, while it has powerful antiseptic properties, is very soluble and highly irritating ; but the combination of cyanide of zinc with it has the same sort of effect, but in a much higher degree, as the albumen of the sero-sublimate

[1] *British Medical Journal*, October 25, 1884 (p. 301 of this volume).

[2] This is a double salt of a very peculiar constitution. It has been specially investigated by Professor Dunstan, who concludes that it has the following formula : $4ZnCy_2$, $HgCy_2$. See *Trans. Chem. Soc.*, 1892, p. 666. The best way of preparing it was described by Professor Dunstan in the *Pharmaceutical Journal*, third series, vol. xx, No. 653.

gauze had upon the bichloride. The combination with zinc keeps the cyanide of mercury from being dissolved away, and also prevents it from irritating. It is, so to speak, chained down by the cyanide of zinc with which it is combined. The double salt is very little soluble in blood-serum, requiring between two and three thousand parts to dissolve it; and thus a small quantity of it will last a long time in spite of a free flow of discharge through it. It thus fulfils the condition of persistent storage. It is at the same time practically unirritating; wounds heal under its immediate contact without the necessity for a protective layer interposed. Then, as to the essential question of its antiseptic virtues. Small as is the quantity which serum dissolves, it proves amply sufficient to prevent bacteric development. Thus in one experiment some serum of horse's blood containing 1–5,000th part of the salt remained clear and odourless for more than a fortnight at the temperature of the body in spite of inoculation with putrid material, and even 1–10,000th part prevented all growth for ten days. When mixed with serum and corpuscles, it prevents putrefaction in smaller quantity than any other antiseptic with which I am acquainted. The greater the amount of albuminoid substances in any solution, the more severely is the antiseptic tested; and when the red corpuscles are mingled with the serum, as is the case in the first twenty-four hours after the infliction of a wound, a much larger amount of the antiseptic is needed than with serum only. Thus four times as much corrosive sublimate is required to prevent putrefaction in serum and corpuscles as in serum. Now, the double cyanide answers the purpose in half the quantity that is necessary with corrosive sublimate. As an illustration of the practical value of this material, I may mention a single experiment, not hitherto published. I packed a piece of glass tube with gauze charged with 3 per cent. of the double salt, and poured into it serum and corpuscles obtained by whipping pig's blood. I then inoculated one end of the saturated gauze with a drop of septic serum, and kept it at the temperature of the body, with provision for preventing evaporation. After the lapse of five days I found the entire mass of gauze pure in odour and without bacteric development, as tested by microscopic examination of stained cover-glass preparations of the contained blood. Meanwhile a piece of unprepared gauze similarly treated showed bacteric development within twenty-four hours.

But here I must remind you of the essential difference, which must always be kept in view in considering antiseptic agents, between germicidal and inhibitory power; that is to say, between the capability of destroying the life of microbes and that of preventing their growth while the agent remains in contact with them. These two properties are by no means similarly proportioned to each other in all antiseptics. Thus, cyanide of mercury is far superior to the

bichloride in inhibitory power, but very inferior to it as a germicide. And the double cyanide of mercury and zinc, while admirable as an inhibitor, is very feeble as a germicide; so that we can have no security that materials charged with it may not contain living organisms. Hence if gauze charged with the double cyanide were applied dry to a wound, the time might come when, if the discharge were free, the salt, in spite of its slight solubility, might be all washed out of the deepest parts of the dressing; and as soon as this should be the case, living microbes contained in it would be free to develop towards the wound. In order to guard against this risk, we treat the gauze before using it with a reliable germicide. That which we now use for the purpose is the 1 to 20 solution of carbolic acid, which, besides being thoroughly effective, has the further advantage that it soon flies off from the dressing and leaves nothing in contact with the wound but the unirritating double cyanide and cotton fabric.

And now I wish to correct a mistake I made in a former publication.[1] For the purpose of destroying any microbes that there might be in the gauze, I recommended a solution of corrosive sublimate, 1 to 4,000. Now we have seen that the 1 to 4,000 sublimate lotion is not nearly so powerful as a germicide as we then supposed. But it further appears that such power as it possesses is almost entirely lost as soon as the bichloride comes in contact with the cyanide of mercury and zinc, when a curious soluble triple compound[2] is formed which has extremely slight germicidal action.[3] The triple salt seems also to be highly irritating; and thus, when we used the bichloride of mercury, we failed almost entirely to obtain the object for which we employed it, and at the same time lost some of the goodness of the double cyanide, part of which was washed out in the process, while the resulting solution might cause troublesome irritation. Soon after I first described this dressing, a surgeon at one of our hospitals came to me and said he had been using it, and found great inconvenience from it. He had applied it to a scalp wound, and the whole of the skin covered by the dressing was excoriated. I found he had applied it soaking wet with bichloride lotion, and we are now able to understand the irritation that resulted.

It is quite unnecessary to have the gauze wet with the 1 to 20 carbolic lotion; mere dampness is sufficient. It may be conveniently moistened as follows: The gauze is commonly sold in pieces of three or six yards, folded lengthwise in eight layers. These are unrolled, and half the number to be moistened are sprinkled roughly with the lotion. The wet and dry pieces are then superposed alternately, and the whole rolled firmly together; and in a few minutes the

[1] See p. 319 of this volume.
[2] See Varet, *Comptes Rendus*, 1888, vol. cvi, p. 1080.
[3] For the determination of this fact I am indebted to my colleague, Professor Crookshank.

entire mass will be uniformly damp. This may be done by a nurse, who then folds the gauze up in a piece of macintosh cloth in which it is kept till it is required for use, the precaution being taken of turning over the edge of the jaconet so as to prevent the cotton from coming in contact with the gauze, and abstracting the carbolic lotion by capillary attraction. Used in this way the double-cyanide gauze may be absolutely trusted for excluding mischievous microbes; and we have seen that it contains the antiseptic element excellently stored up, and that it does not irritate; and when I add that it is all that can be desired in absorbing power, you will see that it approaches very closely to our ideal. And having now employed it constantly for over four years, both in hospital and in private practice, with thoroughly satisfactory results, I feel entire confidence in recommending it to you.

Here is a sample of the gauze ready for use. It is, you observe, of mauve colour, whereas the pure cyanide of mercury and zinc is a white impalpable powder. I have fully explained elsewhere the reasons for using a dye,[1] but I may here shortly recapitulate them. When the pure salt is diffused in water, and a piece of gauze is charged by drawing it through the liquid and dried, it is found that the powder dusts out of the gauze on the slightest touch, and irritates the nostrils extremely. I first remedied this defect by means of starch; and having observed that starch in solution in water becomes attached to the particles of the double salt and completely precipitated with it, it occurred to me that perhaps some colouring matter might behave in the same manner as the starch, and that thus it might be possible to dye the colourless salt, and so have the means of judging, by the tint of the gauze charged with it, whether or not it was uniformly distributed in the fabric. I found on trial that various dyes did indeed behave as I hoped, including colouring matters so different as Prussian blue, logwood, and various aniline dyes. But, what I had not at all anticipated, it turned out that in the case of some of these dyes, when the coloured precipitate was diffused in water and the gauze was drawn through the mixture and dried, without the use of any starch, the objectionable dusting was avoided. The particles of dye, though in extremely small proportion to those of the salt, attached them, as it would appear, to the fabric.

When I last published on the subject,[2] I recommended haematoxylin for this purpose. But I have since ascertained that the effect is produced still more satisfactorily by an aniline dye, the hydrochlorate of mauveine, known in commerce by the name of purified rosalane.[3] I have here a sample of the

[1] See p. 325 of this volume.

[2] Vide loc. cit.

[3] This dye may be obtained from Messrs. Meister, Lucius, and Brüning, of Hoechst-on-Main. I may here publicly express my thanks to Dr. Perkin, to whom the world is indebted for the aniline dyes, for

mauve-coloured powder, the dyed cyanide, as supplied by Messrs. Morson, of Southampton Row. For charging gauze it is diffused with pestle and mortar in 1 to 20 solution of carbolic acid in the proportion of about 30 grs. to a pint; and the gauze, which must be of thoroughly absorbent quality, is drawn, in a thickness of about eight layers, through the liquid, which is conveniently placed in a trough having a bar near its lower part, beneath which the gauze is made to pass, care being taken that the liquid is kept perpetually stirred to prevent precipitation of the salt. The gauze is then hung up to dry at the temperature of the air. The carbolic lotion is used in preference to water, both because the powder is very much more easily diffused in it and because it is desirable that any dirty material which the gauze may happen to contain may be sterilized. A very cheap kind of carbolic acid will answer, and the solution that drains from the gauze when it is hung up may be used again for the same purpose. It thus scarcely adds to the expense of the preparation.

This is a very simple process. For a whole year I prepared my own gauze, for use in hospital as well as in private practice, before I had satisfied myself completely as to its value. For hospital use I would advise that the gauze should be prepared in the institution, so as to save the manufacturer's charges. In that case it may be taken down and wrapped in macintosh when only partly dry, avoiding the trouble to the nurses in moistening it.

Gauze may also be easily charged at a few minutes' notice for emergency in private practice. I have here a 6-yd. piece of unprepared absorbent gauze folded lengthwise in eight layers. I soak this thoroughly with 1 to 20 carbolic lotion, and dust some of the powder roughly over one surface with a pepper-box. I then roll it together, and kneading it for a minute or two with the fingers, produce, as you see, a sufficiently uniform diffusion of the salt throughout the mass, as indicated by the colour.[1] If this were done by a nurse before the commencement of an operation, and the wet gauze were wrapped in a folded sheet to absorb redundant moisture, it would be ready for use when required. A 6-yd. piece would be an ample dressing for many cases. Now I see by the amount that has gone from the pepper-box that not more than one-fifth of an ounce has been used, and as Messrs. Morson supply the dyed cyanide at 20*s.* per lb., this implies a cost of only 3*d.*, so that it cannot be regarded as expensive.[2] If you have no

his kindness in ascertaining for me the chemical composition of rosalane. It is used in quantity equal to ½ per cent. of the weight of the double cyanide, and is applied in watery solution, in which the salt, after being freed from excess of cyanide of mercury by repeated washing, but before it has been dried, is thoroughly diffused by stirring. The salt as it precipitates carries the dye down with it, and is afterwards dried at a moderate temperature.

[1] A pair of leather gloves may be worn to avoid staining of the hands, or the dye may be washed from the fingers with spirit of wine.

[2] I found on weighing this piece of gauze when it was dry that it was needlessly heavily charged,

absorbent gauze at your disposal, linen rags, which are excellent in absorbing quality, may be quite well charged in a similar manner. This old towel which has been so prepared, if folded a few times, would make a perfectly satisfactory dressing. Bandages which it is desirable to render efficiently antiseptic, such as one that is to be applied next the skin for keeping down the soft parts in a stump after amputation of the thigh, may be charged on the same principle.

When a free discharge is anticipated we apply a piece of thin macintosh, sponged with carbolic lotion, over the exterior of the dressing, to prevent the blood and serum from passing directly through it. This arrangement no doubt interferes somewhat with the inspissation of the discharges by evaporation, but this is a matter of indifference when the dressings are efficiently antiseptic.

There is another use to which the dyed cyanide powder may be often advantageously put, namely, treating it with enough of the 1 to 20 carbolic lotion to make a sort of soft mud or cream which may be applied with a camel's-hair brush to parts where there is very little space between the wound and some source of septic contamination. I have by this means been repeatedly able to avoid suppuration in the vicinity of the anus, as I otherwise might have failed to do. The store of the antiseptic salt upon the skin prevents the microbes from working their way into the wound under the narrow strip of dressing alone available. There are also situations, such as the pubes, where the cyanide cream applied to the hairs converts them with great advantage into a part of the antiseptic dressing.

I may be asked how it was that I obtained uniformly good results when I used corrosive sublimate solution for the purpose of producing a germicidal effect upon the gauze ; for I do not exaggerate when I say that during nearly two years in which I followed this practice I did not meet with a septic failure when I had an unbroken skin to deal with and a fair field around for the dressing. This success was no doubt partly due to the slight solubility of the double salt preventing it from being washed out of the deeper parts of the gauze. But I attribute it also to another circumstance. I invariably washed a substantial mass of the gauze which was to be applied next the wound in 1 to 20 carbolic lotion, in order to get rid of the irritating bichloride which it contained. I thus—though unintentionally—effectually sterilized, not only this portion of the gauze, but also neighbouring parts into which the redundant carbolic liquid soaked. And this mode of procedure, though not so perfect as the systematic moistening of the entire mass, is a rough-and-ready way of attaining much the same result.

In changing the dressings we make it an invariable rule to cover the wound

containing 7 per cent. of the salt instead of 3 per cent., which is that ordinarily used. Thus the salt required for such a dressing does not really cost 1½*d*.

with something reliably antiseptic before we wash surrounding impure parts, so as to avoid the chance of defiling the wound with them. For these washings we use the 1 to 40 carbolic lotion. As to the times for changing the dressings, it is no doubt true that that which is applied immediately after the operation might in most cases be left untouched for several days. Nevertheless, when discharge is free, I prefer, as a rule, to remove the first dressing when the first twenty-four hours have passed. We thus get rid of the serum and corpuscles, which, while they constitute the largest amount of discharge which occurs in the case, test, as we have seen, our antiseptic dressings the most severely. The discharge being still moist near the wound at this period, the gauze is lifted from it without disturbing it in the slightest degree ; and I never knew a patient fail to express himself as feeling more comfortable when the first dressing had been changed. There are, however, special cases, like a stump after amputation of the thigh, where an exception may be made on account of the disturbance of the wound that the changing of the dressing would involve.

In conclusion, I may remark that it pleases me, as the years pass, to see the hope which I expressed at the International Congress in London eleven years ago in course of fulfilment, namely, that the use of the antiseptic system would gradually spread by leavening action throughout the world. At the same time, I am sorry sometimes to observe that unnecessary trouble is often taken in some directions while essential points are disregarded in others ; so that, with the best intentions, the best results are not always obtained. I venture to hope that this address may be of some use to you in directing your attention to the essential conditions of success.

ON SOME POINTS IN THE HISTORY OF ANTISEPTIC SURGERY

[*Lancet*, 1908, vol. i, p. 1815; and *British Medical Journal*, 1908, vol. i, p. 1557.]

[The following unfinished letter to Sir Hector Cameron was written early in 1906, before the delivery of his *Lectures on the Evolution of Wound Treatment*, but never sent to him. I have been assured that it would have sufficient interest for some readers to warrant its publication.]

My dear Cameron.—It seems superfluous for me to write anything to you with reference to your coming lectures.[1] But perhaps in what I shall say, there may be here and there points which may interest you.

In treating surgical cases antiseptically, I always endeavoured to avoid the direct action of the antiseptic substance upon the tissues, so far as was consistent in the existing state of knowledge with attaining the essential object of preventing the development of injurious microbes in the part concerned.

In compound fracture, to which in 1865, I first put in practice the antiseptic principle, I applied undiluted carbolic acid freely to the injured part, in order to destroy the septic microbes already present in it; regarding the caustic action which I knew must occur as a matter of small moment compared with the tremendous evil which it was sought to avoid. But when this had once been done, no further direct action of the antiseptic upon the tissues occurred. The carbolic acid formed with the blood a dense chemical compound which, together with some layers of lint steeped in the acid, produced a crust that adhered firmly to the wound and the adjacent part of the skin. This crust was left in place till all danger was over, its surface being painted from time to time with the acid, to guard against the penetration of septic change into its substance. Meanwhile, in the undisturbed wound the beautiful result occurred that the material of the crust within it, and the portions of tissue which had been destroyed by the caustic, were replaced by living tissue formed at their expense.

That dead tissue, when protected from external influences, was so disposed of, was a most important truth new to pathology; and it afterwards suggested the idea of the catgut ligature.

[1] 'The Dr. James Watson Lectures delivered at the Faculty of Physicians and Surgeons of Glasgow in February, 1906.' Glasgow 1907.

I do not remember whether you saw the case that led me to apply the antiseptic principle to abscess. The patient was a woman above the middle period of life, with lumbar abscess. Taught by the disastrous results that sooner or later followed the evacuation of such abscesses, whether by valvular opening or by cannula and trocar, I left the case undisturbed; till one day, on looking at it, I found that nothing but epidermis seemed to intervene between the pus and the external world, so that if left for another day it would in all probability burst.

I therefore resolved to open it and apply a dressing which should imitate, as much as circumstances permitted, that which we used in compound fractures. The pus which escaped on incision was as thick as any I ever saw. Mixing some of it with undiluted carbolic acid, I applied some layers of lint soaked with the mixture to the wound and surrounding skin, and covered them with a piece of thin block-tin moulded to proper shape, such as we used for covering the crust in compound fracture. This metal covering, which prevented loss of carbolic acid by evaporation and soaking into surrounding dressings, was fixed by strapping, and a folded towel was bandaged over it to absorb discharge.

Next day, on changing the dressing, I was greatly astonished to see nothing escape from the incision except a drop or two of clear serum. What was now to be done? I had no longer any pus to mix with the carbolic acid. But it occurred to me that I might make a satisfactory crust by mixing carbolic acid with glazier's putty. Accordingly I sent to the dispensary for some whiting and boiled linseed oil, and making a solution of one part of carbolic acid in four of the oil, rubbed it up with whiting in a mortar, thus making a carbolic putty. This I spread on a piece of block-tin and applied it as I had done the first dressing. There never was any further discharge of pus; the serous oozing diminished rapidly, and before long healing was complete.

In that case, as there was no spinal curvature, I could not be sure that the abscess was connected with the vertebrae. But similar results afterwards followed the same treatment where discharge of bone showed that such connexion existed, and also in suppuration of the hip-joint, whether attended with shortening of the limb or not, scrupulous care being taken to keep the affected part completely at rest. The time required for final closing of the sinus was, however, generally much longer than in the first case.

Precisely the same beautiful result, so entirely novel and so full of deep interest both for pathology and practice, was seen when acute abscesses were treated in the same way; the only difference being that in the acute cases the serous oozing which followed evacuation of the pus came much more rapidly to a conclusion.

In order to ensure freedom of escape for the serum, a narrow strip of lint soaked with a solution of carbolic acid in four parts of olive oil was inserted in the incision. But the antiseptic substance was never from first to last applied to the cavity of the abscess, as such treatment could only have been productive of needless irritation.

I continued to use a strip of lint as a drain for about five years with perfectly satisfactory results. But in 1871, having opened a very deeply seated acute abscess in the axilla, I found to my surprise, on changing the dressing next day, that the withdrawal of the lint was followed by escape of thick pus like the original contents.

It occurred to me that in that deep and narrow incision, the lint, instead of serving as a drain, might have acted like a plug, and so reproduced the conditions present before evacuation. Taking a piece of the india-rubber tubing of a Richardson's spray producer that I had used for local anaesthesia at the operation, I cut holes in it and attached knotted silk threads to one end, so improvising a drainage-tube. This I put to steep for the night in a strong watery solution of carbolic acid, and introduced it in place of the lint on changing the dressing next morning. The withdrawal of the lint had been followed by discharge of thick pus as before ; but next morning I was rejoiced to find nothing escape unless it were a drop or so of clear serum. This rapidly diminished, and within a week of the opening of the abscess I was able to take leave of my patient, the discharge from the abscess cavity having entirely ceased.

After that case I used drainage-tubes as a rule in the treatment of abscess. But it is well to remember that if such a tube should not be at hand, a narrow strip of lint, sterilized of course with some trustworthy antiseptic solution, will in almost every case answer the purpose equally well.

The crude carbolic acid which, under the name of German creosote, was supplied to me by my colleague, Dr. Anderson, Professor of Chemistry in the University of Glasgow, was a brown liquid which had been adulterated with water, and this lay on the top as a clear layer, destitute of any flavour of carbolic acid. This led me in my first paper on compound fracture to speak of carbolic acid as absolutely insoluble in water.[1] But when it was afterwards produced in a comparatively pure condition in colourless crystals, it proved to be capable of being taken up by water, though twenty parts were required for the purpose. The watery solution, however, though weak numerically, showed itself to be exceedingly potent as an antiseptic. Having applied it to a foul sore in the palm of the hand, I found, on changing the dressing next day, that all putrefactive odour had disappeared.

[1] See p. 4 of this volume.

This enabled me to use carbolic acid for washing wounds after operations, and so to extend the application of the antiseptic principle to surgery in general. In the state of knowledge at that early period it seemed imperative to apply a powerful germicide to the wound before closing it. To use undiluted carbolic acid for operation-wounds, as I had done in compound fracture, was out of the question ; and carbolic oil, though I did indeed try it, was ill adapted for the purpose. But the watery solution could be satisfactorily used not only for washing the wound, but also for purifying the surrounding skin, the hands of the operator, and the instruments.

The entire absence of carbolic acid in the layer of water on the ' German creosote ' with which I made my first attempts with compound fractures indicates that there were present in the crude product substances for which the acid had incomparably greater attraction than it had for water. When purified from these substances, it is indeed soluble in water, but only in small amount ; and being so feebly held by water, it is free, when in watery solution, to act upon other matters for which it has stronger attraction. Thus was explained the remarkable germicidal energy of a lotion containing only a twentieth part of carbolic acid, as illustrated by the foul sore in the hand before referred to.

With linseed oil, on the other hand, the acid could be mixed in any proportion, and being firmly held by the oil, it was mild in action, though present in the large proportion of 1 to 4, as used in the carbolic putty. The 1 to 4 carbolic oil is bland when applied to the tip of the tongue, whereas the 1 to 20 watery solution is intolerably pungent.

The acid in the watery solution, while potent in action when applied, is soon dissipated, whereas it is slow in leaving the oil. Hence the watery solution, powerful but transient in operation, was admirably adapted for application to a cut surface as a detergent, while the carbolic putty, bland in action and serving long as a store of the antiseptic, could be used with good effect not only for abscesses, but also as an external dressing for operation-wounds ; and for that purpose I long employed it. The putty was used in a layer spread on calico, freely overlapping the skin around the wound, and covered with a folded cloth to absorb the serum that flowed from beneath its edges. Although this mode of dressing gave place in time to others which were more convenient, the change effected under its use at that early period was of the most striking character : healing without suppuration, pain, or fever, instead of being the rare exception, became the rule, and operations were safely performed which had previously been utterly prohibited on account of the danger that attended them ; while pyaemia and hospital gangrene, which had before been disastrously rife, were banished from my wards.

Epidermis is a substance for which carbolic acid has special attraction ; and this, coupled with the facility with which the acid blends with oily matters, renders it peculiarly fitted for purifying the skin about the seat of operation and the surgeon's hands. Another property which aids its action as a detergent is its great penetrating power, not limited by the products of its chemical action upon organic substances.

I used the 1 to 20 watery solution for rendering the patient's skin and the hands of myself and my assistants aseptic throughout the 40 years during which I practised on the antiseptic principle, and I never had any reason to doubt its efficacy. No long time is required for its action. In my private practice the purification of the skin was, as a rule, not begun till I entered the patient's room to perform the operation. The part concerned was then thoroughly washed with the 1 to 20 carbolic solution, and was kept covered with lint soaked with the same lotion while the instruments were being attended to and the anaesthetic administered ; the whole process occupying only about a quarter of an hour. Yet experience showed that this brief period was sufficient.

It may perhaps be argued that under the carbolic putty or any other dressing containing carbolic acid, that volatile agent was perpetually acting on the skin, and may have made up for deficiences in the original purification. But during several years before I gave up practice, the dressings did not owe their virtues to any volatile antiseptic.

I may mention in illustration one of my latest operations. The patient was a lady advanced in years, with a large ventral hernia below the umbilicus. It was producing serious symptoms ; and attempts to reduce it having failed, her condition had become exceedingly grave. I only began to disinfect the skin when she was already partly under the influence of the anaesthetic. The umbilicus contained some drops of opaque liquid of a highly offensive character. I cleansed its folds carefully with the 1 to 20 carbolic solution, and washed the skin over and around the sac with the same lotion. The sac was opened by a median incision, the upper end of which extended to the umbilicus. Into further details of the operation I need not enter. On changing the dressing (of cyanide gauze) it appeared that, in her frail condition, the margins of the skin at the upper end of the incision had lost their vitality over an extent of about half an inch in length and one-tenth of an inch in breadth at each side. I afterwards left the dressing unchanged for several days, when I found that the sloughs, the upper ends of which encroached on the umbilicus, so foul before the operation, had been replaced by new living tissue, and complete cicatrization had occurred without the formation of a particle of pus.

I cannot but think it a happy circumstance that the substance which

I employed first in endeavouring to apply the antiseptic principle should have been so admirably adapted for detergent purposes. And it has grieved me to learn that many surgeons have been led to substitute needlessly protracted and complicated measures for means so simple and efficient.[1]

As an instance of trouble misapplied in this matter, may be mentioned preliminary washing with soap and water. If carbolic acid is the disinfectant used, such washing is not only wholly unnecessary, but is, I believe, positively injurious; as it must tend to check the penetration of the germicide into the substance of the epidermis, by saturating it with water for which carbolic acid has so little affinity. That this practice is superfluous is, I venture to think, proved by my experience, as I never in any case adopted it.

The incomparably greater attraction of carbolic acid for epidermis than for water was strikingly illustrated by an experiment not hitherto published.

[Here my letter was broken off, in consequence of other engagements. But I afterwards wrote to Sir Hector Cameron what I had intended to say on this subject and he was good enough to incorporate my remarks in his second lecture (see *British Medical Journal*, April 6, 1907, p. 799).]

'The avidity with which carbolic acid seizes upon epidermic tissues was strikingly illustrated by an experiment which he related in an unpublished address to the medical students of Glasgow, delivered in 1894.

'Having discovered a method by which the amount of carbolic acid present in a watery solution could be determined,[2] he packed a test-tube closely with hair of the human head, and added just enough five per cent. solution of carbolic acid to cover it, eight times the weight of the hair being required for the purpose. Half an hour later he poured out some of the liquid, and applied the test; when it was found that already nearly half the carbolic acid had been withdrawn by the hair from the watery solution.

'Considering that the hair was only an eighth part of the weight of the solu-

[1] The fear sometimes expressed of poisonous effects from carbolic acid, as used in antiseptic surgery, is, so far as my experience goes, entirely groundless.

[2] 'In the course of some work on the preparation of catgut for surgical purposes, he observed that if a weak solution of chromic acid in water is mixed with carbolic lotion, the mixture, which is at first a pale straw colour, gradually grows very much darker during the next few hours. This fact afforded the means of estimating the quantity of carbolic acid in a watery solution. Making a mixture of equal parts of the weak chromic liquid and a five per cent. watery solution of carbolic acid to serve as a standard of comparison, and at the same time making a corresponding mixture of the chromic liquid with the carbolic solution to be tested, and ascertaining how much the standard had to be diluted in order to bring its tint down to equality with that of the mixture containing the liquid to be tested, an estimate could be formed of the amount of carbolic acid present in the latter. Lord Lister informs me that, on going over the subject again recently, he ascertained that hair retains this remarkable power of withdrawing carbolic acid from a watery solution after all fatty matter has been removed from it by prolonged steeping in sulphuric ether.'

tion, this was certainly very remarkable. The hair must thus have become charged with about a sixth of its weight of the antiseptic[1]; and if a larger quantity of the lotion had been used, the proportion would have been still greater.'[2]

[1] 'Hair thus highly charged with carbolic acid by washing with five per cent. solution, may sometimes be turned to account in surgery of the scalp as an effective and unirritating antiseptic dressing. This may be illustrated by one of Lord Lister's latest cases. The patient was a lady with numerous atheromatous tumours scattered over the scalp. To have shaved around each of these would have caused a very inconvenient loss of hair; but this was avoided by washing freely with the lotion about each tumour, and simply passing a comb along the line where the incision was to run, the hair being replaced in position after the removal of the cyst. The several tumours having been so dealt with, a cap of folded cyanide gauze was bandaged over the head, and when this was removed some days later, all the wounds were found to be healed.'

[2] *On the Evolution of Wound-treatment during the last Forty Years*, p. 71, and *British Medical Journal*, April 6, 1907, p. 799.

PART IV

SURGERY

REPORT OF SOME CASES OF ARTICULAR DISEASE OCCURRING IN MR. SYME'S PRACTICE, EXEMPLIFYING THE ADVANTAGES OF THE ACTUAL CAUTERY

[*Monthly Journal of Medical Science*, August 1854.]

CASE I.—*Omalgia ; Application of the Actual Cautery ; Cure.*

MARGARET ASHTON, aet. 25, admitted the 25th of October, 1853 ; a servant ; has generally enjoyed good health, and has a very robust appearance. Four months ago, after exposure to wet and cold in washing, she had a severe fit of shivering, and was seized a few days after with pain in the right shoulder, just below the acromion, so severe that she could scarcely lift the arm ; this lasted about twelve hours, and was followed in the course of the next day by intense pain in the left shoulder, below the back part of the acromion. From that day till her admission, she was unable to raise the arm ; the pain was for the first two months extreme, keeping her as if 'in the fire all night', and banishing sleep almost entirely. During the last two months she has rested from work, and has suffered less. On admission she complained of constant gnawing pain in the left shoulder, and extending down the limb as far as the elbow, and sometimes to the fingers ; when in the sitting posture she held the affected limb with the other hand, to ease the pain ; the arm was also affected with a feeling of numbness and weakness ; and although the shoulder was not very tender on pressure, and very gentle passive motion of the arm could be performed, through a considerable angle, without pain, yet any attempts on her own part to move it produced great aggravation of her sufferings. As a result, no doubt, of habitual disuse, the muscles about the shoulder were much atrophied, and this caused a remarkable apparent prominence of the bony points, viz. the spine of the scapula, the acromion, the anterior border of the outer part of the clavicle, and the head of the humerus. The shoulder had an appearance that suggested at first sight the idea of dislocation.

On the 3rd of November, the patient being under the influence of chloroform,

Mr. Syme cauterized thoroughly the skin over the anterior and posterior aspects of the joint, rubbing a red-hot cautery iron freely backwards and forwards four or five times over each part. It had the effect of raising and rubbing off the cuticle, but did not char the skin. An hour afterwards the patient was suffering but little pain.

Nov. 4.—Said, with a smiling countenance, that she slept well last night, the first time for four months, and feels now no pain save that of the burns.

Nov. 5.—A poultice was applied yesterday; the pain of the burn is now gone, and she feels *no pain at all.* Says that she has not only lost all pain, but also that the feeling of numbness is gone from the limb, and that she seems to have more power in it. The burned parts present a white sloughy appearance.

The poultice was continued till the sloughs separated, when simple cerate was substituted for it, with the view of retarding, rather than promoting cicatrization.

Nov. 12.—To-day she has been trying to lift the arm, and felt none of the old pain in the attempt.

Jan. 31, 1854.—She has to-day left the infirmary. She has for some time past been gradually acquiring more and more power in the limb; she can move the arm backwards and forwards for a considerable extent, and even raise it slightly. The movements of the forearm are free; there is no tenderness whatever about the shoulder. The return of the use of the limb has been accompanied with a restoration of the fullness of the muscles, so that there is now no difference between the contour of the two shoulders. She continues quite free from spontaneous pain.

I saw her again towards the end of May. She was still quite free from pain, and there remained only some stiffness about the joint that prevented her from raising the arm to the full extent.

Case II.—*Disease of Shoulder-joint; Actual Cautery; Cure.*

Lily Kay, aet. 50, admitted the 23rd of March, 1854. Has generally enjoyed good health, except that for the last twelve years she has suffered inconvenience from what she supposed to be rheumatism in the right shoulder, characterized by shooting pain, occurring more especially when she attempted to lift anything. In January last the limb became completely disabled from increase of the pain, which now assumed a gnawing as well as a shooting character, and also began to be felt in the elbow-joint, and in the arm, forearm, and hand. At this time she first observed the existence of swelling about the shoulder-joint.

The pain continued to increase till the time of her admission into the infirmary, when it was exceedingly severe; not constant, but frequently keeping

her awake at night. She was unable to raise the arm from the side, and had a sense of weakness in the limb, and some stiffness of the hand. There was considerable swelling about the shoulder-joint, which was tender on pressure, particularly at the anterior and posterior aspects. On the day of admission Mr. Syme applied the actual cautery freely over the anterior and posterior parts of the joint, the patient being under chloroform. From this time she lost the old pain entirely, or at least was uncertain whether that which she still felt was not altogether that of the burn ; and though the pain of the burn was considerable till the sloughs separated, yet it was much less distressing than the old pain, for which it was substituted, so that she slept much better than before the application of the cautery. The sloughs came away on the 1st of April, on which day she had a slight return of the old pain near the wrist, but it has not occurred again, and she is now (the 4th of April) quite easy. The swelling about the shoulder has almost entirely disappeared, and there is little, if any, tenderness ; the sores are granulating healthily.

April 14.—Continues quite easy.

She was discharged on the 27th of April ; I saw her about a month after, and she still continued free from pain.

Case III.—*Disease of Wrist-joint ; Actual Cautery ; Cure.*

Janet Archibald, aet. 32, admitted the 2nd of November, 1853. Rather a weakly subject. In October last she 'took a shivering', without any particular exposure to cold, and a pricking pain came on in the left wrist, which increased for a time, and was accompanied with swelling. She applied poultices medicated with acetate of lead, and under their use a great improvement had taken place at the end of five weeks, when she got fresh cold in it, as she says, and it became excessively painful ; the pain continued ever after till her admission, and although its extreme severity was then somewhat mitigated, yet it kept her awake a good deal at night ; it was partly dull and heavy, and partly of a shooting character, and extended down through the hand and fingers. There was also an occasional tingling sensation in the fingers, and a sense of unnatural weight in the limb. A great degree of swelling existed about the wrist-joint, particularly on the dorsal aspect, and this part when manipulated gave a feeling very like that of fluctuation, so that her medical attendant had been desirous to open what he had supposed a collection of matter there.

Mr. Syme regarded the condition of the wrist as almost hopeless, but as he thought suppuration had not yet occurred, he determined to give the limb a chance with the actual cautery, which he accordingly applied on the dorsal aspect in two lines, crossing one another over the articulation. The pain and

swelling both diminished greatly during the first four weeks after the cauterization; some aggravation of the symptoms then occurred for a time, but as the sore was still open, Mr. Syme thought it unnecessary to interfere further, and a gradual improvement afterwards took place, till at the time of her leaving the infirmary (the 14th of February, 1854) there was scarcely any swelling and very little pain.

I saw her again on the 10th of June; there was then no swelling whatever about the wrist, and no uneasiness except a painful feeling of weakness when she exerted it much.

CASE IV.—*Disease between the Atlas and Axis; Actual Cautery applied with great benefit.*

Thomas Smith, aet. 27, admitted the 20th of June, 1854. Generally enjoyed good health till eighteen months ago, when a stiffness of the neck came on without any assignable cause, with pain when he turned round his head on the pillow; the pain increased greatly, and deprived him altogether of sleep for seven weeks, during which time he lost three stone in weight. There was severe pain in the head as well as in the neck, aggravated to an extreme degree by either nodding or turning of the head, particularly the latter, which, indeed, he at last never did without turning the rest of the body also. He applied to numerous medical men in Birmingham, where he lives; and blisters and caustic issues were repeatedly applied to the back of the neck, but never gave more than very slight and very transient relief, and he says that from the commencement of his complaint he never had one minute's freedom from pain, except during sleep, till he came here.

At this time he was, according to his own account, about as bad as he had been at all. His countenance wore a peculiar expression of mingled suffering and apprehension, as Mr. Syme expressed it. He complained of severe pain in the neck and head, aggravated by any sudden movement, so that there was a great constraint about all his actions. He always kept his head bolt upright except when in bed, and could neither lie down nor get up without supporting his head with his hands; he never turned his head without the rest of the body, but gentle nodding was not very painful. There was great swelling of the upper part of the neck, and he could only open his mouth a little way; deglutition was extremely difficult, and a remarkable prominence of the bodies of the upper cervical vertebrae was to be felt in the pharynx.

On the day after his admission, Mr. Syme applied the actual cautery over the spinous processes of the upper cervical vertebrae; the man was not under chloroform, and said he hardly knew whether the pain was greater even at the

moment than what he had experienced from caustic issues, and immediately afterwards he told us that he did not feel the pain of the burn at all. Next day he found less pain in moving the head, and in two or three days his countenance assumed a cheerful aspect. A steady daily improvement has since taken place in his symptoms, and at the present time (the 15th of July) he has no pain whatever when he sits at rest, and can also use strong and active exertion without uneasiness, and no longer requires to support his head in lying down or rising; he can turn his head round pretty freely and look up to the ceiling, and it is only in sudden movements of the neck that he feels any pain at all. The swelling of the neck has greatly subsided, and he can open his jaws wide, and swallow with comparative facility. The sore on the neck is almost healed, and he talks of leaving the hospital in a few days as cured.

Remarks.—The above cases speak for themselves; and I might add several others, that exemplify in an equally striking manner the beneficial effects of the actual cautery in certain forms of articular disease. It will be observed that it is by no means so painful a remedy as is generally supposed, and also that its good effects are more than can be attributed to the mere discharge of pus from the sore which it produces, seeing that a great improvement commonly occurs within a few hours of its application, and long before suppuration is established.

It is now many years since the use of this means of counter-irritation was introduced into Great Britain by Mr. Syme; but although a constant series of successful cases have since continued to demonstrate its value to those who have witnessed his practice, yet I am satisfied that it has not hitherto been sufficiently generally appreciated. Case IV is an example of its efficacy against a most formidable disease, where caustic issues had been long tried in vain. I believe many limbs and lives have been sacrificed that might have been saved by the actual cautery, and by it alone; and having been myself very strongly impressed with the importance of the subject, I should be truly glad if any surgeon who may have hitherto overlooked it, should be induced by the above report to inquire more closely into its merits.

ON AMPUTATION

[*Holmes's System of Surgery*, vol. iii, third edition. London, 1883.]

Part I

Amputation is often regarded as an opprobrium of the healing art. But while the human frame remains liable to derangement from accident or disease, the removal of hopelessly disordered parts, in the way most conducive to the safety and future comfort of the sufferer, must ever claim the best attention of the surgeon. Indeed, the progress of medical science, while furnishing the means of curing some affections once regarded as hopeless, and thus in one sense restricting the field for the application of amputation, has in another point of view extended that field, by improving the mode of operative procedure, and divesting it of much of its terror and danger ; so that whereas in former times the removal of a limb was only resorted to in cases of the most serious nature, it is now often practised when the unoffending member is merely a source of inconvenience.

It is instructive to trace the history of the improvement of this department of surgery.

Hippocrates (B.C. 430) recommended only a very rude kind of amputation, consisting of cutting through mortified limbs at some joint, 'care being taken not to wound any living parts.' [1]

On the other hand, Celsus, who seems to have lived at the commencement of the Christian era, advised that the removal of gangrenous limbs should be effected between the dead and living parts, and so as rather to take away some of the healthy textures than leave any that were diseased ; and as he interdicted amputating through an articulation, his operations must often have been performed entirely through sound tissues. He directed that the soft parts should be divided with a knife down to the bone, and then dissected up from it for some distance, so as to allow the saw to be applied at a higher level. The rough surface of the sawn bone was then to be smoothed off, and the soft parts, which, as he tells us, will be lax if this plan be pursued, were to be brought down so as to cover the end of the bone as much as possible. This method seems calculated to afford good results ; particularly as it appears probable from his writings that

[1] Hippocrates, *de Articulis*, p. 639 of the Sydenham Society's translation.

Celsus employed the ligature for arresting haemorrhage after amputation,[1] and dressed the stump in a manner favourable to the occurrence of primary union.

Archigenes, who practised in Rome shortly after the time of Celsus, paid special attention to the control of haemorrhage during the performance of the operation ; and appears to have been the first to employ for this purpose a tight band or fillet encircling the limb above the site of amputation. But while in this he did good service, he applied the red-hot iron to the surface of the stump and also neglected the dissection of the soft parts from the bone, advised by Celsus, though compensating to a certain extent for this omission by retracting the integuments before dividing them.[2]

Galen, who was in truth more of a physician than a surgeon, declined still more from the Celsian precepts, and reverting to the practice of Hippocrates, advised amputating through the dead tissues and applying the cautery to the

[1] On this interesting point in surgical history I am disposed to agree with the author of the article 'Amputation' in Rees's *Cyclopaedia*, in opposition to the prevalent opinion that Celsus employed the ligature only in ordinary wounds, and used the actual cautery in amputations. The directions of Celsus regarding amputation are contained in his chapter on the treatment of gangrene, in which the only mention of haemorrhage is the statement that patients often die of it during the performance of the operation (*in ipso opere*), referring doubtless to profuse bleeding resulting from ignorance of the circulation of the blood, and of any means of controlling it in the limb. Certainly this expression is no proof that the cautery was used rather than the ligature ; for the former is the more speedy method of the two. Neither is the absence of allusion to the ligature in this passage any evidence against its employment after amputation ; for the argument would apply equally to the cautery, and no one doubts that one of these two means was used. Celsus, who is remarkable for his extremely concise style, leaves us to refer to his previous chapter on wounds, in which the subject of haemorrhage is very ably discussed. In slight cases pressure with dry lint, and a sponge wrung out of cold water, is recommended, or if this does not answer, lint steeped in vinegar is to be used ; but any portion of dressing retained in the wound is said to do mischief by causing inflammation ; and on the same principle caustics and other powerful styptics, though very efficient in arresting the bleeding, are prohibited because they produce a crust, which acts like a foreign body. In more severe cases the vessels are to be tied ; and finally, 'when the circumstances do not even admit of this,' the red-hot iron may be used as a last resort.

The only thing that seems to me to give any colour for doubt upon this subject, is the manner in which the ligature is described, 'venae quae sanguinem fundunt apprehendendae, circaque id quod ictum est duobus locis deligandae intercidendaeque sunt' ; language which seems rather to apply to a partially divided blood-vessel than to one completely severed ; but as the context shows that the ligature, as used by Celsus, was applicable in the majority of cases, and to more vessels than one in the same wound, it can hardly be conceived possible that the practice was restricted to the very rare case of partial division.

Again, there can be little doubt that in drawing down the soft parts over the bone after amputation, Celsus aimed at primary union, the great advantages of which are strongly insisted on in the same admirable chapter on wounds ; but it is certain that he knew that the use of the cautery would have destroyed any chance of union by first intention.

One argument that has been urged on the other side is, that if he had employed the ligature in amputation, it would hardly have been neglected by his successors : but the slowness of the surgeons of the sixteenth and seventeenth centuries to adopt it, in spite of the strenuous advocacy of Paré, with all the advantages of a printed literature, show how little weight is to be attached to this objection. The utter neglect, during the Middle Ages, of the Celsian method of amputation, and of his simple mode of treating wounds, may also be mentioned as analogous cases.

[2] Sprengel's *History of Medicine*, French translation, vol. ii, p. 81, and vol. vii, p. 312.

residue of the mortified part ;[1] and for several centuries after his time either this method or others equally rude and often much more barbarous continued to be employed.

During the Middle Ages, the ligature, though used for ordinary wounds, was never thought of in amputation, and whatever may have been the practice of Celsus in this respect, there is no doubt that the great French surgeon Ambroise Paré, when he so applied it, in the middle of the sixteenth century, had all the merit of originality. But, though he urged its superiority over the cautery with able argument, supported by his extensive experience in both military and civil practice, yet his teaching failed for a long time to influence surgeons generally, either in his own country or in other parts of Europe.

The principal reason for this appears to have been that the fillet, which was the means still in use for controlling the bleeding during the operation, did not answer its purpose effectually even in the ablest hands : so that the dread of haemorrhage led most surgeons to prefer the cautery as a more expeditious method than the ligature. We even find Fabricius of Aquapendente repeating, in 1618, Galen's timid doctrine of the danger of amputating through living parts at all ;[2] and in 1633 the celebrated Fabricius Hildanus, though describing the ligature, states that the time which it occupies, and the consequent loss of blood, make it suitable only for the robust and plethoric, and declares that he ' cannot sufficiently extol the excellence ' of the *cauterium cultellare,* or red-hot knife, by which the orifices of the vessels were sealed while they were divided.[3]

In consequence of this same fear of bleeding, the great object at this period seems to have been to accomplish the work of severance of the limb as speedily as possible, and this was often done without any attempt whatever to provide a covering for the bone. Scultetus, in 1655, depicted the performance of amputation of the hand by chisel and mallet ; and Purmannus, in his *Chirurgia Curiosa,* written as late as 1696, mentions having seen legs removed by two different surgeons by modifications of a barbarous instrument of the Middle Ages, a sort of guillotine, ' which, by its great weight and sharpness, cuts at once the skin, flesh, and bones asunder ' ; but he states that it splintered the bone, and *therefore,* ' all things considered, the ancient way of cutting through the flesh with a knife, and through the bone with a saw, is more practicable, safe, and certain.'[4]

As an example of the ordinary practice of the seventeenth century may

[1] *Galeni ad Glauconem,* lib. ii, cap. xi.

[2] *Hieronymi Fabricii ab Aquapendente Opera Chirurgica,* pars i, cap. xcvi.

[3] ' Porro excellentiam hujus cauterii non satis extollere possum,' *Gul. Fabricii Hildani Opera omnia,* lib. de Gangraena et Sphacelo.

[4] Purmannus' *Chirurgia Curiosa,* English translation, book iii, chap. xii.

be mentioned that of Richard Wiseman, Sergeant-Surgeon to King Charles II. A fillet having been tightly applied, for the threefold purpose of checking haemorrhage, rendering the limb less sensitive by pressure on the nerves, and steadying the soft parts, which were retracted by an assistant, he carried a crooked knife by a single circular sweep down to the bone, which was divided with the saw at the same level, and the bleeding was arrested by the cautery, or some kind of styptic.[1]

Thus the mode of amputation employed by the father of British surgery only two centuries ago was precisely that used fifteen hundred years before by the Roman Archigenes. And very unsatisfactory were the results which it commonly afforded. The soft parts were insufficient, even in the first instance, to cover the end of the bone, which was accordingly cauterized, with the object of accelerating its inevitable exfoliation, and in the further progress of the case it tended to become more and more exposed by the contraction of the muscles; and even if the patient survived the protracted suppuration that ensued, he suffered more or less from the inconveniences of what has been called the sugar-loaf stump, being in the shape of a cone, the apex of which was formed by the prominent bone, covered either by a sore which refused to heal, or by a thin pellicle of cicatrix, very liable to abrasion.

A great step towards a better order of things was made in 1674 by the French surgeon Morel, in the invention of the tourniquet,[2] which though at first but a rude contrivance, being a stick passed beneath the fillet and turned round so as to twist it up to the requisite degree of tightness, furnished the basis for the greatly improved instrument devised in the early part of the following century by his distinguished countryman, J. L. Petit. This consisted essentially of two metallic plates, which could be separated from one another by means of

[1] The ligature, though known to Wiseman, seems not to have been adopted by him. After describing different modes of applying it, in a way that shows pretty clearly that he had not practised them, he writes, 'But the late discovery of the royal styptic hath rendered them of less use. But in the heat of fight it will be necessary to have your actual cautery always ready, for that will secure the bleeding arteries in a moment, and fortify the part against the future putrefaction.'—*Chirurgical Treatises*, book vi.

[2] English surgeons might dispute with the French the honour of the invention of the tourniquet. In a work written in 1678, published in 1679, entitled *Currus Triumphalis e Terebintho*, Mr. James Young, of Plymouth, gives an account of a similar contrivance, apparently produced independently by himself. He describes it as 'a wadd of hard linen cloth, or the like, inside the thigh, a little below the inguen: then, passing a towel round the member, knit the ends of it together, and with a battoon or bedstaff, or the like, twist it till it compress the wadd or boulster so very straight on the crural vessels that (the circulation being stopped in them) their bleeding, when divided by the incision, shall be scarce large enough to let him see where to apply his restrictives' (p. 30). Further on in the book he states that the same principle is applicable with advantage in amputations of the upper limb. But as he does not inform us how long he had used this expedient before he wrote the account of it, the credit of priority must of course be accorded to Morel.

a screw, so as to tighten a strap which was connected with them and also encircled the limb : and it is upon this principle that the ordinary screw tourniquet is still constructed. From this time forward, except in amputations performed near the trunk, haemorrhage during the operation ceased to be an object of dread, and surgeons were at liberty to consider other questions besides mere rapidity of execution.

The improver of the tourniquet, and our own great countryman Cheselden, seem to have conceived independently of each other the idea of performing amputation by ' double incision ' ; in which the skin and fat were first cut through by a circular sweep of the knife and retracted for about an inch, when the muscles and bone were divided as high as they were exposed.[1]

But this, though a great improvement, had only the effect of diminishing the cicatrix without covering the bone ; [2] and Louis, another eminent Parisian surgeon, believed that in the thigh the objects sought might be better attained by dividing all the soft parts at once, and sawing the bone at a higher level. In order to allow the muscles to contract freely when divided, he avoided the use of the tourniquet, and was the first to employ in its place digital compression of the femoral artery at the groin. He pointed out the important circumstance that the muscles on the posterior aspect of the thigh, being divided far from their origin at the pelvis, contract to a much greater extent than those at the anterior part of the limb, which are connected with the bone where they are cut ; and he showed that, the soft parts having been severed to the bone by a circular incision and drawn up with a linen retractor, the saw might be readily applied two and a half inches higher up, after the knife had been carried through the attachments of the anterior muscles.[3] This method was amputation by double incision on a different principle ; and though, in truth, a revival of the practice of Celsus, was not less valuable than the plan of Cheselden and Petit, and seems to have afforded results superior to theirs.[4]

Louis, however, was content if the stump when healed was free from conical

[1] It is difficult to determine to whom the priority belongs in this matter. Petit in his posthumous work states, ' Je suis le premier qui ait coupé les chairs en deux temps ' ; and also, 'J'ai imaginé de couper les chairs en deux temps ' ; and Dieffenbach, in his *Operative Surgery*, gives 1718 as the date of the introduction of the double incision by Petit. On the other hand, Cheselden as distinctly claims the original idea in the following passage in his notes to Gataker's translation of *Le Dran's Surgery :* ' The thing that led me to do this was what has too often happened—the necessity of cutting off the end of the stump the second time. This operation I proposed to my master when I was his apprentice ; but he treated it with neglect, though he lived afterwards to practise it when he had seen me perform it in the same hospital.' This proposal must have been made before 1711, when, at the age of twenty-two, he began to lecture on anatomy.

[2] This is well illustrated by the drawing of a stump given by Cheselden in *Le Dran's Surgery*, for the purpose of showing the good effects of the double incision.

[3] *Mémoires de l'Académie de Chirurgie*, vol. ii, p. 286. [4] Ibid., vol. iv, p. 60.

projection,[1] and did not aim at forming a complete covering for the bone. This was effectually done about a quarter of a century later by Alanson of Liverpool, by dissecting up the integuments for some distance and then dividing the muscles obliquely, so that they formed a hollow cone, in the apex of which the bone was sawn 'about three or four fingers' breadth higher than was usually done'. The effect of this was to 'fully cover the whole surface of the wound with the most perfect ease';[2] but in the hands of other surgeons the oblique division of the muscles proved to be a matter of considerable difficulty, and the object was accomplished as efficiently and more simply by Mr. Benjamin Bell, of Edinburgh,[3] and Mr. Hey, of Leeds, by a combination of the methods of Cheselden and Louis; or, as Mr. Hey expressed it, 'with a triple incision,'[4] in which the skin and fat were first divided circularly and dissected up for some distance, then the muscles were cut at a higher level, and these were retracted so as to permit the bone to be exposed and sawn considerably higher. Mr. Hey added the advice to cut the posterior muscles somewhat longer than the anterior, to compensate for their greater contraction; and thus towards the end of last century, 'the circular operation,' as it is termed, may be said to have been brought to perfection.

Meanwhile a different principle had been long before suggested and acted on. So early as 1678, Mr. James Young, of Plymouth, described 'a way of amputating large members, so as to be able to cure them *per symphysin* in three weeks, and without fouling and scaling the bone'. The directions given for this method, the 'first hints' of which he says he had 'from a very ingenious brother of ours, Mr. C. Lowdham of Exeter', are as follows: 'You are with the catling, or some long incision-knife, to raise (suppose it the leg) a flap of the membranous flesh covering the muscles of the calf, beginning below the place where you intend to make excision, and raising it thitherward of length enough to cover the stump; having so done, turn it back under the hand of him that gripes; and as soon as you have severed the member, bring this flap of cutaneous flesh over the stump, and fasten it to the edges thereof by four or five strong stitches.'[5] Eighteen years later, Verduin, a surgeon of Amsterdam, ignorant apparently of what Lowdham had done, provided like him a covering for the end of the stump from the calf; but, instead of cutting from below upwards, and only raising the integuments, he thrust a knife behind the bones at the part

[1] 'L'amputation la plus parfaite est, sans contredit, celle dans laquelle les chairs qui forment l'extrémité du moignon conservent assez de longueur pour se maintenir au niveau du bout de l'os.' Op. cit., vol. iv, p. 41.

[2] Alanson's *Practical Observations on Amputation*, 2nd edit., p. 16.

[3] Benjamin Bell's *System of Surgery*, 7th edit., vol. vii, p. 260.

[4] Hey's *Practical Observations*, 3rd edit., p. 527.

[5] James Young's *Currus Triumphalis e Terebintho*, p. 108. A copy of this interesting book exists in the library of the Royal Medical and Chirurgical Society of London.

where he intended to divide them, and cutting downwards formed a muscular flap, which he afterwards supported by an apparatus devised for the purpose of pressing the cut surfaces together so as to check bleeding without the use of either cautery or ligature.[1] This machine being complicated and unsatisfactory was rejected in 1750 by M. Garangeot,[2] who, substituting the ligature for it, but retaining in other respects the method of Verduin, brought amputation of the leg to the form in which it is still often practised at the present day.

The same principle was applied to the thigh, in 1739, by Ravaton, of Landau; but instead of one long flap he made two short ones. Having divided all the soft parts circularly, he thrust a knife down to the bone on the anterior aspect of the limb, a hand-breadth higher up, and cut down to the circular wound; and, having made a similar longitudinal incision behind, dissected up the square lateral flaps thus formed, and sawed the bone where it was exposed at their angle of union, and brought them together after tying the vessels.[3]

Vermale, surgeon to the Elector Palatine, soon afterwards formed the flaps more easily, and of a shape better adapted for union, by introducing a knife at the front of the limb and pushing it round the bone at one side, so as to make it emerge at the opposite point behind, and then cutting a flap of rounded form by carrying the knife in a curved manner downwards and outwards, the same process being repeated on the other side.[4]

The flap operation, performed either by cutting from without inwards or by transfixion, was occasionally employed by various surgeons in the latter half of last century; but found its most strenuous advocate in the late Mr. Liston, and at one time seemed likely to supersede the circular method altogether. Its great merit in those days of painful surgery was its facility and speed; for the flaps were cut with great rapidity, and when they were drawn up by the assistant, the bone was exposed with the utmost readiness at the part where it was desirable to divide it; whereas, in the circular operation, to dissect up the ring of integuments was a somewhat troublesome and tedious process, especially in a limb increasing in thickness upwards like the thigh, and the use of a retractor was often necessary, in order that the saw might be applied at a sufficiently high level.

As regards the immediate results of the two methods, the principal difference between them was that the flaps, when formed by transfixion, contained a large amount of muscle, while the circular mode furnished a covering chiefly from the integument. In this respect the flap operation was at first supposed to have

[1] *Mémoires de l'Académie de Chirurgie*, vol. ii, p. 244. [2] Ibid., p. 261.
[3] Ravaton's *Traité d'Armes à feu*, p. 405; also *Mémoires de l'Académie*, vol. ii, p. 251.
[4] *Le Dran's Surgery*, Gataker's translation, p. 431.

a great advantage, as providing a muscular cushion for the end of the stump. But this opinion was shaken by further experience. The muscular part of the covering, no longer discharging its normal physiological function, degenerates and dwindles, while the integument tends to become thicker and firmer, so that the ultimate results of the flap and circular operations present no material difference. On the other hand, at the time of the performance of the operation, the method by transfixion has the great disadvantage that the muscular element in the flap is almost always redundant, and has to be tucked back to permit the edges of the skin to be stitched together, the natural result being tension and confinement of discharges and consequent inflammatory disturbance. In the very case in which the flap operation was first employed, viz. in the upper part of the leg, the muscular mass proved very inconvenient from its redundancy when the calf was largely developed ; and even under more favourable circumstances the heavy and contractile flap was apt to shift from its position or to drag down the skin of the front of the leg, so as to stretch it on the cut end of the tibia, and induce ulceration. Hence Mr. Liston himself, so early as 1839, preferred in muscular subjects a short posterior flap and an anterior one of the same length, composed of integument only ;[1] and in the latter period of his practice he changed this for the following modification of the circular operation, which was also suggested independently by Mr. Syme, and was used by him for many years in all cases of amputation in this situation. The skin and fat are divided by two crescentic incisions with the convexity downwards, so as to form short antero-posterior flaps of the integument, which is then dissected up considerably higher than their angle of union, after which the operation is completed as in the ordinary circular method.[2] This plan gives essentially the same result as the circular mode, while the raising of the integument is facilitated, and its edges can be accurately adapted to each other without any of the puckering that occurred at the angles of the wound after the old operation ; and experience shows that when the soft parts have been divided in this way they are quite as favourably disposed for primary union as when cut more smoothly in the form of flaps.

In the lower part of the thigh also, the presence of the contractile element in the flaps was found to be injurious by increasing the disposition to protrusion of the bone, from the action of the powerful hamstring muscles, cut so far from their origin at the pelvis. Mr. Syme accordingly adapted his modification of the circular method to that situation ;[3] and I can testify to the sufficiency of the covering which it afforded.

[1] Liston's *Elements of Surgery*, 2nd edit., p. 786.
[2] Syme's *Principles of Surgery*, 5th edit., p. 108.
[3] Ibid., p. 170.

The longer time required for this operation than that by flap was rendered a matter of no moment by the discovery of anaesthesia in surgery, in the year 1846.[1] Independently of the relief from bodily and mental suffering procured by this great event, it must be regarded as an era in the history of amputation, of at least equal importance with the invention of the tourniquet; because, pain being abolished during the operation, as well as dangerous haemorrhage, surgeons are now, in the great majority of cases, deprived of all excuse for sacrificing anything, either in plan or execution, to mere rapidity of performance, and are enabled to regard simply what will most promote the two great ultimate objects in amputation—safety to life, and usefulness of the stump.

With regard to the latter object, it was till lately an understood thing that the end of the stump was not adapted for bearing any part of the weight of the body. Being tender from the presence of the cicatrix, it was not allowed by the instrument-makers to touch the artificial limb at all; the apparatus being applied partly to the sides of the stump, but chiefly to some bony prominence resting on the upper edge of the socket—the tuberosity of the ischium when the thigh is concerned, and in the leg the internal tuberosity of the tibia, the head of the fibula, and especially the lower border of the patella.

To this general rule, however, a striking exception was presented by the amputation at the ankle devised by Mr. Syme, in which the bones are divided just above the malleoli, where they present a broad surface for diffusing the pressure over the integument of the heel turned up to cover them, specially fitted by the character of its epidermic investment and subcutaneous fibro-adipose cushion for bearing the weight of the body, while the cicatrix lies well forward out of reach of pressure. The result is that the patient can stand on the end of the stump as on the natural sole; and when the deficient spring of the arch of the foot is compensated by some elastic material contained in a very simple boot, the limb proves nearly as useful as in its normal condition.

Subsequent experience has shown that similar advantages may be attained to a greater or less degree in stumps formed by amputation higher up the limb. It is easy by proper management to ensure the cicatrix falling out of reach of compression by the end of the bone; and the integument, though tender in the first instance, gradually acquires a brawny and callous character when subjected to regulated pressure, like the skin over the dorsal aspect of the cuboid bone in talipes varus, and thus becomes able to bear the whole or part of the weight of the body according to the breadth of the cut surface of the bone, and the consequent diffusion of the pressure. Indeed, stumps possessing these qualities were occasionally obtained as long ago as the time of Alanson, who, speaking

[1] See the essay 'On Anaesthetics' (printed in vol. i, p. 135).

of the condition of a patient on whom he had performed amputation above the ankle by posterior flap, says: 'He has been several voyages to sea, and done his business with great activity. He bears the pressure of the machine totally upon the end of the stump, and has not been troubled with the least excoriation or soreness.'[1] But it is easy to understand why such results were altogether exceptional so long as the covering for the ends of the bones was provided by a posterior flap, which, from the force of gravity and the preponderating power of the posterior muscles over those at the anterior aspect of the limb, must always tend to drop from its original position, and leave some part of the bone to be covered only by cicatrix. And independently of this, in the case of the leg, the tibia being covered in front merely by the skin, a scar placed anteriorly is much more likely to suffer from pressure against the bone than one situated posteriorly. The amputation of the ankle is, indeed, by posterior flap; but the full rounded cushion formed by the cup-shaped integument of the heel renders this an entirely exceptional case. It is plain, therefore, that with reference to fitness of the stump for bearing the weight of the body, preference should be given to an anterior flap, which moreover has the great advantage of allowing a dependent opening for the escape of discharge.

The recognition of the advantages of the anterior flap is due to the labours of two English surgeons, the late Mr. Teale, of Leeds, and Mr. Carden, of Worcester, working independently of each other, and proceeding by different methods. Mr. Teale, who had the priority in publication, formed a long anterior and short posterior flap in the following manner. Having ascertained by measurement the semi-circumference of the limb where the bone was to be divided, he first traced with pen and ink upon the skin four lines of that length; two longitudinal, extending downwards along the sides of the limb, and two transverse, of which one joined in front the lower ends of the longitudinal lines, while the other ran across behind from one longitudinal line to the other at the distance of a quarter of their length from their upper extremities. Two rectangular flaps of very unequal lengths being thus mapped out, he raised them, including the muscles as well as the integuments, by cutting from without inwards, and sawed the bone at their angle of union; then, after tying the vessels, he bent the long anterior flap upon itself, that it might 'form a kind of pouch for the end of the bone', turning up its lower edge to meet that of the short posterior flap, to which it was carefully adjusted and united by a few points of suture, some stitches being also introduced where the edges of skin met at the sides of the stump.[2]

Experience with this method has shown that in properly selected cases it gives admirable results; the patient being often able to rest his entire weight

[1] Alanson, *On Amputation*, p. 133.

[2] Teale, *On Amputation*, pp. 34 et seq.

upon the end of the stump ; and even where this is not fully the case, the distribution of the pressure between the end of the stump and the bony prominences which formerly alone sustained it greatly increases the comfort and steadiness of locomotion.

Nevertheless it must be admitted that Mr. Teale's operation has serious drawbacks. Precise accuracy of execution being essential to its success, it demands a degree of time and pains which, under ordinary circumstances, would certainly not be grudged, if really necessary, but which most surgeons would be glad to be saved, and which sometimes, as in the pressure of military practice, could not well be given. Again, the cut surface is more extensive than with ordinary modes of amputation, involving a larger number of vessels to secure, and also, under some conditions of healing, a more profuse suppuration. But the greatest objection to this method with a view to its general application is the high division of the bone which would frequently be required in order to form the long anterior flap. This defect is of course most marked when the limb is of considerable thickness at the seat of amputation, and shows itself in its most exaggerated form in the thigh of a muscular subject. Thus in a particular instance, where the development was by no means extraordinary, the dimensions were such that, supposing the anterior transverse incision made at the level of the upper border of the patella, it would have been necessary, in order to preserve Mr. Teale's proportions, to saw the bone eleven inches further up, or full five inches higher than if the modified circular operation had been performed. This would seriously have increased the danger, which is always greater the nearer the seat of amputation is to the trunk,[1] while, in case of recovery, the short stump would have been very inferior in usefulness on account of the slightness of the leverage it could have exerted in controlling the movements of an artificial limb.

The same disadvantage would often be experienced in applying the method to the leg. Near the ankle, indeed, where the limb is small and the anterior flap short in proportion, the operation is comparatively free from this objection. But if the circumstances of the case should render it necessary to amputate higher in the limb, the rapid increase of the thickness of the calf would necessitate a high division of the bone greatly out of proportion to the extent of the injury or disease of the soft parts. In a leg of about average development the amputation at Mr. Teale's seat of election, dividing the bones just below the calf, would require the integuments to be sound to the level of the tip of the internal malleolus. But if the skin happened to be unsound to a quarter of an inch

[1] This principle has been pithily expressed by Dieffenbach in the words ' zollweise steigt die Gefahr '. (*Operative Chirurgie*, vol. ii, p. 822.)

above that level, the bones would have to be divided an inch higher; and a difference of three-quarters of an inch in the skin would involve a loss of two inches of the bones; and, again, an affection of the integuments implicating less than two inches above the tip of the malleolus would require a division of the bones full four inches above Teale's seat of election. And in the last-named situation, where the calf is thickest, the very long flap, consisting in the greater part of its breadth of skin alone, would be very liable to suffer from sloughing.

From considerations like these some of the stanchest advocates of Mr. Teale's method are now disposed to restrict it to the lower part of the leg and just above the knee, where, by turning to account the integument over the patella, which is not used in ordinary operations, the anterior flap may be made of the requisite length without specially high division of the bone.

Mr. Carden proceeded upon a much more simple plan, forming a rounded anterior flap of integument only, without any posterior flap, and retracting the soft parts somewhat from the bone before dividing it with the saw; 'thus forming a flat-faced stump with a bonnet of integument to fall over it.'[1] This practice he began as early as 1846, nine years before Mr. Teale first employed his rectangular operation; and though refraining from publication, he obtained from that time forward most admirable results, both in safety to life and the amount of pressure that could be borne by the end of the stump.

It was principally at the knee, where amputation had not previously been much practised, that Mr. Carden applied his principle. The operation at this situation is thus described by him. 'The operator, standing on the right side of the limb, seizes it between his left forefinger and thumb at the spots selected for the base of the flap, and enters the point of the knife close to his finger, bringing it round through skin and fat below the patella to the spot pressed by his thumb; then turning the edge downwards at a right angle with the line of the limb, he passes it through to the spot where it first entered, cutting outwards through everything behind the bone. The flap is then reflected, and the remainder of the soft parts divided straight down to the bone: the muscles are then slightly cleared upwards and the saw is applied' through the bases of the condyles. 'Or the flap may be reflected first, and the knee examined, particularly if the operator be undetermined between resection and amputation. In amputating through the condyles, the patella is drawn down by flexing the knee to a right angle before dividing the soft parts in front of the bone; or if that be inconvenient, the patella may be reflected downwards.'[2]

This operation, when contrasted with amputation in the lower third of the

[1] See *On Amputation by Single Flap.* By Richard Carden, F.R.C.S., &c., p. 6. This is a reprint of an article in the *British Medical Journal*, April 1864.

[2] Op. cit., p. 6.

thigh, presents a remarkable combination of advantages. It is less serious in its immediate effects upon the system, because a considerably smaller portion of the body is removed, and also because, the limb being divided where it consists of little else than skin, bone, and tendons, fewer blood-vessels are cut than when the knife is carried through the highly vascular muscles of the thigh; the popliteal and one or two articular branches being, as a general rule, all that require attention, so that loss of blood is much diminished. In the further progress of the case the tendency to protrusion of the bone, which often causes inconvenience in amputation in the thigh, is rendered comparatively slight by the ample extent of the covering provided, and also by the circumstance that the divided hamstrings slip up into their sheaths, so that the posterior muscles have comparatively little power to produce retraction. The superiority of the operation is equally conspicuous as regards the ultimate usefulness of the stump, which from its great length has full command of the artificial limb, while its extremity is well calculated for sustaining pressure, both on account of the breadth of the cut surface of the bone divided through the condyles and from the character of the skin habituated to similar treatment in kneeling. Considering, therefore, that this procedure can be substituted for amputation of the thigh in the great majority of the cases both of injury and disease formerly supposed to demand it, 'Carden's operation' must be regarded as a great advance in surgery.

It is also of great value with reference to the general question of the best mode of amputating in the lower limb. It confirms completely the conclusion which was, indeed, obvious enough from theoretical considerations, that there is no special virtue in the rectangular shape of the flaps advised by Mr. Teale, but that the advantages claimed for his method may be attained by much more simple means.

Nevertheless to extend the method by anterior flap of skin alone to the thigh and leg, as advised by Mr. Carden, does not seem to me judicious. A flap of integument alone, sufficiently long to cover the entire diameter of the limb, must be liable to the risk of sloughing; and I cannot but think it wise, when the muscular element is available for the purpose, to follow Mr. Teale's example by including it in the composition of the flap. An operation thus intermediate between those of Carden and Teale, with a rounded muscular anterior flap somewhat shorter than Teale's, and compensating for its diminished length and for the absence of a posterior flap by retracting the muscles before applying the saw, was practised in the thigh by Mr. Spence, of Edinburgh, before Mr. Carden published, and yielded very good results.[1] But this operation involves as high

[1] *Edinburgh Monthly Journal*, November 1859.

a division of the bone as Mr. Teale's, and it therefore became an important question whether its advantages might not be attained by some method free from this objection. The essential object to be aimed at is that, while the covering for the bone shall be ample, the tender cicatrix shall be placed sufficiently far back on the end of the stump to be well out of the way of pressure between the end of the bone and the bottom of the socket of the artificial limb. And if, consistently with attaining this object, the anterior flap could be shortened and eked out with a short posterior flap, it is plain that in exact proportion to the extent to which this was done would be the length of bone gained, with corresponding diminution of danger and increase of usefulness of the stump. Now it fortunately happens, both in the calf of the leg and in the thigh, that the bone lies far forward among the muscles, so that even its posterior surface is considerably anterior in position to the longitudinal axis of the limb. Hence a flap as long as two-thirds of the diameter of the limb would ensure the scar being considerably behind the point of pressure; while a posterior flap half as long as the anterior one would be sufficient to complete the covering. The posterior flap, being short, may be made of integument only, without any risk of sloughing, thus getting rid of the bulk, weight, and contractility of a posterior muscular flap. On the other hand, the anterior flap, being still somewhat lengthy, should be raised so as to contain a good deal of muscle, which will be useful not only by ensuring sufficient vascular supply, but also by increasing the thickness of the cushion below the bone; while any tendency to retraction that it possesses (small compared with that of the posterior muscles) will be counteracted by the force of gravity, through which it will naturally tend to occupy its proper place.

Such was the plan of amputating which I ventured to recommend for the thigh and the calf in the first edition of this work, on theoretical grounds which subsequent experience has only tended to confirm. The details of the method, as applied to these two situations respectively, will be found described in subsequent pages.

Before considering the operations best adapted for particular cases of amputation, it will be well to allude in a general way to the necessary instruments, and the mode of using them.

The amputating knife should have a straight and strong back, and a sharp point, near which the edge should present a gentle convexity. In the old circular amputation, a curved knife with a blunt extremity was employed to divide the integument at one continuous sweep; but as the modified operation is always preferable, in which the skin is cut in the form of short semilunar flaps, this somewhat clumsy implement may now be entirely dispensed with. For a flap operation performed by transfixion, the blade should be about half as long again

as the diameter of the limb ; but when the soft parts are cut from without inwards, a much shorter knife will answer the purpose, and should therefore be preferred, as the movements of the smaller instrument can be directed with greater precision and speed. For removing a finger or toe, something intermediate between the tapering bistoury often used in France and the old round-bellied English scalpel will be found to combine the advantages of both, without the inconveniences of either, being equally adapted for piercing and cutting.

In using the knife, the young practitioner will have to unlearn some of the habits he has acquired in anatomical study. The object being now simply to divide the resisting textures efficiently, the stroking and scratching movements of the dissecting room must be changed for a free sawing motion : and for this purpose the knife must be held firmly in the hand, instead of being kept in the feeble position best suited for the investigation of delicate structures.

There is another error to which the habits of dissection may lead, far more serious than a cramped and awkward use of the knife, viz. that of directing the edge of the instrument towards the skin in raising a flap of integument. Such a practice, necessary in anatomy, in order to leave the subcutaneous structures intact, will, if carried into amputation, most seriously endanger the vitality of the flap, which derives its supply of nourishment from vessels ramifying in the fat, and must perish if those vessels are extensively divided through scoring of the *tela adiposa*. I am satisfied that integument designed to form a covering for the stump is often made to slough for want of scrupulous attention to this simple point.

The skin should always be cut perpendicularly to its surface, for if it is bevelled off to a thin edge, it is not only unsuited in shape for adaptation with a view to primary union, but the margin may slough for lack of nutriment.

In transfixing a limb, the direction of the knife must of course be changed as it passes round the bone, in order that it may emerge at the opposite aspect ; but it is desirable that this should be done in a continuous manner ; for if the instrument be thrust in for a certain distance, and then partially withdrawn and made to follow a new track, the punctured wound first made may cause very troublesome haemorrhage, if a considerable arterial branch happen to be divided in it.

In passing the knife round a bony prominence, such as the shoulder, care must be taken to hold the limb in such a position as shall relax the parts that are to be pierced, otherwise what might be quite easy may prove impossible ; and in the latter part of the process, when the point of the knife is advancing in a greatly altered direction, it is important to keep the back rather than the edge directed outwards, in order to avoid cutting the base of the flap.

In amputating at a joint, if the tissues are healthy, the division of the soft parts completes the process, there being no need to take away the articular cartilage, which is almost as favourably circumstanced for healing as vascular structures. Thus, when a finger is removed at the metacarpo-phalangeal joint, the whole wound may unite by first intention; or if suppuration occurs, the cartilage undergoes a change into granulations by a process so speedy as hardly to delay the cure.

The saw, for dividing the bone in other cases, should be broad-bladed, with a stout back, like the 'fine saw' of the carpenter, and should have small but well-set teeth. In applying the instrument, its heel being placed upon the bone, previously cleared of soft parts by a circular sweep of the knife, it should in the first instance be drawn with firm pressure towards the operator, so as to make a groove which it will have no disposition to quit in the first forward stroke. The bone is thus cut precisely at the place desired.

The assistant who holds the limb must take care not to press it forcibly upwards, otherwise the saw will become locked; nor must he draw it downwards to any great degree, or the bone will break and splinter towards the last. But the operator should always be so placed as to be able to control with his left hand the part which he removes. Should any projecting portion be left, it must be removed with a pair of bone-pliers, which may be substituted entirely for the saw when the bone is of very small size, as in the fingers. In using them, the flat surface should always be directed towards the parts that are to be preserved, as the other sides of the wedge-shaped blades crush the bone while they divide it.

The tenaculum, long universally employed for seizing the bleeding vessels in order to tie them, has been superseded by the catch-forceps, which, like the bone-pliers, were introduced into surgical practice by the late Mr. Liston. Besides being always more convenient, they have the great advantage of making the surgeon independent of an assistant in cases of emergency. The ligature should be tightly and securely tied, by reversing in the second half of the knot the relation that the ends of the thread had to one another in the former half, or, in the language of sailors, by making a 'reef-knot'. The larger arteries should be drawn a little way out of their sheaths, as the best means of avoiding nervous trunks and other unnecessary tissue. The principal veins also should be tied; the dread of exciting phlebitis by such treatment having proved entirely groundless. As regards smaller vessels, the old rule was to tie only such as furnished a distinct pulsating stream. But as the catgut ligature with short-cut ends has none of the inconveniences of the long threads of silk or flax formerly employed, there is now no objection to tying mere oozing-points, however numerous; and

this practice has the great advantage that it banishes all risk of reactionary haemorrhage.

The catgut, of course properly prepared to fit it for surgical purposes, should be used of as slender quality as will bear the strain of tying ; except in the case of advanced atheroma, when the finer kinds may be found to cut through the degenerated tissues of an arterial trunk, and a thicker sort must then be employed for the principal vessels. If the ligature cannot be made to hold when applied round the point of the forceps in the usual way, as when fibrous tissue is condensed by inflammatory infiltration, the difficulty may always be overcome by threading a fine curved needle with catgut with both ends long, and passing it so as to take a substantial hold of the tissues at the site of the bleeding-point, cutting off the needle, and tying the two pieces of gut one at each side. The bleeding vessel will be sure to be included in one of them.

Torsion is preferred by some surgeons ; but, though it is admirable for many wounds, particularly about the face, those who have tried both in amputation will, I think, agree that the ligature is more unfailing and on the average more expeditious.

In the second edition of this work I recommended a practice which I had adopted for some years with great advantage, viz. raising the limb into the vertical position and pressing it firmly from the extremity towards the trunk with the view of emptying it of venous blood, and then tightening as rapidly as possible a screw tourniquet, previously kept perfectly loose. The contrast between the ' almost bloodless ' division of the tissues under such circumstances and the gush of venous blood which attended the operation when the tourniquet had been applied in the horizontal or dependent position of the limb was extremely striking. Soon afterwards Professor Esmarch, of Kiel, published his bloodless method, which consisted of forcing the blood out of the limb by means of an elastic bandage applied continuously from the distal extremity to a point some distance above the site of the intended operation, and then applying another elastic band just above, to serve as a tourniquet and maintain the bloodless condition when the continuous bandage was removed. By these means the limb is rendered absolutely ex-sanguine at the seat of operation.

There can be no doubt of the great advantage of the upper elastic band, which follows up any yielding of the soft parts and maintains continuously a perfectly effective constriction ; whereas with the common tourniquet, if the operation was protracted, and especially if the tissues were unusually yielding through inflammatory or oedematous infiltration, the inelastic strap had to be further tightened again and again in consequence of recurrence of bleeding. Esmarch's elastic tourniquet has thus entirely superseded the old instrument.

But for emptying the limb of its blood the method of elevation seems to me preferable, if it is used in such a way as to obtain its full advantages, with a view to which it is essential to understand the *modus operandi*. Though I first employed elevation, as others had occasionally done before me, with the object merely of emptying the limb of its venous blood, I saw before long that much more than this was really done. If the elevated position was maintained for a sufficient length of time, the perfectly blanched appearance of the skin implied that arterioles as well as veins were emptied in a manner that could not be accounted for on merely hydraulic principles by the effect of gravity upon the blood; and being led to inquire into the matter experimentally, I ascertained that when a limb is raised, the first effect of gravity in emptying and relaxing the veins is followed by a gradual contraction of the larger as well as smaller arteries of the limb under the influence of the vasomotor nervous system; the effect reaching its maximum in about four minutes.[1] If, therefore, the limb is kept raised to the utmost for about that length of time, care being taken not to press upon any part containing a venous trunk, and the elastic tourniquet is then rapidly applied, a degree of bloodlessness of the site of operation is obtained which is practically as good as that of Esmarch's method, while it is free from two objections which attend the latter. One of these is referred to by Esmarch, viz. that it is inapplicable in case of putrid infiltration of the tissues, on account of the risk of forcing septic matter into the interstices of sound tissues; and I may add that I should feel considerable hesitation in applying the continuous elastic bandage to a part affected with soft malignant tumour, fearing the possibility of the disease being diffused by the upward pressure through venous or lymphatic channels. The method by position, on the other hand, is applicable to all cases. The other objection to Esmarch's method which many surgeons have complained of, and which has induced some to abandon it, is a liability to reactionary haemorrhage. From this also the method by position is free. Esmarch's original elastic band, consisting of a tube of caoutchouc about as thick as the finger, or a somewhat thinner solid rod of the same material, is, I believe, the best for the thigh; because, while it is exceedingly effective, the abrupt constriction which it produces cannot injure the nervous trunks, well protected as they are by an abundant padding of muscles. But in the case of the arm, where the soft parts are comparatively scanty in proportion to the bone, serious paralytic effects have followed the use of the elastic tourniquet in this form. These are, however, entirely avoided by employing for the upper limb, in accordance with Von Langenbeck's suggestion,

[1] See an Address on the Influence of Position on the Local Circulation, *British Medical Journal*, June 21, 1879 (reprinted in vol. i, p. 176).

a flat elastic bandage, the pressure of which is more diffused. The elastic tourniquet, whichever form is used, should be put well on the stretch, and wound quickly three or four times round the limb to ensure efficiency of its action. The elastic bandage is fixed by means of a pin ; the rod or tube by tying in a bow pieces of stout tape previously well secured to its extremities. This may be done very simply by tying the tape very tightly round the end of the tube or rod bent into a loop, which cannot escape from the grasp of the ligature.

The only inconvenience attending the elastic tourniquet as compared with the old instrument is that it cannot be relaxed and tightened at pleasure to show the bleeding-points, but must be removed once for all. In practice, however, this difficulty is overcome by searching for the principal arteries in the places indicated by anatomical knowledge, and, when these have been secured, tying all points from which any venous blood oozes, by which means the vein and its accompanying artery will be both included. When this has been done, it will often be found that not a single vessel requires attention when the constricting band has been removed. But to guard against the chance of any having escaped notice, the main artery of the limb must be subjected to digital compression.

The strength of the assistant on whom this duty devolves is often early exhausted by unnecessary exertion ; for the current through an artery lying over a bone, or some other resisting texture, is completely arrested by a very moderate amount of pressure directed exactly to the proper part.

A stump after amputation is dressed on the same general principles as other wounds. When there is much tendency to muscular contraction with its attendant risk of protrusion of the bone, as in the lower part of the thigh, this disposition is greatly checked and repose of the stump promoted by a bandage applied smoothly and moderately firmly from above downwards, while an assistant draws down the soft parts. In the thigh and also in the leg great advantage is derived from bandaging upon the posterior surface of the stump outside the dressing a trough of Gooch's splint on which the stump rests smoothly, being rendered independent of movement or irregularity of the pillow. The end of the stump should not be much raised, as too great elevation interferes with free discharge, and increases through gravity the tendency to retraction of the soft parts.

Amputations in the Upper Extremity

The upper limb, independently of its smaller size, and the consequent less shock to the system from the operation, is more favourably circumstanced for amputation than the lower, in consequence, apparently, of its possessing a better

vascular supply and superior vital power. Thus, it is a more serious thing to amputate a toe than a finger, and to take away the arm at the shoulder-joint is a much safer proceeding than to cut off a leg below the knee, even though a larger wound be inflicted, and a larger portion of the body removed, in the former case than in the latter. The more advanced in life the patient is, the more do these differences show themselves. But if circumstances admit of the septic element being effectually excluded, such considerations have comparatively little of the weight formerly attached to them.

The particular amputations in the upper extremity will be most conveniently considered in the order in which they occur from below upwards. The distal phalanges, though very liable to injury and disease, rarely require amputation ; for the removal of crushed portions of bone in the former case, or exfoliation in the latter, will generally leave a useful end to the finger. If it be wished, the phalanx may be readily taken away by opening the joint across its dorsal aspect, and, after getting the knife round the base of the bone, forming a palmar flap, by cutting from within outwards. Or the palmar flap may be first cut by transfixion ; and this being held up by an assistant, the operation is completed by cutting straight through the articulation. If the whole distal phalanx be crushed, amputation through the second phalanx will be best performed by cutting from without inwards two rounded lateral or antero-posterior flaps, and dividing the bone with pliers.

Removal of the entire finger is generally preferable to leaving the first phalanx by itself, which, besides being unseemly, would be a mere incumbrance, except in the index-finger ; and even there it is of service only in some few handicrafts. For the middle, or the ring-finger, the operation is best performed according to the following definite rule. The adjoining fingers being held aside by an assistant, the surgeon cuts from the prominence of the knuckle in a straight line towards the middle of the web on one side ; but, just before reaching the web, carries the knife inwards to the fold between the finger and the palm, and, after making a similar incision on the other side, accomplishes the disarticulation. The edges of the skin will be found to meet exactly on approximation of the adjoining fingers, which should be kept tied in that position, to avoid disturbing the process of union. Remarkably little deformity results from this operation, so that removal of the head of the metacarpal bone for the sake of appearance is quite uncalled for. If, however, it is at any time necessary on other grounds to take away a portion of the metacarpal bone, this can be readily done by the same method, except that the incisions are made to start from the place on the back of the hand where the bone is to be divided by the cutting-pliers.

The index-finger may be removed in a similar manner, care being taken, in

making the incision on the side next the thumb, to carry the knife from the point of the knuckle in a longitudinal direction to near the level of the web between the fingers, before sloping it off towards the palm, otherwise the flap will be insufficient to cover the raw surface. A preferable method, however, is to make dorsal and palmar flaps of rounded form, by cutting from the web between the fingers to a point on the opposite side of the articulation at a sufficiently high level to allow the end of the metacarpal bone to be taken off obliquely with pliers, so as to get rid of what would cause an unseemly prominence. But if it be necessary to remove a considerable portion of the metacarpal bone, the former method, with the dorsal part of the incision extended upwards, will be the best.

Similar rules apply to the little finger ; and, in cases requiring it, the whole metacarpal bone may be removed, by commencing the incision a little above the articulation with the *os unciforme,* so as to give space for dividing the ligaments after clearing the bone of the muscles which surround it.

Any portion of the thumb is valuable for opposition to the fingers ; but, if necessary, the whole of it may be taken away by cutting in a curve, with the convexity downwards, from the web connecting it with the forefinger to the opposite side of the joint, both on the dorsal and palmar aspects, raising the rounded flaps, and disarticulating. The whole metacarpal bone may be removed along with the thumb on a similar plan, by entering the knife a little above the articulation with the trapezium, and cutting first longitudinally, and then with a gentle curve to the web, on each side of the bone, then dissecting up the flaps, and dividing the ligaments of the joint.[1] This operation has been often performed for tumour of the metacarpal bone ; but from a case published by Mr. Syme, it would appear that under such circumstances a useful thumb may be preserved by excising the bone affected.[2]

The thumb alone or a single finger, being far more useful than any substitute should always be retained if possible in cases of injury ; an artificial hand being afterwards used, provided with a claw, against which the single digit left may be pressed so as to hold objects firmly.

Amputation at the wrist-joint may be performed by cutting across the back of the wrist from one styloid process to the other, in a line presenting a slight concavity downwards, in accordance with the form of the articulation, opening the joint on its dorsal aspect, then shaping a rounded flap in the palm, raising

[1] For removing the thumb or little finger with the metacarpal bone, other modes of operating, somewhat more rapid, but in other respects disadvantageous even when applicable, were recommended before the introduction of anaesthesia. At present, it appears only necessary to mention such as are calculated to give the best results.

[2] *Observations in Clinical Surgery*, p. 38.

it to the joint, and disarticulating. Another method is to cut the palmar flap from within outwards after disarticulation ; but the prominence of the pisiform bone prevents this from being satisfactorily accomplished.

Amputation in the forearm may be performed by antero-posterior flaps. In front, where the muscles are in larger amount, transfixion may be adopted ; but behind, the presence of the two bones prevents this, except near the wrist, where it may be effected, provided the soft parts have their natural laxity, by pinching up the skin, and passing the knife as close to the radius and ulna as possible, when, after the integument has fallen back to its usual position, the extremities of the wound will be placed so far forward that the knife can be introduced through them in forming the anterior flap. But it is probably always well to cut the dorsal flap from without inwards, and to raise it so that it shall consist chiefly of integument, in order that redundancy of muscle and consequent tension may be avoided. The surgeon standing on the (patient's) left side of the limb, and holding it with the dorsal surface towards him, enters the knife a little to the palmar side of the bone that is the further from him, and cuts through the skin and fat so as to shape a rounded dorsal flap, terminating the incision a little to the palmar side of the nearer bone, where he at once pushes in the point of the knife, so that it may pass in front of the bones and emerge at the place where the operation was commenced, and cuts a fleshy palmar flap from within outwards. He then dissects up the dorsal flap ; and the soft parts being drawn back by an assistant, clears both bones thoroughly about three-quarters of an inch higher up, and applies the saw. The interosseous artery, which is apt to retract beside the unyielding interosseous membrane, must always be secured as well as the radial and ulnar trunks ; and if the median or ulnar nerve is exposed in the palmar flap, it should be shortened with scissors, to prevent the occurrence of painful symptoms as the stump heals.

There is no objection to amputation at the elbow-joint, in cases adapted for it. The most eligible plan is to cut a large anterior flap from within outwards, after transfixing the partially extended limb in front of the joint, bearing in mind that the line of the articulation is oblique to the axis of the humerus, and is considerably further below the internal than the external condyle. The flap being then held up by an assistant, the points of transfixion are connected posteriorly by a semicircular stroke of the knife, which, besides dividing the integument, probably detaches the radius, and a few touches with the point of the instrument will sever the connexions of the ulna. The assistant should keep the skin of the back of the arm drawn upwards during the operation.

Amputation of the arm presents a good example of the double-flap operation by transfixion. The point of the knife being entered at one side of the limb,

avoiding the site of the brachial vessels and nerves, is pushed on in front of the bone ; and then, by slightly raising the handle, is made to emerge at a place exactly opposite. The anterior flap is then cut with a brisk sawing movement of the instrument, which is first directed longitudinally for a short distance, and then turned gradually towards the surface, and brought out perpendicularly to the integument. The flap is now lightly raised by the assistant, without any traction, for this would interfere with transfixion behind the bone, which is effected through the extremities of the wound already made, and the posterior flap is cut like the anterior. The assistant now retracts the flaps firmly, when a circular sweep of the knife exposes the bone about an inch above the angle of union of the flaps, and another similar turn of the instrument prepares it for the application of the saw. The edges of the wound meet accurately when brought together, producing a symmetrically rounded stump. But when the muscles are largely developed, it is well to avoid the inconvenience occasioned by their redundancy, by cutting the flaps from without inwards, or by employing the modified circular method.

Amputation at the shoulder-joint is an operation which yields very satisfactory results, as was strikingly shown by the experience of the late Baron Larrey, who, during the wars of the first Napoleon, saved ninety out of a hundred cases, in spite of the very unfavourable circumstances of military practice.[1]

Of the various methods that have been proposed, that of Lisfranc is the most expeditious. The arm being raised so as to relax the deltoid, the point of a long-bladed knife is introduced about midway between the coracoid and acromion processes, and thrust round the outer side of the joint till it comes out within the posterior fold of the axilla (or, if the left limb be the subject of operation, the direction of transfixion is reversed), when a large muscular external flap is rapidly cut ; and this being held up by an assistant, and the arm drawn downwards and forwards, the joint is opened by cutting firmly upon the head of the bone,[2] which is then raised from its socket so that the knife may be passed round it, and carried downwards along the inner surface of its neck and shaft, followed by the other hand of the assistant, which grasps the tissues that lie between the track of the instrument and the axilla, so as to prevent bleeding from the main artery, when it is divided in the completion of the short internal flap.

This operation, however, is rarely available in practice. Its satisfactory performance requires the leverage of the humerus, which is generally broken in

[1] *Mémoire de Chirurgie militaire*, par le baron D. J. Larrey, tome iv, p. 434.

[2] Strictly speaking, this is Dupuytren's modification of the method of Lisfranc, who depressed the arm at the commencement of the operation, and opened the joint during the transfixion ; but this was a less easy proceeding, though shorter by a few seconds in very expert hands.

cases of injury demanding removal of the limb, in which also the parts necessary for the large external flap are often encroached on ; and in tumour of the bone, which is the other affection that most frequently calls for amputation in this situation, transfixion becomes inadmissible.

On the other hand, Larrey's mode of operating, by lateral flaps of equal size, proved almost always applicable in his cases of gunshot-wound, while it was as secure against haemorrhage as that of Lisfranc. Thrusting the point of a knife of moderate length down to the bone immediately below the acromion process, Larrey first made a longitudinal incision about two inches in length, from the extremity of which he cut in a curved line at each side of the limb to the fold of the axilla ; then dissected up the muscular flaps so as to expose the articulation completely, a finger of an assistant being placed upon the divided circumflex artery ; and, having severed the connexions of the head of the humerus, passed the knife round it, and kept the instrument close to the inner side of the bone, till, turning the edge towards the surface, he last of all divided transversely the tissues intervening between the axillary folds, containing the artery, previously commanded by the hand of the assistant following the knife.[1]

This operation is improved by dividing the structures between the folds of the axilla obliquely, as part of the internal flap, the lower portion of which is reserved to be cut from within outwards, at the conclusion of the operation : the result being two precisely similar semilunar flaps, meeting above at the acromion and below at the posterior fold of the axilla, adapted for immediate union throughout their length, and presenting as small a wound as is consistent with an efficient covering.

When the bone is broken near the joint, it will be found useful to adopt Mr. Syme's expedient of introducing the finger into a longitudinal wound in the capsule, for the purpose of drawing down the head of the bone so as to gain access to its attachments. In some cases of tumour it may be necessary to raise all the soft parts, including the axillary vessels, from without inwards ; when haemorrhage must be restrained by compression of the subclavian artery over the first rib, by the thumb of an assistant pressed down behind the collar-bone.

Sometimes it may be best to make a large superior flap, cut from without inwards, containing the whole width and chief length of the deltoid muscle ; but circumstances will often arise in which no regular rule can be followed, and the parts that happen to be sound must be turned to the best advantage, accord-

[1] During one period of his practice, he formed the lower parts of the flaps by transfixing from the end of the longitudinal incision to the borders of the axilla, and cutting from within outwards : but the method given in the text is that to which he ultimately gave the preference. See Larrey's *Clinique chirurgicale*, 1829, p. 503.

ing to the judgement of the operator. Even when a large raw surface is left, the granulating process will complete the cure, as is well illustrated by some of Larrey's cases, which terminated satisfactorily after extensive loss of the soft parts of the shoulder and removal of portions of the scapula.

Amputations in the Lower Extremity

The distal phalanx of the great toe may be removed in the same way as that of a finger. When one of the smaller toes is in a condition requiring amputation at all, it should be taken away entirely, since any portion left would be likely to prove inconvenient from being tilted upwards. The operation is exactly similar to that for a finger ; but it must be borne in mind that the articulation with the metatarsal bone, which is the starting-point for the incisions, is much further behind the web than the corresponding joint in the hand, in proportion to the size of the digit.

When the whole great toe is removed, or the little toe, the prominent part of the head of the metatarsal bone must be cut off by an oblique application of the bone-pliers, as it would prove inconvenient if left. The longitudinal part of the incision in the soft parts should be placed on the dorsum of the foot, to avoid the inconvenience that might arise from pressure on a scar at the lateral aspect. In amputating the great or little toe, together with the whole metatarsal bone, it is best to proceed as in the analogous operation for the little finger, the incision being commenced on the dorsum of the foot, about a quarter of an inch behind the articulation with the tarsus, and carried longitudinally to near the metatarso-phalangeal joint, where it bifurcates to embrace the root of the toe. The knife, which should be a strong one, is then applied with a short sawing action close to the metatarsal bone and its articulation with the toe, so as to clear them completely ; and the ligamentous attachments of the base of the bone are lastly divided with the point of the instrument. In the case of the great toe, it is especially important to keep the knife well under command, and avoid thrusting its point deeply into the sole ; for this, besides inflicting unnecessary punctures, may wound the plantar artery at a part difficult of access. This mode of removing the great or little toe and its metatarsal bone, though not so rapid as that of dissecting up a flap from the side of the foot, then cutting between the toe to be removed and the adjoining one, and disarticulating, has the great advantage of avoiding any scar in the sole.

If more metatarsal bones than one require removal, the incision must be begun in the same way, but made to include the roots of all the toes concerned, so as to form a dorsal and a plantar flap ; and even in case of caries in the articulation between the tarsus and metatarsus at one side, a useful foot may be left

after taking away the bones affected, by means of a similar incision commenced further back.

The separation of the whole metatarsus from the tarsus is an operation seldom called for ; but it is evident, from the account given by the late Mr. Hey, of Leeds,[1] who introduced it, that it affords excellent results. When the state of the soft parts permits, the ends of the exposed tarsal bones should be covered with a long flap from the sole, turned up to unite with the dorsal integument, cut very short ; so that the cicatrix, being on the upper part of the foot, may be out of the way both of pressure in walking and of contact with objects in front of it. In performing the operation, it must be remembered that the tarso-metatarsal articulations are not in a regular line, but that the base of the second metatarsal bone is locked between the first and third cuneiform bones, of which the former is the more prominent, and is connected laterally with the second metatarsal by a very strong interosseous ligament. To divide this ligament, Lisfranc adopted the plan of thrusting an amputating knife obliquely downwards and backwards between the first and second metatarsal bones into the substance of the sole, the tissues of which served as a fulcrum, supporting the point of the instrument, when its edge was urged forcibly between the bases of the bones by pushing the handle backwards. This, however, is a needlessly rough proceeding ; for by pressing firmly back between the bases of the bones a strong and short knife, such as ought to be used for the rest of the operation, the ligament may be cut without difficulty ; after which all the articulations are readily separated by scratching through the dorsal and other ligaments with the point of the knife, while the metatarsus is strongly depressed.

The secret of facility in the operation lies in hitting the line of the articulations ; but this is readily enough done by finding first the joints of the first and fifth metatarsal bones, and bearing in mind that the others lie in a line between them, slightly convex forwards, interrupted by the recession of the second bone. The prominence of the base of the fifth metatarsal indicates the situation of its joint, and, if the parts be in a natural condition, the articulation of the first metatarsal with the first cuneiform can also be felt. Should inflammatory thickening obscure the position of the latter, it might be well to measure the distance of the corresponding joint from the internal malleolus on the sound foot ; or assistance may be derived from the circumstance that the joint lies midway between the malleolus and the metatarso-phalangeal articulation.

These points having been precisely ascertained, the surgeon grasps the fore part of the sole with his left hand, placing the tip of the forefinger at one of the joints, and the thumb at the other, to mark their position, and cuts firmly across

[1] Hey's *Observations*, p. 555.

the dorsum of the foot in a line slightly convex forwards, a little anterior to the articulations, taking care that the incision commences and ends fairly in the sole. He then opens the joints of the first and fifth metatarsal bones, so as to ensure finding the line of the articulations afterwards, and next shapes a long plantar flap by an incision extending from the extremities of that already made along the sides of the foot and roots of the toes, dissects up the flap from the bones, and completes the disarticulation in the manner above described.

When the anterior part of the sole is unsound, a shorter plantar flap and a proportionately longer dorsal one may be made, as recommended by Sir Astley Cooper.[1]

Sometimes the proceeding may be greatly simplified by sawing through the metatarsal bones a little anterior to their bases, and so avoiding disarticulation altogether. This method would probably have another advantage, from making the stump of the foot longer and therefore a more effectual lever for opposing the muscles which act upon the calcaneum through the tendo Achillis ; for experience has shown that when the foot is much shortened, the heel is apt to be drawn up, so as to cause the end of the stump to point more and more towards the ground, producing lameness or entire inability to walk. This has been noticed especially after Chopart's amputation through the tarsus, which is consequently an undesirable operation, even in cases of injury : while in caries it is further objectionable, because the part of the tarsus left behind, though apparently sound at the time, may become affected with the same disease at a later period.

If it be wished, however, Chopart's operation may be performed on the same principle as Hey's, by making a very short dorsal flap, and a plantar one reaching to the balls of the toes, to cover the exposed anterior surfaces of the astragalus and os calcis. The articulation between them and the navicular and cuboid bones will be found in a line running across the foot, through a point midway between the external malleolus and the base of the fifth metatarsal bone.

In the amputation at the ankle devised by Mr. Syme, the bones of the leg are divided just above the bases of the malleoli, a covering for the osseous surfaces being provided from the integument of the heel ; the result being a stump admirably fitted for bearing the weight of the body. At the same time, the parts likely to originate carious disease are completely got rid of ; so that this operation is calculated to supersede entirely that of Chopart, besides taking the place of amputation of the leg in the majority of the cases formerly supposed to demand it.

The operation should be performed as follows. Provision being made against

[1] *Surgical Lectures*, edited by Tyrrell, vol. ii, p. 432.

haemorrhage by the pressure of the thumb and finger of an assistant, placed respectively on the middle of the fore part of the limb and behind the tibia, about two inches above the joint, so as to control the anterior and posterior tibial arteries, or by an elastic tourniquet above the knee, and the foot being held at right angles to the leg, the surgeon puts his left hand behind the heel, with the finger and thumb on the places where the incisions are to commence and terminate ; these being the tip of the external malleolus and the point exactly opposite on the inner side, i.e. not at the tip of the internal malleolus, but considerably below and behind it. With a knife, short and strong both in blade and handle, he now cuts down to the bone across the sole, from one of these points to the other, in a plane either vertical or sloping slightly towards the heel when that part is unusually prominent ; and then, extending the foot, joins the horns of this incision by another running as straight as possible across the front of the ankle. He next dissects up the posterior flap from the os calcis, keeping the edge of the knife close to the bone with the guidance of the left thumb-nail, till the point of the calcaneum is fairly turned, when he proceeds to open the joint in front, divides each lateral ligament with a stroke of the knife applied between the malleolus and astragalus, and completes the removal of the foot by severing the tendo Achillis. He then prepares the bones of the leg for the application of the saw ; taking care, when cutting behind the tibia, to keep close to its surface, from which the posterior tibial artery is separated only by a little loose cellular tissue ; and lastly, he takes off the malleoli along with a slice of the intervening part of the tibia, sawing exactly perpendicularly to the axis of the limb—that is to say, directing the saw vertically and transversely while the leg is kept horizontal.

It is a common mistake to make the inner end of the incision at the internal malleolus, instead of opposite the extremity of the outer one. This has two bad effects : it renders the flap unsymmetrical, and, what is far worse, it makes it unnecessarily long, and thus introduces an element of difficulty and risk into an easy and safe operation. For when the incision is carried forwards into the hollow of the foot, it becomes a most troublesome task to turn back the integument over the prominence of the heel ; and the knife being thrust the operator knows not where, the subcutaneous tissue containing the vessels on which the skin depends for its nourishment is punctured and scored, and perhaps the point of the instrument itself appears occasionally through the skin itself, while the flap is subjected to violent wrenching in the effort to draw it back over the bony projection. Under such a combination of unfavourable circumstances, it is but natural that it should slough.

On the other hand, when the flap has been made as above directed, in

accordance with the latest recommendations of the author of the operation,[1] it applies itself with perfect uniformity to the surface it is designed to cover, and has no disposition to shift to one side in the after progress of the case ; and every stroke of the knife by which it is raised being made under the eye of the surgeon, without any forcible traction, it is as little liable to slough as any other portion of integument with an equally broad base and an equally rich vascular supply. Even the integrity of the posterior tibial artery, though desirable, is by no means essential, provided the rest of the subcutaneous tissue has been left uninjured. Many persons, in discussing the merits of this operation, seem to assume as an axiom that sloughing of the flap must occasionally take place ; but I am persuaded from very extensive experience that, if the skin of the heel be sound, such an occurrence will always be the fault of the surgeon.

Hence the various modifications of the original method that have been suggested, though commonly discussed chiefly with reference to a fear of sloughing, must be judged of entirely on other grounds. Thus the plan introduced by the late Dr. Richard Mackenzie, of Edinburgh, of making the base of the flap at the inner side, that it may have a more free supply of blood from the posterior tibial artery, is not to be regarded as a substitute for the simpler method of a posterior flap ; yet it proves useful in case of unsoundness of the integument on the outer side of the heel ; and it is probable that an external flap might be made with equal advantage if the internal aspect of the limb were affected. At the same time it may be worth while to remark that the mere presence of sinuses at either side is no ground for deviating from the original procedure ; and, further, that no degree of complication of sinuous tracks ought to induce the surgeon to amputate in the leg and deprive his patient of the greatly superior stump afforded by Mr. Syme's amputation.

The operation of the late Professor Pirogoff, of Petersburg, in which the posterior part of the os calcis is sawn off and turned up as part of the flap, to unite with the cut end of the tibia, has the disadvantage in cases of caries that it entails a risk of recurrence of disease in the portion of the calcaneum remaining. It is also more complicated than Mr. Syme's method, from the necessity of accurate adjustment of the osseous surfaces, with a view to the best position for the posterior flap. For this purpose both bones are cut obliquely ; the tibia in a plane looking somewhat backwards as well as downwards, and the os calcis in one that is directed somewhat upwards as well as forwards ; so that when the cut surfaces are applied to each other, the dense plantar integument covering the lower part of the calcaneum is presented downwards for supporting the weight of the body, rather than the thin skin over the posterior aspect

[1] See Mr. Syme's Clinical Lectures in the *Lancet*, 1854.

of the bone. If these points are attended to, Pirogoff's amputation gives a thoroughly useful stump in cases of injury. But I am not aware that it has any advantages over that provided by Syme's operation, and the increased length of the stump which it produces is rather objectionable than otherwise; for with the original operation, the space afforded for the artificial foot is not more than the maker finds convenient.

When the ankle-joint is affected with caries, the saw should be applied at a higher level than usual to the tibia and fibula, and the vertical articular surfaces by which the joint is continued upwards between those bones should be removed with cutting pliers, to guard against recurrence of disease in that situation.

In cases which do not admit of Mr. Syme's operation, amputation immediately above the ankle should be performed if possible, in preference to that at 'the seat of election', a little below the knee; for, although the use of the knee-joint may be retained even with a very short stump, the longer one gives greater command over the artificial limb, and the operation involves less risk to life.

Different methods may here be employed. One mode is to make a short semilunar anterior flap cut from without inwards, and a large posterior one formed by transfixing behind the bones and cutting downwards and outwards, the saw being applied a little above the bases of the flaps; or antero-posterior skin flaps of equal length may be made, and the bones divided somewhat higher up. Or again, the modified circular operation [1] is applicable in this situation.

But the method by longer anterior flap is greatly to be preferred to any other, on account of the excellent covering it affords, with the cicatrix out of the way of pressure, enabling the stump to sustain the whole or a considerable part of the weight of the body on its extremity. The principles on which the operation should be performed have been already fully discussed in former pages,[2] but a modification of the plan there indicated is called for on account of the difficulty of retracting the soft parts from the bones. This arises especially from the intimate attachment of the muscles to the fibula; but if these are divided through an extension upwards of the outer longitudinal incision, no difficulty is experienced, unless the tissues are condensed by inflammatory thickening, in effecting retraction of the remaining soft parts from the tibia without dividing the skin at the inner side to a higher level than the typical operation demands. Another point requiring special attention in the leg, as compared with the thigh, is the raising of the anterior flap. The anterior tibial artery, on which the flap depends for its nutrition, lies close to the inter-

[1] See p. 385.

[2] See pp. 387 et seq.

osseous membrane, and would be very liable to be punctured during the dissection if we did not follow Mr. Teale's advice in conducting it. He pointed out that in consequence of the looseness of the cellular connexions of the interosseous membrane, there is no difficulty in separating the parts in front from its surface with the finger-tip, while dividing with the knife the attachments of the muscles to the bones.[1] In this way, the vessel is secured from any chance of injury.

Immediately above the ankle the operation is performed as follows. The diameter of the limb having been ascertained by spanning it, a straight longitudinal incision of that length is made at the inner side of the leg, and on the outer aspect another similar incision directly over the fibula and extending about an inch higher up. The lower ends of these incisions are connected by cutting across the front of the limb in a direction transverse in the main, but rounded off where it joins the lateral lines. The knife is next carried round the back of the limb to the bones from the upper end of the internal incision to a point exactly opposite on the outer side, which will be about an inch below the upper end of the outer incision; the instrument being carried in a line slightly convex downwards, so as to form a very short posterior flap. The anterior flap is then raised in the manner above mentioned, including everything in front of the bones and interosseous membrane; after which the tibia and fibula are cleared as high as the level of the upper end of the outer incision, the finger-tip being still used in detaching the parts anterior to the interosseous membrane.

In order to avoid splintering the fibula, it is best to saw both bones at the same time, and to finish the fibula before the tibia. The sharp angle of the spine of the tibia being apt to cause ulceration of the skin over it, should be removed; and the most convenient way of doing this is to commence with sawing obliquely for a short distance from a point about half an inch above the place where the bones are to be divided transversely. Supposing effectual antiseptic treatment employed, the cutaneous margins of the flaps may be stitched very closely, except at the upper end of the outer incision, which is left open for the drain, and serves admirably for the purpose, as it leads directly from the cut surfaces of the bones, and is dependent in position from the circumstance that the limb reposes on its outer side. Accurate stitching is desirable elsewhere, in consequence of the disproportion of the sizes of the two flaps, which, however, is diminished by making a short posterior flap as advised.

In amputating through the calf on the same principle, the operation is similar, except that, for reasons before discussed,[2] the anterior flap need not be longer than two-thirds of the diameter of the limb; but, to compensate

[1] See *Medical Times and Gazette*, July 6, 1861.

[2] See p. 391.

for its diminution, the posterior flap must be made at least half as long as the anterior, by carrying the knife round the back of the limb at an angle of forty-five degrees through the integuments, and dissecting them up to the level of the upper end of the inner part of the incision, before cutting towards the bones, so as to get rid of the heavy and contractile mass of the sural muscles.

The old flap operation is still employed in the calf by many surgeons, being very readily accomplished by drawing the knife in a segment of a circle across the front of the leg from one bone to the other, transfixing behind them, and cutting first downwards and then gradually outwards, next dissecting up the anterior flap of integument, and clearing and dividing the bones at the level of its base. But it is, as we have seen,[1] a most undesirable proceeding, on account of the bulk of the muscular mass from the calf turned up to cover the ends of the bones. Mr. Spence met this objection by shaving off a considerable portion from the face of the posterior flap after forming it. But though this was undoubtedly a great improvement, it could not give to the operation the advantages of the method by longer anterior flap.

When there is not enough sound integument to admit of the latter method, the modified circular operation of Mr. Syme[2] proves highly valuable, enabling us to form out of the smallest amount of materials a short stump, which is preferable to any that can result from operating higher up in the limb, the patient either retaining the use of the joint or resting his weight with great security and comfort upon the bent knee.

The great merits of Mr. Carden's amputation through the condyles of the femur have been already fully discussed.[3] I cannot but agree with him that the patella should always be removed. In cases of injury it may seem a tempting thing to leave it, sawing off its articular surface, that it may unite with the divided end of the femur ; but having tried this plan before Mr. Carden published, I have found that while it *may* result in an admirable stump, it is sometimes attended with serious inconvenience, from the patella being tilted up from its proper position by the action of the quadriceps extensor. Besides this, the presence of the patella interferes with the adequacy of the covering for the end of the femur, and makes it needful to borrow more integument from the front of the leg than is otherwise requisite. And as regards the ultimate result, when the sawn extremity of the femur has been rounded off by ossific deposit, it proves little, if at all, inferior to the patella for bearing the weight of the body. The only objection to Carden's operation, as described by him,[4] is the occasional occurrence of more or less sloughing of the long anterior flap of skin, in spite of faultless operating. It is plain that the risk of sloughing

[1] See p. 385. [2] Ibid. [3] See p. 387. [4] Ibid.

would be diminished if the flap could be made shorter by not carrying the horns of the incision by which it is formed so high up the limb ; and on making experiments on the dead body several years ago, to ascertain to what extent this could be done without disadvantage, I found that it is by no means difficult, when the parts are in their natural condition, to accomplish the operation without making any anterior flap at all, the integuments in front being divided transversely at the level of the lower end of Mr. Carden's flap. I also found it advantageous to form a short posterior skin-flap, both for the sake of coaptation of the cutaneous margins without puckering, and as a useful addition to the covering for the end of the stump.

With this modification, the operation is performed as follows. The surgeon first cuts transversely across the front of the limb from side to side at the level of the anterior tuberosity of the tibia, and joins the horns of this incision posteriorly by carrying the knife at an angle of forty-five degrees to the axis of the leg through the skin and fat. The limb being elevated, he dissects up the posterior skin-flap, and then proceeds to raise the ring of integument as in a circular operation, taking due care to avoid scoring the subcutaneous tissue ; and, dividing the hamstrings as soon as they are exposed, and bending the knee, he finds no difficulty in exposing the upper border of the patella. He then sinks his knife through the insertion of the quadriceps extensor, and having cleared the bone immediately above the articular cartilage and holding the limb horizontal, he applies the saw vertically and at the same time transversely to the axis of the limb (not of the bone), so as to ensure a horizontal surface for the patient to rest on. The popliteal artery and vein are then secured, and any articular or other small branches that may require it.

When the soft parts are thickened and condensed by inflammation, the integuments cannot well be reflected above the patella with such incisions of the skin. But the difficulty may be got over by cutting into the joint as soon as the ligamentum patellae is exposed, and at once removing the leg by dividing the ligaments and hamstrings ; after which the soft parts can be retracted from the femur sufficiently to permit the application of the saw. The arteries having then been secured, the patella is dissected out at leisure.

As thus performed, Carden's operation takes a little more time and pains than when the integument is divided in the form of an anterior flap ; but these are well rewarded by the ample covering for the bone, the small external wound, and the perfect security against sloughing.

Some surgeons speak highly of amputation through the knee, leaving the articular portion of the femur and the patella, a covering being provided by forming a large anterior and short posterior skin-flap from the leg, the result

being that the patient rests his weight upon the broad rounded end of the bone while the patella is drawn up by the quadriceps to occupy the hollow between the condyles in front.[1] There can hardly, I think, be two opinions as to the superiority of Carden's method to this procedure for carious disease of the knee-joint; and in cases of injury, when the integuments are sound as far as five inches below the patella, which is the length of the long anterior flap according to the method hitherto recommended,[2] a satisfactory though very short stump may be made below the knee. But from my experience with Carden's operation I feel sure that the amputation through the knee may be much improved by dividing the integument in the circular fashion, slightly modified to permit neat adjustment of the cutaneous margins, in which case it would not only be freed from the risk of partial sloughing of the anterior flap which is admitted by its advocates,[3] but, the posterior integument being made to take a larger share in forming the covering, it would not be needful to go so far down the limb in front, and thus the operation would become available for cases of injury reaching too high in the limb to permit amputation below the knee. And in order to ensure complete adequacy of the covering, the saw might be carried through the middle of the articular end of the femur so as to flatten it without interfering with its breadth, and thus in all probability improve rather than impair the fitness of the end of the stump for bearing the weight of the body. On this matter, however, I cannot as yet speak from personal experience.

In amputation of the thigh, if we except cases in which the soft parts are affected at one side only, where a covering may be advantageously provided from the sound side, the flaps should always be antero-posterior, because, the flexor muscles being no longer counteracted by the weight of the limb, the bone tends to become tilted forwards, so that its extremity would be apt to show itself in the anterior angle of lateral flaps.

In the lower half of the thigh, the method by longer anterior flap, on the principles before considered,[4] will be found easy of execution and excellent in results. Two straight incisions are made through the skin and fat along the lateral aspects of the limb, parallel to its anterior surface, and equal in length to two-thirds of its diameter, and their inferior extremities are connected in front by a straight transverse cut, curved upwards near its ends to join the longitudinal ones, so as to shape out a moderately long rectangular flap with rounded angles, if we may so speak. The knife is then passed round the back

[1] See especially a paper on Amputation at the Knee-joint, by Mr. Pollock, *Medico-Chirurgical Transactions*, 1870.

[2] See Mr. Pollock, ibid. [3] Ibid. [4] See p. 388.

of the thigh at an angle of forty-five degrees to its axis, marking out a short posterior skin-flap, which is at once dissected up, the limb being well elevated by an assistant. The anterior flap is next raised so as to contain a moderate amount of muscle, and the soft parts being well retracted, the knife is swept circularly through the muscles, so as to expose the bone for the application of the saw about two inches above the angle of union of the flaps.

The incisions should always be made as far down in the limb as the state of the soft parts permits ; and the skin over the patella, if available, will be employed with great advantage as part of the anterior flap.

For restraining haemorrhage the elastic tourniquet must be applied as high as possible in the thigh, and if its constriction is found to interfere with the due retraction of the soft parts, it is best to saw the bone in the first instance where it is easily reached, and, after securing the vessels and removing the tourniquet, expose the bone at the requisite level, and saw off an additional portion, held steady with a pair of strong forceps.

When digital compression is resorted to, the hands should grasp as much of the circumference of the limb as possible, while the thumbs are placed one above the other over the vessel, as it lies on the pelvis, midway between the symphysis pubis and the iliac spine.

Even in the upper part of the thigh, although the object of having a stump capable of bearing weight upon its extremity is no longer to be considered, the operation above described will be found to yield better results than that by transfixion, by avoiding the redundancy of muscle which is the great defect of the latter method. Nor need this plan involve greater loss of blood. For the posterior flap, being only cutaneous, can be raised without material bleeding ; and the anterior flap, after being shaped by carrying the knife through the skin and fat, may be completed by transfixion, while comparatively little retraction of the soft parts is required, in consequence of the short-cut muscles having little tendency to cause protrusion of the bone. Moreover, all bleeding during the performance of the operation may be effectually prevented by the elastic band applied in the manner to be described in connexion with the next amputation.

Amputation at the hip-joint has of late years been divested of much of the danger that formerly attended it ; so that it now ranks among the well-established operations of surgery.

What may be termed the classical method is to form a large anterior flap by transfixion, disarticulate, and cut a short posterior flap, also from within outwards. The thigh being somewhat fixed, to relax the soft parts of the front of the limb, the point of a knife with a blade fully a foot in length is entered midway between the anterior-superior spinous process of the ilium and the

great trochanter, supposing the left side operated on, and passed in front of the bone till it emerges near the tuberosity of the ischium, or in the opposite direction if the right limb be concerned. The knife is then carried longitudinally with a rapid sawing movement, followed by the fingers of one hand of an assistant, which are introduced into the wound so as to compress the femoral artery securely between them and the thumb, previously placed over it in the groin, his other hand being employed to lift up the large anterior flap as soon as it is completed. The limb being now extended and abducted, the surgeon opens the capsule of the joint by cutting firmly upon the head of the bone; and as this starts from its socket, he divides the round ligament and the posterior part of the capsule; and lastly, the thigh having been adducted, to draw the trochanter down out of the way of the knife, he completes the severance of the limb by cutting downwards and backwards through the muscular mass at the back of the thigh.

Attention is now at once directed to the bleeding vessels of the posterior flap, fed by the internal iliac, which are covered in the first instance with a folded cloth, or, what is better, by the tips of the fingers of an assistant; and when they have been tied the femoral trunk and any of its branches which may require it are secured in the anterior flap.

But though I have described this mode of operating, captivating as it is by its brilliant swiftness of performance, I do not desire to recommend it. Many years ago I was much impressed with a circumstance that I witnessed in the practice of one of my colleagues in Glasgow. He amputated below the trochanters by antero-posterior flaps for malignant sarcoma of the lower part of the femur; but the part of the bone removed being examined after it had been sawn longitudinally while the vessels were being secured, the disease was found to extend up to the part where it had been divided in the amputation. The surgeon therefore seized the remainder of the femur with powerful forceps and dissected it out from its socket. This was done with great facility and with scarcely any loss of blood; and it occurred to me that, if the same procedure were adopted when it was intended from the first to disarticulate, shock, which is one of the great dangers of amputation at the hip-joint, would surely be greatly diminished; for we could not suppose that the powerful impression produced upon the nervous system by that operation performed in the usual way could be due either to the removal of the head of the bone or to the mere extent of the cut surface as such. The correctness of this view has been since strikingly demonstrated by the practice of Mr. Furneaux Jordan, of Birmingham, who, in cases suitable for such a procedure, first divides the soft parts circularly low down in the thigh, and then dissects out the bone from among the muscles

and from the acetabulum through a long incision on the outer aspect of the limb, where the soft parts are comparatively thin and the blood-vessels inconsiderable; a long boneless stump being the result. Now such an operation involves both disarticulation and the formation of an exceedingly extensive wound; yet Mr. Jordan's anticipations of increased safety of this method as compared with the old one seem to have been fully realized. Ever since the Glasgow experience to which I have referred, I have myself proceeded on the principle which it suggested; and while it does not seem to me necessary to push it to the extreme degree advocated by Mr. Jordan, I would advise the following as the method to be generally adopted.

Supposing the right limb operated on, the knife is entered at the posterior part of the great trochanter and carried down longitudinally for about eight inches (if the patient be an adult male), and then drawn across the limb in front and behind through skin and fat, in the form of two crescentic incisions which meet at the inner side of the limb at a point an inch or two lower down than the extremity of the outer longitudinal cut. The semilunar flaps mapped out by the crescentic incisions are then dissected up as in a modified circular operation, the integument being raised about two inches higher than their angle of union at the inner side of the thigh; after which the muscles are divided where they are exposed and the head of the bone dissected out.

Such a mode of operating, besides the diminished danger from shock, has the great advantage of making truly aseptic treatment easy, instead of almost impossible, as it is when the copious sero-sanguineous discharge which takes place from so large a wound is poured out within a very few inches of the anus, which is the case after the ordinary operation, with the dependent angle of the wound close to the tuberosity of the ischium. After the operation which I have advised, the inner end of the wound having been closely stitched and drainage-tubes introduced at its outer part, there is sufficient space for an effectual antiseptic dressing, which will often be a matter of life and death where so large an extent of irritable and absorbent surface is concerned.

The longer time occupied by the operation is of no consequence now that we have the means of dealing efficiently with the once dreaded haemorrhage. For this purpose I advised in former editions of this work the use of the aortic tourniquet. This instrument, however, has two defects. In the first place, when the aorta deviates to any considerable extent from its normal median or nearly median position, the tourniquet is somewhat difficult of adjustment, and instead of retaining its position by the clamping action of the screw which presses down the pad, it tends to slip to one side on the rounded body of the lumbar vertebra, and must be held in place by a very careful and steady assistant.

And, in the second place, an inexperienced or nervous surgeon may be tempted to screw down the rigid instrument with needless violence and damage the intestine by so doing.

Mr. Davy, of the Westminster Hospital, has suggested a very ingenious mode of compressing the common iliac artery by introducing into the rectum one end of a smooth wooden cylinder two feet in length and about an inch in diameter passed in sufficiently far to permit it to be pressed down upon the vessel on the brim of the true pelvis when the other end or handle of the instrument is carried to the thigh of the opposite side, and then raised so that the rod may act as a lever for which the anus serves as a fulcrum.[1] In most cases in which Davy's lever has been employed it has answered to admiration.[2] But it is intelligible that in case of a short mesorectum it might be impossible without undue force to effect compression of the iliac trunk on the right side; and of course if the coats of the rectum were unsound, the instrument would be wholly inapplicable. Accordingly, I lately heard of a case in which a gentleman specially conversant with the use of the lever failed to bring it into effective action; and another case has been mentioned to me where death resulted from mischief done by the end of the rod working in the dark.

Hence I believe it to be wiser to adopt here also the principle of Esmarch's elastic compression. It may be applied either to the aorta or to the extreme upper part of the limb. For the aorta a pad of sufficient size, such as a pincushion, adjusted over the vessel about the level of the iliac crests, is pressed down by elastic bands, which, however, ought not to encircle the body directly and so cause inconvenient constriction of the waist, but should be connected with the ends of a rigid object placed transversely beneath the back and extending laterally sufficiently far to protect the sides of the body from compression. A narrow piece of board with two lateral notches at each end would answer the purpose quite well for an emergency as a substitute for the curved piece of stout iron with rings or hooks at the ends recommended by Esmarch.

When the elastic band is applied to the *limb* for amputation at the hip-joint, special arrangements must be adopted to keep it well out of the way of the knife, and also to prevent it from slipping down and becoming useless when the support of the head and neck of the bone is withdrawn by disarticulation. The following method will be found to answer perfectly. An elastic band having been provided sufficiently strong to require the full force of the surgeon to stretch it to twice its length,[3] and long enough to encircle the upper part of

[1] See *British Medical Journal*, May 18, 1878.

[2] See Mr. Pearce Gould, *Transactions of Clinical Society of London*, 1879.

[3] About three of the ordinary rods of red caoutchouc, placed side by side and tied together at their ends, will be found to answer the purpose for an adult.

the limb when in the relaxed condition, and with tapes securely connected with its ends, is placed with one end of the elastic part under the sacrum, while the tape of that end is brought round the pelvis between the crest of the ilium and the great trochanter of the side opposite to that to be operated on, and held perfectly firmly in the vertical position by an assistant. The surgeon then, standing on the side for operation, puts the band fully on the stretch in a direction transverse to the body and brings it up into the vertical position immediately below the iliac crest. Holding it in his left hand (if the right limb is concerned), he next passes his right hand round behind the limb, which has been previously placed in the vertical position to expel its blood, and, changing hands, encircles the thigh as near to the perineum as possible, the scrotum being held well to the other side by an assistant. The surgeon's end of the elastic band being now over the groin, he takes the other tape from his assistant and ties the two tapes together in a reef-bow over the sound side. Another point requires attention. Two pieces of bandage, each about two feet in length, are placed longitudinally upon the skin before the elastic band is applied, one of them over the groin, the other well behind the great trochanter ; the middle of each piece of bandage being in the situation where the elastic band is to go. And when the elastic band has been applied, the lower end of each of these pieces of bandage is drawn up so as to convert them into two loops by means of which, in the hands of a steady assistant, the elastic tourniquet is kept drawn well up both at Poupart's ligament and behind the trochanter. If this arrangement is well carried out, the whole operation, including disarticulation, may be done uninterruptedly. Nevertheless, I think it prudent to retain the resistance of the head and neck of the femur so long as the tourniquet is in operation, by sawing through the bone below the trochanters, and at once securing all the vessels that show themselves on the cut surfaces. The tourniquet is then removed while an assistant compresses the femoral at the groin : and when any branches still requiring attention have been tied, the remainder of the bone is seized with strong forceps and dissected out. With the incisions which have been recommended this will be found a matter of the utmost facility and attended with little if any haemorrhage.[1]

[1] The article on Amputation was first published in the 1st edit. of Holmes's *System of Surgery*, vol. iii, 1862. It afterwards appeared in the 2nd edit., vol. v, 1871, and in the 3rd edit., vol. iii, 1883. In its later appearances, while retaining its original features, it was altered in various details in accordance with the progress of knowledge.

ON EXCISION OF THE WRIST FOR CARIES

[*Lancet*, 1865, vol. i, pp. 308, 335, 362.]

To save a human hand from amputation, and restore its usefulness, is an object well worthy of any labour involved in it. When caries affects the shoulder or the elbow, the limb is preserved by excision of the diseased joint, and the brilliant success of these operations naturally suggested a similar procedure for the wrist. The first attempt of this kind appears to have been made as early as the close of the last century by the younger Moreau, who, however, gives but few details of his case. In 1839, a German surgeon, named Dietz, is said to have removed all the carpal bones, together with the ends of the radius and ulna, on account of caries.[1] But as such an operation must necessarily have been very painful and protracted, we cannot wonder that it was not repeated till after the introduction of chloroform, when, in 1849, Heyfelder, of Erlangen, excised the wrist-joint for disease, and he has been followed by many surgeons, both British and foreign, who have adopted various methods of effecting their object.

The results of this practice, however, have not proved encouraging. For although several instances of success have been put on record, it is generally admitted that these are quite exceptional,[2] and amputation is now again considered by most surgeons the appropriate treatment for caries of the carpus.

About two years ago a more hopeful view of the subject was suggested to me by a case of injury under my care in the infirmary. The patient was a young man, seventeen years of age, who had fallen about fifty feet down the shaft of a coal-mine, and, besides fracture of the left thigh, had sustained a compound dislocation of the wrist of the same side, the articular ends of the radius and ulna protruding anteriorly for about an inch and a half through a large irregular wound. I sawed off the exposed portions of the bones, and placed the limb on a splint ; and, commencing passive movement of the fingers early, and maintaining it perseveringly, I had the satisfaction of seeing him, at the end of five months, with a hand nearly as supple and strong as the other, the chief difference between them being that the wrist of the injured side was rather more slender than the sound one.

This case appeared to me to throw light upon excision of the wrist for disease. In the first place, it was clear that no operation, intentionally performed,

[1] See O. Heyfelder, *Operationslehre und Statistik der Resectionen*. p. 202.

[2] See Erichsen's *Science and Art of Surgery*, 4th edit., 1864, p. 768. Holmes's *System of Surgery*, vol. iii, 1862, p. 812.

would do such violence to the tendons as must have been inflicted in that accident, both on the flexors through which the ends of the bones were so rudely thrust and on the extensors wrenched out of their sheaths in spite of the secure connexions of the annular ligament. Hence the favourable issue of this case indicated that the tendons might be very freely dealt with in gaining access to the carious bones without inducing stiffness of the fingers, provided the after-treatment were rightly conducted.

And in the second place, the fact that a useful hand had been retained after the loss of so large a portion of the bones, suggested that the same happy result might follow removal of the whole articular apparatus of the wrist ; that is to say, excision of all the carpal bones, together with the ends of the radius and ulna, and the bases of all the five metacarpal bones.

If this were done, recurrence of the disease, the grand cause of disappointment in excision of the wrist, would, as I hoped, be avoided ; and the operation would be placed on a par with excision of the elbow, which, if properly performed, may be relied on with almost absolute security for complete extirpation of the caries. I have long believed that the reason of the remarkable success attained in this respect by excision of the elbow is that the surgeon (when operating in the manner to which I allude) takes away in all cases, however limited the disease may seem, the entire surface covered with cartilage. For it is in the cartilage that caries commonly takes its origin, and even parts of it which may appear sound in a carious joint seem apt to be affected in an insidious, incipient degree, and if left behind may lead to recurrence of the complaint. But, in excising the wrist, all that has hitherto been aimed at has been to take away such portions of the bones as are found to present unhealthy characters, leaving behind more or less of the articular surfaces, which, from the forearm to the metacarpus, may be viewed with reference to caries as forming a single complicated joint, though subdivided in health into three synovial sacs. On the other hand, if the whole of the structures thus liable to morbid action were cleared out, there seemed good reason to hope that success in excision of the wrist for caries might become the rule instead of the exception.

A few months later two cases of caries of the wrist presented themselves for treatment, and, after some experiments upon the dead body, I resolved to test the new principle upon them, and operated upon both on the 16th of April, 1863. Since that time the practice of our large infirmary has afforded me frequent similar opportunities, which have enabled me gradually to improve both the operative procedure and subsequent management, and also to judge fairly of the ultimate results. These having proved fully equal to my theoretical anticipations, I now feel called upon to bring the subject under the notice of

my professional brethren ; and first I will give shortly some illustrative cases, reserving meanwhile the details of the treatment.

Case 1.—Elizabeth M'K——, a millworker, aged forty, in good general health, was admitted on the 27th of October, 1862, on account of suppuration of the right carpus, resulting from the hand having been violently pinched in a door. Pus was discharged from openings at the back of the wrist, and the carpal bones were felt to grate upon one another on manipulation.

The disease being of traumatic origin, I hoped it might subside if the limb were kept at rest upon a splint, and free exit were provided for the discharge. This treatment was persevered with for upwards of five months, but proved unavailing ; and she also continued to suffer considerable pain. Accordingly, on the 16th of April, 1863, I removed the carpus, and at the same time took off so much of the bones of the forearm and of the five metacarpal bones that the interval between them where they were divided measured two inches and a half. The bones of the carpus and the metacarpal bones of all the fingers proved to be extensively eroded by caries.

Seven weeks after the operation the limb was almost healed and promised a most satisfactory result, when, being an ignorant woman, and mistaking our efforts to maintain the flexibility of the fingers for attempts to break them she ran away from the hospital, and did not show herself again for nearly five months, during which time she had kept the fingers extended and motionless upon the splint she took out with her. Consequently they were almost absolutely rigid, and the movements of the thumb were also extremely limited, so that the hand was nearly useless, while, from the position in which it had been habitually held, it had acquired some tendency to droop towards the ulnar side. It was, however, soundly healed ; and, through repeated forcible movement under chloroform to break down the adhesions of the tendons, and the use of a leather splint to support the palm and ulnar border of the hand without interfering with the thumb or fingers, it improved remarkably, and when she left the hospital in April 1864 she could use it for wringing a cloth or knitting a stocking. The improvement has since been progressive. In August it was found that without the splint she could readily lift a kettle of water weighing six pounds, implying a most satisfactory command of the muscles over the newly formed articulation. At first I had aimed at anchylosis of the wrist, but was now much better pleased to see that it retained the power of flexion and extension, eversion and inversion, pronation and supination. Even now (March 1865) the limb is still increasing in strength, in proof of which she lately raised with outstretched hand a pail of coals weighing 16½ pounds. She has for the last

six weeks entirely discarded the support, having found the hand exactly as strong without it. The new wrist is now as firmly knit as the sound one, but more slender in consequence of the radius and ulna having been so freely resected.

Case 2.—Margaret W——, aged fourteen, a sewing-machine worker, was admitted on the 20th of March, 1863, when she stated that a swelling had appeared five months previously on the back of her right hand, which, however, remained free from pain till within about three weeks, when suppuration occurred. An incision was made by her medical attendant, but this failed to relieve her; and when she came into the infirmary she was still suffering severely, while there was also considerable swelling of the hand.

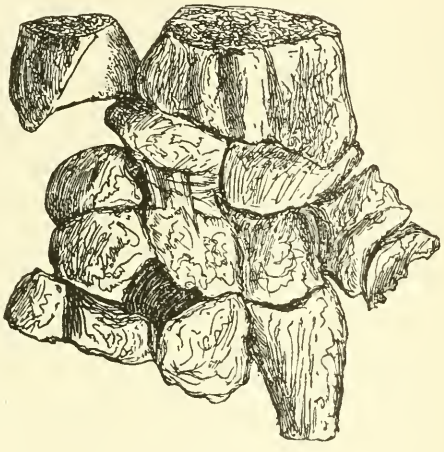

Fig. 1.

The limb was placed on a splint and poulticed, but additional abscesses formed and opened, and at length the probe distinctly indicated caries of the carpus.

On the 23rd of May I excised the parts represented in Fig. 1. The carpus was chiefly affected by the disease, but the metacarpal bones of all the fingers were implicated, and the radius was anchylosed to the scaphoid and semilunar. Constant attention being subsequently paid to supporting the wrist and bending the fingers, she progressed steadily, though slowly. Thus seven weeks after the operation, the hand no longer drooped when the arm was extended horizontally; three months and a half later she could take up a roll of bandage between the finger and thumb; and when three months more had elapsed she knitted part of a stocking without using any splint. About this time, as she had never learned to write, the nurse of the ward taught her the art, which, being a clever girl, she soon learned; and half a year afterwards I received from her a letter, well written with the affected hand, requesting a certificate of soundness for the satisfaction of her old employer, who was about to re-

engage her. In August 1864 I saw her again. She was then employed at the sewing-machine, earning ten shillings a week, with the expectation of eleven shillings before long, as she was considered one of the best hands at the work. She still wore a leather support for the palm, but without it could lift a heavy weight with the arm horizontal. She stated that there had been no discharge from the hand for the last two months ; and that the only way in which she could convince her friends of the nature of the operation she had undergone was by showing to them her two arms extended side by side, the affected limb measuring two inches less than the other from the elbow to the finger-tips. Lastly, in December 1864, I learned that the hand was still constantly increasing in strength, and that she was on her full wages.

Case 3.—William C——, aged eighteen, a clerk, was admitted on the 14th of January, 1864. Two years previously I amputated the great toe of his right foot on account of strumous disease. The wound was slow in healing, and in walking with a stick he thinks he over-exerted the right wrist, which became swollen and disabled, though for a long time free from pain. The treatment employed failed to arrest the disease, and at length suppuration occurred ; and a probe introduced through one of the openings by which the matter escaped passed down to carious disease in the carpus. The hand had now been useless for a twelvemonth, and I recommended him to submit to incision, for which purpose he came into the infirmary.

On the 16th of January I removed the carpus, together with the articular ends of the adjacent bones. His progress afterwards was satisfactory. Within seven weeks of the operation he could bend the fingers, and raise the metacarpus by muscular action at the same time ; and five weeks later he left the hospital, able to pick up light objects with the unsupported hand, and to execute to some extent all the natural movements of the wrist-joint. In August, after four months more had elapsed, the actions of the wrist were much more free, and the new joint was so secure that without any splint he could support a kettle of water weighing six pounds and a half upon the radial border of the hand with the arm extended horizontally, and easily lifted a chair with the arm vertical. He bent the fingers imperfectly at the knuckles, but moved their other joints and both those of the thumb very freely, and he could hold a pen so as to write with considerable steadiness. In September all discharge finally ceased. The hand has since remained perfectly sound ; and when I saw him in December 1864 there was nothing in its appearance to attract attention. He was engaged in a situation where little writing was required, but the hand was becoming more and more serviceable for that purpose. Its grasp also was con-

siderably stronger, and he used it occasionally to work the bellows of a forge. He still derived benefit from a palmar support with which he had been furnished, but he had gone without it for a week at a time, and promised soon to be entirely independent of it.

Case 4.—Helen M——, aged fourteen, a schoolgirl, admitted on the 19th of February, 1864, attributed the disease, which had appeared seven months before in her left wrist, to a violent squeeze experienced at that time. Three months after the accident it suppurated, and was opened on the dorsal aspect by a medical man ; but its condition became rapidly aggravated, and at length her parents sent her to the infirmary to have the hand amputated. That this was the only feasible treatment was certainly a most natural conclusion from the appearance presented by the affected part. The hand was enormously swollen both on the palmar and dorsal aspects, and drooped helplessly from ligamentous relaxation ; while the fingers were almost fixed in a semiflexed position. There were several sinuses on the back of the hand, and in front of the wrist a deep ragged grey sore as large as a half-crown, and another smaller ulcer on the palm. Her general health at the same time was much reduced.

But most unpromising as the case appeared, I determined to give the hand a chance, and at the same time test fully the capabilities of the new method. On the 5th of March I removed the carious mass, when the metacarpal bone of the middle finger proved to be so extensively affected that it was necessary to drill it with the gouge into a mere tube, which must have reached to near the knuckle, as a portion of the cartilage in connexion with the epiphysis was removed by the instrument.

The result turned out satisfactory, and she left the hospital on the 30th of July, with the sores and sinuses almost healed, and able to move all the fingers freely, and also, at an earlier stage than in any previous case, to raise the knuckles above the level of the forearm by muscular effort. In October she could support a kettle of water on the radial border of the hand, and her general health was completely re-established. In the middle of December she could take up a quart bottle full of water, holding it by the neck between the forefinger and thumb. She was herself disposed to dispense with the palmar support, but was recommended to continue it, so as to favour as much as possible the rapid increase of strength and usefulness. There had been no discharge for the last month, except a little moisture on the removal of a scab ; and the part once so greatly deformed was nearly natural in appearance. When I last saw her (March 1865), she told me she was learning to work at a sewing-machine, and found her hand thoroughly useful for the purpose.

Case 5.—Thomas M——, aged twenty-one, a miner, was admitted on the 8th of July, 1864. About six months before, when suffering from small-pox, he was seized with inflammation in the right tibia and the left carpus, resulting in necrosis of the former and caries of the latter. When he came into the hospital the back of the wrist was swollen, and presented two sinuses through which a probe could be passed down to the diseased bone. The hand was extremely feeble, and drooped when the arm was extended horizontally. It was very painful, interfering seriously with his night's rest, and his general health was otherwise much deranged, his pulse being 135, and his appetite impaired, while he was constantly bathed in perspiration.

On the 16th of July I extracted some exfoliations from the affected tibia, and also removed from the wrist the parts represented in Fig. 2. A carious

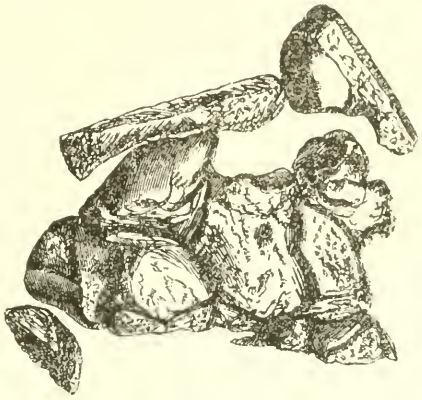

Fig. 2.

cavity occupied the place of the semilunar bone, and the adjacent part of the cuneiform was excavated. The other carpal bones, except the trapezium, were anchylosed into one mass. Two days after these operations his pulse had fallen eleven beats, and after two days more he was recovering his appetite, and had lost his perspirations, while his former anxious expression was exchanged for that of cheerfulness. The improvement in his general health continued, and the hand made most satisfactory progress. Six weeks and six days after the excision it was soundly healed, and the swelling had entirely disappeared. He could move all the joints of all the fingers and both those of the thumb, and performed to some extent all the natural actions of the wrist. A fortnight later he stood the test of lifting a weight of six pounds and a half with the unsupported hand in the horizontal position of the forearm. In November, four months after the operation, the movements had so greatly increased in freedom and firmness, and the hand had so thoroughly natural an appearance, that it was with difficulty some of my medical friends could be persuaded that the

carpus had been removed at all, more especially as a growth of new bone from the radius presented considerable resemblance to the prominence of the os magnum.

On the 13th of February he entirely discarded the leather support, which for some time he had only worn at night as a safeguard, and now (March) his hand has a powerful grasp, and is in all respects nearly, if not quite, as useful as ever.[1]

Case 6.—Mary Ann L——, aged nineteen, a millworker, was admitted on the 22nd of October, 1864. Her case differed from any of the preceding, in being extremely acute. Ten days before her admission, pain came on at night in the right wrist without any assignable cause, and increased from day to day till it became agonizing. When I first saw her the whole hand and the neighbouring part of the forearm were greatly swollen and fiery red, and contained a considerable amount of pus, which I evacuated by very free incisions. This relieved her only temporarily; and, when she had been in the hospital twelve days, it was obvious that some very decided treatment was called for. During the whole of that time she had taken nothing but water, and had slept very little, so that her flesh and strength were rapidly diminishing; and, on manipulation of the wrist, it felt like a bag of loose grating bones.

Though I feared that the tendons might have sloughed from vicinity to such intense inflammation, and that the case was likely to do little credit to the operation, I felt it my duty to remove the carpus with the neighbouring articular surfaces, which I did on the 2nd of November. The carpal bones were found almost entirely detached from one another, and all of them, as well as those of the forearm and the metacarpal bones of the fingers, were eroded by ulceration. The tissues beneath the extensor tendons were so disorganized as to break down readily under the finger; but, happily, the tendons themselves had not suffered seriously, as was proved by the event.

The second night after the operation she slept without an opiate, and on the following day took some beef-tea; and from this time forward she regained her strength and flesh as quickly as she had previously been losing them. The cavity produced by excision of the bones consolidated with great rapidity. After three weeks she could pick up a roll of bandage with the finger and thumb of the unsupported hand; and on the 28th of December (eight weeks from the operation) cicatrization was complete. The wrist was then already firmer than in any previous case at that period, so that the base of the hand could not be moved from side to side at all, though she could herself perform flexion and extension, pronation and supination, with increasing freedom. She could also use every joint of the fingers and thumb, which were growing more and more supple as they gradually lost the thickening of tissue which the acute inflammation

[1] For a further notice of this case see p. 199.

had induced. At the present time (March 1865), though she wears a leather support as a measure of precaution, she can write a fair hand without it, and also employs herself frequently with knitting or crochet-work.

In reading the above cases it will have been observed that the later ones show a superiority over the earlier, both in their rate of progress and in their results. This is due principally to successive improvements which experience has suggested in the mode of treatment.

In the earlier cases I made two longitudinal incisions, both on the dorsal aspect of the limb, one at the radial, the other at the ulnar side, sacrificing in the radial incision the extensor of the second joint of the thumb ; then divided the extensors of the carpus opposite the wrist-joint ; and having detached the tendons sufficiently from the radius and ulna, removed the articular ends of those bones by means of a small saw and cutting-forceps applied transversely. Next, after separating the tendons from the carpus, I sawed or clipped through the metacarpal bones of the fingers, so as to extract their extremities together with the greater part of the carpus in a single piece, dissecting out afterwards any articular portions that remained.

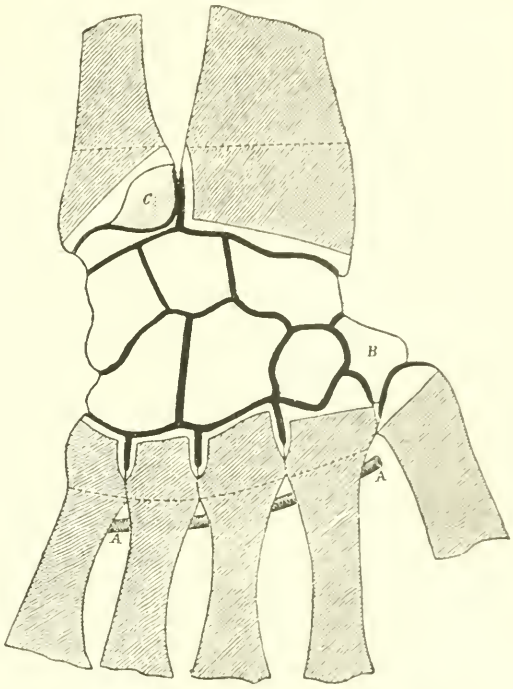

FIG. 3.—*A A*, deep palmar arch ; *B*, trapezium ; *C*, articular surface of the ulna, over which the radius moves.

This method proved far from perfect, both in the way in which the bones were dealt with and in the mode of gaining access to them.

As regards the bones, it was objectionable in two ways. In the first place, the bones being divided transversely so as to include all the cartilage-covered surfaces, a needlessly large amount was removed both from the radius and ulna and from the metacarpus. In a case of disease apparently limited to the carpus, the essential principle of the operation would be carried out by merely taking away what is represented by the unshaded parts of the accompanying diagram (Fig. 3), taken from a faithful sketch of the bones of the right wrist, the thick lines indicating the extent of the articular surfaces. But the original method sacrificed in all cases at least as much bone as is included between the dotted

lines of the same diagram, and it will be readily understood that the greater loss both of length and breadth of the bones must have interfered seriously with the process of consolidation, and impaired the ultimate strength of the hand. This point may be further illustrated by comparing the sketches in Figs. 1 and 2, of which the former exhibits the parts removed according to the first method in Case 2, and the latter the portions excised on an improved plan in Case 5, which was very similar as regards the extent of the disease.

But a greater objection to the original procedure lay in the fact that, the bones being divided in the dark, there was serious risk of leaving behind some portions of the disease; for it is, of course, impossible to know beforehand the precise extent of the caries, and when the bones have been confused by operative interference, it is somewhat difficult to judge accurately of their condition.

In order to attain completely the twofold object of taking away all the disease, and leaving behind all bone that may be relied upon as sound, it is desirable that both the radius and ulna and the metacarpal bones should be presented untouched for examination; and in my later cases this condition has been fulfilled in a thoroughly satisfactory manner by removing the carpus in the first instance, when the free space so afforded permitted me to deal methodically and surely with the other bones.

The soft parts, too, were by no means in the best possible condition after the original operation. In exposing the ends of the bones, and especially the radius, for the application of the saw or pliers, the tendons were separated from their sheaths to an extent which the preliminary removal of the carpus renders quite unnecessary. They consequently acquired a disposition to contract adhesions to neighbouring parts, which occasioned a great deal of needless trouble in the after-treatment.

Again, the division of the extensors of the carpus opposite to the wrist-joint gave less power of raising the hand than was afterwards obtained by cutting them long at their insertions into the metacarpus, and so imitating as nearly as possible the natural arrangement. This point seemed deserving of consideration when the progress of some of the cases had proved that those muscles will regain command over the hand. The idea was first acted on in Case 3, that of William C——, and the result in him and in all that have followed him has shown that it is well worthy of attention.

I also found that by properly planning the radial incision it was quite unnecessary to sacrifice the extensor secundi internodii pollicis, and in the more recent cases the second joint of the thumb has commonly been moved with perfect freedom, whereas in the earlier ones the first joint only was capable of any material motion.

Lastly, it appeared that to have both the incisions on the dorsal aspect of the limb was by no means the best arrangement ; for, while the radial incision must necessarily be at the back of the hand, that on the ulnar side is advantageously made towards the palm, where it gives the most ready access to the palmar surface of the carpus, and avoids injury to the tendons of the extensor carpi ulnaris and the extensor minimi digiti, while it affords a dependent opening for the escape of discharges from the cavity.

The foregoing discussion of the defects of my first mode of operating will, I trust, prevent other surgeons from going over the laborious ground of gradual improvement over which I have travelled, while it will enable the reader to appreciate the advantages of the method which I now venture to recommend.

The operation is performed in the following manner : Chloroform having been administered, a tourniquet is placed upon the limb to prevent oozing of blood, which would interfere with the careful scrutiny to which the bones must be subjected. Before the operation is commenced, any adhesions of the tendons are thoroughly broken down by freely moving all the articulations of the hand. The radial incision is then made in the situation indicated by the thick line (*L L*) in the accompanying diagram of the anatomy of the back of the hand (Fig. 4). This incision is planned so as to avoid the radial artery, and also the tendons of the extensor secundi internodii pollicis and indicator. It commences above at the middle of the dorsal aspect of the radius, on a level with the styloid process, this being as close to the angle where the tendons meet as it is safe to go. Thence it is at first directed towards the inner side of the metacarpo-phalangeal articulation of the thumb running parallel in this course to the extensor secundi internodii ; but on reaching the line of the radial border of the second metacarpal bone it is carried downwards longitudinally for half the length of the bone, the radial artery being thus avoided, as it lies somewhat further to the outer side of the limb. These directions will be found to serve, however much the parts may be obscured by inflammatory thickening. The soft parts at the radial side of the incision are next detached from the bones with the knife guided by the thumb-nail, so as to divide the tendon of the extensor carpi radialis longior at its insertion into the base of the second metacarpal bone, and raise it, along with that of the extensor carpi radialis brevior previously cut across, and the extensor secundi internodii, while the radial artery is thrust somewhat outwards. This prepares the way for the next step, which is the separation of the trapezium from the rest of the carpus, by means of cutting forceps applied in a line with the longitudinal part of the incision—a procedure which, as experience shows, does not endanger the radial artery. The removal of the trapezium is reserved till the rest of the carpus has been taken away,

when it can be dissected out without any considerable difficulty; whereas its intimate relations with the radial artery and its secure connexions with neighbouring parts would cause a great deal of trouble at an earlier stage of the operation. The soft parts on the ulnar side of the incision are now dissected up from the carpus as far as is convenient, the hand being bent back to relax the extensor tendons of the fingers. The separation of these is, however, best effected from the ulnar incision, which must be made very free. The knife is

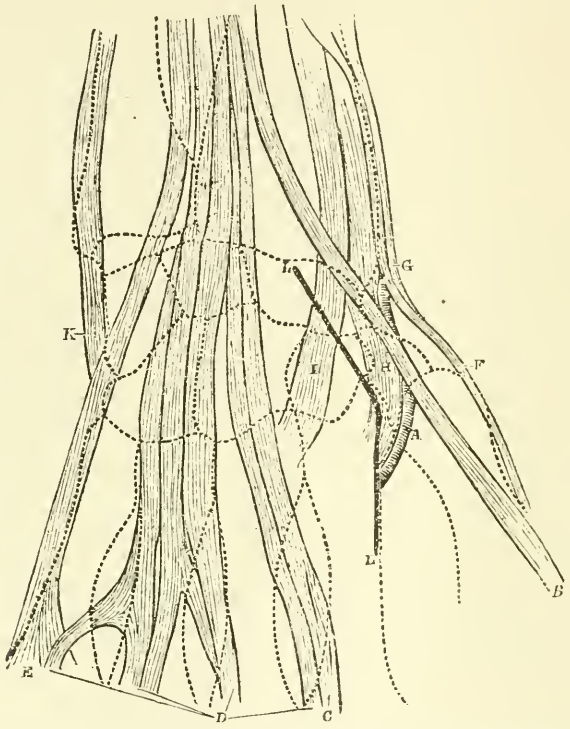

FIG. 4.—*A*, radial artery. *B*, tendon of extensor secundi internodii pollicis. *C*, indicator. *D*, extensor communis digitorum. *E*, extensor minimi digiti. *F*, extensor primi internodii pollicis. *G*, extensor ossis metacarpi pollicis. *H*, extensor carpi radialis longior. *I*, extensor carpi radialis brevior. *K*, extensor carpi ulnaris. *L L*, line of the radial incision.

entered at least two inches above the end of the ulna, immediately anterior to the bone, and is carried downwards between it and the flexor carpi ulnaris, and on in a straight line as far as to the middle of the fifth metacarpal bone at its palmar aspect. The dorsal lip of this incision is then raised, and the tendon of the extensor carpi ulnaris is cut at its insertion into the fifth metacarpal bone, and is dissected up from its groove in the ulna, care being taken to avoid isolating it from the integuments, which would endanger its vitality. The extensors of the fingers are then readily separated from the carpus, and the dorsal and internal lateral ligaments of the wrist-joint are divided; but

the connexions of the tendons with the radius are purposely left undisturbed. Attention is now directed to the palmar side of the incision. The anterior surface of the ulna is cleared by cutting towards the bone so as to avoid the artery and nerve ; the articulation of the pisiform bone is opened, if that has not been already done in making the incision, and the flexor tendons are separated from the carpus, the hand being depressed to relax them. While this is being done, the knife is arrested by the process of the unciform bone, which is clipped through at its base with pliers. Care is taken to avoid carrying the knife further down the hand than the bases of the metacarpal bones ; for this, besides inflicting unnecessary injury, would involve risk of cutting the deep palmar arch, the position of which is shown in Fig. 3. The anterior ligament of the wrist-joint is also divided, after which the junction between the carpus and metacarpus is severed with cutting-pliers, and the carpus is extracted by seizing it from the ulnar incision with a serviceable pair of sequestrum forceps, and touching with the knife any ligamentous connexions that may remain undivided. The hand being now forcibly everted, the articular ends of the radius and ulna will protrude at the ulnar incision, and are carefully examined and treated according to their condition. If they appear sound or very superficially affected, the articular surfaces only are removed. The ulna is divided obliquely with a small saw, so as to take away the cartilage-covered rounded part over which the radius sweeps, while the base of the styloid process is retained. The ulna is thus left of the same length as the radius, and this greatly promotes the symmetry and steadiness of the hand, the angular interval between the bones being soon filled up by fresh ossific deposit. The end of the radius is then cleared sufficiently to permit a thin slice to be sawn off parallel to the general direction of the inferior articular surface. For this purpose it is scarcely needful to disturb the tendons in their grooves on the back of the bone, the bevelled ungrooved part being enough to remove, and thus the extensor secundi internodii pollicis may never appear at all. This may seem a refinement ; but the freedom with which the thumb and fingers can be extended, even within a day or two of the operation, when this point is attended to, shows that it is important. The articular facet on the ulnar side of the bone is then clipped away with bone forceps applied longitudinally. If, on the other hand, the bones prove to be deeply carious, the pliers or gouge must be used with the greatest freedom ; for it is of course far better to take away too much bone than too little, and my earlier cases, as well as some more recent ones to which I have not yet alluded, prove that a useful hand will result in spite of very extensive excision. The metacarpal bones of the fingers are next dealt with on the same principle, each being in its turn closely investigated, the second and third being most readily

reached from the radial incision, the fourth and fifth from the ulnar side. If they seem sound, the articular surfaces only are clipped off, the little facets by which they articulate with one another being removed by the longitudinal application of the pliers, as is indicated in Fig. 3. On the other hand, we have had in Case 4 an illustration of what may be required when the disease proves extensive; for it may be remembered that in that case it was necessary not merely to take away the whole base of the metacarpal bone of the middle finger, but to drill its entire shaft into a hollow tube, and yet a sound and most useful hand was retained.

The trapezium is next seized with a strong efficient pair of forceps, and dissected out so as to avoid cutting the tendon of the flexor carpi radialis, which is firmly bound into the groove on its palmar aspect, the knife being also kept close to the bone elsewhere to preserve the radial artery. The thumb being then pushed up longitudinally by an assistant, the articular end of its metacarpal bone is cleared and removed. This may seem a superfluity, as this bone articulates with the trapezium by a separate joint. But besides the possibility of its being affected through its immediate vicinity to the other articulations, the symmetry of the hand is promoted by reducing it to the same level as the other metacarpal bones. Lastly, the articular surface of the pisiform bone is clipped off, the rest of the bone being left, if sound, as it gives insertion to the flexor carpi ulnaris, and affords attachment to the anterior annular ligament, and may serve other useful purposes in the palm. But if there is any suspicion of its unsoundness, it must be dissected out completely. The same applies to the process of the unciform. It may be observed that the extensors of the carpus are the only tendons divided; for the flexor carpi radialis is connected with the second metacarpal bone below its base, and so escapes. But if it should be cut, there is no doubt that, like the extensors, it would acquire new and secure attachments. The tourniquet being now removed, it will probably be found that either no vessel at all requires ligature, or merely one or two superficial branches. The radial incision is stitched closely throughout, and also the ends of the ulnar incision, as it is desirable that union should take place there, and more especially over the end of the ulna; but the middle of this incision must be kept open by pieces of lint introduced lightly into the wound to give support to the extensor tendons, and to ensure a wide opening into the cavity, which may serve for the free exit of the pus which must necessarily be formed there. The limb is placed upon a suitable splint, and dressed with some porous material, arranged so as to avoid pressure upon the lines of incision, in order that it may absorb without obstructing the discharge.

To the general reader the above description will, I fear, have proved weari-

some ; but to any one about to perform the operation, all the details will, I believe, be found well worthy of attention. The procedure consists, in fact, of a series of operations, each one of which must be executed with scrupulous care. But none of them will present any difficulty to a surgeon who has refreshed his knowledge of the anatomy of the parts, and carefully studied the various successive steps of the process. The operation is, however, necessarily tedious ; and no one ought to undertake it who is not prepared to bestow upon it a great deal of patient attention. But, considering the importance of its object, its tediousness must not be regarded as an objection, more especially as the surgeon alone feels the disadvantage. For the tourniquet prevents the loss of a drop of blood beyond what is in the veins of the hand at the outset, and the patient sleeps tranquilly under chloroform ; and if this is given by any intelligent assistant in accordance with the safe and simple principles which I first learned in the Edinburgh school—and have since done my best to diffuse,[1] but which, I regret to say, are still too little appreciated by the profession—it is a matter of entire indifference whether its administration is continued half an hour or an hour and a half. Under such circumstances, anything like hurry is as uncalled for as it would be fatal to success.

The after-treatment also requires much care, and has undergone great improvement through experience ; and, indeed, the superior results obtained in the more recent cases are due even more to this cause than to the better method of operating.

The principal objects to be kept in view are, to maintain flexibility of the fingers by frequently moving them, and at the same time to procure firmness of the wrist by keeping it securely fixed during the process of consolidation.

To the latter indication I paid scrupulous attention from the first ; and hence I have in no instance met with any approach to the flail-like condition of the new joint which otherwise would certainly have occurred. Indeed, my anxiety to avoid interference with the process of repair at the wrist led me at first to abstain from moving the knuckles, and to restrict the exercise of the fingers to their middle and distal joints. Consequently, in the earlier cases the movements of the knuckles are still very limited ; experience having shown that any one joint which is not freely and frequently moved is apt to become permanently rigid. Another circumstance which interfered at first with my obtaining the best results was a needless dread of suppuration of the opened sheaths of the tendons, which made me afraid to disturb them during the first week. But I was gratified to find, in case after case, that nothing of the sort

[1] For an investigation of these principles see an article on Anaesthetics, by the author, in Holmes's *System of Surgery*, vol. iii, third edition (reprinted at page 135 of vol. i).

ever occurred—a fact which must, I suppose, be attributed to the entire absence of tension in the soft parts, ensured by the free removal of the bones. Thus I have gradually grown more bold, and now do not scruple to ask the patient to demonstrate his command over the joints of the thumb and fingers on the first day, and make a point of commencing passive motion on the second day, whether the inflammation has subsided or not; and from this time forth it is continued daily till it ceases to be necessary. In executing these movements each finger is both flexed and extended to the full degree which is possible in health, care being taken that the metacarpal bone concerned is held quite steady, to avoid disturbing the wrist.

By proceeding in this way, even though the fingers have been previously stiff, it is easy to maintain the suppleness produced by the free movement under chloroform immediately before the operation as recommended above. For an adhesion only one day old yields without much force on the part of the surgeon or much pain to the patient, whereas a very few days will give it such firmness as will require great violence to rupture it.

The splint which I used originally was a flat one, on which the hand lay with the fingers extended. But I have found it a great improvement to have the hollow of the palm supported upon an obtuse-angled piece of thick cork (Fig. 5, *C*) attached to the splint, a convenient cement for the purpose being gutta-percha fused with a hot iron. The hand thus lies semiflexed, which is its natural position of repose; and has also the advantage that the fingers are midway between the extremes of flexion and extension into which it is necessary to bring them in the daily passive movements, while a certain range of voluntary motion is also permitted, which the patient should be encouraged to exercise frequently during the day. At the same time, this position is best adapted for allowing the extensors of the carpus to acquire fresh attachments. Lastly, the palm being applied to the sloping surface of the cork, the splint is kept from slipping upwards; while any movement in the opposite direction is prevented by giving the turns of bandage which encircle the wrist a purchase upon a transverse bar of cork (Fig. 5, *D*) attached to the under surface of the splint about the level of the knuckles. The great essential as regards the wrist—that of perfect steadiness—is thus effectually secured, the hand remaining fixed throughout the day, however freely the fingers be moved. While the patient is confined to bed the limb should rest upon a sloping piece of wood or desk, which is much more steady than a cushion, the inner condyle being well padded with cotton to avoid bed-sore.

The bar of cork beneath the splint has the further advantage of allowing the thumb to fall below the level of the rest of the hand into the position in

which it is most serviceable for opposition to the fingers. For if it be pushed upwards by resting on a board or cushion, it will be apt to retain permanently its unnatural attitude; its basis of support, removed in the operation, being reproduced in accordance with its altered circumstances. For the same reason, the piece of cork on which the hand rests should be well hollowed to receive the ball of the thumb. The thumb itself is apt to become drawn, in time, towards the index-finger, which would greatly impair its usefulness; but this is readily avoided by taking the precaution of keeping the thumb, from the first, completely extended by a substantial pad of cotton in the angle between it and the fore-finger, the pad being of course removed once every day for exercise of the joints.

Pronation and supination, also, must not be long neglected; and as the

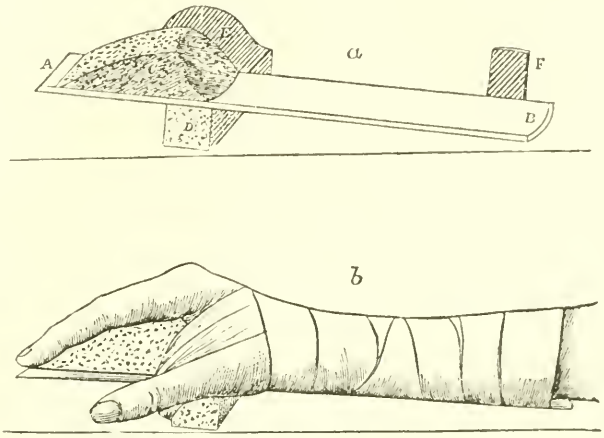

FIG. 5.—*a* represents the splint, of which *A B* is the wooden part; *C*, the piece of cork to support the hand; *D*, the transverse bar of cork beneath; *E*, the ledge of gutta-percha for the ulnar border of the hand to rest on; *F*, another ledge of gutta-percha. *b* shows the limb bandaged to the splint.

new wrist acquires firmness, flexion and extension, abduction and adduction, should be occasionally encouraged.

As to the length of the period during which passive motion may be required, the rule must be to continue it till the disposition to contract adhesions finally ceases, and this may be after a few weeks or not till months have elapsed.

When the patient leaves his bed and carries his arm in a sling, the weight of the hand will make it gradually droop to the ulnar side, unless it is properly supported. This is conveniently done by affixing two ledges of gutta-percha (Fig. 5, *E* and *F*) to the ulnar side of the splint—one for the border of the hand to rest on, and another, towards the upper part of the forearm, to keep the splint from shifting laterally

When the hand acquires strength enough to be useful, more free play for the fingers should be allowed by cutting off the part of the splint on which they

rested—viz. all of it beyond the knuckles, leaving only enough to support the hollow of the palm. The thumb must also be left free for use during the day, but at night it should still be kept extended with the pad before mentioned ; and if there be any defect in extension of the fingers, they may be kept bound at night to a piece of thick gutta-percha applied to the back of the limb, and bent upwards at an angle at the knuckles. The gentle and continuous traction of the elasticity of the gutta-percha will soon correct the fault.

With regard to the dressing, after the first twenty-four hours I have found a poultice the best application for a few days, and afterwards lint soaked in a solution of some stimulating agent, such as sulphite of potash, which I tried some time ago, on theoretical grounds, for the treatment of sores, and have found preferable to the ordinary astringents, diminishing the amount and fetor of the discharge, and producing a very healthy state of the granulations. It may be used in the proportion of ten grains to an ounce of water.

Even after the hand is healed some support will still be required for a considerable period, and this may be conveniently made of bend leather, accurately moulded to the anterior aspect of the limb, and reaching from about the middle of the forearm to the level of the knuckles, which it rises to support, its ulnar border being turned up for the side of the hand to rest on, while at the radial side it gets a purchase on the base of the metacarpal bone of the thumb. A few turns of bandage, or a laced piece of soft leather, above the wrist, will be sufficient to keep it securely in position, the apparatus scarcely showing at all at the back of the hand.

This support must be worn till the patient feels the wrist exactly as strong without it as with it. It is a most serious mistake to lay it aside too early. Case 1, in which after the lapse of a year and three-quarters the necessary condition for abandoning it was at last fulfilled, is a striking instance of the advantage derived from its persevering employment. Considering the very large amount of bone removed in that case, I confess I hardly hoped for such perfect firmness as was ultimately attained ; and I feel sure that if the rule I have given had not been followed, the result would have been very different. The use of the support, far from hampering the motions of the fingers, favours their usefulness. For it seems to be a principle in physiology that the nerves refuse to call the muscles into action unless they can do so with effect ; and so, when the wrist has not the firmness mechanically necessary for the efficient action of the fingers, their movements are feebly executed ; but when the wrist is firmly supported, the motor apparatus of the hand is, so to speak, encouraged to its best efforts, and recovery of the power of the limb is greatly promoted.

One or more sinuses may remain open for a long time, just as after excision

of the elbow-joint, without anything being wrong with the bones; as, for example, in the case of Margaret W—— (Case 2), in which they did not finally close for more than a year after the operation. Or, again, the persistence of sinuses may depend upon small exfoliations, and these may prove extremely slow in separating. This may be illustrated by

Case 7, that of James M'G——, sixteen years of age, whose right wrist I excised on the 21st of November, 1863, on account of disease of spontaneous origin, which had attained to much the same exaggerated degree as in Helen M—— (Case 4), and presented similar apparently hopeless appearances: the hand, greatly swollen and discoloured, and with numerous sinuses and a grey palmar sore, hanging helpless at an angle of about sixty degrees, with the fingers stiff and clawed. The radius and ulna were very freely resected, and it was necessary to apply the gouge to the third and fourth metacarpal bones on account of extension of the disease below their bases. All went on well after the operation, except that a sinus remained in each line of incision, and a probe introduced into that on the ulnar side passed down to bare bone of irregular surface. This made me fear a return of the disease; but, as the wrist was growing firm and the hand useful, I did not interfere, and after the lapse of ten months a small exfoliation escaped from the ulnar aperture. The probe, being then introduced, no longer came in contact with any bone; and now, three months later, the sinus has become reduced to an almost invisible aperture, which yields only a minute drop of limpid liquid, while the new joint at the wrist is all that can be desired both in firmness and flexibility.

Hence so long as swelling and discharge diminish, and the strength of the hand increases, no interference is called for. But should the opposite conditions present themselves, recurrence of the disease must be suspected, and the part must be submitted to a thorough exploration, which should not be too long delayed, since caries reappearing at a limited spot will spread in time to all the bones.

This course I have found it necessary to adopt in more than one instance, as, for example, in the following case, which was in some respects the worst I have had to deal with.

Case 8.—Mrs. C——, aged twenty-five, a married woman, came to the infirmary for the purpose of having her right hand amputated, on account of spontaneous disease of the carpus of two years' standing, attended for eighteen months with constant discharge, and for the last six months with such severe pain as to deprive her to a great extent of her night's rest, while the effect upon her general health was marked by her wasted and sallow aspect, impaired appetite, and rapid pulse.

Some idea of the appearance of the wrist may be gathered from the accompanying illustration (Fig. 6), taken from a photograph, and also from the fact that the wrist measured nine inches and three-quarters in circumference, whereas the sound one was slender both from natural conformation and from emaciation. The surface of the swollen part was studded wth eight sinuses, through which the probe could be passed down to diseased bone in the forearm, the carpus, and the metacarpus. The hand drooped when the arm was extended, and the fingers were entirely useless. On the 8th of June, 1864, I performed the operation, which proved very laborious on account of the condensation of the soft parts and the extraordinary enlargement of the radius from inflammatory hypertrophy, which made it impossible to protrude it as usual at the ulnar incision for the application of the saw, while its texture was so hard as to require considerable force with the most powerful cutting-pliers to divide it.

Fig. 6.

For seven weeks all went on perfectly well, so that at the end of that time the circumference of the wrist was diminished by an inch, and she could readily pick up a light object with the finger and thumb of the unsupported hand, which drooped but slightly below the horizontal level. She was also quite free from uneasiness, and her general health was greatly improved. Unfortunately, however, the sores, which were previously healing kindly, were now attacked by hospital gangrene ; and though this was checked in about five days by the application of nitric acid and other measures, the previously satisfactory progress was no longer observed ; and, when a month had elapsed without improvement, I resolved to investigate the cause. Having put her under the influence of chloroform, I opened up the line of the ulnar incision, and, finding the end of the ulna again carious, removed it with pliers ; but was pleased to find, on introducing my finger into the cavity that still existed, that the large cut surface of the radius was smoothly covered with granulations, as also were the ends of the metacarpal bones. I therefore brought the edges of the incision together by stitches, except a part sufficient for the escape of discharges, and placed the limb again upon the splint. From that time forth she has advanced satisfactorily ; and when she last came from her native town to see me (December 1864), the discharge had almost entirely ceased, the swelling was greatly reduced, and although the end of the radius was still very large, and the wrist measured eight inches and a quarter in circumference, the border of the bone was to be felt immediately beneath integument of normal thickness and consistence, and

the hand had a thoroughly natural appearance. She could extend it unsupported without any droop, and even raise it above the level of the forearm by muscular effort. She had discarded the sling, and, by help of a light splint, found the hand of much use to her, the movements of the thumb and fingers being very satisfactory.

In this case return of the disease appeared to be produced by an attack of hospital gangrene, and I am inclined to attribute to the same cause a similar occurrence in the following instance.

Case 9.—Alexander C——, aged eighteen, a calenderer, was affected with spontaneous caries of the ulnar side of the right carpus, for which I performed excision of the wrist on the 22nd of July, 1864. Four days afterwards the ulnar incision was affected with hospital gangrene, which was subdued in the course of a few days by local treatment. The hand then progressed very favourably up to a certain point, and in a few weeks he could freely flex and extend all the joints of his fingers, which before the operation he could scarcely move, and had perfect use of his thumb and a strong wrist. The ulnar incision, however, refused to heal completely, and the soft parts in the vicinity continued thickened, and towards the close of the year the swelling there appeared to be on the increase. Suspecting some return of the disease, I opened up the ulnar incision on the 2nd of December, but could not find anything amiss. The wound, however, continued remarkably languid, making in three months scarcely any progress in healing; and at length, on passing the probe down to the pisiform bone, I felt it bare and rough. Accordingly, on the 28th of February, 1865, I put the patient under chloroform, and, after extending the incision sufficiently to gain access to the pisiform, I dissected out the whole of that bone, which was the only diseased part that I could discover. During the month that has since elapsed, the healing, so long delayed, has made good progress, and as there is now no discharge from the deeper parts, I hope to see the hand before long soundly cicatrized.

There is yet a third case with which hospital gangrene appears to have had the same relations.

Case 10.—Jane S——, aged twenty-three, a millworker, with idiopathic caries of the right carpus, had the wrist excised on the 23rd of March, 1864. For two months and a half the appearance and power of the hand promised an excellent result, when it was seized with hospital gangrene, and from that time it gradually deteriorated, till at length, on the 26th of August, I explored it under chloroform, and found very extensive disease in both bones of the forearm and in the metacarpus. I removed with cutting-pliers the affected

parts, so far as I was able to judge of their extent ; but I omitted to examine the metacarpal bone of the thumb, and, whether in consequence of this or not I will not pretend to say, the operation did not prove successful ; and on the 29th of January, 1865, being loth to sacrifice the hand, I re-excised a second time, and found carious disease again present in the ends of all the bones, including the metacarpal bone of the thumb, which this time I made a point of examining, reopening the radial as well as the ulnar incision. The operation, however, was an extremely troublesome one, and far from being as definite and satisfactory as a primary excision ; and even now, although she can move the thumb and fingers, and it is evident that the hand would be a thoroughly useful one if it would heal, I feel by no means satisfied that it will do so.

The lesson to be learned from such a case is the necessity of using all means by which recurrence of the disease may be avoided, and not too long delaying exploration of the wrist when it is suspected.

It remains to say a few words regarding the mode in which the new joint at the wrist is produced. It appears that the bones of the forearm and of the metacarpus become approximated, partly by shortening of the limb and partly by the growth of new osseous material from their divided ends ; and I find that, as a general rule, about half an inch in length of new bone is formed, and that the rest is effected by shortening. The new bone appears to be generally developed in about equal proportions from the radius and ulna and from the metacarpus, but sometimes in an irregular manner. Thus in two instances I have observed a process grown from the ulnar side of the radius, and received between two lateral portions from the metacarpus, so that a secure joint on an entirely new principle was the result.

With proper care on the part of the surgeon, perfect symmetry of the hand can be always ensured ; for the radius and ulna above, and the metacarpus below, being divided in parallel lines, the shrinking of the new material between them draws the hand equably upwards towards the forearm. In this respect the operation above recommended has a great advantage over any partial procedure in case of disease apparently confined to one side of the carpus, independently of the paramount consideration of its greater security of eradicating the caries. And any scruple which I might once have felt in recommending total excision for limited disease has been entirely removed by the usefulness of the hands which have been subjected to it.

It will no doubt appear desirable that I should allude, however briefly, to the rest of the cases that have come under my care. They are five in number, and include some of the best and some of the worst. Among the former is to be mentioned, first, my last case, that of E. P——, aged thirteen, a strumous

girl, whose right wrist was excised on the 27th of November, 1864, for caries limited to the lower and outer part of the carpus and the base of the second metacarpal bone, which appeared to be affected with tubercular deposit. The hand had been useless for a year, but is now already useful as well as sound and of perfect shape, with better movements than in any former case at the same stage.

Next is James B——, aged twelve, with disease of the left wrist, limited to the lower and ulnar side of the carpus and the fifth metacarpal bone, which was enormously thickened and diseased in its interior throughout almost its entire length. Rest and constitutional treatment having been tried in vain for five months, I proceeded to operate on the 14th of August, 1864, and, instead of adopting what would, I believe, be the usual course (amputation of the little finger and its metacarpal bone, with the probability of the disease continuing in the carpus), I excised the wrist, leaving the little finger, though it was necessary to drill its metacarpal bone into a tube with the gouge. He has now a beautiful and most useful hand, which is constantly increasing in strength, though there still remains a sinus over the base of the fifth metacarpal bone, from which a small exfoliation recently escaped.

Third in order must be placed Andrew C——, aged nine, with disease of the left wrist of eight months' standing, and in so severe a form as to call for immediate treatment. The caries extended from the forearm to the metacarpus, and had produced great destruction in the carpus. The operation was performed on the 26th of November, 1864, and the result has been very satisfactory as regards the improvement in the general health, the strength of the wrist, and the mobility of the thumb and fingers. But there have remained hitherto (four months) two small sores unhealed, and these within the last few days have been affected with hospital gangrene. This has, however, been checked by the application of carbolic acid, and I hope has not penetrated to the bones.

Lastly, I have to record two deaths. Neither of these, however, was directly connected with the operation. One of the patients, Alex. S——, a stone-mason, aged forty-six, the first I operated on, was in truth not a fit subject for excision, except as a means of relieving him of the agonizing pain which he endured from disease of the right wrist; for he was affected with advanced phthisis as well as other complaints, and died of these seven weeks after the operation, which, however, had made him free during the interval from his previous suffering.

The other fatal event occurred also in one of my early cases. The patient Neil C——, aged twenty-one, had the right wrist excised on the 4th of July, 1863, for extensive strumous caries. But in consequence, as I suppose, of

the imperfection of my first mode of operating, the disease recurred, and, after waiting in the vain hope of improvement for about six months, I explored the bones and performed re-excision. A few weeks later—namely, on the 28th of February, 1864, a spot of redness appeared on the forearm, about midway between the wrist and elbow, and next day he had a severe rigor. I was at once called to see him, and, finding evidence of inflammation of a superficial vein leading upwards from the red spot to the elbow, I thought there might be a chance of arresting the pyaemia in its outset by amputating the arm, which I did about three inches below the shoulder, within an hour and a quarter of the occurrence of the first rigor. On examining the veins, I found a mixture of pus and blood in the vessels of the forearm, while those of the arm appeared quite sound. Sulphite of potash, in ten-grain doses, was now administered every three hours, with the view of counteracting the poisonous effect of any septic matter already introduced into the circulation, and this was continued till the 5th of March. After this time he improved for several days in general health, while the stump was progressing favourably; but just as I was congratulating myself on a cure of pyaemia, pulmonary symptoms appeared, and carried him off on the 23rd of March, twenty-three days after the amputation. Whether the affection of the lung was an aggravated condition of the phthisis under which he laboured, or an unusually remote effect of the pyaemia, I unfortunately had not the opportunity of ascertaining by post mortem examination.

In no instance have I been troubled with secondary haemorrhage or any other bad symptom immediately referable to the operation, which appears to be a peculiarly safe one.

A general review of the cases above related may be stated shortly as follows. I have excised the entire articular apparatus of the wrist in fifteen patients. Of these, two have died of causes independent of the operation, and of the remaining thirteen, one is in an unsatisfactory condition, but not hopeless, two afford good hope of a satisfactory termination, which in the remaining ten may be said to have been already arrived at.

On comparing these results with those of previous practice, bearing in mind that the cases include all varieties of carious disease, sometimes in the most aggravated form ever likely to be presented, and also that they have been treated under the disadvantages of hospital atmosphere, so that I have had to contend in no less than six instances with hospital gangrene and in one with pyaemia it will, I trust, appear that the principles which have guided me are sound, and afford the means of removing one of the greatest opprobria of modern surgery.

CLINICAL LECTURE ON A CASE OF EXCISION OF THE KNEE-JOINT, AND HORSEHAIR AS A DRAIN FOR WOUNDS, WITH REMARKS ON THE TEACHING OF CLINICAL SURGERY

Delivered at King's College Hospital, December 10, 1877.

[*Lancet*, 1878, vol. i, p. 5.]

GENTLEMEN.—I bring this little girl before you to-day because it is important that you should not only see the patients when they first come under our care in the hospital, not merely have the diagnostic features of their diseases pointed out to you, hear the appropriate treatment discussed, and witness any operations that may be performed, but also follow the after-progress of the cases, and further, because by bringing her into the theatre I can show you what I wish you to notice regarding her very much better than by taking you to her bed in the ward.

Let me remind you of the essential features of the case. As she was brought before you ten days ago, the left knee was bent considerably beyond a right angle, the leg being in fact at an angle of about 45° with the thigh, and we were given to understand that this condition of things had existed from the age of three years, when she was affected with a disease of the knee-joint up till the time of her admission to the hospital at the age of ten. The scar of a sinus was present at one side, but it had long since healed. The limb in that position was of course worse than useless. I also pointed out that it was atrophied; or, to speak more correctly, had lagged behind the other in growth; so that the fibula was $1\frac{1}{2}$ inches shorter than the other, and there was a difference of eleven-sixteenths of an inch between the two feet as measured from the point of the calcaneum to the end of the great toe.

I may remark that this atrophy, or lagging behind in development, seems to be interesting as explaining, in part at least, the corresponding fact after excision of the knee. If that operation is performed in early childhood, it is often observed that as the patient grows to adult life the affected limb is more or less considerably smaller than the sound one. This has been supposed to be due to taking away too much of the ends of the bones so as to deprive them of their epiphyses, but a case like the present points to another explanation.

Here no portions of bone at all had been taken away, no active disease had been present for several years, and the only abnormal circumstance was that the limb had been in a condition incapable of being used like the other. In consequence of this want of use, not only had the muscles atrophied, a thing which you would all have anticipated, as the converse of the hypertrophy that occurs in the blacksmith's arm, but all the textures, including the bones, had grown in a less degree than in the healthy limb. Similarly, after excision, although the operation be successful, and perfect anchylosis between the femur and tibia be attained, the limb is not so vigorous as the other, and in proportion to its diminished activity may its growth be interfered with. I lately saw a case in private practice which illustrates this point still more strikingly. The patient was a boy who had experienced fracture of both bones of the leg in the lower third when a child. The fracture had been overlooked, and the bones had united in a faulty position, so that the foot was considerably inverted. The boy therefore could only walk upon the outer edge of his foot, and that with a very limping gait, except by the aid of an apparatus which, though it enabled him to tread fairly on the sole of his foot, was in itself necessarily cumbrous ; and the result had been a shortening of the limb, as compared with the other, altogether out of proportion to the effect of the curved position in which the bones had united ; and, just as in the case before you, the foot also was smaller than its fellow. There the interference with full development induced by imperfect action of the limb was still more plainly illustrated than in this little girl, because in the former there had been no disease at all from first to last, but merely the crippling influence of an injury.

To return to the case of the little girl. We had to deal with a limb which was not only useless from its bent position, but which had been so retarded in its growth that, even if perfectly extended, it must be shorter than the other. Hence it was a matter of the utmost importance that the means which should be used to produce extension should add as little as possible to the existing deficiency in length. The joint was not anchylosed, but the hamstrings became extremely tight on any attempt at extension. We therefore proposed to divide the hamstrings by subcutaneous tenotomy, but I led you to fear that this step might not be sufficient to enable us to restore the straight position ; for I mentioned to you the fact first brought prominently forward by Prof. Volkmann, of Halle,[1] that in cases like this, in which the knee remains for a long time in a bent position, the lower end of the femur, no longer supported as usual by the articular surface of the tibia, may experience disproportionate growth in the downward direction, often to a very considerable extent. Meanwhile the

[1] See a translation of Prof. Volkmann's paper in the *Edinburgh Medical Journal*, vol. xx, p. 794.

lateral ligaments remaining of normal shortness, while the articular portion of the femur is abnormally lengthened, the tibia becomes locked against the femur when extension is attempted, and the application of violence for the purpose could only lead to backward dislocation. Accordingly we found that after free division of all the hamstrings, together with all tight bands of popliteal fascia, the tibia did become locked in the way I had anticipated, when we tried to straighten the limb.

The abnormal length of the end of the femur being presumably the essential obstacle to extension, I proceeded to reduce it, opening into the joint with a semilunar incision anteriorly without dividing the lateral ligaments, and paring away successive portions of the articular part of the femur until, some superfluous fibrous tissue of new formation having been also removed from the surface of the tibia, I was at length able to effect complete extension, but not without a degree of pressure of one osseous surface against the other which I should not have felt justifiable without antiseptic means.

The manner in which drainage was provided is a point worthy of your attention. Next to the importance of the avoidance of putrefaction in wounds is the prevention of tension by providing a free escape for effused blood and serum. This we have hitherto generally done by means of the caoutchouc drainage-tube of Chassaignac. But in the present case such a tube would have been unsuitable, because the natural position for the drain was that it should run between the ends of the bones which, as we have seen, were pressed together so that the calibre of a caoutchouc tube would have been altogether obliterated, and the drain in a most important part of its course rendered useless. Under these circumstances I used a drain of horsehair, because such a drain operates by capillary attraction through the interstices between the hairs, and those interstices cannot be obliterated by pressure, seeing that the hairs are not individually compressible.

The drain was introduced in a manner which you will often find useful. It may frequently happen that the most dependent part of a wound may have no opening in the skin to correspond with it: thus after excision of the mamma it may turn out, when the operation is concluded, that the wound presents a pocket extending considerably further back than the outer angle of your incision. Under such circumstances it is desirable to make an opening for the exit of the drain at the most dependent part. Now, if this were done by a puncture with the knife, some arterial branch of considerable size might be wounded, involving the necessity of freely enlarging the wound to secure the bleeding-point. But if you take a pair of dressing-forceps, and bore steadily from within outwards, the conical extremity of the instrument will slip past any arterial

branch or nervous trunk without injuring it, and when at length it is apparent that there is nothing but skin between the instrument and the surface, the tough integument is divided with a knife over the point of the forceps, and the blades being forcibly expanded so as to enlarge somewhat by laceration the opening which has been made in the muscles, or other deeper textures, the drain is seized between the blades of the forceps, and drawn into place. So in the present case the most eligible position for a dependent opening was at the outer aspect of the limb, where the use of a knife would have involved the risk of injuring the external popliteal nerve, or of dividing some articular arterial branch. Any such difficulty was avoided by employing the dressing-forceps in the manner described.

It is only right that I should mention, when alluding to the horsehair drain, that its use did not originate with myself. We were led to its adoption in the following manner. Mr. Chiene, of Edinburgh, suggested some time ago the employment of catgut as a substitute for the caoutchouc tube. He hoped by this means to provide adequate drainage through capillary attraction, and at the same time, by virtue of the proneness of the catgut to absorption, to do away with the necessity for the withdrawal of the drain from time to time, which there is when the caoutchouc tube is used, whether for the purpose of shortening the tube or substituting a small one for a large. Mr. Chiene's anticipations were to a considerable extent realized. In all cases in which the wound remained aseptic the absorption of the deeper part of the catgut drain, and consequent falling off of the part outside the wound, might be reckoned on as a matter of course ; and in several cases in which the catgut was so used, both by Mr. Chiene and afterwards by myself, the drainage proved adequate and satisfactory. Mr. White, of the Nottingham General Infirmary, afterwards substituted horsehair for catgut ; not because it was supposed to be superior, but because, whereas the prepared catgut is a somewhat expensive article, a horse's tail is a very cheap one. A notice of this use of horsehair was published by Mr. White's house surgeon, Dr. L. W. Marshall, in the *Lancet* of the 2nd of December, 1876 ; and in the following month it was employed by myself in the Edinburgh Royal Infirmary, in a case of chronic bursitis of the sheaths of the flexor tendons at the wrist, in which it seemed likely to be peculiarly serviceable. In this affection the bursa is distended both above the wrist and in the palm, the cavities thus constituted being connected by a constricted passage under the annular ligament ; and it is desirable that both the expanded parts should be opened to give exit to the fibrinous concretions which are generally present (varying in size from that of a millet-seed to that of a small bean), and, further, that drainage should be provided for effused serum, the operation being

performed antiseptically, in order to avoid the very serious inflammatory disturbance and suppuration which are otherwise apt to occur. I had previously used the caoutchouc tube as a drain in such a case, but I found a difficulty from the liability of the tube to be compressed by the tendons. This might, I thought, be overcome by the use of the horsehair drain, which at the same time would, for this particular purpose, be superior to one of catgut, because the catgut would probably be absorbed before the necessity for drainage would be over. Accordingly I cut down above the wrist, making my way between the tendons of the flexor sublimis to the distended sheath of the flexor profundis, and, as soon as this was opened, passed in a large bullet-probe, somewhat curved, slipped it along under the annular ligament, and pressed it forcibly towards the palm, so as to perforate the palmar fascia while avoiding injury to the palmar arch, and, having divided the skin over the point of the probe, dilated the opening in the fascia with dressing-forceps, and then passed into the eye of the probe a substantial drain of horsehair, which had been well purified by steeping in a 1 to 20 solution of carbolic acid, and withdrew the probe, leaving the horsehair drain in its track. The drain answered admirably, and presented the further great advantage that it could be reduced in bulk in accordance with the diminution of the serous discharge, by drawing out as many hairs as might be desired; and in the course of three weeks, the last portions of the drain having been withdrawn, the wound healed without the occurrence of suppuration from first to last.

While the horsehair has the advantage over the catgut that it can be used when necessary over a longer period, it has, in some cases, the converse superiority that it can be not only reduced in bulk, but withdrawn altogether at an earlier period than is required for the absorption of the catgut; for the catgut, in process of organization and absorption, becomes more or less incorporated with surrounding tissues through the medium of the cells of new formation which invade it, and, if an attempt is made to withdraw the drain in whole or in part, there will often occur inconvenient oozing of blood through the rupture of newly formed vessels. And if, on the other hand, the drain is left intact till the parts of the catgut within the wound are entirely absorbed, there remains a small granulating sore at the place of exit of the drain, which may retard for some days the complete healing of the wound. Further, the threads of the catgut, as they undergo organization, are increased in bulk by the formation of the new cells, and their interstices are liable to be more or less choked, so as to interfere with effective drainage. The horsehairs, on the other hand, lie unchanged among the tissues, and their interstices remain to the last as effective as they were at the outset.

The next case in which I used the horsehair drain was one which you your-

selves witnessed—viz. that of transverse fracture of the patella, treated by laying open the joint, drilling the fragments obliquely, and tying them together by means of strong silver wire. Being apprehensive that blood and serum might be effused into the joint to such a degree as to produce inconvenient tension unless a free exit was provided, I resolved to introduce a drain at a dependent part of the articular cavity ; but I feared that, if a caoutchouc tube was used, it might be rendered inefficient by being compressed between the condyle of the femur and the neighbouring tissues. I therefore had recourse to the horsehair, introducing into the posterior and outer part of the joint a drain, about a quarter of an inch in thickness, by means of the dressing-forceps employed as before described. It worked to admiration ; for though there was, indeed, in the first twenty-four hours, a very copious sanguineo-serous effusion, as shown by the soaking of the antiseptic gauze, yet not the slightest swelling of the joint occurred, and, after nine days, the small remains of the drain, which had been previously reduced at successive periods, were withdrawn, to allow the puncture to close. The drain of horsehair was as pure and white [1] as if it had been merely dipped in water ; having been washed quite clean of the blood which first occupied its interstices by the colourless serum which, after the cessation of the original sanguineous effusion, had been the only discharge. I was so much impressed with the satisfactory working of the horsehair drain in that case that we have since employed it in preference to the caoutchouc tube in all our wounds, and have had good reason to be pleased with the change. (If it be necessary to reintroduce a horsehair drain, it is readily done by taking a wisp of hair of half the thickness required, bending it in the middle at a sharp angle over a probe, and tying a piece of carbolized silk round it close to the probe, on withdrawal of which the drain is left with a rounded end which passes readily into the interior of the wound.)

In the case of this little girl the horsehair drain has worked perfectly well in spite of the pressure to which it was subjected. The flow of blood and serum was, in the first twenty-four hours, extremely free, but there was no appearance of the retention of any of it within the wound. On the occasion of the last dressing, two days ago, more than half of the drain was removed. That dressing took place after an interval of three days, and it would be superfluous to change the dressing to-day, were it not that we may, perhaps, be justified, by the further diminution of the discharge, in withdrawing the remainder of the drain entirely so as to permit its track to close.

[1] I used white horsehair in this case simply because I did not happen to have at hand any of the black, which is generally preferable, because the individual hairs are thicker, while the dark colour has the advantage of making them more conspicuous, especially when they are used for sutures.

I will now expose the limb before you. We take care that this is done under a full cloud of spray. We removed at the last dressings both the stitches of relaxation in the shape of thick wire sutures taking a substantial hold, and the stitches of coaptation, of horsehair, including only the margins of the wound. You observe that cicatrization is almost complete, while there is not the appearance of a particle of pus. The skin is still, as it has been all along, free from inflammatory blush or puffiness. The child has suffered no more uneasiness than would have been anticipated had forcible extension been practised in a much less severe case,without the infliction of an external wound, and her constitutional diturbance has been equally trivial. The position of the limb is even better than at the conclusion of the operation, thanks to the effect of the elasticity of a substantial mass of cotton-wool bound down over the knee outside the antiseptic dressing, while we have the satisfaction of reflecting that the bones of the limb have been shortened only by the extent of the abnormal downward growth of the femur; and I think those of you who have had experience in surgery will allow that it would have been unjustifiable to have aimed at such a result without the use of antiseptic measures. If a joint is excised without such means, all prudent surgeons would agree that enough of the bones ought to be removed to ensure absence of tension.

On raising the limb, I find that the gauze dressing presents evidence of discharge, which, though of the nature of colourless serum, is still in sufficient quantity to make it prudent to retain the drain. We may, however, remove half of what yet remains, and you observe that I do this by withdrawing successive hairs without causing the least uneasiness to the child.

Allow me to direct your attention to the splint on which the limb is placed. It is a piece of Gooch's splint, a material introduced into surgery by Mr. Gooch, formerly a surgeon at Norwich, and exceedingly convenient for purposes like the present. It is made slightly longer than the limb, and as broad as the semi-circumference of the thigh, cut obliquely at its upper end to correspond to the line from the perineum to the great trochanter, and at its lower end it is excavated into a horseshoe to receive the point of the heel. Its flexibility in the transverse direction permits it to form a trough which is well padded with a substantial folded sheet made thicker opposite the tendo Achillis, and when it is bandaged to the limb, the horns of the horseshoe, together with the padding, form a satisfactory support to the sides of the ankle. The foot is kept slightly above the level of the groin, and a piece of thin macintosh cloth over the part of the padding towards the nates sheds the discharge and prevents it from soiling the padding, while the exact quantity of effused serum can be correctly estimated. In the course of a short time, when the discharge becomes trifling

or *nil*, a bandage steeped in waterglass (a mixture of the silicates of soda and potash) will be wound round the limb as it lies in the splint, so as to ensure absolute immobility.

Now, gentlemen, these various matters have been much more easily demonstrated to you here than they could have been in the ward. I was much struck with the difference between the theatre and the ward in this respect when showing in the ward to some strangers, after our lecture this day fortnight, the case of large granulating sore which I have brought before you here on several occasions. Our class is not a large one, numbering only fifty, and I suppose not half that number accompanied me to the ward. Yet in order to show the ulcer, it was necessary that those gentlemen should arrange themselves in two rows, so as to form an alley to admit the light from the window, and even then they stood in one another's way, and only those who were very near the bed could see what would have been shown without any difficulty to the whole class at once in this place. In connexion with that case I may make some further remarks regarding the mode of teaching which we employ.

Let me remind you of the various important matters which that ulcer has afforded the opportunity of demonstrating. First, you recollect how putrid the sore was at the outset, and how we succeeded in purifying it once for all by applying to the epidermis soaked with putrid discharge a strong watery solution (1 to 20) of carbolic acid, which has a special power of penetrating the epidermis, and to the granulations a solution of chloride of zinc (40 grains to an ounce), which experience has shown to have an energetic antiseptic effect upon foul granulations. That we did really purify the sore by this application was proved to you by the fact that, being afterwards dressed with lint containing boracic acid, which is the mildest of our antiseptics, with a piece of prepared oiled silk interposed between it and the granulations, to protect them from the antiseptic, mild as it was, and to ensure constant moisture of the surface, yet when dressed after an interval of a week, the oiled silk, instead of being putrid as it would have been in twenty-four hours under a piece of ordinary lint, had no odour except that of oiled silk itself. The pus had remained free from putrefaction for that long period, though not directly acted on by an antiseptic at all.

You have also had demonstrated to you on that sore some very important truths regarding the properties of granulations. You saw me clip away with scissors a portion of the surface without occasioning the slightest pain to the patient, proving that the granulations constituted a protective layer destitute of sensibility.

Again, we made an accurate pattern of the ulcer in gutta-percha tissue,

and on comparing it with the sore a week later we found that the pattern was already considerably larger than the granulating surface together with the cicatrizing margin already forming round it. Thus you had ocular evidence of the truth that granulations have a tendency to shrink, this being one of the means by which sores are diminished in extent in the healing process.

You also observed how, when the ulcer was protected, as far as was in our power, from irritation, by excluding both putrefaction and the direct action of the antiseptic, the formation of the epidermic pellicle at the edge proceeded with a rapidity never seen under water dressing.

Lastly, how instructive was the result obtained by skin-grafting. You saw that whereas before this operation was performed cicatrization took place only at the edge of the sore, a thin superficial layer of integument, involving little more than epidermis, having been removed with a sharp knife from the inner side of the arm, and the shaving having been cut up on the thumb-nail into small bits, which were placed in succession, with the raw surface downwards, on the granulations, the grafts so planted became each one a centre of epidermic growth on the sore. Thus was illustrated the general fact in pathology, that new structures formed in the repair of injuries are composed only of tissues similar to those in the immediate vicinity, and the equally fundamental fact in physiology, that severance of a part from connexion with the body is not followed by immediate loss of its vitality.

You remember also how, having sprinkled the granulating surface with a sufficient number of grafts, we placed upon the sore the remaining portion of the shaving, about as large as a fourpenny-piece, and this, as you afterwards saw, took root and adhered by its entire under-surface, thus teaching us two great truths. First, it showed that the surface of granulations, if thoroughly healthy, may unite not merely with granulations, but with a freshly cut surface, combining, so to speak, union by second intention with union by first intention. And, in the second place, it afforded of itself conclusive evidence of a most important pathological fact not yet universally recognised, that granulations have no inherent tendency to form pus ; for, before sufficient time had elapsed to cause the death of the portion of integument as the result of its severance from vascular connexion with the rest of the body, all pus-formation from the granulations on which it was placed must have ceased ; and not pus-formation only, but serous oozing also, which would have been equally incompatible with union of the two surfaces. No sooner did this piece of living dressing, perfectly unstimulating, chemically or mechanically, protect the granulations, than pus-formation and exudation of liquor sanguinis were alike suspended.

These, you may say, are very simple matters. Some of them, at least,

you might all have done for yourselves. Any one of you might, as a dresser, clip away a piece of granulations and see that the proceeding was painless, or any of you might equally easily make a pattern of a granulating sore and prove to himself its shrinking tendency. You might perhaps have opportunities for performing skin-grafting ; and might, for aught I know, draw for yourselves the inferences to be deduced from it.

But, on the other hand, you might very likely fail to do some or all of these things even in the entire course of your studentship ; and if you do not learn these matters when students, you may perhaps never learn them at all. Some of you may become in course of time ' pure physicians ', and in that case you will have no opportunity of studying the healing of sores ; and yet it is a subject which concerns the physician as well as the surgeon. If the intestines become ulcerated in typhoid fever, the sores must heal by granulation and cicatrization in a manner precisely similar to that which occurs in an ulcer of the leg. But the physician has no opportunity of witnessing this healing process during life ; and when he sees its effects on post mortem examination, they are probably marred by the results of decomposition. And so with a multitude of other things, which it is easy for me to prove to you by demonstration here, but which the physician can only learn by inference. For medical diseases differ from surgical diseases not so much in their nature as in their situation ; and the same great principles of pathology, and to a large extent of practice also, must guide alike the physician and the surgeon.

Now, these great principles may often be illustrated by extremely simple facts, such as those which you have witnessed in that ulcer. But such simple and rudimentary, or, so to speak, homely, truths are not only much more easily demonstrated in the theatre than in the ward, but would very likely never be taught in the ward at all. In ward visits the surgeon passes from bed to bed, and points out the most striking features of interest in the various cases ; but matters of everyday experience, though concerned with the most fundamental principles of our art, are not likely to receive attention except from some one who is appointed to discharge the duty of impressing upon his class by way of demonstration, not only points of unusual interest, but the most commonplace facts, which, though less attractive, are, in truth, more important to the student.

Thus our clinical course resembles in so far a systematic one that it is our duty, as the material at our disposal permits, to illustrate all departments of general surgery *ab initio* every session. And meeting you so frequently as I do —twice a week—with an attendance on your part as regular as is given to a systematic course, I am encouraged to keep my eyes open throughout the session for the materials requisite for such illustrations.

But though sound general principles are the most important things that we can discuss together, they are, of course, far from being all that we consider. Every case of special interest is brought before you, its diagnosis is carefully considered, and the method of treatment to be adopted is discussed in all its details; and then, if an operation has to be performed, whether, as is often the case, in the course of the lecture, or at some other time, you are prepared to profit by watching its performance, having all the steps of the procedure clearly in your minds beforehand.

I may take this opportunity of expressing my sincere regret that certain expressions which I employed before I left Edinburgh should have seemed capable of interpretation as casting the remotest possible slur on the surgeons of this metropolis. Nothing certainly was further from my intention. I did, indeed, while speaking under circumstances peculiarly difficult and embarrassing, allow an expression to escape my lips which I should not have uttered under any circumstances had I supposed that my remarks were likely to be published; and I am truly sorry for the needless offence which I have thus given. For the leading surgeons of London no one, I venture to say, entertains higher respects than myself. I referred not to the London teachers, but to the system on which clinical surgical lectures were given in London; which, so far as my knowledge extended, seemed to me essentially inferior to that in use in Edinburgh; partly because they were not demonstrative, and partly because, being given at rarer intervals and in conjunction with one or more colleagues, they could not, from the nature of things, approach to the characters of a complete course.

Not that I wish to underrate such clinical lectures in London as I refer to. In proportion to the ability and experience of the lecturer such discourses have their high value. But referring, as they do, to cases which are not present before the student, and which many of the audience may perhaps never have seen at all, they might often, except for the effects of voice and manner, be as well read as attended. Such lectures are in reality far more ambitious and involve greater talent and literary effort than ours, which are comparatively humble performances, standing much in the same relation to a course of systematic surgery as anatomical demonstrations to lectures on anatomy. But, simple as they are, they fill a place in the medical curriculum which, I believe, is second in importance to no other, and which cannot be filled adequately either by clinical lectures otherwise conducted, or by bedside teaching or tutorial instruction.

My own conviction of the importance of the subject is, at least, sufficiently shown by the fact that upon the question whether or not arrangements

could be made to enable me to conduct my course here exactly in the same manner that, following the example of Mr. Syme, I had found so advantageous in Edinburgh, depended my acceptance or otherwise of the highly honourable offer of a clinical chair in King's College.

[In publishing this lecture I wish to add two remarks in order to avoid misunderstanding. First, that I do not omit bedside instruction, and always warn my class that no lectures can possibly take the place of their own individual work at the bedside, since it is essential, in order that the student may become a competent practitioner, that he should handle diseases as well as see them, and not only witness their treatment by others, but be personally concerned in their management by holding dresserships, &c., in our hospitals. Secondly, I desire to add that, since I used the expressions in Edinburgh above referred to, I have been informed that clinical surgical teaching in London has undergone considerable changes since I was a student, both as regards giving it a more demonstrative character, and in greater frequency and regularity of meetings of the classes. The London schools are both numerous and independent, and the changes to which I allude have, I understand, taken place in different degrees in different institutions. Hence, I can quite understand that my general remarks, made, as I would repeat again, without any view to publication, may have done individual injustice, for which no one could be more sorry than myself.]

AN ADDRESS ON THE TREATMENT OF FRACTURE OF THE PATELLA

Delivered at the First Meeting of the Session (1883) of the Medical Society of London.

[*British Medical Journal*, 1883, vol. ii, p. 855.]

Sir Joseph Fayrer, and Gentlemen.—Some time ago, Mr. Holmes remarked to me that it would be well for me to place before the profession statistics of the operations which I had performed for fracture of the patella. And when you, sir, did me the honour to request that I should open this session of the Medical Society with a paper, it occurred to me that I could hardly do better than act on Mr. Holmes's suggestion. But, before entering on the strict subject of the communication which I have the honour to bring before you, it will be advisable to make some prefatory remarks regarding the circumstances that led me to it. In March 1873, my friend Dr. Hector Cameron, of Glasgow, recommended to my care, in the Edinburgh Infirmary, a case of ununited fracture of the olecranon. Dr. Cameron had formerly been my house surgeon in the Glasgow Royal Infirmary, and I had afterwards for several years the great advantage of his assistance in private practice; and he reminds me that I had often expressed to him the opinion that the use of a metallic suture, antiseptically applied, which we had employed in ununited fracture of the shafts of the long bones, ought, in suitable cases, to be extended to the olecranon and patella. The patient to whom I refer presented himself to Dr. Cameron in the out-patient department of the infirmary; and, as he had not at that time beds in the institution, and therefore could not operate himself, he sent him to me. He was a man thirty-four years of age, who, five months previously, had received a blow from a policeman's baton on the left elbow. This occasioned great swelling, which seems to have concealed the true nature of the case from the medical man whom he first consulted. On admission, there was a considerable interval between the olecranon and the shaft of the bone; and, although the limb was muscular, it was comparatively helpless, as he could not extend the forearm at all without the aid of the other hand. On the 28th of the month I made a longitudinal incision, exposing the site of the fracture, and, at the same time, bringing into view the articular surface of the humerus; and, having pared away the fibrous material from

the fractured surfaces, I proceeded to drill the fragments, with a view to the application of the suture. The fracture was oblique from before backwards, as indicated by this diagram. I found no difficulty, with the proximal fragment, in making the drill appear upon the fractured surface at a little distance from the cartilage (see *b*, Fig. 1), but with the other fragment the obliquity of the position in which the drill had to be placed was so great that, instead of the end of the drill emerging at the fractured surface, as I had intended, I found it had entered into the substance of the humerus (*d*, Fig. 1). I therefore withdrew the drill, and substituted for it a needle (*c d*), passing the eyed end in first. Then, with a gouge, I excavated an opening (*e*) upon the fractured

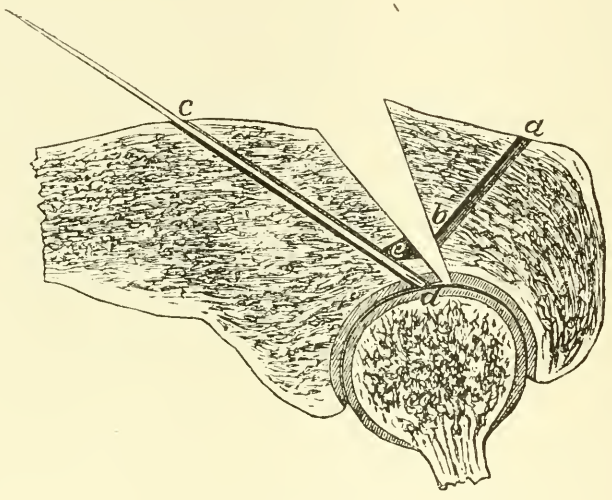

Fig. 1.

surface, opposite to the drill-hole (*b*) on the other surface, until the needle was exposed. Withdrawing the needle, I introduced a silver wire in its place, and I had no difficulty, by means of forceps passed into the excavation made by the gouge, in drawing out the wire. I was then able to pass it on through the other drilled opening, and thus the two fragments were brought into apposition. The ends of the wire were twisted together and left projecting at the wound. Healing took place without suppuration or fever, and the wire was removed on the 19th of May, seven weeks after the operation. The wound made for its extraction soon healed, and the patient returned to Glasgow; and I afterwards had the satisfaction of learning that he was wielding the hammer in an iron shipbuilding yard with his former energy.

I have had two other cases of ununited fracture of the olecranon; and, as these are closely allied to the subject of my paper, I may refer briefly to them. One was a man forty-five years of age, incapacitated for his occupation

as a plasterer by inability to extend the right elbow completely. The ununited fracture of nine weeks' standing was oblique laterally. It was treated as in the last case; the operation presenting no difficulty. It was performed on the 20th of March, 1878. The wound healed without suppuration, but the wire was not completely removed; for, the loop having broken near the twist, the twisted part was alone taken away, and the loop left behind. It never caused any inconvenience, and he afterwards wrote to us from his home at Bristol that he was able to follow his old employment.

The third case was that of a gentleman thirty-three years old, who had consulted no fewer than eighteen surgeons on account of the weakness of his left arm, caused by ununited fracture of the olecranon. I operated on the 28th of July, 1881, paring the broken surfaces as in the other cases, using a chisel and hammer for the purpose; and, having drilled the fragments with a common bradawl, I brought them together with moderately stout silver wire. In this case, however, I did not leave the ends of the wire projecting from the wound; but, having given them one complete twist (or two half twists), cut the ends off short, and hammered the twisted part down flat upon the bone with this small hammer.

The advantage of this practice was strikingly exemplified by the difference in the course of this case from its predecessors. Instead of keeping him under treatment for several weeks until the wire could be removed, I was enabled to allow him to return, fifteen days after the operation, to his home in Wales, with a sound cicatrix; and, trusting to the connecting loop of wire, permitted him to use the elbow freely. I afterwards learned that he was able to drive a four-in-hand as well as ever.

The practice of cutting the ends of the wire short, and hammering down the twist upon the bone, is one to which I shall have to refer again in connexion with my later cases of fracture of the patella. It is in every respect an advantage. The hammering down of the twist renders it more secure than if it is left projecting, to be moved by every shifting of the dressing, and perhaps broken, as in the second of the cases just referred to. We also get rid of a source of disturbance, and sometimes of considerable uneasiness, in the wound. The time of healing is greatly shortened, and the knowledge that the loop of wire securely holds the fragments in position allows the use of the joint to be commenced much earlier than when we have only the organic bond of union to trust to. The practice is also of the highest value in ununited fracture of the shafts of the long bones. The thickness of the wire must be proportioned to the force to which it is to be subjected. For the olecranon, that which I have here is amply sufficient, only about one twenty-fifth of an

inch in diameter. For the shaft of a femur of an adult male, a piece like this, about one-tenth of an inch in thickness, which I have had specially prepared with a view to a case of ununited fracture of the femur which we expect to operate upon in the hospital this week, is requisite, in order to resist with certainty the enormous force of the great muscles of the thigh. The pieces of bone which I hold in my hand were removed, in August 1881, from a case of badly united fracture of the femur. The patient was a gentleman from Rio de Janeiro. The fracture had occurred about the junction between the middle and upper thirds of the bone; and it had been so badly united that the fragments overlapped very much, and also were at a considerable angle with each other, so that the limb was extremely distorted, as well as much shortened. Bloodlessness of the operation having been provided for by elevation of the limb and the application of an elastic tourniquet, I cut down from the outer aspect of the thigh upon the seat of fracture. Then, with a periosteum-detacher, I separated the soft parts from the place of junction of the fragments; and, in the next place, I went through the extremely laborious process of cutting through the osseous union (which was of almost ivory hardness) parallel to the axes of the two overlapping fragments. This having been at length accomplished, and the soft parts still further detached, I found that I was able not only to get the limb into a straight position, but also, by very moderate extension, so to reduce the riding of the fragments that, by sawing off comparatively small portions, I was able to bring their extremities into apposition and apply my suture. This was done with wire of about this same thickness. The limb was put up at first with a long splint. I need not enter into details regarding the after-treatment; but I may say this, that he was a weakly man, and it was some months before absolutely firm union was obtained. It would have been extremely embarrassing to have had the ends of the wire sticking out from the wound all that time. On the contrary, it was a very great comfort to have no occasion to think about the wire; and ultimately he left for South America, with a perfectly straight limb, almost of the same length as the other, and, at the same time, with thoroughly firm union.

But to return to the immediate subject of my communication. Ever after my first case of ununited fracture of the olecranon, I was on the look out for a fracture of the patella to treat on the same principle. Dr. Cameron, however, anticipated me. In October 1876, being now full surgeon in the Glasgow Infirmary, he admitted a man with transverse fracture of the patella. He treated him, in the first instance, in the ordinary way, and dismissed him at the end of eight weeks with a pretty short and strong ligamentous union. Eleven days later, however, he reappeared, having ruptured the fibrous band

by a violent movement during a state of intoxication. The fragments were then found widely separated. Dr. Cameron again treated him on ordinary principles for eight weeks, at the end of which time the fragments were still so widely apart, and the limb so feeble, that Dr. Cameron determined to cut down and apply the wire suture antiseptically. This he did on the 5th of March, 1877. On making a longitudinal incision, he exposed a condition of the parts, of which, through his kindness, I am able to show you a sketch, viz. a ligamentous union one inch in length, connecting pretty equal-sized fragments, with nipple-like projections extending from their attenuated margins much thinned by absorption. He cut away the fibrous material, and, having pared the edges of the fragments, and drilled them in two situations with a bradawl, he connected them with two sutures of stout silver wire (as shown here in another drawing), the ends of the wires being left projecting at the wound. At the same time, he introduced an independent drain into the joint. The wound healed without suppuration or fever; and, though osseous union was not obtained—which, as Dr. Cameron remarks in his report of the case, was not to be wondered at, considering the thinned state of the surfaces—yet he had the satisfaction of discharging the patient with close approximation of the fragments, and a thoroughly useful limb.

In October of the same year, 1877, a patient with transverse fracture of the patella was admitted under my care in King's College Hospital. He was a man forty years of age, who, while riding on horseback, had his horse stumble and fall. He was thrown over the horse's head, falling on the right knee. He could not rise, and was brought to the hospital. In the first instance, I attempted with this patient to bring the upper fragment down, so that it should be in contact with the lower. For this purpose I applied an apparatus, into the details of which I need not enter further than to say that it was so arranged that the upper fragment, by means of weights and pulleys, was drawn down. Four days later, however, I found that there was still a quarter of an inch interval between the fragments, and I suggested to the patient the operation of cutting down and applying the wire suture. This, however, he would not then consent to, and preferred returning home to be under the care of his ordinary medical attendant. Eight days later, or fourteen days after the accident, he was readmitted, expressing a wish to be operated upon. On the 26th of October, I accordingly proceeded to operate, making a vertical incision, about two inches in length, over the patella, exposing the fragments, which were then one inch apart. My inability to bring down the upper fragment into contact with the lower became explained when the parts were exposed; for there were found between the fragments extremely firm coagula,

with fibrous tissue, fascial and periosteal, mingled with them, constituting so firm a mass as to make it quite impossible for the two fragments to be brought into contact. The clots having been completely cleared away from between the fragments and from the interior of the joint, I applied a common bradawl in the middle line of the patella, drilling each fragment obliquely so as to bring out the drill upon the broken surface a little distance from the cartilage. Pretty stout silver wire was then passed through the drilled openings, and the fragments thus strung upon it were pushed firmly home, and so brought accurately into apposition. Before they were brought together, however, an arrangement was made for the drainage of the joint. This was done on the same principle in all the cases that I have to record, and I may therefore describe the matter once for all. A pair of dressing-forceps, with the blades closed, were introduced from the wound made into the anterior part of the joint to the most dependent part of the outer aspect of the articulation. The instrument was then forcibly thrust through the synovial membrane, the fibrous capsule, and the fascia, until the point of the forceps was felt under the skin. An incision was then made with a knife through the skin upon the end of the dressing-forceps, so as to allow it to protrude. The blades of the forceps were then expanded so as to enlarge the opening which they had made in the deeper structures without risk of causing haemorrhage. The drain was then seized in the forceps that protruded through the wound, and drawn into the joint. The ends of the wire were now twisted together, and the twisted ends brought out at the wound, which was closed with sutures and a small drain inserted. I need hardly say that in this case, as in Dr. Cameron's, antiseptic treatment was employed throughout. It is unnecessary for me to enter into details as to the progress of this case. We have here the temperature-chart for as long as it was thought worth while to have it recorded, and you will see that it indicates, after a little temporary disturbance immediately after the operation, an entirely afebrile condition. The wounds healed without any suppuration. At the end of eight weeks, the wire was removed by an incision through the cicatrix. Eight days later, the wound made for the removal of the wire had healed. At the end of ten weeks from the operation, the patient was allowed to get up, and, though no passive motion had been employed, he could move the limb freely through an angle of about thirty degrees. Two days later he was discharged, and, unfortunately, nothing has been heard of him since. I saw him once in a cart a few days after he was dismissed, but I have not been able to learn any further tidings of him. This, I believe, is the first instance of a recent case of fracture of the patella being treated by wire-suture antiseptically applied.

My next case occurred two years later. William T——, a coal-porter, thirty-seven years of age, was admitted on the 13th of December, 1879. He was a muscular man. The patient slipped on the 9th of December, while carrying a sack of coals, and felt something give way in one knee. On endeavouring to rise, he found himself unable to do so. On admission into the hospital, the right patella was found fractured transversely, the interval between the fragments being about an inch. There was a considerable amount of effusion into the joint. On the 15th of December, that is to say six days after the accident, I proceeded to operate, making a longitudinal incision, as in the last case, about two inches long, over the patella. The lips of the wound being held apart with blunt hooks, a hole was drilled in each fragment in the median line. Stout silver wire was passed, and secured by half-turns. A drainage-tube was introduced, as in the last case. And here, again, there is no need for my entering in detail into the reports. We have before us the temperature-chart. For the first fortnight after the operation, there was what we may call an absolutely afebrile state. During the rest of the time of his residence in the hospital, there were two accidental elevations to 100·5°, but nothing to indicate anything serious. The wound healed, as in the last case, without any suppuration. Six weeks after the operation he was allowed to get up. Eight weeks after the operation an incision was made through the cicatrix for the removal of the wire, the loop of which was cut, and the wire withdrawn. On the 23rd of February, that is a fortnight later, the bandages which had been previously applied to the leg were removed, and it was found that he could bend the limb to a right angle; he could walk well, and was able to kick. On the 22nd of February, 1883, this patient showed himself. We then took the following notes: 'The patella seems perfectly natural, except a trifling irregularity of outline on one side. It is evidently osseously united. The movements of the joint are perfect, from complete flexion to extension. He can kick vigorously. He says the joint is just as trustworthy as ever. He frequently carries a weight of 220 lb. for upwards of 100 yards, and he walks without the slightest limp.'

[The patient was now introduced and exhibited to the Society. He said his limb was as good as ever, and that it never failed him.]

I have not seen the man since the time I have referred to. Except for the linear cicatrix, no one would be able to tell that this patient had had anything wrong with his knee at all.

The next case was one of ununited fracture. The patient, Joseph R——, aged twenty-two, was admitted into the hospital on the 27th of September, 1880. He is a soldier, and stated that, on the 3rd of June of that year, while

running across a green, he slipped upon a piece of turf, and, in trying to recover his balance, he snapped his knee-pan and fell. He was taken to a military hospital; and the limb was put in splints, which were kept on for seven weeks, a starched bandage being applied after their removal. On admission, the fragments were found separated by an interval of three-quarters of an inch. There was a firm fibrous band of union between them. The knee, however, was quite stiff in consequence, apparently, of the rigidity of the extremely atrophied quadriceps extensor. He was quite unable to walk. If he attempted to do so, he held the leg in his hand. He complained of frequent uneasy sensations in the knee. On the 22nd of October, I cut down over the patella, as in the former case, making an incision about two inches in length. Having cut away the fibrous tissue between the fragments, I refreshed the osseous surfaces, and then, having provided for drainage as before, drilled the fragments, and drew them together with stout silver wire. Nothing occurred worthy of notice until four weeks after the operation, when I proceeded to attempt passive motion. Without chloroform, this proved impossible; but, under chloroform, I used considerable force, when, the rigid quadriceps extensor refusing to yield, the twist of the wire gave way; the cicatrix, which had quite healed except where the wire projected, opened; and there was heard a sound of air being sucked into the joint, the fragments becoming, at the same time, widely separated. I injected the joint with an antiseptic solution, a procedure which I candidly confess I should now regard as probably superfluous. However, such was done; and, six days later, when all disturbance caused by this second injury and the irritation of the antiseptic injection had passed off, I proceeded to operate upon him a second time. Chloroform was again administered, and an incision similar to the former one was made, the cicatrix being laid open. I then found that the ends of the wire were lying in place, ready to be twisted together. I found, at the same time, a considerable mass of coagula present. These I cleared away from between the fragments and from the joint, twisted the wire again, and thus we had a second operation in one patient. The after-progress was as in the other cases. Here we have a doubly long temperature-chart, because we have two cases in one, so to speak; but there was no deviation whatever from the normal state; neither as the result of one operation nor the other has any febrile condition as regards temperature been produced. In due time the wire was removed, and, the wound made through the cicatrix having healed, he was discharged eight weeks after the second operation. He was then able to walk, but with a stiff knee, with scarcely any mobility. I did not attempt any more passive motion after previous experience, and while I was well pleased to see

that he could walk with a stiff knee, whereas he could not walk at all on admission, I hardly ventured to hope for anything better. However, on the 22nd of February, 1883, he presented himself, when the following report was made: 'The patella is perfectly natural, except a little irregularity of the two borders opposite the seat of the fracture. The surface is quite smooth. There is no interval between the fragments. There is evident osseous union. The degree of flexion is increasing. He can bend to an angle of 60°, and extend again completely. He laid aside his stick last summer, and returned to his work as a gardener seven months ago. He can do anything except kneel. In getting over a paling, he has to throw the leg over in a partially extended position. He walks with a barely perceptible limp.' We may now say that he walks with no limp at all.

[The patient was introduced and exhibited. He could now bend his knee nearly to a right angle, and said that he was on his legs all day, and was quite equal to his work as a gardener.]

The great interest in this case seems to me to lie in the improvement that has occurred without any passive motion on the part of the surgeon, and as a mere result of the natural actions of the limb; from such a very rigid state of things as there was when he left the hospital to the condition of mobility which we now witness.

The next case was also one of ununited fracture. The patient, Martha F——, aged forty-three, was admitted on the 5th of November, 1880, on account of an accident which had happened eight weeks before, when she felt something snap in her knee, while trying to save herself from falling. She was unable to walk or move the joint, which quickly swelled up to a large size. No splints had been used, and no treatment of any kind employed. On admission, there was fullness over the knee, and a transverse fracture of the patella to be discovered in the middle of the bone. The fragments were separated one inch. There was fluid in the joint, as indicated by fluctuation. On the 12th of November, I operated as in the last case, paring away the fibrous material between the fragments, and refreshing the osseous surfaces with cutting pliers. The fragments were then drilled obliquely in the middle line, and brought into apposition by means of a suture of stout silver wire. This, however, could only be done when the limb was raised high into the air so as to relax the quadriceps, she being a stout woman, with powerful muscles. Of course, antiseptic dressing was employed; and, as was done in all these cases, the limb was placed in a trough of Gooch's splint, with the upper end oblique, corresponding to the line from the tuberosity of the ischium to the great trochanter, and the lower end excavated in the form of a horseshoe, while the horns of the horseshoe

were well padded, to support the sides of the foot. With regard to this patient, again, I have to show a temperature-chart free from any febrile indication. In eight weeks after the operation the patient was allowed to get up, and she walked about on crutches. She was discharged three weeks later, able to bend the knee slightly, lowering and raising the foot three or four inches.

On the 14th of February the patient was readmitted, to have the wire extracted; and, five days later, she was finally discharged. It will be remembered that this was a case of ununited fracture; and that, when the patient left the hospital, she could only move the foot through a distance of about four inches. On the 22nd of February of this year we had an opportunity of seeing her, and then the following note was taken: 'There is perfect union of the fragments. She can walk from Drury Lane to Billingsgate and back, and walks without a limp. She can bend the knee to a right angle, and says the mobility increases every day. From the position at a right angle, she can raise the leg in a perfectly natural manner to the extended position. She is a stout, heavy woman. She cannot kneel.'

[The patient was now introduced and exhibited. There seems to be a perfect patella; there is nothing abnormal to be felt about it. She says she can do everything except kneeling.]

The next case was one of recent fracture. William G——, sixty-two years of age admitted on the 21st of June, 1881, a healthy man, but a pretty hard drinker. On the morning of his admission he slipped, put out his right leg to save himself, fell, and could not rise. His leg doubled under him. On admission the patella was found fractured transversely, with one inch of interval between the fragments, which could be brought together with difficulty. The knee was considerably swollen. On the 24th, that is three days after the accident, I operated upon him. The operation was conducted precisely as in the cases that have been before described, except that, unlike the two last, there was no necessity for refreshing the fragments. It was merely necessary to take away the clots of blood and any effused fluid from the interior of the joint. A few hours after the operation this patient became delirious, and we were apprehensive of delirium tremens. However, he was quieted with a dose of opium, and gave no further trouble. The temperature-chart here, as in the last case, exhibits entire absence of febrile indication. In this patient, for the first time, the ends of the wire were cut short, and the twist hammered down. The wounds healed without suppuration. Exactly six weeks after the operation he was allowed to get up, and I had the satisfaction of exhibiting him before some of the members of the International Medical Congress, and

showed them that he could raise his limb, from a position of flexion almost at a right angle, to the completely extended posture. I have not seen him from that time until to-day. Having learned that he was employed in a Birmingham establishment, I wrote to my friend Mr. Chavasse, of that city, who replied as follows : ' I saw G. this morning, and have made arrangements for him to leave by the 2 o'clock train to-morrow afternoon. He will be at your house just before 8 o'clock. In case he should not keep his appointment, there is a faint linear cicatrix present over the joint. The knee can be flexed as well as the other. There is bony union of the patella, a faint ridge being felt on the point of union. No suture is to be detected by the touch. At his work, as a stamper, all day long, he works a pulley with the affected limb, which raises a weight of 60 lb. In damp weather he feels very slight inconvenience in the site of the old fracture.' Happily our patient has kept his appointment, so that we are able to see him to-day.

[The patient was introduced and shown. He said he could do anything with the limb the same as with the other. He worked a stamp-hammer, and he could work that hammer, weighing one hundredweight, all day long. There was no difference whatever between the movements of the two knees.]

There is a barely perceptible cicatrix, and he has a perfect patella.

My sixth patient was a woman, Elizabeth C——, fifty-seven years of age. She also was a recent case, admitted on the 21st of October, 1881. Going downstairs, she fell with her left leg bent under her, striking the knee against the ground. She was brought at once to the hospital. The left knee was very much swollen from effusion, hot, and tender ; the fragments of the patella were felt, separated from each other about one inch. I believe it is generally wise to let any distinct inflammatory appearances that exist, as the immediate result of the accident, pass off before the operation. In the former case there were no such, or scarcely any ; therefore we operated three days after the accident. In this case they were manifest, and we allowed a week to pass. The operation was performed just as in the last case. The ends of the wire were twisted with two half-turns, that is to say, one complete turn. The ends were cut short, and then, with the small hammer, the twist was hammered flat down upon the patella.

Here we have the temperature-chart. As we very commonly find, in cases antiseptically operated upon, the first effect of the operation is a depression of temperature. On the evening of the first day there was a depression ; on the following day, a little rise, but that rise only reached 101° ; after that, there was nothing to indicate anything febrile. Two weeks after the operation passive motion was commenced. The wounds were then so very nearly healed

that the milder antiseptic boracic lint was used for the superficial sore which remained. Three weeks after the operation the limb was bent to a right angle; it was painful to her, but caused no disturbance of the fragments. Four weeks after operation, passive motion was again employed; the wounds were completely healed. Six weeks after the operation the patient was allowed to get up, and could already walk fairly. Three days later she was discharged; she could then walk very well. On the 23rd of February of this year we took this note regarding her: 'The patella appears perfectly natural, with the exception that the wire is felt, causing the slightest projection under the skin, which, however, moves freely over it. The movements of the joint are perfect; from complete flexion at a very acute angle to perfect extension. She kneels as she used to do, and only occasionally the wire comes into contact with anything, and then she feels the skin over it tender. She can walk any distance, as before the accident, and does so without a limp.'

[The patient was now introduced to the Society, and confirmed in every respect the previous report.]

My last case is a man sixty-seven years of age, John P——, admitted into King's College Hospital on the 6th of January of the present year. He had fallen from an omnibus on the previous day, striking his knee upon the ground. There was transverse fracture of the patella, and great swelling of the joint. Six days after the accident I proceeded to operate. On making a vertical incision and exposing the seat of fracture, I found a condition of things which, possibly, had I known it, might have induced me to abstain from operating. I found that the lower fragment was very small, and was comminuted. There was one entirely loose piece, as big as a filbert, and another about half that size, which, of course, had to be extracted. Such a condition, taken in connexion with the advanced age of the patient, did not promise well for satisfactory union. Having sponged the clots out from between the fragments and from the joint, and established a drain, I drilled; but as the lower fragment was so exceedingly small, I was obliged to drill, not through it, but through the ligamentum patellae; and, by this means, I was able to bring the large upper fragment into contact with the raw surface below. Now, however, I do not regret having operated upon this poor man. You will see that his temperature-chart is free from any febrile indication; and the wounds healed without any suppuration. Here, as in the last two cases, the ends of the wire were cut short, and the twist hammered down upon the patella. Three weeks and four days after the operation, the wound was nearly healed. The knee was bent through an angle of thirty degrees, and from that position the patient could himself raise it to complete extension; a thing which, with-

out the wire, and without our feeling that the wire was in a very secure form, we should not have thought of permitting. Four weeks after the operation, the wound was healed. The knee was then bent to an angle of forty degrees. Eight weeks after the operation, the patient was discharged, having been for a considerable time before (I have not got it exactly recorded when) allowed to walk about the ward, and walking well. You will observe, gentlemen, that this is my most recent case. He has not had the same time for improvement that the others have had.

[The patient was now introduced, walking up the room without the slightest limp. He said he was getting on 'first-rate'. He found the leg continue to get stronger, and he was able to move it better. He noticed that it was getting stronger every week; he might say every day.]

I confess I am surprised to see how complete a patella this man has, considering how extremely small the lower fragment was. There must have been considerable new osseous formation from the periosteum, because here we feel the central part of the wire, and yet between that and the lower border there is what appears to be a substantial lower fragment connected with the upper one by osseous union. Now we may say he has a perfect patella.

These, gentlemen, are all the cases of fracture of the patella on which I have operated, and I consider it fortunate that I am able to bring before you six out of the seven.

I should like now to say a few words as to the method of operating. The wire employed should be, as I have said, pretty stout, about one-sixteenth of an inch in diameter. I have not found it needful to use more than a single suture of such wire. It is applied in the vertical plane, in the course of the longitudinal incision over the middle of the bone; and in recent cases no dissection of the soft parts from the patella is necessary. It seems important that the cartilaginous surface of the bone should be left quite smooth, or, in other words, that the fragments should be exactly at the same level at their lower part. We cannot be perfectly sure, when we drill, that the bradawl will come out exactly at a corresponding point on the two surfaces. Supposing that on one side the instrument should have come out too far down, it may be into the cartilage (as at *d*, Fig. 2) instead of a little above it. We do not regard that at first, but pass the wire through each drill-hole the moment the drill is withdrawn, and then on that side on which the hole has come too far down, by means of the bradawl, we simply chip away a little of the material that is above the wire until the wire comes to be in a position exactly opposite to the hole on the other side, leaving a gap below, as indicated by the dark shade at *d*. This is a perfectly simple matter; at the same time, it might

possibly not occur to any one during the operation. Here (Fig. 3) we have the wire represented twisted, and the twist hammered down. The twist always goes to one side, and, being on the other side in this instance, is not shown in the section represented by the diagram.

I should like, gentlemen, with your permission, to make some few general remarks. I think it must be admitted that these cases show that the mode of treatment which I have recommended, when applied to recent transverse fractures of the patella, affords a means of restoring the joint to, practically, a perfectly natural condition, provided only that no disaster occurs. That, however, is a tremendous proviso, and no one is more conscious of it than myself. Before I made the incision in the first case that I have recorded to-night, I remarked to those who were assembled in the theatre that I considered no man justified in performing such an operation, unless he could say,

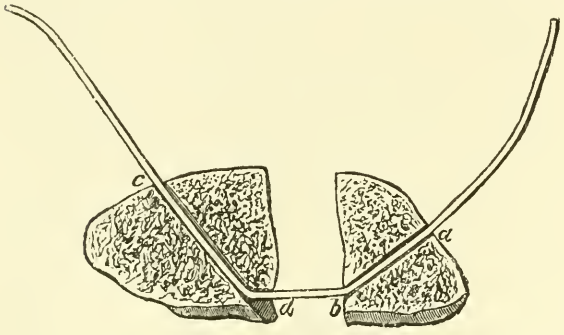

Fig. 2.

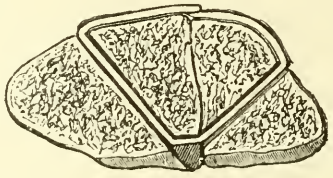

Fig. 3.

with a clear conscience, that he considered himself morally certain of avoiding the entrance of any septic mischief into the wound. Supposing, on the other hand, that a man can say that with a good conscience, then I conceive that he is not only justified, but bound to give his patient the advantages that we see are to be derived from this method of procedure. We know, of course, very well that, by ordinary means of treatment, patients often recover with exceedingly useful limbs. Every now and then, osseous union is obtained; it is a thing which I used to pride myself formerly on striving to get, and I have got such a thing, but it was rather rare, and it was obtained by a very tedious process; and, if ligamentous union occurred, we never had any security that what was a very short ligamentous union when the patient was discharged might not be a long ligamentous union at a later period. A gentleman consulted me with transverse fracture of the patella a couple of years ago. He happened to come to me just as I was about to start for my autumn holiday. I did not care to operate upon him, and throw the responsibility of the after-

treatment upon another, and it happened that the case seemed especially favourably circumstanced for osseous union. There was nothing between the fragments ; the two could be brought into apposition with the utmost facility. I applied an apparatus which appeared to have the effect of maintaining perfect immobility of the fragments ; and, when I came back from my holiday, I found them as I had left them, with no interval between them, and I hoped there might be osseous union. Eight weeks after the accident the patient was allowed to get up and walk, and I was told that he was walking exceedingly well. Six weeks after this, I asked him to come and report himself, and I was sorry to find that there was already half an inch of separation of the fragments. He was then about to start for India. What the separation may be now, I do not know. It is true that there may be a very useful knee-joint, with a very considerable length of fibrous union. Still, when there is a great length of fibrous union the knee is not equal to the original.

Some of our experience with these cases, where we had the opportunity of inspecting the actual state of things, will serve to explain the uncertainty of the results of ordinary treatment. We have found, for instance, such dense masses of clot so mixed with fibrous tissue as to make it quite impossible that the fragments should be brought into apposition. I found in one case, also, such a tilting up of one of the fragments that it would have been perfectly impossible to get the osseous surfaces in contact by any other means than direct operative procedure. Considering, therefore, the great inconvenience which results in many cases when the treatment is conducted on ordinary principles, I believe that if we can really say that we are morally certain that we do not subject the patient to risk, we are in duty bound to give him the benefit of this method. It has been said that it is not justifiable in recent cases, though it is justifiable in ununited cases where there is a useless limb. I must confess that if I believed I was subjecting a patient to serious risk to life, I should not feel justified in operating on the ununited case, and the ununited case is in every respect worse as a subject of operation than the recent. The ununited case has the fragments probably dwindled by absorption, and these fragments, already dwindled, have to be pared. There is a separation, it may be a very considerable separation, and this has to be overcome, it may be by dividing the quadriceps extensor, an operation of difficulty ; and in proportion to the length and difficulty of the operation is the chance that the surgeon may forget some point of importance with regard to the antiseptic element. Then, again, when you come to divide the quadriceps extensor, you divide a very vascular structure, and you may have haemorrhage ; and, further, when the fragments are brought into apposi-

tion they very likely are at considerable tension, and the tension may be apt to cause, through the nervous system, an inflammatory disturbance, and this tends to weaken the parts, and to diminish the power of resistance by which the natural tissues are able to combat the entrance of septic agencies, even though they be in contact with the part. With the recent case, on the other hand, everything is favourable. We have a wound involving no bleeding; and there is no need to pare the fragments. All you have to do is to sponge away the clots, and the surfaces are ready for coaptation. The drilling is a matter of the utmost simplicity. It is an operation of no difficulty, it does not take long; it does not cause anxiety to the surgeon; there is no shock to the system, and no tension. In every respect, the circumstances are favourable as regards the operative procedure. And then, when we come to consider the chances of successful antiseptic management, if there is in the whole body a situation which is well adapted for antiseptic treatment it is this; for of all the conditions requisite according to our present methods of procedure, that which is most important is that the skin on all sides round about the wound should be able to be amply overlapped by the antiseptic dressings. Here we have the wound in the middle of a long limb; from the groin to the foot we may have our antiseptic envelope. Then again we have this envelope surrounded with a secure bandage, and the bandaged dressing encased in a splint, and even if you come to have the patient delirious, as one of my patients was, or supposing a patient to be very curious, as some patients will be, it would puzzle him to get the wound exposed under these circumstances. Now there are wounds so circumstanced that you cannot well guard against this risk. I had a gentleman lately under my care with a psoas abscess; he was very intelligent, and seemed duly impressed with the importance of the antiseptic management; and yet his brother, who was a medical man, coming in one day, saw him drawing the dressing aside, and peeping at the wound. Now a man cannot peep at a wound in connexion with a fracture of the patella; it is so circumstanced mechanically that he cannot do it; and I believe that if we use the means that we have now at our disposal, we may say, with a safe conscience, if we use them aright, that we do not subject the patient to risk, not to anything like so great a risk as patients used to be subjected to not many years ago when they had fatty tumours removed in general hospitals in London. We must all of us remember cases in which, after such operations, erysipelas or diffuse suppuration came on, or some other 'unhealthy action', which, of course, was nobody's fault, but the patient died.

I have referred to a case of ununited fracture of the olecranon where

eighteen surgeons had been previously consulted. I trust no one here will suppose that I mentioned this circumstance for the purpose of glorifying myself. I mentioned it in order to emphasize what I believe to be the truth, that by antiseptic means we can do, and are bound to do, operations of the greatest importance for our patients' advantage, which, without strict antiseptic means, the best surgeon would not be justified in recommending. How wise those eighteen gentlemen were in counselling against operative interference, provided they were not prepared to operate strictly antiseptically, I think we must be all agreed. As regards the operative procedure in that case, it was of the most simple character; any first year's student could have done the operation exactly as well as myself; and, therefore, I trust I shall not be misunderstood by its being supposed that I came here to extol my own skill. That which justified me in operating in that case was simply the knowledge that strict antiseptic treatment would convert serious risk into complete safety.

I should have liked, if time had permitted, to have said a few words as to what seem to be the essential points as regards antiseptic treatment. If I say any words at all now, they will be exceedingly few. I should just like to make this remark, however, that nowadays antiseptic treatment is not a very complicated business, either in theory or in practice. First, as to theory, we do not require any scientific theory to enable us to believe in antiseptic treatment. You need not believe in the germ theory at all; if you are not convinced of the truth of the germ theory of putrefaction and of septic agencies generally, no matter whatsoever with reference to antiseptic practice. All that you have to believe is that there are such things as putrefaction and other septic agencies, and that our wounds are liable to these, and that they are very pernicious, and that these things come from without, and that we have the means of preventing them by various chemical agencies. That is all that we require; and I think anybody who knows the present state of surgical practice must admit these to be truisms. It has sometimes been a great grief to me to think that, because gentlemen are not convinced of the truth of the germ theory out and out, therefore they lay aside antiseptic treatment altogether. And then as to practice; it is not a very difficult thing to wash your hands in a carbolic solution, and have your instruments in this carbolic solution for a quarter of an hour before you operate. It is not a very difficult thing to wrap round the limb a suitable envelope of antiseptic material. What I believe to be one of the most important things of all is, strictly to maintain this rule inviolate, which I insist upon with my dressers, and which I confess I have insisted upon more of late years than I used, and that is,

always when we change a dressing, invariably first to cover the wound with something pure; not to wash the surrounding parts with antiseptic solution, and then, after this has been done, put a dressing on the wound; but before we begin to defile the lotion at all, put on the wound what is pure, and, last thing of all, wash the surrounding parts, which, though they look the same to our eyes, are different *toto coelo*. The edges of the dressing are septic; the wound, if it is as it ought to be, is aseptic. I have known such a thing, for instance, as for a gentleman, in dressing a stump after amputation of the thigh, to wash the perineum with a rag dipped in the carbolic lotion one in forty; and then, having so washed the perineum, immediately to squeeze the rag over the wound. Gentlemen, that makes you laugh; but I assure you these are the kind of things that are constantly going on, and disasters happen in consequence; and gentlemen with whom things go wrong invariably say that with them everything has been perfectly done—a thing which, for my part, I am always loath to say. I am not likely to have many more years of active surgical work; and I have felt that when you, sir, gave me this opportunity, it was my duty to speak what I believe to be the truth; for I feel it to be a grievous thing that patients should be hurried out of their lives, or deprived of usefulness of limbs, simply for want of sufficient earnestness with regard to the endeavour to obtain complete exclusion of septic agencies from wounds, according to our present lights and our present knowledge. Gentlemen, I thank you most heartily for your cheers; for there was a time when such remarks might have met with a different reception.

REMARKS ON THE TREATMENT OF FRACTURES OF THE PATELLA OF LONG STANDING

[*British Medical Journal*, 1908, vol. i, p. 849, and *Lancet*, 1908, vol. i, p. 1049.]

SHORTLY before I retired from practice I devised a method of dealing with fractures of the patella of long standing which gave very satisfactory results. I failed to publish it at the time ; but a surgical friend having asked my advice in a case of that kind, I wrote for him a detailed description of the procedure, preserving a copy of my letter, which I venture, even at this late period, to reproduce.

Fisher's Hotel,
Pitlochry, Scotland.
September 15th, 1895.

My dear Dr. ———,

I should have written to you long ago regarding Miss ——'s case, had I not known that you could not deal with it till you had returned home after your autumn holiday in Europe ; Mr. —— having given me your address in London up to the 15th inst. The limb is no doubt useful as it is ; but it is very far from being as strong as we should wish to see it ; and assuming, as I do in your case, that the surgeon can look with confidence to an aseptic condition of the wound, I think an endeavour should be made to improve it. Indeed, there is good reason to believe that it will be made as serviceable as ever. I doubt, however, whether you will be able to bring the fragments together without some special mode of procedure. When you visited my wards in King's College Hospital, now several years ago, I showed you a young man on whom I had operated on account of a fracture of the patella of long standing with considerable separation of the fragments, where I had attained my object by means of free division of the quadriceps extensor muscle. This, however, is a pretty severe measure, and involves, at the best, more or less weakening of the muscle and a long cicatrix.

Since that time I have greatly improved on that practice, and have succeeded without touching the quadriceps by proceeding in two stages. The idea was suggested to me by a case published by Dr. Lucas-Championnière, of Paris, who, being unable to get the fragments into apposition, wired them together nevertheless, and left the wire in as an adjuvant connecting medium.

It occurred to me that, although such a use of the wire did not seem likely to be very satisfactory as a permanent arrangement, yet it might probably be adopted with great advantage as a temporary expedient ; and that, after the quadriceps had been gradually stretched by the use of the limb in the position so produced, the fragments might by a second operation be brought into contact, and might then be wired in the usual way after paring the opposed surfaces. The first case in which I put this idea in practice was a sufficiently testing one. The patient was a young woman of very stout and heavy build, who had fractured both patellae, one of them four years, the other three years before she consulted me. The fragments were, in both limbs, considerably separated ; and in the

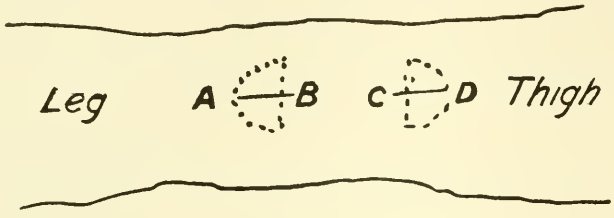

Fig. 1.

left, where the bone had been injured four years previously, the separation was so great (about 5 inches), and the upper fragment, at the same time, so very small, that I at first despaired of being able to do anything.

But on the right side, the upper fragment being of good size and the separation more moderate, I determined to give the plan a trial. I made two short longitudinal incisions (*A B* and *C D*) over the two fragments (shown in dotted line, Fig. 1), and having exposed them by a little dissection, drilled two holes in the upper one, and passed through them, from without inwards, the ends of a piece of the usual stout silver wire, so that, when the ends were pulled upon, the middle of the loop of wire would press upon the surface of the fragment, thus (Fig. 2) :—

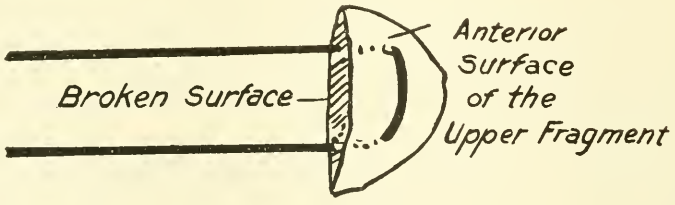

Fig. 2.

Next, passing into the lower incision a blunt instrument (a broad raspatory), I detached from the front of the femur the soft parts lying between the incisions,

consisting, of course, only of skin and fat, as the muscle was absent at that part. Then passing a strong pair of forceps from the lower incision under the skin till their blades appeared in the upper incision, I seized the ends of the wire and drew them down into the lower incision. I then drilled two holes in the lower fragment and passed the ends of the wire through them from within outwards, and, after drawing the upper fragment well down, secured them in the usual way and cut the ends short. The immediate result, so far as the fragments were concerned, is indicated in this diagram (Fig. 3). The incisions in the skin

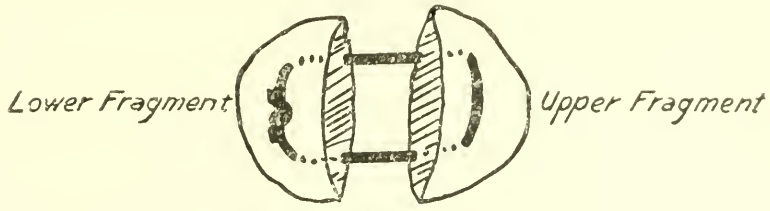

Fig. 3.

were then brought together by sutures, and a dressing (the double-cyanide gauze) applied.

In drawing down the upper fragment I found a great advantage from the use of a very strong sharp hook (Fig. 4), the point of which was inserted in the tendon of the quadriceps at its attachment. By this means I was able to exert much greater traction upon the bone than can be done by simply pulling upon the wire ; and, in order to relax the quadriceps as much as possible, the limb

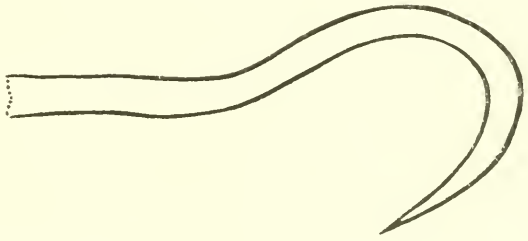

Fig 4.

was placed in the vertical position before the fragment was pulled down. The dressing having been put on, a trough of Gooch's splint was applied to the limb still in the elevated position, and the same attitude was maintained as the patient was removed to the ward, and continued by attaching the end of the splint to a rope connected with the tripod and pulley used in applying Sayre's plaster-of-Paris jacket. This position of the limb did not cause the patient material inconvenience, and after two or three days the rope was slackened a little so as to allow the end of the splint to come down an inch or two, and the same

thing was repeated every two days or so, till the limb could be placed quite horizontal.

This preliminary operation has taken a long time to describe ; but in execution it is of the simplest character, no paring of the broken surfaces being done at this stage, and there being almost no bleeding and no shock. The wound having healed (I need hardly say without suppuration), the patient was allowed to leave her bed, and left the hospital soon after to practise using the limb.

Before long she was readmitted, and the second operation was performed. The lower cicatrix was opened and the wire removed, and two interrupted wire sutures placed in the tracks of the previous continued one ; the fragments, of course, being this time pared to clear them of fibrous tissue of new formation, and produce smooth surfaces for coaptation. This was all satisfactorily effected, though not without the use of the powerful hook and the vertical position of the limb. The result was restoration of the use of the joint in a manner so satisfactory that I determined to try the same procedure in the other limb. The only difference which I made in this case was that, as the upper fragment was too small to bear drilling, I passed the ends of the wire, in dealing with that fragment, through the tendon of the quadriceps just above the upper border of the bone, the lower fragment, which was, of course, very substantial, being drilled as in the other limb. By this means, aided by the vertical position of the limb and the hook, I was able to bring down the upper fragment very satisfactorily, so much so that I did not feel it needful to have the patient use the limb in walking before proceeding to the second operation, but did this before she left her bed, soon after the wounds had healed. In the second operation I applied two interrupted sutures, passing them, as in the first operation, through the track in tendon and bone which the first wire had occupied.

The continued wire suture, which was first used by Dr. Hector C. Cameron, of Glasgow, has the great advantage, where much traction has to be made upon the fragments, that the pressure of the wire is distributed over the anterior surface of the bone instead of being concentrated upon the limited portion of tissue included in an interrupted stitch. And this is peculiarly valuable in old cases of fracture ; in which, as a result of long disuse, the bone undergoes interstitial atrophy that sometimes makes it very soft. But this suture has the disadvantage that it may tilt the fragments so that their anterior edges do not come well into contact with each other, as indicated in this diagram (Fig. 5).

This was my reason for preferring the interrupted suture in the second stage of the proceeding. In the first stage, in which the principal dragging probably takes place, this circumstance is a matter of no consequence, as accurate apposition of the fragments is not then aimed at ; and in any case admitting of being

dealt with by a single operation, though not without much traction, this defect of the continued suture might be readily got over by introducing a superficial central stitch of comparatively thin wire to ensure coaptation of the anterior margins of the fragments.

The effect of the second operation in the above case was such as would have surprised me if I had not seen in other cases how substantial and strong a patella may result from wiring after thorough paring of the surfaces, even when one of the fragments is of quite insignificant size. Without my notes I cannot say how long it is since the case was treated, but I think it must be about four years. And I lately had the satisfaction of hearing from the patient that she could walk well, with strong and supple knee-joints.

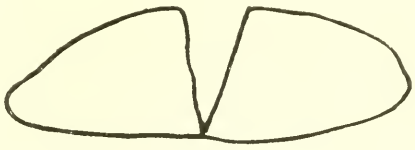

Fig 5.

In Miss ——'s case I would suggest that you should, in the first instance, before making any incision, but after she has been placed under an anaesthetic, apply the strong sharp hook to the border of the upper fragment through the skin, and, with the limb vertical, ascertain to what extent the quadriceps will yield immediately. If the upper fragment came pretty well down, you would proceed to do all by one operation, but if there were the slightest doubt as to the feasibility of this, you would adopt the preliminary procedure as in the above case.

Supposing you to be doing all by one operation, and that it seems desirable to employ Cameron's continued suture, as the wire will be left in permanently, it would be better to introduce its ends first into the lower fragment and fix them over the upper one, where the fixed ends could not cause inconvenience in kneeling. The strong hook would keep the upper fragment well down during the fixing process. If I had published this case, as I ought perhaps to have done, I should not have had to inflict upon you so large an amount of manuscript.

I remain,

Ever truly yours,

(Signed) Joseph Lister.

PART V

ADDRESSES

AN INTRODUCTORY LECTURE

(ON THE CAUSATION OF PUTREFACTION AND FERMENTATION)[1]

Delivered in the University of Edinburgh, November 8, 1869.

[Edinburgh, 1869 (Pamphlet).]

GENTLEMEN.—I stand before you affected with very mingled feelings. On the one hand, I cannot but feel proud to have been called to occupy a Chair which, without disparagement to others, must be allowed to have been, during the last thirty-six years, the one most influential for good in this the most important medical school in the British dominions. But the exultation which I might otherwise naturally feel is heavily dashed by the thought that the circumstance which led to my promotion was the retirement of the man to whom the lustre of the Edinburgh Chair of Clinical Surgery has been from first to last entirely due. I am well aware that he has made the place, not the place him. And though in his presence I must not say all that I otherwise should, I cannot refrain from expressing my conviction that, whether regarded as a scientific and practical surgeon, or as a teacher of those principles which he has done more than any other man in this century to establish, he has been without a rival in the world. Hence, in addition to the grief which I feel in common with you all at the cause of his resigning the Chair which he had so long adorned, I am oppressed with a humbling sense of my own insufficiency; of my weakness, compared with his giant strength of mind and purpose; of my utter inability to fill his place. I can only strive, by the blessing of God, to do my best among you, relying, as I know I may, upon your generous sympathy. At the same time, we may all rejoice that our old master is still among us, to cheer us by his presence and aid us by his counsel; and it is a source of great satisfaction to myself that, as I have the privilege of free access to his inexhaustible store of wisdom and experience, he will, in some sense, through me be still your teacher.

[1] This lecture was not originally intended for publication, and was for the most part delivered extempore.

But, leaving these personal considerations, let us turn to the subject that lies before us. Clinical surgery is, strictly speaking, surgery at the bed-side; surgery illustrated by cases in hospital, as distinguished from surgery taught systematically in the class-room. The importance of clinical or bedside study cannot be overestimated. It is the very keystone, without which all the rest of the educational structure, being merely preparatory, would be absolutely useless. It is to surgery or medicine what dissection is to anatomy. It confers a familiar acquaintance with the nature of disease, and an instinctive knowledge of the appropriate treatment, without which, a man, however accomplished otherwise, would be utterly unfit to practise the profession. But how, it may be asked, can a course of lectures be delivered upon this principle? Can it be possible to take a class of the size of my present audience from bed to bed in a ward, and profitably teach them there? To do this would certainly be impossible. Remarks made at the bed-side are doubtless highly valuable to those who hear them and who see their subject; but it is only a few at a time who can be thus taught. Hence clinical lectures commonly degenerate into the reading of details of cases, with remarks upon them, which, for the great majority of those who hear them, lack the genuine element of clinical interest.

This difficulty was happily overcome by Mr. Syme. Though it was impossible to take a large class to the bed-side of the patient, it was easy in most instances to bring the patient before the class, collected in the operating theatre, where they could all see the salient features of the case, and hear not only the remarks of the teacher, but the patient's own account of his symptoms, and witness the treatment then and there put in practice; or, if it was thought desirable to defer the operation to another day, they were prepared to watch its various steps with intelligence and profit, after having heard the principles of the procedure fully discussed. Such a course of instruction is truly clinical, and, if rightly conducted, possesses a vividness of interest and permanence of impression peculiar to itself. Having witnessed its advantages when in Edinburgh, I have followed this system in Glasgow, and shall continue to pursue it here. But invaluable as such lectures may be made, you must not suppose that attendance upon them will do all that is needful for you in the way of clinical study. You must not only see diseases and watch their treatment by others, but handle them and be personally concerned in their management. Facilities for this are presented by the hospital offices of dresser, clerk, and house surgeon, and no man should consider himself justified in assuming the serious responsibilities of practice without having availed himself largely of such opportunities, either in our infirmary or in some other similar institution.

But to return to the course before us. There are some details regarding the mode in which you may attend it to the greatest advantage, which I shall reserve till we next meet. And now, as the place where we are assembled forbids my entering at once upon demonstrative surgery, I propose to devote the remainder of this hour to the endeavour to convince you, so far as the limited time at our disposal permits, of the truth of the germ theory of putrefaction, the basis of a new mode of treatment which finds its application in all departments of practice; so that without understanding it we cannot advance satisfactorily in the consideration of individual cases. I allude to the antiseptic system. This system of treatment consists of such management of a surgical case as shall effectually prevent the occurrence of putrefaction in the part concerned. When this is really secured, surgery becomes something totally different from what it used to be; and injuries and diseases formerly regarded as most formidable, or even hopeless, advance quietly and surely towards recovery. Of this system the germ theory of putrefaction is the pole-star which will guide you safely through what would otherwise be a navigation of hopeless difficulty.

The germ theory declares that the putrefaction of organic substances under atmospheric influence is not effected, as used to be supposed, by the oxygen of the air, but by living organisms developed from germs floating in the atmosphere as constituents of its dust.

The first great step towards the establishment of this theory was the discovery of the yeast plant in 1838 by Cagniard-Latour, who, having detected in yeast a microscopic fungus, the *Torula Cerevisiae*, which appeared to be the essential constituent of the ferment, attributed the resolution of sugar into alcohol and carbonic acid to the disturbing influence of the growing organism.[1] In the following year, Schwann of Berlin published the results of a remarkable investigation into the cause of putrefaction (in the course of which, by a coincidence such as is not uncommon in the history of science, he, too, had independently discovered the yeast plant), and he related experiments which showed that a decoction of meat might remain for weeks together free alike from putrefaction and from the development of infusoria or fungi in a flask containing air frequently renewed, provided that the atmosphere was subjected to a high temperature at some part of its course towards the containing vessel.[2] Hence he concluded that putrefaction was caused by the growth of organisms springing from germs in the air, the heat preventing the putrefactive change by depriving the germs of their vitality. In other words, he propounded the

[1] See *Comptes Rendus*, tom. iv. p. 905.

[2] See Poggendorf's *Annalen*, vol. xli. art. xvi.

germ theory of putrefaction. These experiments of Schwann's appear to me to prove conclusively that oxygen, as ordinarily understood by chemists, cannot of itself occasion putrefaction. It is true, indeed, that, if you attempt to repeat the experiments, you may meet with failure. But it must be remembered that merely negative results go for nothing here, if the positive evidence rests on satisfactory authority. This is a point which has been too little borne in mind in the discussion of this subject. If we consider what the germ theory assumes, how minute the putrefactive particles are supposed to be, and how universally present in the atmosphere, and in the dust which adheres to all objects exposed to it, it is easy to understand failure in such experiments consistently with the truth of the theory. But it is *impossible* to understand success in any single instance, consistently with the falsehood of the theory. If in any one case it really happened that a decoction of meat remained without putrefaction for weeks together, though freely exposed to air, unaltered, except by having been temporarily subjected to a high temperature, this is enough to show that oxygen, as known to chemists, is not the sole cause of the change in question.[1] One genuine successful experiment out of a thousand is enough to establish that point.

Schwann's observations, however, did not receive the attention which they appear to me to have deserved. The fermentation of sugar was generally allowed to be occasioned by the *Torula Cerevisiae*; but it was not admitted that putrefaction was due to an analogous agency. And yet the two cases present a very striking parallel. In each a stable chemical compound, sugar in the one case, albumen in the other, undergoes extraordinary chemical changes under the influence of an excessively minute quantity of a substance which, regarded chemically, we should suppose inert. As an example of this in the case of putrefaction, let us take a circumstance often witnessed in the treatment of large chronic abscesses. In order to guard against the access of atmospheric air, we used to draw off the matter by means of a cannula and trocar, such as you see here, consisting of a silver tube with a sharp-pointed steel rod fitted into it, and projecting beyond it. The instrument, dipped in oil, was thrust into the cavity of the abscess, the trocar was withdrawn, and the pus flowed out through the cannula, care being taken by gentle pressure over the part to prevent the possibility of regurgitation. The cannula was then drawn out with due precaution against the reflux of air. This method was frequently successful as to its immediate object, the patient being relieved

[1] Such experiments are peculiarly likely to fail in the hands of those who perform them with the object of confuting the germ theory. In fact, a belief in the theory is almost essential in order that the experimenter may be sufficiently keenly alive to the subtle sources of failure

from the mass of the accumulated fluid, and experiencing no inconvenience from the operation. But the pus was pretty certain to reaccumulate in course of time, and it became necessary again and again to repeat the process. And unhappily there was no absolute security of immunity from bad consequences. However carefully the procedure was conducted, it sometimes happened, even though the puncture seemed healing by first intention, that feverish symptoms declared themselves in the course of the first or second day, and, on inspecting the seat of the abscess, the skin was perhaps seen to be red, implying the presence of some cause of irritation, while a rapid reaccumulation of the fluid was found to have occurred. Under these circumstances, it became necessary to open the abscess by free incision, when a quantity, large in proportion to the size of the abscess, say, for example, a quart, of pus escaped, fetid from putrefaction. Now, how had this change been brought about? Without the germ theory, I venture to say, no rational explanation of it could have been given. It must have been caused by the introduction of something from without. Inflammation of the punctured wound, even supposing it to have occurred, would not explain the phenomenon. For mere inflammation, whether acute or chronic, though it occasions the formation of pus, does not induce putrefaction. The pus originally evacuated was perfectly sweet, and we know of nothing to account for the alteration in its quality but the influence of something derived from the external world. And what could that something be? The dipping of the instrument in oil, and the subsequent precautions, prevented the entrance of oxygen. Or even if you allowed that a few atoms of the gas did enter, it would be an extraordinary assumption to make that these could in so short a time effect such changes in so large a mass of albuminous material. Besides, the pyogenic membrane is abundantly supplied with capillary vessels, through which arterial blood, rich in oxygen, is perpetually flowing; and there can be little doubt that the pus, before it was evacuated at all, was liable to any action which the element might be disposed to exert upon it.

On the oxygen theory, then, the occurrence of putrefaction under these circumstances is quite inexplicable. But if you admit the germ theory, the difficulty vanishes at once. The cannula and trocar having been lying exposed to the air, dust will have been deposited upon them, and will be present in the angle between the trocar and the silver tube, and in that protected situation will fail to be wiped off when the instrument is thrust through the tissues. Then when the trocar is withdrawn, some portions of this dust will naturally remain upon the margin of the cannula, which is left projecting into the abscess, and nothing is more likely than that some particles may fail to be washed off

by the stream of outflowing pus, but may be dislodged when the tube is taken out, and left behind in the cavity. The germ theory tells us that these particles of dust will be pretty sure to contain the germs of putrefactive organisms, and if one such is left in the albuminous liquid, it will rapidly develop at the high temperature of the body, and account for all the phenomena.

But striking as is the parallel between putrefaction in this instance and the vinous fermentation, as regards the greatness of the effect produced, compared with the minuteness and the inertness, chemically speaking, of the cause, you will naturally desire further evidence of the similarity of the two processes. You can see with the microscope the torula of fermenting must or beer. Is there, you may ask, any organism to be detected in the putrefying pus? Yes, gentlemen, there is. If any drop of the putrid matter is examined with a good glass, it is found to be teeming with myriads of minute jointed bodies, called vibrios, which indubitably proclaim their vitality by the energy of their movements. It is not an affair of probability, but a fact, that the entire mass of that quart of pus has become peopled with living organisms as the result of the introduction of the cannula and trocar; for the matter first let out was as free from vibrios as it was from putrefaction. If this be so, the greatness of the chemical changes that have taken place in the pus ceases to be surprising. We know that it is one of the chief peculiarities of living structures that they possess extraordinary powers of effecting chemical changes in materials in their vicinity, out of all proportion to their energy as mere chemical compounds. And we can hardly doubt that the animalcules which have been developed in the albuminous liquid, and have grown at its expense, must have altered its constitution, just as we ourselves alter that of the materials on which we feed.

The only question, therefore, that remains to be answered is, Whence have these vibrios originated? Have they sprung, like higher animals and plants, from pre-existing similar organisms, or have they arisen spontaneously out of the pus from an alteration in its physical constitution, determined in some inexplicable manner by the introduction of a cannula and trocar?

All analogy, gentlemen, is in favour of the former view. The doctrine of spontaneous or equivocal generation has been chased successively to lower and lower stations in the world of organized beings, as our means of investigation have improved. I remember a conversation I once had, when a student, with an elderly gentleman, not indeed belonging to our profession, on the subject of mites in cheese. He believed that they grew out of the cheese from some change in its substance as the result of keeping; and the view which I advocated, that they had sprung from the eggs of pre-existing mites, seemed

to him preposterous. But when the microscope is applied to these creatures, and we see that they rank in the type of their organization with spiders or crabs, and that they are similarly provided with organs of reproduction, it seems to us as absurd to suppose that they have arisen from a mere alteration in the cheese as it would be to imagine that crabs could spring spontaneously out of a piece of dead fish or other garbage upon which they prey. Yet though no physiologist doubts that cheese-mites do arise from parentage, it must be confessed that there is some difficulty in accounting for their almost invariable occurrence in some kinds of cheese kept for a sufficient length of time. Whether the eggs are transferred by the hand of the cheesemonger, or whether the adult mites migrate from cheese to cheese, may be matter for curious discussion.

But though with creatures as large, comparatively speaking, as the cheese-mite, it may not be very easy to explain the extensive diffusion of their ova, this difficulty becomes less and less the more minute the organism. If a vessel containing preserved fruit is left exposed to the air, the surface of the preserve soon becomes covered with mould, and it is then found to have a 'mouldy' flavour—implying alteration in its chemical constitution. The mould itself has a flavour of its own, and it has developed, in part at least, at the expense of the preserve. If the mould is examined microscopically, it is seen to be just as distinctly a vegetable as a cabbage is, and far more abundantly provided with reproductive apparatus. Supposing it to be the ordinary blue mould, the blue tint is simply the colour of the fructification. This is in accordance with a general law in the organic world, that so far from any deficiency appearing in the arrangements for reproduction in the lower forms of life, so as to make it difficult to account for their originating from parents, the lower the organism the more lavishly is this provided for. In some animals low in the scale of being we find, besides the formation of ova, a faculty of self-multiplication by segmentation, or, as it is termed, fissiparous generation. For what purpose, I venture to ask, can be this ample provision for reproduction of the lowest species by parentage, if they can spring spontaneously out of the materials in which they grow ?

Now, in the case of the blue mould, the sporules, besides being produced in incalculable multitudes, are of extreme minuteness, and constitute a very fine dust, which cannot fail to be wafted and extensively diffused through the air. If a ray of sunlight were to shoot through this room, we should see the sunbeam peopled with motes. But the particles of dust which are rendered visible to the naked eye by being so illuminated, are gross indeed compared with the sporules of such a fungus. Some of them are complicated organic

structures, such as pieces of hair or vegetable fibre ; and if these are suspended in the air, still more must microscopic spores be so, though their extreme minuteness makes it less easy to distinguish them from particles of inorganic matter. Hence it appears that, for the lowest forms of life, as for the highest the notion of spontaneous generation is simply gratuitous and uncalled for.

But although from these considerations we may be led pretty surely to infer, on the one hand, that the atmosphere is pervaded by the germs of minute organisms, and, on the other hand, that without such germs the organisms could not take their origin, it would be highly desirable to obtain positive evidence on both these points, if indeed it is attainable.

Such evidence has been afforded of late years by the beautiful researches of Pasteur. From among his numerous experiments, I will select one set as peculiarly instructive. A number of glass flasks, with attenuated necks, were partially filled with a decoction of yeast, filtered so as to be perfectly clear and transparent. Each was then boiled for a certain length of time, with the object of destroying any organisms existing in the decoction, or adhering to the interior of the vessel, and during ebullition the neck was hermetically sealed, so that when the vessel cooled, a vacuum was produced in the part previously occupied by air. A certain number of such a series of flasks were then opened in a particular locality, as, for example, a lecture-room such as this, by breaking the narrow neck of each, after scratching it with a file. Air rushed in to fill the vacuum, after which the neck was immediately sealed again with the blow-pipe. As the result of the introduction of this limited amount of air, the previously transparent liquid in a considerable proportion of the flasks was seen to present, in the course of the next few days, a cloudiness indicative of the first appearance of the growth of torulae and other organisms, which afterwards continued to increase. But if a set of such flasks were opened in a situation where atmospheric germs might be expected to be few, if any, a different result was obtained. M. Pasteur was at the pains to take such flasks to the Mont Anvert, in Switzerland, and open them in wind blowing from a glacier, taking special care, by exposing the neck to the flame of a spirit-lamp when filing it, and breaking it with long forceps similarly treated, to guard as much as possible against the introduction of living organisms from the instruments employed, or from his own person. The pure air thus introduced had indeed, in one flask out of twenty, the effect of inducing, very slowly, an appearance of organic development. But in all the rest the liquid remained perfectly unchanged for an indefinite period. On the other hand, if the flasks were opened in a situation where the air, though in one sense pure, might be expected to abound in minute life, viz. under the shade

of trees in the country, organisms formed in sixteen out of eighteen flasks, and presented a great variety in their nature.[1] These experiments, which rest not only on the high authority of M. Pasteur, but also on the unimpeachable corroborative testimony of a Committee of the French Academy of Sciences, including the celebrated Milne Edwards, prove conclusively both that the gases of the air cannot of themselves occasion the growth of organisms, even in a very favourable nidus for their development, and also that in regions inhabited by plants or animals, whether in cities or in the country, nearly every cubic inch of atmosphere really does contain living germs floating in it.

But there is one other experiment related by Pasteur,[2] which is in some respects even more striking. A flask is prepared similar to those already described, except that, after the introduction of the decoction of yeast, the neck is not only drawn out into a pretty narrow tube, but bent at various angles. The fluid is then boiled as in the former experiments; but the end of the neck, instead of being sealed, is left open, so that air passes into the flask on withdrawal of the lamp. The vessel being then left undisturbed, the diurnal changes of temperature, involving alternate expansion by day and condensation at night of the gases in the flask, necessitate a daily interchange between the air in the body of the flask and the external atmosphere. Yet the fluid, though exposed in this way to air perpetually changed, remains for an indefinite period quite transparent, without trace of organic development. There can be but one interpretation of this fact. The oxygen, whether in its ordinary condition or that of ozone, with all the other atmospheric gases, including any which may exist in such small quantities as to be undiscoverable by the chemist, must pass, each in its own proportion, unchanged into the body of the flask. It is impossible that a dry glass tube can stop any gas. For though the tube is moist from condensation of aqueous vapour in the first instance, it is soon dried by the air that passes in and out through it. It is, therefore, inconceivable that any atmospheric gas can have been arrested by the tube. But it is conceivable, considering the very gradual character of the movements of the air in consequence of the diurnal changes, that dust, even though very fine, may be arrested by the angles. We may, perhaps, wonder that particles of such extreme minuteness as the germs of atmospheric organisms should be so detained. But no one can say it is impossible, and no other possible explanation presents itself. The experiment proves with certainty that the gases of the air, however abundantly supplied, are of themselves unable to originate the growth of torulae and the other minute organisms which appear in decoction of yeast

[1] See *Annales des Sciences Naturelles*, 1861 and 1865.

[2] This experiment is attributed by Pasteur to M. Chevreul.

freely exposed to the atmosphere; and also that the essential source of such development must be suspended particles or germs. But in order to render the experiment, if possible, still more conclusive, the Committee of the Academy completed it by sealing the end of the neck of the flask, after the fluid had remained clear for a sufficient length of time to show that no organisms could grow in it, and inverting and shaking the vessel till some of the liquid passed into the angles of the bent tube, after which the flask was again left to itself. And now, gentlemen, occurred something which you may perhaps be disposed to regard as too good to be true, but which is true nevertheless. In the course of no long time the fluid in the angles of the tube exhibited indications of organic growth, demonstrating that the sources or germs of such development had, as a matter of fact, been arrested there.

This experiment charms us alike by its simplicity and perfect conclusiveness. Here is evidence indeed, which, if the facts be admitted, cannot be gainsaid. But though I could not doubt the authority on which it rested, I felt desirous, if possible, to bring it to bear more directly upon the subject of putrefaction. The fluid which seemed most likely to answer the purpose, combining transparency with a high degree of putrescibility, was urine, and I accordingly made it the subject of the experiment to which I now desire to direct your attention.[1] Two years ago last month, I introduced portions of the same specimen of fresh urine into four flasks, of which two are before you. The body of each vessel was about one-third filled with the liquid. After the introduction of the fluid, the necks of three of them were drawn out into tubes rather less than a line in diameter, and then bent at various acute angles, as you observe in one of these. In the other the neck was drawn out to a calibre if anything rather finer, but cut short and left vertical, as you see it. The liquid in each flask was then boiled for five minutes, the steam issuing freely from the open end of the narrow neck. The reason for boiling it so long is that, as Pasteur has shown, merely raising this fluid to the temperature of 212° Fahr., and then allowing it to cool, is not enough to kill all the organisms it may contain. It is necessary to maintain the elevated temperature for about five minutes to ensure complete destruction of their vitality.[2] The lamp being then removed, air of course passed in to take the place of the condensed aqueous vapour. And during the two years that have since

[1] Since making the experiment I have learned that Pasteur had previously performed it with urine.

[2] See *Comptes Rendus*, vol. l, p. 306. It follows that if any germs were drawn into the body of the flask with the air that rushes in on the withdrawal of the lamp, they would retain their vitality in the hot liquid, and develop in it when it had cooled. I have elsewhere expressed the opinion that the germs contained in the air which is thus rapidly admitted in the first instance must be arrested by the drops of water which appear in the angles of the tube immediately on the cessation of ebullition, just as the particles of dust in inspired air are stopped by the mucus of our bronchial tubes. See *British Medical Journal*, July 18, 1868 (page 58 of this volume).

elapsed, a considerable fraction of a cubic inch of fresh air has entered every night into the body of each flask to exert its influence upon the liquid. In the case of the flasks with contorted neck, the air moving to and fro through the tube soon dried the moisture which was at first deposited within it ; and any of you may see, after lecture, that in the one before you the neck is dry as well as open from end to end, so that it could present no obstacle to any gaseous constituent of the atmosphere. Nevertheless, though thus freely exposed to the action of the gases of the air for so long a period, including two unusually hot summers, the urine still retains its original straw colour and perfect transparency, presenting neither cloud, scum, nor sediment ; and the only change that I can detect in it is, that of late, as a result, I presume, of the slow evaporation that has been going on in consequence of the perpetual change of air, some very minute shining crystals have been deposited upon the sides of the glass. Similarly unaltered are the contents of the other two similar flasks which I have not thought it needful to bring here. But very different is the appearance of the urine in this other flask, whose neck, short and vertical, was calculated to admit particles of dust as well as gaseous material. The transparent straw colour has given place to a muddy brown, with abundant sediment, including the *débris* of different fungi, which have long since ceased to grow, poisoned, no doubt, by the acridity of the liquid, the pungently ammoniacal character of which may be readily ascertained by placing the warm hand for a moment upon the body of the flask, while one nostril is kept above the orifice.

Soon after the commencement of the experiment, this short-necked flask had a really beautiful appearance. Two different kinds of fungi presented themselves—one of exceedingly delicate structure growing rapidly from the bottom of the vessel, so as to occupy in no long time the greater part of the bulk of the liquid ; the other a dense blue mould floating at the surface, and extending slowly in concentric rings. Meanwhile the fluid gradually assumed a deeper and deeper amber tint, indicative of progressive change in its chemical composition.

In the case of the flasks with bent necks I was not content with observing the completely unchanged appearance of the contained urine. Half a year after the experiment was begun I poured out about half an ounce of the clear contents of one of them into a wine-glass for examination. Its odour was perfectly sweet, and its reaction faintly acid ; and under the microscope a careful search with an excellent glass of high power failed to detect vibrio, bacterium, or any other organism. The lowest known forms of organic development and the slightest approach to putrefactive change had been alike prevented by simply filtering the air of its floating molecules.

Yet the urine which had so long remained unaltered under the free influence of the gaseous constituents of the atmosphere proved as prone as ever to the usual effects of exposure to the air as soon as particles of dust could gain access to it ; for the wine-glass having been covered to prevent evaporation, I found the fluid in two days with a dunghill odour, and loaded with minute microscopic organisms, and a few days later different kinds of fungi visible to the naked eye were growing in it.

Gentlemen, I commend these facts to your candid and impartial judgement, beseeching you to form your own opinions regarding them. The minds which you bring to bear upon this subject to-day are very much the same as they will be throughout your lives. An observation which any one of you may make now will serve in after life to illustrate a course of lectures, should he occupy a position corresponding to that which I have now the honour to hold. And you are as competent as you ever will be to draw logical inferences from established data. Do not, then, let any authority shake your confidence in knowledge so obtained.

Throughout the course on which we are entering I shall endeavour, as far as possible, to place before you simple facts—trusting that, in estimating their significance, you will be ever guided by that which our dear master has so constantly striven to inculcate as our leading principle, the love of Truth.

ON THE INTERDEPENDENCE OF SCIENCE AND THE HEALING ART

Being the Presidential Address to the British Association for the Advancement of Science Liverpool, 1896.

[*Report of the Association.*]

My Lord Mayor, My Lords, Ladies, and Gentlemen.—I have first to express my deep sense of gratitude for the great honour conferred upon me by my election to the high office which I occupy to-day. It came upon me as a great surprise. The engrossing claims of surgery have prevented me for many years from attending the meetings of the Association, which excludes from her sections medicine in all its branches. This severance of the art of healing from the work of the Association was right and indeed inevitable. Not that medicine has little in common with science. The surgeon never performs an operation without the aid of anatomy and physiology; and in what is often the most difficult part of his duty, the selection of the right course to follow, he, like the physician, is guided by pathology, the science of the nature of disease, which, though very difficult from the complexity of its subject-matter, has made during the last half-century astonishing progress; so that the practice of medicine in every department is becoming more and more based on science as distinguished from empiricism. I propose on the present occasion to bring before you some illustrations of the interdependence of science and the healing art; and the first that I will take is perhaps the most astonishing of all results of purely physical inquiry—the discovery of the Röntgen rays, so called after the man who first clearly revealed them to the world. Mysterious as they still are, there is one of their properties which we can all appreciate—their power of passing through substances opaque to ordinary light. There seems to be no relation whatever between transparency in the common sense of the term and penetrability to these emanations. The glasses of a pair of spectacles may arrest them, while their wooden and leathern case allows them to pass almost unchecked. Yet they produce, whether directly or indirectly, the same effects as light upon a photographic plate. As a general rule, the denser any object is the greater obstacle does it oppose to the rays. Hence, as bone is denser than flesh, if the hand or other part of the body is placed above the sensitive film enclosed in a case of wood or other light material at a suitable distance from the source of the rays, while they pass with the utmost facility through the uncovered parts of the lid of the box and powerfully affect the plate beneath, they are arrested to a large

extent by the bones, so that the plate is little acted upon in the parts opposite to them, while the portions corresponding to the muscles and other soft parts are influenced in an intermediate degree. Thus a picture is obtained in which the bones stand out in sharp relief among the flesh, and anything abnormal in their shape or position is clearly displayed.

I need hardly point out what important aid this must give to the surgeon. As an instance, I may mention a case which occurred in the practice of Mr. Howard Marsh. He was called to see a severe injury of the elbow, in which the swelling was so great as to make it impossible for him by ordinary means of examination to decide whether he had to deal with a fracture or a dislocation. If it were the latter, a cure would be effected by the exercise of violence which would be not only useless but most injurious if a bone was broken. By the aid of the Röntgen rays a photograph was taken in which the bone of the upper arm was clearly seen displaced forwards on those of the forearm. The diagnosis being thus established, Mr. Marsh proceeded to reduce the dislocation ; and his success was proved by another photograph which showed the bones in their natural relative position.

The common metals, such as lead, iron, and copper, being still denser than the osseous structures, these rays can show a bullet embedded in a bone or a needle lodged about a joint. At the last conversazione of the Royal Society, a picture produced by the new photography displayed with perfect distinctness through the bony framework of the chest a halfpenny low down in a boy's gullet. It had been there for six months, causing uneasiness at the pit of the stomach during swallowing ; but whether the coin really remained impacted, and if so, what was its position, was entirely uncertain till the Röntgen rays revealed it. Dr. Macintyre, of Glasgow, who was the photographer, informs me that when the presence of the halfpenny had been thus demonstrated, the surgeon in charge of the case made an attempt to extract it, and although this was not successful in its immediate object, it had the effect of dislodging the coin ; for a subsequent photograph by Dr. Macintyre not only showed that it had disappeared from the gullet, but also, thanks to the wonderful penetrating power which the rays had acquired in his hands, proved that it had not lodged further down in the alimentary passage. The boy has since completely recovered.

The Röntgen rays cause certain chemical compounds to fluoresce, and emit a faint light plainly visible in the dark ; and if they are made to fall upon a translucent screen impregnated with such a salt, it becomes beautifully illuminated. If a part of the human body is interposed between the screen and the source of the rays, the bones and other structures are thrown in shadow

upon it, and thus a diagnosis can be made without the delay involved in taking a photograph. It was in fact in this way that Dr. Macintyre first detected the coin in the boy's gullet. Mr. Herbert Jackson, of King's College, London, early distinguished himself in this branch of the subject. There is no reason to suppose that the limits of the capabilities of the rays in this way have yet been reached. By virtue of the greater density of the heart than the adjacent lungs with their contained air, the form and dimensions of that organ in the living body may be displayed on the fluorescent screen, and even its movements have been lately seen by several different observers.

Such important applications of the new rays to medical practice have strongly attracted the interest of the public to them, and I venture to think that they have even served to stimulate the investigations of physicists. The eminent Professor of Physics in the University College of this city (Professor Lodge) was one of the first to make such practical applications, and I was able to show to the Royal Society at a very early period a photograph, which he had the kindness to send me, of a bullet embedded in the hand. His interest in the medical aspect of the subject remains unabated, and at the same time he has been one of the most distinguished investigators of its purely physical side.

There is another way in which the Röntgen rays connect themselves with physiology, and may possibly influence medicine. It is found that if the skin is long exposed to their action it becomes very much irritated, affected with a sort of aggravated sun-burning. This suggests the idea that the transmission of the rays through the human body may be not altogether a matter of indifference to internal organs, but may, by long-continued action, produce, according to the condition of the part concerned, injurious irritation or salutary stimulation.

This is the jubilee of Anaesthesia in surgery. That priceless blessing to mankind came from America. It had, indeed, been foreshadowed in the first year of this century by Sir Humphrey Davy, who, having found a toothache from which he was suffering relieved as he inhaled laughing gas (nitrous oxide), threw out the suggestion that it might perhaps be used for preventing pain in surgical operations. But it was not till, on the 30th of September, 1846, Dr. W. T. G. Morton, of Boston, after a series of experiments upon himself and the lower animals, extracted a tooth painlessly from a patient whom he had caused to inhale the vapour of sulphuric ether, that the idea was fully realized. He soon afterwards publicly exhibited his method at the Massachusetts General Hospital, and after that event the great discovery spread rapidly over the civilized world. I witnessed the first operation in England under ether. It was performed by Robert Liston in University College Hospital, and it was a complete success. Soon afterwards I saw the same great surgeon amputate

the thigh as painlessly, with less complicated anaesthetic apparatus, by aid of another agent, chloroform, which was being powerfully advocated as a substitute for ether by Dr. (afterwards Sir James Y.) Simpson, who also had the great merit of showing that confinements could be conducted painlessly, yet safely, under its influence. These two agents still hold the field as general anaesthetics for protracted operations, although the gas originally suggested by Davy, in consequence of its rapid action and other advantages, has taken their place in short operations, such as tooth extraction. In the birthplace of anaesthesia ether has always maintained its ground ; but in Europe it was to a large extent displaced by chloroform till recently, when many have returned to ether, under the idea that, though less convenient, it is safer. For my own part, I believe that chloroform, if carefully administered on right principles, is, on the average, the safer agent of the two.

The discovery of anaesthesia inaugurated a new era in surgery. Not only was the pain of operations abolished, but the serious and sometimes mortal shock which they occasioned to the system was averted, while the patient was saved the terrible ordeal of preparing to endure them. At the same time the field of surgery became widely extended since many procedures in themselves desirable, but before impossible from the protracted agony they would occasion, became matters of routine practice. Nor have I by any means exhausted the list of the benefits conferred by this discovery.

Anaesthesia in surgery has been from first to last a gift of science. Nitrous oxide, sulphuric ether, and chloroform are all artificial products of chemistry, their employment as anaesthetics was the result of scientific investigation, and their administration, far from being, like the giving of a dose of medicine, a matter of rule of thumb, imperatively demands the vigilant exercise of physiological and pathological knowledge.

While rendering such signal service to surgery, anaesthetics have thrown light upon biology generally. It has been found that they exert their soporific influence not only upon vertebrata, but upon animals so remote in structure from man as bees and other insects. Even the functions of vegetables are suspended by their agency. They thus afford strong confirmation of the great generalization that living matter is of the same essential nature wherever it is met with on this planet, whether in the animal or vegetable kingdom. Anaesthetics have also, in ways to which I need not here refer, powerfully promoted the progress of physiology and pathology.

My next illustration may be taken from the work of Pasteur on fermentation. The prevailing opinion regarding this class of phenomena when they first engaged his attention was that they were occasioned primarily by the oxygen of the

air acting upon unstable animal or vegetable products, which, breaking up under its influence, communicated disturbance to other organic materials in their vicinity, and thus led to their decomposition. Cagniard-Latour had indeed shown several years before that yeast consists essentially of the cells of a microscopic fungus which grows as the sweetwort ferments ; and he had attributed the breaking up of the sugar into alcohol and carbonic acid to the growth of the micro-organism. In Germany, Schwann, who independently discovered the yeast plant, had published very striking experiments in support of analogous ideas regarding the putrefaction of meat. Such views had also found other advocates, but they had become utterly discredited, largely through the great authority of Liebig, who bitterly opposed them.

Pasteur, having been appointed as a young man Dean of the Faculty of Sciences in the University of Lille, a town where the products of alcoholic fermentation were staple articles of manufacture, determined to study that process thoroughly ; and as a result he became firmly convinced of the correctness of Cagniard-Latour's views regarding it. In the case of other fermentations, however, nothing fairly comparable to the formation of yeast had till then been observed. This was now done by Pasteur for that fermentation in which sugar is resolved into lactic acid. This lactic fermentation was at that time brought about by adding some animal substance, such as fibrine, to a solution of sugar, together with chalk that should combine with the acid as it was formed. Pasteur saw, what had never before been noticed, that a fine grey deposit was formed, differing little in appearance from the decomposing fibrine, but steadily increasing as the fermentation proceeded. Struck by the analogy presented by the increasing deposit to the growth of yeast in sweetwort, he examined it with the microscope, and found it to consist of minute particles of uniform size. Pasteur was not a biologist, but although these particles were of extreme minuteness in comparison with the constituents of the yeast plant, he felt convinced that they were of an analogous nature, the cells of a tiny microscopic fungus. This he regarded as the essential ferment, the fibrine or other so-called ferment serving, as he believed, merely the purpose of supplying to the growing plant certain chemical ingredients essential to its nutrition not contained in the sugar. And the correctness of this view he confirmed in a very striking manner, by doing away with the fibrine or other animal material altogether, and substituting for it mineral salts containing the requisite chemical elements. A trace of the grey deposit being applied to a solution of sugar containing these salts in addition to the chalk, a brisker lactic fermentation ensued than could be procured in the ordinary way.

I have referred to this research in some detail because it illustrates

Pasteur's acuteness as an observer and his ingenuity in experiment, as well as his almost intuitive perception of truth.

A series of other beautiful investigations followed, clearly proving that all true fermentations, including putrefaction, are caused by the growth of micro-organisms.

It was natural that Pasteur should desire to know how the microbes which he showed to be the essential causes of the various fermentations took their origin. It was at that period a prevalent notion, even among many eminent naturalists, that such humble and minute beings originated *de novo* in decomposing organic substances; the doctrine of spontaneous generation, which had been chased successively from various positions which it once occupied among creatures visible to the naked eye, having taken its last refuge where the objects of study were of such minuteness that their habits and history were correspondingly difficult to trace. Here again, Pasteur at once saw, as if by instinct, on which side the truth lay; and, perceiving its immense importance, he threw himself with ardour into its demonstration. I may describe briefly one class of experiments which he performed with this object. He charged a series of narrow-necked glass flasks with a decoction of yeast, a liquid peculiarly liable to alteration on exposure to the air. Having boiled the liquid in each flask, to kill any living germs it might contain, he sealed its neck with a blowpipe during ebullition; after which, the flask being allowed to cool, the steam within it condensed, leaving a vacuum above the liquid. If, then, the neck of the flask were broken in any locality, the air at that particular place would rush in to fill the vacuum, carrying with it any living microbes that might be floating in it. The neck of the flask having been again sealed, any germs so introduced would in due time manifest their presence by developing in the clear liquid. When any of such a series of flasks were opened and resealed in an inhabited room, or under the trees of a forest, multitudes of minute living forms made their appearance in them; but if this was done in a cellar long unused, where the suspended organisms, like other dust, might be expected to have all fallen to the ground, the decoction remained perfectly clear and unaltered. The oxygen and other gaseous constituents of the atmosphere were thus shown to be of themselves incapable of inducing any organic development in yeast-water.

Such is a sample of the many well-devised experiments by which he carried to most minds the conviction that, as he expressed it, *la génération spontanée est une chimère*, and that the humblest and minutest living organisms can only originate by parentage from beings like themselves.

Pasteur pointed out the enormous importance of these humble organisms in the economy of nature. It is by their agency that the dead bodies of plants

and animals are resolved into simpler compounds fitted for assimilation by new living forms. Without their aid the world would be, as Pasteur said, *encombré de cadavres*. They are essential not only to our well-being, but to our very existence. Similar microbes must have discharged the same necessary function of removing refuse and providing food for successive generations of plants and animals during the past periods of the world's history; and it is interesting to think that organisms as simple as can well be conceived to have existed when life first appeared upon our globe have, in all probability, propagated the same lowly but most useful offspring during the ages of geological time.

Pasteur's labours on fermentation have had a very important influence upon surgery. I have been often asked to speak on my share in this matter before a public audience; but I have hitherto refused to do so, partly because the details are so entirely technical, but chiefly because I have felt an invincible repugnance to what might seem to savour of self-advertisement. The latter objection now no longer exists, since advancing years have indicated that it is right for me to leave to younger men the practice of my dearly loved profession. And it will perhaps be expected that, if I can make myself intelligible, I should say something upon the subject on the present occasion.

Nothing was formerly more striking in surgical experience than the difference in the behaviour of injuries according to whether the skin was implicated or not. Thus, if the bones of the leg were broken and the skin remained intact, the surgeon applied the necessary apparatus without any other anxiety than that of maintaining a good position of the fragments, although the internal injury to bones and soft parts might be very severe. If, on the other hand, a wound of the skin was present communicating with the broken bones, although the damage might be in other respects comparatively slight, the compound fracture, as it was termed, was one of the most dangerous accidents that could happen. Mr. Syme, who was, I believe, the safest surgeon of his time, once told me that he was inclined to think that it would be, on the whole, better if all compound fractures of the leg were subjected to amputation, without any attempt to save the limb. What was the cause of this astonishing difference? It was clearly in some way due to the exposure of the injured parts to the external world. One obvious effect of such exposure was indicated by the odour of the discharge, which showed that the blood in the wound had undergone putrefactive change by which the bland nutrient liquid had been converted into highly irritating and poisonous substances. I have seen a man with compound fracture of the leg die within two days of the accident, as plainly poisoned by the products of putrefaction as if he had taken a fatal dose of some potent toxic drug.

An external wound of the soft parts might be healed in one of two ways. If its surfaces were clean cut and could be brought into accurate apposition, it might unite rapidly and painlessly ' by the first intention '. This, however, was exceptional. Too often the surgeon's efforts to obtain primary union were frustrated : the wound inflamed and the retentive stitches had to be removed, allowing it to gape ; and then, as if it had been left open from the first, healing had to be effected in the other way, which it is necessary for me briefly to describe. An exposed raw surface became covered in the first instance with a layer of clotted blood or certain of its constituents, which invariably putrefied ; and the irritation of the sensitive tissues by the putrid products appeared to me to account sufficiently for the inflammation which always occurred in and around an open wound during the three or four days which elapsed before what were termed ' granulations ' had been produced. These constituted a coarsely granular coating of very imperfect or embryonic structure, destitute of sensory nerves and prone to throw off matter or pus, rather than absorb, as freshly divided tissues do, the products of putrefaction. The granulations thus formed a beautiful living plaster, which protected the sensitive parts beneath from irritation, and the system generally from poisoning and consequent febrile disturbance. The granulations had other useful properties, of which I may mention their tendency to shrink as they grew, thus gradually reducing the dimensions of the sore. Meanwhile, another cause of its diminution was in operation. The cells of the epidermis or scarf-skin of the cutaneous margins were perpetually producing a crop of young cells of similar nature, which gradually spread over the granulations till they covered them entirely, and a complete cicatrix or scar was the result. Such was the other mode of healing, that by granulation and cicatrization ; a process which, when it proceeded unchecked to its completion, commanded our profound admiration. It was, however, essentially tedious compared with primary union, while, as we have seen, it was always preceded by more or less inflammation and fever, sometimes very serious in their effects. It was also liable to unforeseen interruptions. The sore might become larger instead of smaller, cicatrization giving place to ulceration in one of its various forms, or even to the frightful destruction of tissue which, from the circumstance that it was most frequently met with in hospitals, was termed hospital gangrene. Other serious and often fatal complications might arise, which the surgeon could only regard as untoward accidents, and over which he had no efficient control.

It will be readily understood from the above description that the inflammation which so often frustrated the surgeon's endeavours after primary union was in my opinion essentially due to decomposition of blood within the wound.

These and many other considerations had long impressed me with the greatness of the evil of putrefaction in surgery. I had done my best to mitigate it by scrupulous ordinary cleanliness and the use of various deodorant lotions. But to prevent it altogether appeared hopeless while we believed with Liebig that its primary cause was the atmospheric oxygen which, in accordance with the researches of Graham, could not fail to be perpetually diffused through the porous dressings which were used to absorb the blood discharged from the wound. But when Pasteur had shown that putrefaction was a fermentation caused by the growth of microbes, and that these could not arise *de novo* in the decomposable substance, the problem assumed a more hopeful aspect. If the wound could be treated with some substance which, without doing too serious mischief to the human tissues, would kill the microbes already contained in it and prevent the future access of others in the living state, putrefaction might be prevented, however freely the air with its oxygen might enter. I had heard of carbolic acid as having a remarkable deodorizing effect upon sewage, and having obtained from my colleague, Dr. Anderson, Professor of Chemistry in the University of Glasgow, a sample which he had of this product, then little more than a chemical curiosity in Scotland, I determined to try it in compound fractures. Applying it undiluted to the wound, with an arrangement for its occasional renewal, I had the joy of seeing these formidable injuries follow the same safe and tranquil course as simple fractures, in which the skin remains unbroken

At the same time we had the intense interest of observing in open wounds what had previously been hidden from human view, the manner in which subcutaneous injuries are repaired. Of special interest was the process by which portions of tissue killed by the violence of the accident were disposed of, as contrasted with what had till then been invariably witnessed. Dead parts had been always seen to be gradually separated from the living by an inflammatory process and thrown off as sloughs. But when protected by the antiseptic dressing from becoming putrid and therefore irritating, a structure deprived of its life caused no disturbance in its vicinity; and, on the contrary, being of a nutritious nature, it served as pabulum for the growing elements of the neighbouring living structures, and these became in due time entirely substituted for it. Even dead bone was seen to be thus replaced by living osseous tissue.

This suggested the idea of using threads of dead animal structures for tying blood-vessels; and this was realized by means of catgut, which is made from the intestine of the sheep. If deprived of living microbes, and otherwise properly prepared, catgut answers its purpose completely: the knot holding

securely, while the ligature around the vessel becomes gradually absorbed and replaced by a ring of living tissue. The threads, instead of being left long as before, could now be cut short, and the tedious process of separation of the ligature, with its attendant serious danger of bleeding, was avoided.

Undiluted carbolic acid is a powerful caustic; and although it might be employed in compound fracture, where some loss of tissue was of little moment in comparison with the tremendous danger to be averted, it was altogether unsuitable for wounds made by the surgeon. It soon appeared, however, that the acid would answer the purpose aimed at, though used in diluted forms devoid of caustic action, and therefore applicable to operative surgery. According to our then existing knowledge, two essential points had to be aimed at: to conduct the operation so that on its completion the wound should contain no living microbes, and to apply a dressing capable of preventing the access of other living organisms till the time should have arrived for changing it.

Carbolic acid lent itself well to both these objects. Our experience with this agent brought out what was, I believe, a new principle in pharmacology—namely, that the energy of action of any substance upon the human tissues depends not only upon the proportion in which it is contained in the material used as a vehicle for its administration, but also upon the degree of tenacity with which it is held by its solvent. Water dissolves carbolic acid sparingly and holds it extremely lightly, leaving it free to act energetically on other things for which it has greater affinity, while various organic substances absorb it greedily and hold it tenaciously. Hence its watery solution seemed admirably suited for a detergent lotion to be used for destroying any microbes that might fall upon the wound during the operation, and for purifying the surrounding skin and also the surgeon's hands and instruments. For the last-named purpose it had the further advantage that it did not act on steel.

For an external dressing the watery solution was not adapted, as it soon lost the acid it contained, and was irritating while it lasted. For this purpose some organic substances were found to answer well. Large proportions of the acid could be blended with them in so bland a form as to be unirritating; and such mixtures, while perpetually giving off enough of the volatile salt to prevent organic development in the discharges that flowed past them, served as a reliable store of the antiseptic for days together.

The appliances which I first used for carrying out the antiseptic principle were both rude and needlessly complicated. The years that have since passed have witnessed great improvements in both respects. Of the various materials which have been employed by myself and others, and their modes of application, I need say nothing except to express my belief, as a matter of long experience,

that carbolic acid, by virtue of its powerful affinity for the epidermis and oily matters associated with it, and also its great penetrating power, is still the best agent at our disposal for purifying the skin around the wound. But I must say a few words regarding a most important simplification of our procedure. Pasteur, as we have seen, had shown that the air of every inhabited room teems with microbes ; and for a long time I employed various more or less elaborate precautions against the living atmospheric dust, not doubting that, as all wounds except the few which healed completely by the first intention, underwent putrefactive fermentation, the blood must be a peculiarly favourable soil for the growth of putrefactive microbes. But I afterwards learnt that such was by no means the case. I had performed many experiments in confirmation of Pasteur's germ theory, not indeed in order to satisfy myself of its truth, but in the hope of convincing others. I had observed that uncontaminated milk, which would remain unaltered for an indefinite time if protected from dust, was made to teem with microbes of different kinds by a very brief exposure to the atmosphere, and that the same effect was produced by the addition of a drop of ordinary water. But when I came to experiment with blood drawn with antiseptic precautions into sterilized vessels, I saw to my surprise that it might remain free from microbes in spite of similar access of air or treatment with water. I even found that if very putrid blood was largely diluted with sterilized water, so as to diffuse its microbes widely and wash them of their acrid products, a drop of such dilution added to pure blood might leave it unchanged for days at the temperature of the body, although a trace of the septic liquid undiluted caused intense putrefaction within twenty-four hours. Hence I was led to conclude that it was the grosser forms of septic mischief, rather than microbes in the attenuated condition in which they existed in the atmosphere, that we had to dread in surgical practice. And at the London Medical Congress in 1881, I hinted, when describing the experiments I have alluded to, that it might turn out possible to disregard altogether the atmospheric dust. But greatly as I should have rejoiced at such a simplification of our procedure, if justifiable, I did not then venture to test it in practice. I knew that with the safeguards which we then employed I could ensure the safety of my patients, and I did not dare to imperil it by relaxing them. There is one golden rule for all experiments upon our fellow men. Let the thing tried be that which, according to our best judgement, is the most likely to promote the welfare of the patient. In other words, Do as you would be done by.

Nine years later, however, at the Berlin Congress in 1890, I was able to bring forward what was, I believe, absolute demonstration of the harmlessness of

the atmospheric dust in surgical operations. This conclusion has been justified by subsequent experience : the irritation of the wound by antiseptic irrigation and washing may therefore now be avoided, and Nature left quite undisturbed to carry out her best methods of repair, while the surgeon may conduct his operations as simply as in former days, provided always that deeply impressed with the tremendous importance of his object, and inspiring the same conviction in all his assistants, he vigilantly maintains from first to last, with a care that, once learnt, becomes instinctive, but for the want of which nothing else can compensate, the use of the simple means which will suffice to exclude from the wound the coarser forms of septic impurity.

Even our earlier and ruder methods of carrying out the antiseptic principle soon produced a wonderful change in my surgical wards in the Glasgow Royal Infirmary, which, from being some of the most unhealthy in the kingdom, became, as I believe I may say without exaggeration, the healthiest in the world ; while other wards, separated from mine only by a passage a few feet broad, where former modes of treatment were for a while continued, retained their former insalubrity. This result, I need hardly remark, was not in any degree due to special skill on my part, but simply to the strenuous endeavour to carry out strictly what seemed to me a principle of supreme importance.

Equally striking changes were afterwards witnessed in other institutions. Of these I may give one example. In the great Allgemeines Krankenhaus of Munich, hospital gangrene had become more and more rife from year to year, till at length the frightful condition was reached that 80 per cent. of all wounds became affected by it. It is only just to the memory of Professor von Nussbaum, then the head of that establishment, to say that he had done his utmost to check this frightful scourge ; and that the evil was not caused by anything peculiar in his management was shown by the fact that in a private hospital under his care there was no unusual unhealthiness. The larger institution seemed to have become hopelessly infected, and the city authorities were contemplating its demolition and reconstruction. Under these circumstances, Professor von Nussbaum dispatched his chief assistant, Dr. Lindpaintner, to Edinburgh, where I at that time occupied the chair of clinical surgery, to learn the details of the antiseptic system as we then practised it. He remained until he had entirely mastered them, and after his return all the cases were on a certain day dressed on our plan. From that day forward not a single case of hospital gangrene occurred in the Krankenhaus. The fearful disease pyaemia likewise disappeared, and erysipelas soon followed its example.

But it was by no means only in removing the unhealthiness of hospitals that the antiseptic system showed its benefits. Inflammation being suppressed,

with attendant pain, fever, and wasting discharge, the sufferings of the patient were, of course, immensely lessened ; rapid primary union being now the rule, convalescence was correspondingly curtailed ; while as regards safety and the essential nature of the mode of repair, it became a matter of indifference whether the wound had clean-cut surfaces which could be closely approximated, or whether the injury inflicted had been such as to cause destruction of tissue. And operations which had been regarded from time immemorial as unjustifiable were adopted with complete safety.

It pleases me to think that there is an ever-increasing number of practitioners throughout the world to whom this will not appear the language of exaggeration. There are cases in which, from the situation of the part concerned or other unusual circumstances, it is impossible to carry out the antiseptic system completely. These, however, are quite exceptional ; and even in them much has been done to mitigate the evil which cannot be altogether avoided.

I ask your indulgence if I have seemed to dwell too long upon matters in which I have been personally concerned. I now gladly return to the labours of others.

The striking results of the application of the germ theory to surgery acted as a powerful stimulus to the investigation of the nature of the micro-organisms concerned ; and it soon appeared that putrefaction was by no means the only evil of microbic origin to which wounds were liable. I had myself very early noticed that hospital gangrene was not necessarily attended by any unpleasant odour ; and I afterwards made a similar observation regarding the matter formed in a remarkable epidemic of erysipelas in Edinburgh obviously of infective character. I had also seen a careless dressing followed by the occurrence of suppuration without putrefaction. And as these non-putrefactive disorders had the same self-propagating property as ferments, and were suppressed by the same antiseptic agencies which were used for combating the putrefactive microbes, I did not doubt that they were of an analogous origin ; and I ventured to express the view that, just as the various fermentations had each its special microbe, so it might be with the various complications of wounds. This surmise was afterwards amply verified. Professor Ogston, of Aberdeen, was an early worker in this field, and showed that in acute abscesses, that is to say those which run a rapid course, the matter, although often quite free from unpleasant odour, invariably contains micro-organisms belonging to the group which, from the spherical form of their elements, are termed micrococci ; and these he classed as streptococci or staphylococci, according as they were arranged in chains or disposed in irregular clusters like bunches of grapes. The German pathologist, Fehleisen, followed with a beautiful research, by which he clearly proved that

erysipelas is caused by a streptococcus. A host of earnest workers in different countries have cultivated the new science of bacteriology, and, while opening up a wide fresh domain of biology, have demonstrated in so many cases the causal relation between special micro-organisms and special diseases, not only in wounds but in the system generally, as to afford ample confirmation of the induction which had been made by Pasteur that all infective disorders are of microbic origin.

Not that we can look forward with anything like confidence to being able ever to see the *materies morbi* of every disease of this nature. One of the latest of such discoveries has been that by Pfeiffer of Berlin of the bacillus of influenza, perhaps the most minute of all micro-organisms ever yet detected. The bacillus of anthrax, the cause of a plague common among cattle in some parts of Europe, and often communicated to sorters of foreign wool in this country, is a giant as compared with this tiny being; and supposing the microbe of any infectious fever to be as much smaller than the influenza bacillus as this is less than that of anthrax, a by no means unlikely hypothesis, it is probable that it would never be visible to man. The improvements of the microscope, based on the principle established by my father in the earlier part of the century, have apparently nearly reached the limits of what is possible. But that such parasites are really the causes of all this great class of diseases can no longer be doubted.

The first rational step towards the prevention or cure of disease is to know its cause; and it is impossible to overestimate the practical value of researches such as those to which I am now referring. Among their many achievements is what may be fairly regarded as the most important discovery ever made in pathology, because it revealed the true nature of the disease which causes more sickness and death in the human race than any other. It was made by Robert Koch, who greatly distinguished himself, when a practitioner in an obscure town in Germany, by the remarkable combination of experimental acuteness and skill, chemical and optical knowledge and successful micro-photography, which he brought to bear upon the elucidation of infective diseases of wounds in the lower animals; in recognition of which service the enlightened Prussian Government at once appointed him to an official position of great importance in Berlin. There he conducted various important researches; and at the London Congress in 1881 he showed to us for the first time the bacillus of tubercle. Wonderful light was thrown by this discovery upon a great group of diseases which had before been rather guessed than known to be of allied nature; a precision and efficacy never before possible was introduced into their surgical treatment, while the physician became guided by new and sure light as regards their diagnosis and prevention.

At that same London Congress Koch demonstrated to us his ' plate culture '

of bacteria, which was so important that I must devote a few words to its description. With a view to the successful study of the habits and effects of any particular microbe outside the living body, it is essential that it should be present unmixed in the medium in which it is cultivated. It can be readily understood how difficult it must have been to isolate any particular micro-organism when it existed mixed, as was often the case, with a multitude of other forms. In fact, the various ingenious attempts made to effect this object had often proved entire failures. Koch, however, by an ingenious procedure converted what had been before impossible into a matter of the utmost facility. In the broth or other nutrient liquid which was to serve as food for the growing microbe he dissolved, by aid of heat, just enough gelatine to ensure that, while it should become a solid mass when cold, it should remain fluid though reduced in temperature so much as to be incapable of killing living germs. To the medium thus partially cooled was added some liquid containing, among others, the microbe to be investigated ; and the mixture was thoroughly shaken so as to diffuse the bacteria and separate them from each other. Some of the liquid was then poured out in a thin layer upon a glass plate and allowed to cool so as to assume the solid form. The various microbes, fixed in the gelatine and so prevented from intermingling, proceeded to develop each its special progeny, which in course of time showed itself as an opaque speck in the transparent film. Any one of such specks could now be removed and transferred to another vessel in which the microbe composing it grew in perfect isolation.

Pasteur was present at this demonstration, and expressed his sense of the great progress effected by the new method. It was soon introduced into his own institute and other laboratories throughout the world ; and it has immensely facilitated bacteriological study.

One fruit of it in Koch's own hands was the discovery of the microbe of cholera in India, whither he went to study the disease. This organism was termed by Koch from its curved form the ' comma bacillus ', and by the French the cholera vibrio. Great doubts were for a long time felt regarding this discovery. Several other kinds of bacteria were found of the same shape, some of them producing very similar appearances in culture media. But bacteriologists are now universally agreed that, although various other conditions are necessary to the production of an attack of cholera besides the mere presence of the vibrio, yet it is the essential *materies morbi* ; and it is by the aid of the diagnosis which its presence in any case of true cholera enables the bacteriologist to make, that threatened invasions of this awful disease have of late years been so successfully repelled from our shores. If bacteriology had done nothing more for us than this, it might well have earned our gratitude.

I have next to invite your attention to some earlier work of Pasteur. There is a disease known in France under the name of *choléra des poules*, which often produced great havoc among the poultry yards of Paris. It had been observed that the blood of birds that had died of this disease was peopled by a multitude of minute bacteria, not very dissimilar in form and size to the microbe of the lactic ferment to which I have before referred. And Pasteur found that, if this bacterium was cultivated outside the body for a protracted period under certain conditions, it underwent a remarkable diminution of its virulence ; so that, if inoculated into a healthy fowl, it no longer caused the death of the bird, as it would have done in its original condition, but produced a milder form of the disease which was not fatal. And this altered character of the microbe, caused by certain conditions, was found to persist in successive generations cultivated in the ordinary way. Thus was discovered the great fact of what Pasteur termed the *atténuation des virus*, which at once gave the clue to understanding what had before been quite mysterious, the difference in virulence of the same disease in different epidemics.

But he made the further very important observation that a bird which had gone through the mild form of the complaint had acquired immunity against it in its most virulent condition. Pasteur afterwards succeeded in obtaining mitigated varieties of microbes for some other diseases ; and he applied with great success the principle which he had discovered in fowl-cholera for protecting the larger domestic animals against the plague of anthrax. The preparations used for such preventive inoculations he termed ' vaccins ' in honour of our great countryman, Edward Jenner. For Pasteur at once saw the analogy between the immunity to fowl-cholera produced by its attenuated virus and the protection afforded against small-pox by vaccination. And while pathologists still hesitated, he had no doubt of the correctness of Jenner's expression *variolae vaccinae*, or small-pox in the cow.

It is just a hundred years since Jenner made the crucial experiment of inoculating with small-pox a boy whom he had previously vaccinated, the result being, as he anticipated, that the boy was quite unaffected. It may be remarked that this was a perfectly legitimate experiment, involving no danger to the subject of it. Inoculation was at that time the established practice ; and if vaccination should prove nugatory, the inoculation would be only what would have been otherwise called for ; while it would be perfectly harmless if the hoped-for effect of vaccination had been produced.

We are a practical people, not much addicted to personal commemorations, although our nation did indeed celebrate with fitting splendour the jubilee of the reign of our beloved Queen ; and at the invitation of Glasgow the scientific

world has lately marked in a manner, though different, as imposing, the jubilee of the life-work of a sovereign in science (Lord Kelvin). But while we cannot be astonished that the centenary of Jenner's immortal discovery should have failed to receive general recognition in this country, it is melancholy to think that this year should, in his native county, have been distinguished by a terrible illustration of the results which would sooner or later inevitably follow the general neglect of his prescriptions.

I have no desire to speak severely of the Gloucester Guardians. They are not sanitary authorities, and had not the technical knowledge necessary to enable them to judge between the teachings of true science and the declamations of misguided, though well-meaning, enthusiasts. They did what they believed to be right ; and when roused to a sense of the greatness of their mistake, they did their very best to repair it, so that their city is said to be now the best vaccinated in Her Majesty's dominions. But though by their praiseworthy exertions they succeeded in promptly checking the raging epidemic, they cannot recall the dead to life, or restore beauty to marred features, or sight to blinded eyes. Would that the entire country and our Legislature might take duly to heart this object-lesson!

How completely the medical profession were convinced of the efficacy of vaccination in the early part of this century was strikingly illustrated by an account given by Professor Crookshank, in his interesting history of this subject, of several eminent medical men in Edinburgh meeting to see the, to them, unprecedented fact of a vaccinated person having taken small-pox. It has, of course, since become well known that the milder form of the disease, as modified by passing through the cow, confers a less permanent protection than the original human disorder. This it was, of course, impossible for Jenner to foresee. It is, indeed, a question of degree, since a second attack of ordinary small-pox is occasionally known to occur, and vaccination, long after it has ceased to give perfect immunity, greatly modifies the character of the disorder and diminishes its danger. And, happily, in revaccination after a certain number of years we have the means of making Jenner's work complete. I understand that the majority of the Commissioners, who have recently issued their report upon this subject, while recognising the value and importance of revaccination, are so impressed with the difficulties that would attend making it compulsory by legislation that they do not recommend that course ; although it is advocated by two of their number who are of peculiarly high authority on such a question. I was lately told by a Berlin professor that no serious difficulty is experienced in carrying out the compulsory law that prevails in Germany. The masters of the schools are directed to ascertain in the case of every child attaining the age of twelve whether revaccination has been practised. If not, and the parents refuse to

have it done, they are fined one mark. If this does not prove effectual, the fine is doubled : and if even the double penalty should not prove efficacious, a second doubling of it would follow, but, as my informant remarked, it is very seldom that it is called for. The result is that small-pox is a matter of extreme rarity in that country ; while it is almost unknown in the huge German army, in consequence of the rule that every soldier is revaccinated on entering the service. Whatever view our Legislature may take on this question, one thing seems to me clear : that it will be the duty of Government to encourage by every available means the use of calf lymph, so as to exclude the possibility of the communication of any human disease to the child, and to institute such efficient inspection of vaccination institutes as shall ensure careful antiseptic arrangements, and so prevent contamination by extraneous microbes. If this were done, ' conscientious objections ' would cease to have any rational basis. At the same time, the administration of the regulations on vaccination should be transferred (as advised by the Commissioners) to competent sanitary authorities.

But to return to Pasteur. In 1880 he entered upon the study of that terrible but then most obscure disease, hydrophobia or rabies, which from its infective character he was sure must be of microbic origin, although no micro-organism could be detected in it. He early demonstrated the new pathological fact that the virus had its essential seat in the nervous system. This proved the key to his success in this subject. One result that flowed from it has been the cause of unspeakable consolation to many. The foolish practice is still too prevalent of killing the dog that has bitten any one, on the absurd notion that, if it were mad, its destruction would prevent the occurrence of hydrophobia in the person bitten. The idea of the bare possibility of the animal having been so affected causes an agony of suspense during the long weeks or months of possible incubation of the disease. Very serious nervous symptoms aping true hydrophobia have been known to result from the terror thus inspired. Pasteur showed that if a little of the brain or spinal cord of a dog that had been really mad was inoculated in an appropriate manner into a rabbit, it infallibly caused rabies in that animal in a few days. If, therefore, such an experiment was made with a negative result, the conclusion might be drawn with certainty that the dog had been healthy. It is perhaps right that I should say that the inoculation is painlessly done under an anaesthetic, and that in the rabbit rabies does not assume the violent form that it does in the dog, but produces gradual loss of power with little if any suffering.

This is the more satisfactory because rabbits in which the disease has been thus artificially induced are employed in carrying out what was Pasteur's greatest triumph, the preventive treatment of hydrophobia in the human subject. We

have seen that Pasteur discovered that microbes might under some circumstances undergo mitigation of their virulence. He afterwards found that under different conditions they might have it exalted, or, as he expressed it, there might be a *renforcement du virus*. Such proved to be the case with rabies in the rabbit; so that the spinal cords of animals which had died of it contained the poison in a highly intensified condition. But he also found that if such a highly virulent cord was suspended under strict antiseptic precautions in a dry atmosphere at a certain temperature, it gradually from day to day lost in potency, till in course of time it became absolutely inert. If now an emulsion of such a harmless cord was introduced under the skin of an animal, as in the subcutaneous administration of morphia, it might be followed without harm another day by a similar dose of a cord still rather poisonous; and so from day to day stronger and stronger injections might be used, the system becoming gradually accustomed to the poison, till a degree of virulence had been reached far exceeding that of the bite of a mad dog. When this had been attained, the animal proved incapable of taking the disease in the ordinary way; and more than that, if such treatment was adopted after an animal had already received the poison, provided that too long a time had not elapsed, the outbreak of the disease was prevented. It was only after great searching of heart that Pasteur, after consultation with some trusted medical friends, ventured upon trying this practice upon man. It has since been extensively adopted in various parts of the world with increasing success as the details of the method were improved. It is not of course the case that every one bitten by a really rabid animal takes the disease; but the percentage of those who do so, which was formerly large, has been reduced almost to zero by this treatment, if not too long delayed.

While the intensity of rabies in the rabbit is undoubtedly due to a peculiarly virulent form of the microbe concerned, we cannot suppose that the daily diminishing potency of the cord suspended in dry warm air is an instance of attenuation of virus, using the term 'virus' as synonymous with the microbe concerned. In other words, we have no reason to believe that the special micro-organism of hydrophobia continues to develop in the dead cord and produce successively a milder and milder progeny; since rabies cannot be cultivated in the nervous system of a dead animal. We must rather conclude that there must be some chemical poison present which gradually loses its potency as time passes. And this leads me to refer to another most important branch of this large subject of bacteriology, that of the poisonous products of microbes.

It was shown several years ago by Roux and Yersin, working in the *Institut Pasteur*, that the crust or false membrane which forms upon the throats of patients affected with diphtheria contains bacteria which can be cultivated outside

the body in a nutrient liquid, with the result that it acquires poisonous qualities of astonishing intensity, comparable to that of the secretion of the poison-glands of the most venomous serpents. And they also ascertained that the liquid retained this property after the microbes had been removed from it by filtration, which proved that the poison must be a chemical substance in solution, as distinguished from the living element which had produced it. These poisonous products of bacteria, or toxins as they have been termed, explain the deadly effects of some microbes, which it would otherwise be impossible to understand. Thus, in diphtheria itself, the special bacillus which was shown by Löffler to be its cause does not become propagated in the blood, like the microbe of chicken cholera, but remains confined to the surface on which it first appeared ; but the toxin which it secretes is absorbed from that surface into the blood, and so poisons the system. Similar observations have been made with regard to the microbes of some other diseases, as, for example, the bacillus of tetanus or lock-jaw. This remains localized in the wound, but forms a special toxin of extreme potency, which becomes absorbed and diffused through the body.

Wonderful as it seems, each poisonous microbe appears to form its own peculiar toxin. Koch's tuberculin was of this nature ; a product of the growth of the tubercle bacillus in culture media. Here, again, great effects were produced by extremely minute quantities of the substance ; but here a new peculiarity showed itself, viz. that patients affected with tubercular disease, in any of its varied forms, exhibited inflammation in the affected part and general fever after receiving under the skin an amount of the material which had no effect whatever upon healthy persons. I witnessed in Berlin some instance of these effects, which were simply astounding. Patients affected with a peculiar form of obstinate ulcer of the face showed, after a single injection of the tuberculin, violent inflammatory redness and swelling of the sore and surrounding skin ; and, what was equally surprising, when this disturbance subsided the disease was found to have undergone great improvement. By repetitions of such procedures, ulcers which had previously been steadily advancing, in spite of ordinary treatment, became greatly reduced in size, and in some instances apparently cured. Such results led Koch to believe that he had obtained an effectual means of dealing with tubercular disease in all its forms. Unhappily, the apparent cure proved to be only of transient duration, and the high hopes which had been inspired by Koch's great reputation were dashed. It is but fair to say that he was strongly urged to publish before he was himself disposed to do so, and we cannot but regret that he yielded to the pressure put upon him.

But though Koch's sanguine anticipations were not realized, it would be a great mistake to suppose that his labours with tuberculin have been fruitless.

Cattle are liable to tubercle, and, when affected with it, may become a very serious source of infection for human beings, more especially when the disease affects the udders of cows, and so contaminates the milk. By virtue of the close affinity that prevails between the lower animals and ourselves, in disease as well as in health, tuberculin produces fever in tubercular cows in doses which do not affect healthy beasts. Thus, by the subcutaneous use of a little of the fluid, tubercle latent in internal organs of an apparently healthy cow can be with certainty revealed, and the slaughter of the animal after this discovery protects man from infection.

It has been ascertained that glanders presents a precise analogy with tubercle as regards the effects of its toxic products. If the microbe which has been found to be the cause of this disease is cultivated in appropriate media, it produces a poison which has received the name of mallein, and the subcutaneous injection of a suitable dose of this fluid into a glandered horse causes striking febrile symptoms which do not occur in a healthy animal. Glanders, like tubercle, may exist in insidious latent forms which there was formerly no possibility of detecting, but which are at once disclosed by this means. If a glandered horse has been accidentally introduced into a large stable, this method of diagnosis surely tells if it has infected others. All receive a little mallein. Those which become affected with fever are slaughtered, and thus not only is the disease prevented from spreading to other horses, but the grooms are protected from a mortal disorder.

This valuable resource sprang from Koch's work on tuberculin, which has also indirectly done good in other ways. His distinguished pupil, Behring, has expressly attributed to those researches the inspiration of the work which led him and his since famous collaborateur, the Japanese Kitasato, to their surprising discovery of antitoxic serum. They found that if an animal of a species liable to diphtheria or tetanus received a quantity of the respective toxin, so small as to be harmless, and afterwards, at suitable intervals, successively stronger and stronger doses, the creature, in course of time, acquired such a tolerance for the poison as to be able to receive with impunity a quantity very much greater than would at the outset have proved fatal. So far, we have nothing more than seems to correspond with the effects of the increasingly potent cords in Pasteur's treatment of rabies. But what was entirely new in their results was that, if blood was drawn from an animal which had acquired this high degree of artificial immunity, and some of the clear fluid or serum which exuded from it after it had clotted was introduced under the skin of another animal, this second animal acquired a strong, though more transient, immunity against the particular toxin concerned. The serum in some way counteracted the toxin or was anti-toxic. But, more than that, if some of the antitoxic serum was applied to an

animal after it had already received a poisonous dose of the toxin, it preserved the life of the creature, provided that too long a time had not elapsed after the poison was introduced. In other words, the antitoxin proved to be not only preventive but curative.

Similar results were afterwards obtained by Ehrlich, of Berlin, with some poisons not of bacterial origin, but derived from the vegetable kingdom ; and quite recently the independent labours of Calmette of Lille, and Fraser of Edinburgh, have shown that antidotes of wonderful efficacy against the venom of serpents may be procured on the same principle. Calmette has obtained antitoxin so powerful that a quantity of it only a 200,000th part of the weight of an animal will protect it perfectly against a dose of the secretion of the poison-glands of the most venomous serpents known to exist, which without such protection would have proved fatal in four hours. For curative purposes larger quantities of the remedy are required, but cases have been already published by Calmette in which death appears to have been averted in the human subject by this treatment.

Behring's darling object was to discover means of curing tetanus and diphtheria in man. In tetanus the conditions are not favourable ; because the specific bacilli lurk in the depths of the wound, and only declare their presence by symptoms caused by their toxin having been already in a greater or less amount diffused through the system ; and in every case of this disease there must be a fear that the antidote may be applied too late to be useful. But in diphtheria the bacilli very early manifest their presence by the false membrane which they cause upon the throat, so that the antitoxin has a fair chance ; and here we are justified in saying that Behring's object has been attained.

The problem, however, was by no means so simple as in the case of some mere chemical poison. However effectual the antitoxin might be against the toxin, if it left the bacilli intact, not only would repeated injections be required to maintain the transient immunity to the poison perpetually secreted by the microbes, but the bacilli might by their growth and extension cause obstruction of the respiratory passages.

Roux, however, whose name must always be mentioned with honour in relation to this subject, effectually disposed of this difficulty. He showed by experiments on animals that a diphtheritic false membrane, rapidly extending and accompanied by surrounding inflammation, was brought to a stand by the use of the antitoxin, and soon dropped off, leaving a healthy surface. Whatever be the explanation, the fact was thus established that the antitoxic serum, while it renders the toxin harmless, causes the microbe to languish and disappear.

No theoretical objection could now be urged against the treatment ; and it has during the last two years been extensively tested in practice in various parts

of the world, and it has gradually made its way more and more into the confidence of the profession. One important piece of evidence in its favour in this country is derived from the report of the six large hospitals under the management of the London Asylums Board. The medical officers of these hospitals at first naturally regarded the practice with scepticism: but as it appeared to be at least harmless, they gave it a trial; and during the year 1895 it was very generally employed upon the 2,182 cases admitted; and they have all become convinced of its great value. In the nature of things, if the theory of the treatment is correct, the best results must be obtained when the patients are admitted at an early stage of the attack, before there has been time for much poisoning of the system; and accordingly we learn from the report that, comparing 1895 with 1894, during which latter year the ordinary treatment had been used, the percentage of mortality, in all the six hospitals combined, among the patients admitted on the first day of the disease, which in 1894 was 22·5, was only 4·6 in 1895; and for those admitted on the second day the numbers are 27 for 1894 and 14·8 for 1895. Thus for cases admitted on the first day the mortality was only one-fifth of what it was in the previous year, and for those entering on the second it was halved. Unfortunately in the low parts of London which furnish most of these patients the parents too often delay sending in the children till much later; so that on the average no less than 67·5 per cent. were admitted on the fourth day of the disease or later. Hence the aggregate statistics of all cases are not nearly so striking. Nevertheless, taking it altogether, the mortality in 1895 was less than had ever before been experienced in those hospitals. I should add that there was no reason to think that the disease was of a milder type than usual in 1895; and no change whatever was made in the treatment except as regards the antitoxic injections.

There is one piece of evidence recorded in the report which, though it is not concerned with high numbers, is well worthy of notice. It relates to a special institution to which convalescents from scarlet fever are sent from all the six hospitals. Such patients occasionally contract diphtheria, and when they do so the added disease has generally proved extremely fatal. In the five years preceding the introduction of the treatment with antitoxin the mortality from this cause had never been less than 50 per cent., and averaged on the whole 61·9 per cent. During 1895, under antitoxin, the deaths among the 119 patients of this class were only 7·5 per cent., or one-eighth of what had been previously experienced. This very striking result seems to be naturally explained by the fact that these patients being already in hospital when the diphtheria appeared, an unusually early opportunity was afforded for dealing with it.

There are certain cases of so malignant a character from the first that no

treatment will probably ever be able to cope with them. But taking all cases together, it seems probable that Behring's hope that the mortality may be reduced to 5 per cent. will be fully realized when the public become alive to the paramount importance of having the treatment commenced at the outset of the disease.

There are many able workers in the field of bacteriology whose names time does not permit me to mention, and to whose important labours I cannot refer ; and even those researches of which I have spoken have been, of course, most inadequately dealt with. I feel this especially with regard to Pasteur, whose work shines out more brightly the more his writings are perused.

I have lastly to bring before you a subject which, though not bacteriological, has intimate relations with bacteria. If a drop of blood is drawn from the finger by a prick with a needle and examined microscopically between two plates of glass, there are seen in it minute solid elements of two kinds, the one pale orange bi-concave discs, which, seen in mass, give the red colour to the vital fluid, the other more or less granular spherical masses of the soft material called protoplasm, destitute of colour, and therefore called the colourless or white corpuscles. It has been long known that if the microscope was placed at such a distance from a fire as to have the temperature of the human body, the white corpuscles might be seen to put out and retract little processes or pseudopodia, and by their means crawl over the surface of the glass, just like the extremely low forms of animal life termed, from this faculty of changing their form, amoebae. It was a somewhat weird spectacle, that of seeing what had just before been coursing through our veins moving about like independent creatures. Yet there was nothing in this inconsistent with what we knew of the fixed components of the animal frame. For example, the surface of a frog's tongue is covered with a layer of cells, each of which is provided with two or more lashing fi'aments or cilia, and those of all the cells acting in concert cause a constant flow of fluid in a definite direction over the organ. If we gently scrape the surface of the animal's tongue, we can detach some of these ciliated cells ; and on examining them with the microscope in a drop of water, we find that they will continue for an indefinite time their lashing movements, which are just as much living or vital in their character as the writhings of a worm. And, as I observed many years ago, these detached cells behave under the influence of a stimulus just like parts connected with the body, the movements of the cilia being excited to greater activity by gentle stimulation, and thrown into a state of temporary inactivity when the irritation was more severe. Thus each constituent element of our bodies may be regarded as in one sense an independent living being, though all work together in marvellous harmony for the good of the body politic. The independent move-

ments of the white corpuscles outside the body were therefore not astonishing ; but they long remained matters of mere curiosity. Much interest was called to them by the observation of the German pathologist Cohnheim, that in some inflammatory conditions they passed through the pores in the walls of the finest blood-vessels, and thus escaped into the interstices of the surrounding tissues. Cohnheim attributed their transit to the pressure of the blood. But why it was that, though larger than the red corpuscles, and containing a nucleus which the red ones have not, they alone passed through the pores of the vessels, or why it was that this emigration of the white corpuscles occurred abundantly in some inflammations and was absent in others, was quite unexplained.

These white corpuscles, however, have been invested with extraordinary new interest by the researches of the Russian naturalist and pathologist, Metchnikoff. He observed that, after passing through the walls of the vessels, they not only crawl about like amoebae, but, like them, receive nutritious materials into their soft bodies and digest them. It is thus that the effete materials of a tadpole's tail are got rid of ; so that they play a most important part in the function of absorption.

But still more interesting observations followed. He found that a microscopic crustacean, a kind of water-flea, was liable to be infested by a fungus which had exceedingly sharp-pointed spores. These were apt to penetrate the coats of the creature's intestine, and project into its body-cavity. No sooner did this occur with any spore than it became surrounded by a group of the cells which are contained in the cavity of the body and correspond to the white corpuscles of our blood. These proceeded to attempt to devour the spore ; and if they succeeded, in every such case the animal was saved from the invasion of the parasite. But if the spores were more than could be disposed of by the devouring cells (phagocytes, as Metchnikoff termed them), the water-flea succumbed.

Starting from this fundamental observation, he ascertained that the microbes of infective diseases are subject to this same process of devouring and digestion, carried on both by the white corpuscles and by cells that line the blood-vessels. And by a long series of most beautiful researches he has, as it appears to me, firmly established the great truth that phagocytosis is the main defensive means possessed by the living body against the invasions of its microscopic foes. The power of the system to produce antitoxic substances to counteract the poisons of microbes is undoubtedly in its own place of great importance. But in the large class of cases in which animals are naturally refractory to particular infective diseases the blood is not found to yield any antitoxic element by which the natural immunity can be accounted for. Here phagocytosis seems to be the sole defensive agency. And even in cases in which the serum does possess antitoxic, or, as it would seem in some cases, germicidal properties, the bodies of the dead

microbes must at last be got rid of by phagocytosis, and some recent observations would seem to indicate that the useful elements of the serum may be, in part at least, derived from the digestive juices of the phagocytes. If ever there was a romantic chapter in pathology, it has surely been that of the story of phagocytosis.

I was myself peculiarly interested by these observations of Metchnikoff's, because they seemed to me to afford clear explanation of the healing of wounds by first intention under circumstances before incomprehensible. Complete primary union was sometimes seen to take place in wounds treated with water dressing, that is to say, a piece of wet lint covered with a layer of oiled silk to keep it moist. This, though cleanly when applied, was invariably putrid within twenty-four hours. The layer of blood between the cut surfaces was thus exposed at the outlet of the wound to a most potent septic focus. How was it prevented from putrefying, as it would have done under such influence if, instead of being between divided living tissues, it had been between plates of glass or other indifferent material? Pasteur's observations pushed the question a step further. It now was, How were the bacteria of putrefaction kept from propagating in the decomposable film? Metchnikoff's phagocytosis supplied the answer. The blood between the lips of the wound became rapidly peopled with phagocytes, which kept guard against the putrefactive microbes and seized them as they endeavoured to enter.

If phagocytosis was ever able to cope with septic microbes in so concentrated and intense a form, it could hardly fail to deal effectually with them in the very mitigated condition in which they are present in the air. We are thus strongly confirmed in our conclusion that the atmospheric dust may safely be disregarded in our operations; and Metchnikoff's researches, while they have illumined the whole pathology of infective diseases, have beautifully completed the theory of antiseptic treatment in surgery.

I might have taken equally striking illustrations of my theme from other departments in which microbes play no part. In fact any attempt to speak of all that the art of healing has borrowed from science and contributed to it during the past half-century would involve a very extensive dissertation on pathology and therapeutics. I have culled specimens from a wide field; and I only hope that in bringing them before you I have not overstepped the bounds of what is fitting before a mixed company. For many of you my remarks can have had little if any novelty: for others they may perhaps possess some interest as showing that Medicine is no unworthy ally of the British Association—that, while her practice is ever more and more based on science, the ceaseless efforts of her votaries to improve what have been fittingly designated *Quae prosunt omnibus artes*, are ever adding largely to the sum of abstract knowledge.

THE THIRD HUXLEY LECTURE[1]

Delivered before the Medical School of Charing Cross Hospital, on October 2, 1900.

When the Council of Charing Cross Hospital did me the great honour of asking me to deliver the third of the lectures instituted by them in memory of Huxley, the illustrious former pupil of their school, at the same time conveying their desire that the subject of it should be my own work, I at first reluctantly declined, on the ground that what I had done had been for the most part already published. But when the Dean, who assured me that he expressed the unanimous wish of his colleagues, urged me to reconsider my decision, I felt unable to refuse compliance with a request so very kindly made. It also occurred to me that, as my papers are scattered through a variety of media of publication, extending over a pretty long period, the earlier ones especially being probably little known to the present generation, it might perhaps be not without interest for me to refer on this occasion to some of the more salient of such observations as bear more or less directly upon the antiseptic system of surgery, while I should at the same time be complying with a wish that has been expressed that I should give some indication of the circumstances that led me to that subject.

As a student at University College I was greatly attracted by Dr. Sharpey's lectures, which inspired me with a love of physiology that has never left me. My father, whose labours (vide 'On the Improvement of Achromatic Compound Microscopes', by J. J. Lister, Esq., *Phil. Trans.*, 1830) had raised the compound microscope from little better than a scientific toy to the powerful engine for investigation which it then already was, had equipped me with a first-rate instrument of that kind, and I employed it with keen interest in verifying the details of histology brought before us by our great master. When I afterwards became house surgeon under Mr. Erichsen, I applied the same means of observation to pathological objects.

One of the earliest records that I find of such work is in the form of sketches of the corpuscles in the pus in a case of pyaemia, which occurred after excision of the elbow in a little boy. The cancellated tissue of the humerus at the seat of operation and the adjacent part of the medullary cavity were seen, on post mortem examination, to be occupied by thick, yellow pus, and similar fluid distended the brachial and axillary veins and their branches,

[1] This lecture was published in the *British Medical Journal* of October 6, 1900. It was reprinted with corrections, in a volume published in February 1907.

including not only those leading from the bone towards the venous trunks, but also those proceeding from other parts of the limb, while the upper part of the axillary was plugged with a firm adhering clot. There was also suppuration in one knee-joint and multiple abscesses in the lungs. I was struck with the fact that the pus was to be found not only in the course of the channels leading from the original seat of mischief to the main trunk, but also in branches along which it must have advanced in the reverse direction in spite of the valves of the veins. The plugging of the axillary seemed also a very noteworthy circumstance. Sédillot had shown that multiple abscesses in the lungs were caused by introducing pus into the veins of an animal; and it seemed probable that the collections of pus in those organs in the present case had been of similar metastatic origin. Yet the plugging of the axillary, shutting off the pus in the veins from the general circulation, seemed inconsistent with such a view. I took careful camera-lucida sketches of the constituents of the pus from the various situations in which it occurred; and I also made a record of the magnifying power employed, by sketching with the camera the scale of a micrometer placed upon the stage of the microscope. And I would venture to recommend this practice strongly to pathologists. The sketch which I then made is as valuable to me to-day as if it had been made yesterday. I see from my drawing what I noted at the time, that the solid constituents of the pus were in no case pus corpuscles such as we then knew them, and I also see that they were not leucocytes. I could not explain at the time the facts that I observed, but subsequent investigation has, I believe, made them intelligible.

An epidemic, as we termed it, of hospital gangrene occurred during my house-surgeoncy, and I was charged with carrying out the treatment. This consisted in scraping away very thoroughly under chloroform the brown pultaceous slough and freely applying acid pernitrate of mercury to the exposed surface. The result was, as a rule, that, when the eschar caused by the powerful caustic separated under poulticing, a perfectly healthy granulating sore was disclosed which healed kindly under ordinary dressings. The only exception to this rule was in the case of a very stout woman, in whom the disease attacked an enormous wound of the forearm caused by an accident which had raised a very large flap of skin. In that case the caustic application removed indeed the pain and the extensive inflammatory blush; but when the slough separated, a small brown spot was seen at one place among the otherwise healthy granulations, and this spread with astonishing rapidity over the entire sore. The treatment was tried again and again with the same result, till, the deep structures of the limb having become seriously involved, Mr. Erichsen

resolved to amputate. On the evening before the day for the operation I again put the patient under chloroform, and, after scraping the sore very thoroughly, allowed the liquid caustic to lie in pools upon it for a quarter of an hour in order to destroy as effectually as possible all material in the sore which might otherwise infect the amputation wound. With a similar object I washed the skin of the limb thoroughly with soap and water, including the shoulder, where it had been decided to perform the amputation. The limb having been removed next day, the stump healed perfectly kindly. Here, as in the other cases, local treatment proved efficacious.

I was greatly struck with the clear evidence which these cases seemed to afford that the disease was of the nature of a purely local poison. In the hope of discovering its nature I examined microscopically the slough from one of the sores, and I made a sketch of some bodies of pretty uniform size which I imagined might be the *materies morbi* in the shape of some kind of fungus. Thus as regards that form of hospital disease, the idea that it was probably of parasitic nature was at that early period already present to my mind.

On visiting Edinburgh by Dr. Sharpey's advice in order to see something of Mr. Syme's practice, I was fascinated by the prominence that he gave to the pathological side of surgery as well as by his rare diagnostic judgement and his surpassing powers as an operator. Under him I had the unexpected great privilege of a second house-surgeoncy, which extended over upwards of a year, and in the great Royal Infirmary I had ample opportunity for observing the behaviour of wounds under the most varied conditions. I was charmed with the superiority of the treatment of recent wounds which I witnessed there over the 'water dressing' which was used at University College after the precepts of Liston, who introduced it in place of what he termed 'filthy unguents'. Water dressing, though cleanly when applied, was invariably putrid within twenty-four hours, and had to be changed daily. Mr. Syme placed pads of dry lint upon the bodies of the flaps, leaving the lips of the wound free for the escape of blood and serum, covering all with a single layer of dry lint and a retaining bandage which gently pressed the cut surfaces together. This dressing was left untouched for four days, during which union by the first intention proceeded undisturbed except in the track of the ligatures upon the blood-vessels, while the discharge found on changing the dressing was scanty and not specially offensive.

But highly successful as this practice was, it could not be continued in the further progress of the case. The ligatures were separated by a process of suppuration, which, even when the tissues had been healthy at the time of operation, became fully established in four days at the latest. The ligatures,

on the other hand, were not fully detached till a later and variable period ; and so long as they remained they perpetuated the formation of pus in the depths of the wound, the retention of which by a dry dressing long continued would have involved disastrous consequences.

Thus, under the best possible management which the knowledge of those days permitted, suppuration was an inevitable attendant on nearly every wound ; and so long as it continued there was no security against the advent of one of the various specially unhealthy conditions, then quite inexplicable, which might ruin the results of the most beautifully planned and executed operations.

The very liberal regulations of the University and College of Surgeons of Edinburgh enabled me, on the expiry of my house-surgeoncy at the infirmary, to start a course of lectures on surgery, qualifying for the examinations of both bodies. The first subject with which I should have to deal was inflammation. The stasis of the blood in the capillaries, as the result of irritating applications, had been long studied in the transparent web of the frog's foot ; and Paget had described similar phenomena in the wing of the mammalian bat. The latest contribution to the subject had been made by Wharton Jones, one of my former teachers at University College, who had received the Astley Cooper Prize for an essay in which observations were recorded leading him to the conclusion that the cause of the arrest of the red corpuscles in the capillaries of an inflamed part was contraction of the arterioles. According to this view, which he supported by very neatly executed experiments, the narrowing of the tubes of supply caused sluggishness of flow in the fields of capillaries supplied by them, and this permitted the red discs to aggregate and so obstruct the channels.

There could be no more doubt of the trustworthiness of Wharton Jones's observations than of the beauty of the drawings with which he illustrated them. But their relation to inflammatory stasis was not so clear ; and I sought further light upon the subject by investigations of my own. My first attempt in this way may be described somewhat in detail. It occurred to me that it would be interesting, instead of the powerful irritants which had been usually applied in such investigations, to try warm water, the mildest of all stimulants to the human body. Having fixed a young frog upon a plate of glass on the stage of a microscope tilted at an angle of about 45°, one of the webs being extended in the field of view, I watched the effect of throwing a few drops of warm water upon the web by means of a syringe. The application of the water was little more than momentary ; and as it flowed off immediately from the sloping surface, I could at once observe the result. This filled me with astonishment, and at first I could not understand what I saw. All appearance of blood-

vessels—arteries, capillaries, and veins—had disappeared; the field being absolutely exsanguine. In a short time the circulation was resumed with greater freedom than ever; and on repeating the experiment I found that the first effect of the stimulus was a state of extreme constriction of the arterioles, which kept back the blood-corpuscles but allowed the liquor sanguinis to pass; so that the capillaries and veins, though retaining their former dimensions, were occupied only by the filtered plasma, itself invisible, while their walls were with difficulty discernible under the low magnifying power that I was using.

Thus was swept away at one stroke the latest theory upon the subject. The condition of contraction of the arterioles, which Wharton Jones had supposed to be the cause of the accumulation of the red corpuscles in the capillaries, had been present in the most perfect conceivable form; but the result had been the very opposite condition.

The explanation of Wharton Jones's mistake became apparent as I proceeded along the path which opened with so much promise. He had never experimented in a perfectly healthy state of the circulation, but had described with great accuracy what could occur only under morbid conditions. For I afterwards learned that the normal temperature of man is deadly to the cold-blooded frog. That animal, which under ordinary conditions exhibits very remarkable persistence of vitality even after somatic death, is killed by being held for about a quarter of an hour in the hand; and if one of its hind feet be similarly warmed, the blood-corpuscles will be found packed and stagnant in the vessels of the webs, as if mustard or any other powerful irritant had been applied to them.

If, on the other hand, in securing the frog for observation under the microscope, scrupulous care was taken to avoid needless exposure of the foot to the warmth of the hands, the threads for fixing the toes being tied by means of long forceps, and each half of the knot done separately, with a fair interval between them, a state of the circulation was seen which is, I believe, even to this day rarely witnessed. The white corpuscles, instead of trailing, more or less sluggishly, along the walls of the venous radicles—the normal condition, according to some modern textbooks—move freely along among the red discs, and these being diffused through a due proportion of liquor sanguinis, the vessels present a pallor which would surprise any one who had seen only the ordinary demonstrations of the circulation, but which might have been anticipated from the appearance, when in health, of the highly vascular sclerotic with its investing conjunctiva, 'the white of the eye'.

Such a method of arranging the foot could not be carried out if the animal

were able to struggle ; but this was effectually prevented in the following way : The frog, wrapped in cold, wet lint, is held in the left hand, and the head, left exposed for the purpose, is depressed with the forefinger so as to stretch the ligament between the occiput and the first vertebra. The junction between the brain and spinal cord is then divided with a tenotome, after which the creature remains perfectly passive as long as may be desired. Comparatively dull though we know sensibility to be in an animal so low in the scale as the frog, it is a comfort to feel that this method must be attended with exceedingly little pain. That caused by the division of the cord is probably almost as momentary as the stroke of the tenotome ; and sensibility as well as motion being abolished in the limbs, the creature cannot feel the tying of the naturally sensitive toes or the subsequent dragging upon them.

This arrangement had the further great advantage of allowing an irritant, even in the form of a drop of liquid, to remain undisturbed at the particular spot to which it was applied, instead of being diffused over the whole web by the movements of the limb. Under these circumstances the highly interesting fact was disclosed that, while the web generally was affected through the nervous system with active congestion, that is to say, with arterial dilatation and consequent very free flow of blood, the characteristic stasis was limited to the area on which the irritant acted directly. In spite of the widening of the tubes of supply, the blood-corpuscles tended to lag more and more behind the liquor sanguinis, till at length complete stagnation occurred. The obstacle to the onward movement of the red discs seemed to be caused by adhesiveness on their part. On careful examination, individual discs were sometimes seen attached to the walls of the vessels. The white corpuscles also showed a tendency to adhere to each other and to the vascular parietes ; and this was seen in all degrees, from the disposition to trail along the venous radicles, before referred to as occurring under slight irritation, to piling up of colourless granular masses of leucocytes large enough to block a venous radicle.[1]

These appearances of the blood-corpuscles in the irritated area were such as were seen in blood examined outside the body between two plates of glass. I had observed similar granular masses of white corpuscles in blood from my own finger, as well as individual leucocytes adhering to the surface of the glass, along which, as has been since observed, they crawl by amoeboid movements.

In the red corpuscles the tendency to mutual adhesion shows itself in

[1] The accumulation of the white corpuscles in the vessels of an inflamed frog's web was described in 1841 by Dr. William Addison and Dr. C. J. B. Williams independently in the *Medical Gazette* of that year.

different forms according to the species of the animal or its state of health. In the frog the prominence of the nuclei leads to very irregular grouping of the oval cells. In man the biconcave circular discs adhere under normal circumstances in that position which enables their moderate degree of adhesiveness to come best into play, the result being the well-known 'rouleaux'. The same is seen in the healthy blood of the cow. But in some animals, e.g. the horse, the adhesiveness of the discs is so great that they stick to one another by the parts that come first into contact, producing dense spherical masses large enough to be visible to the naked eye, like grains of red sand. These, falling rapidly through the lighter plasma, leave the upper part of the liquid free from red corpuscles before coagulation occurs, thus giving rise to the buffy coat, whereas in the cow the delicate network of rouleaux remains suspended, and no buff occurs.

I am greatly surprised to learn that the cause of the buffy coat is stated in some textbooks to be slowness of coagulation. Special slowness of coagulation does not occur in buffing blood; nor, if it did, would it explain the phenomenon. In whipped horse's blood the red discs aggregate into dense masses as in blood freshly drawn, and falling rapidly soon leave a deep layer of serum. In whipped cow's blood, rouleaux forming in the serum as in the plasma, there will be found, if the animal was healthy, only a thin superficial serous layer, even after a lapse of twenty-four hours.

I once drew blood from a donkey into two similar glass vessels, one empty, the other half-full of water. The diluted blood and the undiluted clotted in exactly the same time. But whereas in the normal blood there happened to be an unusually thick layer of buff, comprising nearly two-thirds of the whole mass, the watered blood gave no buff, and the microscope showed that the red corpuscles had lost their natural adhesiveness.

Human blood, as is well known, shows the buffy coat in some states of inflammation. But it may also occur in anaemia.[1] And it may well make our profession humble to reflect that in days within living memory buffing of the clot was regarded as an indication for further withdrawal of the vital fluid by venesection.

To return from this digression: adhesiveness of the corpuscles, both red and white, was seen in the vessels of an irritated area of the frog's web, as in blood outside the body. But in a perfectly healthy part no such condition was observed. A string tied round a frog's thigh of course made the blood in the vessels of the foot motionless; but on the slightest touch of the web the

[1] In the only case of anaemia in which I examined the blood microscopically I found the red discs extremely adhesive.

corpuscles, both red and white, moved along with the plasma with the most perfect freedom.

But I was not altogether satisfied with this evidence of their entire absence of adhesiveness within healthy vessels, because the aggregation of the red discs in the frog is of a somewhat indefinite character. I therefore sought further light upon the point in the mammalian bat. Having placed one under chloroform and extended one of its wings under the microscope, I temporarily arrested the circulation by compressing the main vessels of the limb ; and on examining one of the veins I was much disappointed to see the red corpuscles of its contained blood aggregated. It seemed possible, however, that the part of the membrane which I was examining might be suffering mechanical irritation from pressure between the glass slide on which it rested and the cover-glass which it was necessary to use with the high magnifying power required for the bat's wing. For those were not the days of immersion lenses. I therefore made arrangements to guard against the possibility of such an occurrence ; and now, to my great joy, I beheld the red corpuscles, which lay motionless in a considerable venous channel, distributed uniformly through the plasma, without the slightest appearance of aggregation.

The animal having been killed immediately afterwards, I examined a drop of blood from its heart. The contrast with what I had seen in the healthy living vessel was most striking ; the red corpuscles presenting a degree of adhesiveness such as I had never before seen equalled, whether outside the body or within the vessels. When forced to separate from each other by pressure made upon the cover-glass, they became drawn out like threads of a viscid liquid before becoming completely detached. The animal had been suffering from a bad compound fracture in one of the wings. Whether the great adhesiveness of the red discs of the shed blood was due to inflammation caused by the injury, or whether such a condition is normal to the bat, as it is to the horse and the ass, I do not know.

By such facts it seemed to be established that the stasis of the blood in an irritated area, that is to say, the accumulation of the blood-corpuscles, both red and white, in the vessels of that area, is due to a tendency on their part to adhere to each other and to the walls of the vessels ; that they do this by virtue of an adhesiveness or viscidity which they do not manifest at all within the vessels of a perfectly healthy part, and which, while varying in degree with the severity of the irritation, never seems to exceed that which is observed in blood outside the body.[1]

What was it that induced the blood corpuscles to assume this adhesiveness

[1] Vide *Phil. Trans.*, 1858 (see vol. i, p. 217).

under irritation ? It was clearly not the result of direct action of the irritant upon them. When the inflammatory congestion, as I ventured to term it, was not carried to its extreme degree, the corpuscles, though closely packed, still moving sluggishly along, successive portions of blood, as they passed through the affected spot, were successively affected in the same way, it might be for hours after the irritant had ceased to act. And some of the agencies which produced the effect, such as gentle warmth and mechanical disturbance in the shape of moderate pressure, were not of a nature to act chemically upon the blood-cells, and could not possibly leave behind them among the tissues any active substance.

The tissues, as distinguished from the blood, were therefore the parts primarily and essentially affected by the action of the irritant. And we have seen that, in their relation to the blood-corpuscles, they approached more or less closely, according to the degree of the irritation, the behaviour of ordinary solids, such as glass, as distinguished from the living structures. The natural inference was that they had lost more or less, for the time being, certain special properties which they possessed when in active health as constituent structures of the living body. In other words, certain of their vital functions were temporarily in abeyance. I say temporarily because the extreme degree of inflammatory congestion, in which the capillaries appear as homogeneous scarlet threads of densely packed red discs, is susceptible of complete recovery by resolution if the action of the irritant has not been pushed too far.

The same conclusion followed naturally from a consideration of the properties of irritants. Greatly as they differ in their nature, whether physical, as mechanical violence, heat, and the electric shock, or chemical of the most varied characters, one feature they have all in common ; if pushed far enough they destroy the tissues on which they act. Extreme inflammatory congestion is the state which they produce when their action is just short of the lethal degree ; and it could hardly be doubted that the state of the tissues just short of death must be one of impairment of vital power.

This view was beautifully confirmed by a series of observations to which I was led by a most unlooked-for experience. Before I had adopted the method, which I have described, of obtaining a perfectly tranquil state of the frog's foot, I sought to study the local effect of an irritant by placing on the middle of the web a small piece of moistened mustard, which could not be shifted in position like a drop of liquid when the animal struggled. On removing the mustard after a while to observe its effects, I was astonished to see the part of the web on which it had lain, not only affected with inflammatory congestion, but totally different in colour from the rest of the web in consequence of a dif-

ference of arrangement of its pigmentary constituents. Where the mustard had rested, the pigment appeared as a delicate black network among the tissues, causing an extremely dark appearance ; whereas in the rest of the web it was gathered into small round spots, which interfered little with the pallor of the other structures. I at once saw that I had before me direct ocular evidence of an effect of the irritant upon the tissues. The circumstance that I had applied the mustard to one spot only of the web, had revealed what had escaped the notice of the many previous observers who had studied the circulation in the frog. I afterwards learned that changes of colour due to pigmentary variation had been observed in Germany in the green tree-frog by Von Wittich, who had attributed them to contractions and relaxations of chromatophorous cells, more or less analogous to what is seen on a large scale, visible to the naked eye, in the skin of cephalopods. Very different were the real pigmentary functions in the frog. The colouring matter, which was in the form of granules of extreme minuteness, was contained in cells with offsets that rapidly broke up into ramifications of exquisite delicacy, anastomosing freely with each other and with those of other branches and of neighbouring cells, only visible when the frog was at the darkest, when they appeared, under the highest magnifying power at my disposal (a fine $\frac{1}{12}$ of Powell and Lealand's), as fine homogeneous black lines, in which the closely packed granules were not individually discernible. Under these circumstances the bodies of the cells and their principal offsets were so cleared of pigment as to be almost colourless, so that it was difficult to define their contour.

On the other hand, when the animal was at the palest, the pigment-granules were massed together into a circular disc, which did not occupy the whole of the body of the cell, being apparently grouped round its nucleus, while the offsets and their ramifications were quite colourless. Any intermediate degree between these extremes of complete diffusion and perfect concentration of the pigment-granules might occur, with corresponding differences in the tint of the animal.

Camera-lucida sketching here stood me in good stead. I doubt if any one would have credited my description had I not been able to support it by such evidence. For here was a function entirely new to physiologists. In muscular contraction the entire mass of the cell shrinks, and in ciliary action, the only other visible form of motion then known to occur in animal tissue, the part concerned moves as a whole, so far as we are able to observe it ; in the pigmentary changes the form of the cell remained unaltered, but one of its constituent materials was seen to be transferred from place to place among the rest. But drawings made with the camera of a cell in suc-

cessive stages of concentration of the pigment admitted of only one just interpretation.[1]

These changes in the disposition of the pigment accommodate the tint of the animal to that of surrounding objects. A dark frog placed in a white earthen basin in sunlight soon assumes a dull yellow colour, and a pale one is not long in becoming black in a covered earthen jar.

It was very interesting to find that light produces these effects, not by direct action upon the skin, but indirectly through the retina and optic nerve. A hood of black cloth, carefully arranged so as to exclude light from the eyes without obstructing respiration, entirely prevented a dark frog from becoming pale in bright sunlight. I was naturally desirous of ascertaining through what efferent channels the nervous impulse that caused concentration of the pigment on exposure to light was conveyed from the brain to the foot. Division of the sciatic nerve had no effect whatever upon the colour of the limb. I then tried cutting through all the structures in the thigh except the bone, the femoral artery and vein, and the sciatic nerve. This also had no influence. But when I added to the latter procedure the section of the sciatic, the animal being then pale, it gradually grew dark below the seat of operation, till in no long time it presented from the toes to the wound as great a contrast with the rest of the body as if that part had been covered with a miniature black stocking. Thus the regulation of this function, which is probably closely allied to the action of the cells in nutrition, was not carried on exclusively through special nervous channels, as is the case with the contractions of the voluntary muscles, but one nerve could take the place of others in the duty.[2]

Light was not the only agency that induced pigmentary concentration. It might take place rapidly during struggling of the animal, and I once saw a frog grow pale in its efforts to avoid capture. Here mental emotion perhaps came into play, if we may use such an expression regarding the frog.

It seems quite astonishing that nervous action should make the pigment-molecules rush thus rapidly to the centre of the cell from its remotest and finest

[1] Max Schulze had not yet described the movements of animal protoplasm; and if he had done so, this could have gone but a little way in explaining the phenomena described in the text. The gushing out of homogeneous pseudopodia from the granular body of an amoeba may, however, be of an allied nature. I made attempts to see the movement of the pigment-granules in cells in which concentration was going on; but their extreme minuteness, together with the excessive rapidity of their apparent motion under the high magnifying power requisite, made them generally elude observation. I fancied I saw an indefinite rush of something through the clear space around the already accumulated mass, but I could not be sure. On one occasion, however, I saw some individual granules leave the mass and make excursions into the colourless liquid, as I could not doubt it to be.

[2] Vide *Phil. Trans.*, ibid. (see vol. i, p. 45).

ramifications. Yet a sudden gush of tears or outburst of perspiration, although familiar, is perhaps not less wonderful.

Concentration of the pigment took place, as we have seen, under nervous influence, and diffusion on its withdrawal. But diffusion was no mere passive phenomenon, such as might follow according to any ordinary arrangement of matter, when the agency that caused the grouping of the molecules ceased to operate. The transference of the granules from the body of each cell to its remotest ramifications, and their close packing there, were an act such as a living organism alone could have effected. Pigmentary concentration and diffusion were vital functions of a profound character concerned with the relative distribution of different constituents of the cells. Yet from the very happy circumstance of the conspicuousness of the pigment, the results of their activity could be observed with the utmost facility, and their behaviour in relation to inflammatory congestion easily studied even under a low magnifying power.

The pigment-cells pervade the skin and subcutaneous tissue of which the frog's web consists, and are especially numerous about the blood-vessels, round which their branches twine abundantly. They must, therefore, be acted on along with the vascular parietes by anything applied to the surface of the membrane. And to state shortly the result of many experiments, I found that any agency, physical or chemical, which caused the blood-corpuscles to lag behind the liquor sanguinis in the part on which it operated rendered the pigment cells in that particular area incapable of discharging their functions. Whatever might be their state at the time of the experiment, whether in full diffusion, complete concentration, or any intermediate condition, so they remained in the irritated spot, while in surrounding parts of the web, as in the body generally, they changed as usual in obedience to differences of illumination or other circumstances. At the same time they were not killed : for if the irritation had not been too severe, they recovered their full activity when resolution occurred.[1]

[1] I have in rare instances seen an irritant cause diffusion from a state of concentration as a preliminary effect. This was unmistakably the case on one occasion when mustard was employed. The pigment was in an intermediate (stellate) state when the application was made. In a narrow ring round the mustard, where the volatile oil could only act extremely mildly on the web, the stellate condition gave place to complete diffusion ; whereas under the mass, where the irritant had acted at once with full energy, the stellate appearance remained unchanged. Inflammatory congestion, however, had been produced in the ring of full diffusion as well as in the more strongly irritated area. It happened that complete concentration afterwards took place in the rest of the web, while the irritated areas retained the appearances above described. It seems probable that the diffusion under the slighter irritation may have been the result of the nerves in the irritated part being paralysed before the pigment-cells.

As is commonly the case with more specialized structures, the pigment-cells are a delicate form of tissue, and are more readily killed than other constituents of the web. Hence, if care is not taken to avoid pushing the action of the irritant too far, it will be found, after resolution has taken place, that

Thus the pigment-cells afforded ocular demonstration of the truth, to which I was otherwise led by inference, that an irritant, when producing inflammatory congestion, prostrates for the time being the vital energies of the tissues on which it acts.

It is to be observed that mere paralysis of the nervous apparatus of the irritated area would have been followed by diffusion of the pigment, as occurred after section of the nerves in the thigh, so that the suspension of diffusion as well as concentration shows that the special pigmentary functions had been arrested.

It was of course a familiar fact that nerves may be temporarily paralysed by the direct action of pressure, cold, and other agents upon them. But, so far as I am aware, it had not been known that the tissues generally are liable to be thrown into a state of suspended vital energy by injurious influences.

An experiment upon another form of tissue seems so illustrative of this subject that I am induced to relate it in detail. It was an attempt to study the effect of warmth upon the ciliated epithelium of the frog's tongue. It was easy to obtain the material for examination by gently scraping the surface of the organ and diffusing the product in a drop of water. Individual isolated cells were then to be seen with their cilia in motion, which might continue for a considerable period. But special arrangements were necessary in order to avoid killing them with the warmth, to which, as we have seen, the tissues of the frog are peculiarly sensitive. I succeeded by arranging them in a film of water between two delicate cover-glasses, the whole mass being so thin that it could be very quickly heated and as rapidly cooled. The object being placed under the microscope, I interposed a small cautery at a low black heat between the reflector and the stage and watched the result. The ciliary motion, which had been somewhat languid, became at once increased in rapidity, but resumed its former rate if the cautery was at once withdrawn. If, however, the application was somewhat longer continued, the active motion soon gave place to a state of complete rest, in which the cilia stood straight like the hairs of a brush. The hot iron being removed the instant that this effect was observed, slight indefinite movement soon began to show itself in individual cilia ; and before long all were again in action as before the heat was applied. If the cautery was made somewhat hotter the motionless condition was produced almost immediately, preceded by a momentary period of excessively active motion. But if the warm application was immediately suspended, recovery occurred, as in the former case. And the same experiment might be repeated again

the pigment-cells never recover ; the collections of pigment gradually lose their sharpness of outline and are ultimately absorbed, leaving a permanently white spot in the web.

and again on the same cells with the same results. But if the warmth was allowed to act for a slightly longer time, or the cautery was made still hotter, recovery never took place, and the bodies of the cells swelled up through endosmotic imbibition of water, having lost all life and obeying the ordinary laws of chemistry.

This simple experiment was in various ways instructive. It indicated that ciliated epithelium cells, like the pigment-cells, when acted on by a destructive agency to a degree just short of that which is lethal, are thrown into a state in which their vital functions are suspended but not irretrievably lost. It also showed that the cells which compose the animal organism are individually capable of recovering from this state of suspended vital energy, without any aid from the general circulation or the nervous system.

It further illustrated the important fact that a most injurious agent when operating very mildly may stimulate function without impairing power.[1]

Active congestion, or arterial dilatation with consequent free flow of blood through the capillaries, is an early and prominent symptom of inflammation of a vascular part in man. Unlike the morbid condition which is produced by the application of irritants to the frog's web, it is brought about indirectly through the nervous system. A striking illustration of this was presented in a case which occurred at the period to which I have been referring. A scirrhous mamma had been removed by transverse incisions, together with a considerable amount of integument ; and the cutaneous margins had been brought together, in spite of a good deal of tension, by means of a few stitches. Two days later I found the lips of the wound gaping slightly ; but the sutures, though subjected to much traction, were still holding ; while the skin presented an inflammatory blush extending both upward and downward from the wound, so that it occupied an area of about four inches in breadth. I removed the sutures, and I particularly noticed that no blood escaped from any stitch-track. The procedure occupied about two minutes and (to quote from a note taken at the time) ' no sooner had I done this than I observed that the redness had almost entirely disappeared ; most parts that were before apparently intensely inflamed being now pale '. The irritating agents acted directly on only a minute portion of tissue ; but they induced widespread active congestion, which subsided at once on their removal. Such results could only have been brought about through the agency of the nervous system.

As I before had occasion to remark, active congestion takes place throughout

[1] It would appear that all agents that act with destructive effect upon the tissues produce suspension of vital energy without loss of life when operating in a minor degree. Whether all such agents are also stimulants of function when in a still milder form is quite another question.

the frog's web when an irritant is applied to any part of it. It was therefore possible to study the phenomenon upon that animal. It was not at that time clearly known by what mechanism the constriction of the arterioles was effected, or by what part of the nervous system it was regulated. Kölliker had recently made his great discovery of the fibre-cells of involuntary muscle, and had described them as existing along with elastic tissue in the middle coat of the larger arteries ; but the trustworthiness of his observations had not yet been by any means universally recognised, and the structure of the ultimate arterioles, as compared with the capillaries, had not been ascertained.

I found, on dissecting out the vessels from between the layers of the frog's web, and examining them with a high magnifying power, that whereas the capillaries showed only a thin, apparently homogeneous wall beset with occasional nuclei, the finest arteries exhibited three coats, of which the middle one was composed of muscular fibre-cells wrapped spirally round the lining membrane. A more efficient mechanism for their constriction could hardly be conceived.[1]

As regards the regulation of the arterial contractions, Bernard's classical experiment of inducing turgescence of the vessels of the ear by section of the sympathetic in the neck, and Waller's converse observation that galvanic stimulation of the distal end of the divided nerve made the distended vessels shrink and the ear assume unwonted pallor, had demonstrated the great fact of nervous control over the local circulation ; but it was still a subject of discussion what part of the nervous centres exercised the function.[2]

In entering on the inquiry, I first divided the sciatic nerve, a proceeding which had been stated on high authority to cause relaxation of the arteries of the web. The immediate effect of its division was, indeed, some enlargement of their channels, but this very soon passed off. I then resorted to operations on the roots of the spinal nerves and on the medulla. And, not to weary you too much with details, I may say that removal of all that part of the spinal cord which gives off branches for the hind legs, caused the arteries of the web to relax completely and to remain permanently of about the same calibre as the veins. But if any portion that furnished nerves to the posterior extremities was allowed to remain, whether a little caudal segment or a small anterior part, the arteries resumed, after temporary dilatation, their ordinary and varying dimensions.[3]

It was thus shown that the cerebro-spinal axis is the centre that presides

[1] Vide *Trans. of the Royal Society of Edinburgh*, vol. xxi. part iv (see vol. i. p. 15).

[2] For authorities on this question, vide *Phil. Trans.*, ibid., pp. 607 et seq. (vol. i. p. 27).

[3] Vide *Phil. Trans.*, ibid.

over the contractions of the arteries of the foot, and that the function is exercised by the entire posterior half of the cord.

The very transient effect of section of the sciatic proved that, as is the case with the pigmentary functions, one nerve can take the place of others in the duty. And I found that even after division of all the soft parts of the thigh except the main artery and vein, the vessels of the webs soon recovered their contractile power ; showing that the control of the cord over the arteries can be kept up in an extensive region through quite insignificant nervous twigs. We can thus understand how a flap of skin raised in a plastic operation in the human subject may have its circulation duly regulated through a narrow neck of attachment.

An experiment that greatly pleased Dr. Sharpey would seem to indicate that the vaso-motor function for the hind legs is also discharged by the most anterior part of the cord. A frog having been arranged as before described, the point of a fine needle, curved at the end, was introduced into the wound behind the occiput, so as to prick for an instant the divided medulla. The eye of the observer being over the microscope while this was done, the arteries of the web were seen to contract to complete obliteration of calibre, as in the first experiment with warm water.

The contraction of the vessels caused on that occasion by the application of gentle heat to the web was now explained as a ' reflex action ' through the spinal cord. Their dilatation under irritation remained to be accounted for. In describing that first experiment, I stated that the extreme constriction of the arterioles was followed by relaxation to a larger calibre than they had before the water was applied ; suggesting the idea of fatigue after exertion. I have now to add that, if the warmth was longer continued, the subsequent relaxation was more marked and of longer duration ; and if the water was made somewhat hotter the contraction that preceded the dilatation was so transient as to be barely discernible.[1]

We seem to have here an exact parallel to what occurred as the result of the action of heat upon the ciliated epithelium. And the natural view seemed to me to be that the ganglion cells of the cord concerned in the arterial contractions were affected by the nervous impulse conveyed to them by the afferent fibres according to the same law that governed the direct action of heat upon the epithelium cells ; increased activity or suspension of function being induced according to the degree of energy of its operation. I incline still to believe that this was a correct interpretation of the phenomena of active congestion.

[1] Brief contraction of the vessels, followed by dilatation, had been previously observed by others as the result of the application of irritants to the frog's web.

Inflammatory congestion also may be brought about by nervous agency. This fact being of fundamental importance, and not perhaps universally recognised by pathologists, I may describe briefly two unpublished experiments with regard to it which I did shortly after the time of which I have been speaking. Inflammatory phenomena being of a very languid character in the frog, I had resort to a higher animal. One of the experiments was simply passing a silk thread through a fold of skin in a rabbit's back and knotting the ends together. When forty-eight hours had elapsed, the animal having been killed, I removed the portion of skin concerned and examined its under surface. The thread was covered with a yellow line of lymph, around which there was intense scarlet redness for about a quarter of an inch in every direction, contrasting strongly with the paleness of the healthy structures around. And on microscopic examination I found that this depended (to quote from my notes of the time) 'partly upon ecchymoses, but chiefly upon well-marked inflammatory congestion of the minutest vessels of the subcutaneous tissue and the deeper parts of the skin'.

The other experiment was performed twenty minutes before the first, upon a part of the same animal which, being more sensitive, was more likely to show the effects of nervous disturbance. By means of a fine sewing-needle I passed a delicate thread of silk through the superficial layers of the left cornea, and cut the ends off close with scissors. Next day the eye was much inflamed and its condition was aggravated on the day following, while the other eye remained healthy. As I have already said, the rabbit was killed on the expiry of forty-eight hours. This having been done by pithing, I at once divided the blood-vessels in the neck while the heart was still beating, so as to allow all blood that was free to flow to escape from the head. I then removed the two eyes and cut them both across transversely midway between the cornea and the optic nerve, and compared the inflamed eye with the healthy. I will not detain you with a description of the anterior halves of the globes, though they were very interesting, but will at once speak of their posterior portions, more remote from the part where the exciting cause had been in operation on the left side. In that eye the retina was much more readily detached than in the right, and showed small spots of ecchymosis, while the blood-vessels were more conspicuous. The two chloroids presented a striking contrast; 'the vessel in the right eye' (to quote my notes again) 'being not at all conspicuous, while in the left eye the membrane was scarlet; and this depended not merely upon fullness of red vessels but upon darker tint of their contents in consequence of excess of the corpuscles.'

It is matter for discussion how inflammatory congestion is brought about

by nervous action. It may be doubted whether active congestion could alone give rise to it, though it is by no means inconceivable that the excessive supply of the nutrient fluid might in time exhaust the tissues by over-stimulation, and so bring about more or less of that impairment of vital energy which we have seen reason to regard as the essential cause of the blood-corpuscles lagging behind the liquor sanguinis.

Or it may be that the nerves produce this weakening effect upon the tissues by immediate action upon them. From that point of view, the proof afforded by the pigment-cells of the influence of the nerves over processes going on within cells seemed to me peculiarly interesting. And we can conceive of nervous impulse impairing their energies either by over-stimulation followed by exhaustion, or by immediate prostration of their powers as by an electric shock.

That the latter idea is not altogether out of the question seems to follow from a kind of experience familiar to surgeons. I will mention one instance of this which produced a great impression upon me. A healthy man, in the middle period of life, had been operated on by lateral lithotomy. All went perfectly well till about ten days had elapsed, when the renal secretion, which had passed through the wound since the operation, flowed for the first time through the natural channel. In those days, when lateral lithotomy was the routine treatment of calculus, it was well understood that the mucous membrane becomes in a few days unaccustomed to the urine, which then acts upon it with irritating effect, and the result may be a violent general nervous commotion in the shape of a rigor. This may be immediately followed by complete suppression of secretion by the kidneys ; and if this does not pretty soon pass off it is fatal. Such was the case with the patient to whom I am referring. In spite of hot applications to the skin—which sometimes work like a charm in such circumstances, operating, as it would appear, by distracting, so to speak, the attention of the nervous system from the affected organs—that man died within a few hours of the rigor. On post mortem examination the kidneys presented on section an appearance that I have never forgotten ; scarlet redness throughout what in other respects appeared to be perfectly sound structure.

The previous healthiness of the patient seems to preclude the idea that this grave disorder of the kidneys could have existed before the rigor. We are therefore led to believe that the prostration of vital energy which inflammatory congestion implies was caused by the irritation in the urethra. If such was the case, the remoteness of the kidneys from the source of disturbance makes it certain that the disorder was brought about through the nervous system. This effect could not possibly be produced, like arterial relaxation, by failure of the nerves to act. For we know that the tissues retain their vital

energies for a considerable period after entire severance from the body: as is illustrated by the success of Thiersch's method of skin-grafting. We are therefore driven to the other alternative, and conclude that the inflammatory congestion of those kidneys was caused by nervous action upon the renal tissues. And the suddenness with which the effect was produced strongly suggests the view that the prostration of their vital power was the primary effect of an unwonted nervous impulse.

Abnormal effusion of liquor sanguinis from the vessels, another marked feature of acute inflammatory disturbance in man, would seem a natural result of any degree of inflammatory congestion. I used to illustrate to my class by a simple experiment the enormous increase which takes place in the pressure of a liquid upon the walls of a tube through which it is flowing, when an obstacle is opposed to its passage. When, therefore, the corpuscles begin to block the capillaries the plasma will naturally be forced in undue quantity through their porous walls.

When inflammation assumes an intense degree, the effused liquor sanguinis has the peculiarity of being coagulable, producing by its solidification the characteristic 'brawny' swelling of the parts among which it is poured out. In this respect it differs from the normal plasma forced by pressure through the walls of healthy capillaries as the result of venous obstruction. Here the swelling has the 'doughy' character of oedema, a condition also caused by inflammation of a mild degree. To that point I shall have occasion to refer again.

I designated by the title 'direct inflammation' the morbid state produced by the direct operation of noxious agents upon the tissues, as distinguished from inflammation brought about through the medium of the nervous system. This distinction appears to me to be of great importance; and it enables us to understand what would otherwise be quite unintelligible. One beautiful instance of this is the behaviour of a recent wound in tissues previously healthy. When our means of arresting bleeding were less complete than they are at present, it was no uncommon thing to be summoned a few hours after an operation on account of haemorrhage. It was a sad thing to have to tear asunder the lips of a wound already well glued together by lymph, in order to gain access to the bleeding-point. This lymph was neither more nor less than liquor sanguinis which had been effused from the cut surfaces and had coagulated. From the quality of the effusion we should suppose that we had to deal with inflammation of a very intense character. Yet the lips of the wound were perfectly pale, entirely free from the active congestion which is the very earliest sign of inflammatory disturbance. How could this inconsistency be reconciled? Very

simply, as I believe, by aid of the principles which we have been discussing. Mechanical violence is a noxious agency producing effects proportionate to its degree. A very blunt implement passing through the tissues kills the surface of the parts which it divides ; and in former days we had to poultice a 'contused wound' till the sloughs separated. A sharp knife does not destroy any part of the tissues, but it throws them in a microscopically thin layer into a state of intense inflammatory congestion, attended with effusion of coagulable liquor sanguinis. But this noxious agency is only momentary in its operation. It has no time to cause active congestion through the nervous system, but at once leaves the injured tissues free to recover by virtue of their own inherent powers. If the instrument be very sharp the layer of lymph will be very thin, unless some other disturbing cause come into play. But it is always sufficient in amount to serve the beautiful purpose of adhesion.

It is comparatively rarely that direct inflammation is met with thus pure and simple in practice. The two forms, the direct and indirect, are commonly more or less associated. Thus putrid discharge in a wound is an acrid irritant, as I once experienced personally in the keen smarting of an abrasion on the back of my hand, smeared accidentally with the pus of a stump that I was dressing. Hence during the period that elapses before the divided tissues are clothed with that wonderful protecting layer which we term granulations, such discharge causes direct inflammation in the structures on which it acts immediately, while it also induces in them and in neighbouring parts inflammation through the nervous system.

When Marion Sims had published his remarkable success with the silver suture in gynaecology, I resolved to give it a trial in general surgery. At that time, as assistant surgeon in the Royal Infirmary of Edinburgh, I had charge of the Lock Hospital ; and one of the patients having an atheromatous tumour of the scalp, I removed it and brought the edges of the skin together with a silver stitch. No vessel required ligature, and the wound healed without suppuration. As the suture created no disturbance, I left it *in situ* for about ten days, when I took the patient over to Mr. Syme and showed him the skin about the wire perfectly pale and natural in appearance without a trace of discharge, whereas a silk stitch would within four days have infallibly caused suppuration, with surrounding redness. Mr. Syme at once recognised the importance of the facts, and from that day forward the silver suture was used for all wounds in the clinical wards, until, some years later, antiseptic measures caused it to give place to the more convenient and no longer hurtful silk.

In thinking over this striking difference between the effects of the two kinds of suture, it seemed to me clear that it depended on the silk imbibing

blood and serum, which, undergoing decomposition in its interstices, grew more irritating the longer the process continued; whereas the metal gave no hold to the organic liquids, which were shed unaltered as fast as they were effused. From these and other analogous considerations, I taught my class at that time that decomposition of the organic liquids was the essential cause of suppuration.

The coagulation of the blood, while it is a matter of fundamental importance in physiology, has peculiar interest for the surgeon, on account of the special feature of coagulability of inflammatory exudations and the part played by lymph in the healing of wounds and various other pathological phenomena, such as the sealing of divided arteries by blood-clot. Towards the close of the investigations which I have been describing there was published another successful Astley Cooper Prize essay, *Coagulation of the Blood* having been the subject selected by the judges for the competition. The author of this dissertation, the late Dr. Richardson, propounded the new theory that the solidification of blood shed from the vessels was due to the escape of ammonia, which, as he believed, held the fibrine in solution. I was at first much struck by the evidence with which he supported this view, and my first experiments on the subject were made with a view to strengthening that evidence where it seemed to me weakest.

In one of these, a sheep having been placed under chloroform, I sought by means of a common tourniquet to constrict the thigh so extremely as to prevent the ammonia from escaping when the vessels were divided, and so keep the blood fluid in spite of amputation. Rigidity of the muscles prevented me from carrying out my intention; but I tied a bandage firmly round the foot, below the joint where the butcher removes it, so as to retain the blood, and, as far as might be, the ammonia also. The foot being severed, I took it home, and, having raised a portion of the skin so as to expose a subcutaneous vein, I investigated the state of the blood in it. I found it indeed fluid, with one exception, full of significance, though I did not see its import at the time, viz. where the cord used by the butcher for tying the feet together had pinched the veins against the bone, there, and there only, was the blood in them coagulated. I remember being a little disappointed, as well as puzzled by that appearance. It was not in harmony with the theory in which I was at the time disposed to believe. And yet how replete were the facts with possible instruction! Compression of the veins had certainly given no opportunity for escape of ammonia. It is equally certain that the cord did not make the blood coagulate by any direct action upon it: for the cord, so long as it remained in position, kept the parts of the veins which it compressed empty of blood. It is clear that the effect was due to the action of the cord upon the walls of

the vessels. Not that it had wounded them, nor is there any reason to suppose that it had killed them. No doubt if the animal had been released instead of slaughtered, the veins would in due time have recovered. But the mechanical violence which the hard round cord exerted, being pretty severe and long continued, had prostrated for the time the vital energies of the tissues on which it had acted; and we had, in coagulation of the blood, a repetition of the class of phenomena we had studied in the blood-corpuscles.

But how was it that the blood remained fluid in other parts of the vessels? To my surprise I found that the same continued to be the case for days afterwards. And thus accident led me to recognise what I afterwards found to be the general rule, viz. that the blood, though in mammalia it coagulates soon after death in the heart and main trunks, remains fluid for an indefinite period in minor branches. The clotting in the heart had been an object of familiar observation in post mortem examinations in the human subject, and it seems to have been assumed that the same thing occurred throughout the vascular system.

The sheep's foot, with the blood retained in its veins by a bandage applied before the animal was slaughtered, afforded the opportunity for very simple, but instructive experiments on the nature of the relations between the living vessels and their contained blood. For that the veins retained their life, even after the lapse of more than twenty-four hours after severance of the foot from the body, was shown by their shrinking by muscular contraction on exposure.[1]

Thus I found that a piece of glass introduced into a vein occasioned coagulation in its vicinity. The end of a sewing-needle pushed through the wall of a vein otherwise uninjured, became after a while encrusted with a layer of fibrine deposited upon the part within the vessel, while the rest of the blood in it retained its fluidity.[2] On the other hand, having injected air into the vessels on another occasion, I found seven hours later that their contents were a frothy mixture of blood and air, the walls of whose bubbles were fluid, but solidified when shed. Sir Astley Cooper had been of opinion that the living vessels kept the blood within them fluid by acting in some way upon it—in other words,

[1] As regards the ammonia theory, an experiment which proved universally convincing was this: Having exposed a vein in the sheep's foot, I pressed the blood out of it at one place, and applied liquor ammoniae to the empty portion, protecting neighbouring parts of the vessel from the vapour with olive oil. After sufficient time had passed for the volatile alkali to fly off, blood was allowed to return to the part on which the caustic liquid had acted. There it soon coagulated: the very substance a mere trace of which should have kept it fluid according to the theory in question having brought about its coagulation by injuring the tissues of the living vessel.

[2] The results of experiments of this kind vary considerably according to the time which has elapsed after the foot was removed from the body: for the blood undergoes pretty rapid impairment of its coagulability within the vessels of the severed part, and finally loses it altogether. It then, of course, remains fluid long after the veins have lost all life.

his view implied that the blood had a spontaneous tendency to coagulate, which was held in check by the active operation of the living tubes that contained it.[1] Facts such as I have just mentioned seemed to me to indicate that the ordinary solid was the active agent, determining the formation of fibrine as a thread does the deposition of sugar candy; while the healthy living tissue had the remarkable peculiarity of being destitute of this general aggregating property of solids, behaving rather like the self-repelling particles of gases.[2]

It was not only in vessels of small size, like those of the sheep's foot, that the blood remained fluid in parts severed from the body. I found that the same was the case in veins of the dimensions of the jugular of the ox or the horse, and this in spite of their entire detachment from surrounding structures. The vessel being exposed after the animal had been felled at the abattoir, two ligatures were applied in order to retain the blood in it, after which it was removed and taken home with as little disturbance as possible. The blood in it retained its fluidity for upwards of twenty-four hours, affording opportunity for most instructive experiments. Of these I must content myself with describing one. A portion of an ox's jugular with its contained blood being held vertically, the upper part was removed along with its ligature, and the lips of the now open venous compartment were held apart with forceps by aid of an assistant, while a thin glass tube, of rather smaller calibre than the vein and open at both ends, was passed down into the vessel with the utmost steadiness, so as to disturb the blood as little as possible. The upper end of the tube had been drawn out with the blow-pipe to much smaller size and a short piece of india-rubber tubing adapted, so as to admit of clamping with catch-forceps. When blood appeared at the end of the caoutchouc tube the clamp was applied. The whole apparatus was then rapidly inverted and the piece of vein removed, leaving the blood in a vessel of ordinary solid matter without any contact of living tissue. The glass tube was steadily clamped to a retort stand, and its orifice covered with a loose cap of gutta-percha tissue to exclude dust, after which all was left undisturbed for twenty-four hours. On then turning out the blood, I found it all fluid except a layer of clot about one-eighth of an inch in thickness, which encrusted the interior of the tube, and also a little clot at the surface, which

[1] Brücke, of Vienna who had also competed for the Astley Cooper Prize, had arrived at a similar conclusion. He experimented largely with the turtle's heart, which, as in cold-blooded animals generally, retains its life long after removal from the body: the blood in its cavities at the same time retaining its fluidity. I had not seen Brücke's important essay when the experiments referred to in the text were performed.

[2] It has since been shown by Freund, of Vienna, that an indifferent liquid, such as liquid paraffin, has a similar negative behaviour in relation to coagulation: so that, by proper management, blood may be kept fluid in a vessel of ordinary solid matter having its interior smeared with that substance. Professor Haycraft arrived about the same time at a similar conclusion regarding castor oil.

might be explained by some drying on account of the imperfect fitting of the cap. The fluid part of the blood soon coagulated.

The result of this experiment seemed to me of itself sufficient evidence that the blood requires no action of the living vessels to maintain its fluidity, and that the hypothesis of such action was superfluous.

At the same time the extreme care required in order to ensure the success of such an experiment indicated the subtilty of the influence of an ordinary solid in bringing about coagulation. A very simple experiment, performed in a butcher's establishment on the way from my father's house at Upton to deliver the Croonian Lecture before the Royal Society, illustrates the same thing. I received blood from the throat of an ox into two similar open earthen jars (gallipots), and slowly moved a clean glass stirring-rod through the blood of one of them for a second or two, and then left both vessels undisturbed.

In the course of a few minutes the blood that had been thus gently and briefly stirred was a mass of coagulum, while the unstirred blood was still fluid, except a thin layer of clot encrusting the wall of the jar. In course of time it also coagulated completely. Now we know, from the experiment with the ox's jugular, that coagulation is propagated with extreme slowness, if at all, from a clot in blood perfectly undisturbed. The earlier coagulation of the main mass of the stirred blood was, therefore, not caused by propagation of the process from a layer upon the surface of the jar, but must have been the result of the brief agency of the glass rod.

A little before the delivery of the lecture referred to,[1] I became aware of the recent very important observations of Schmidt, who showed, as had been foreshadowed many years previously by Andrew Buchanan, of Glasgow, that normal liquor sanguinis does not, as had been supposed, contain fibrine in solution but only one constituent of that substance, termed by Schwann fibrinogen, the other constituent being derived from the blood-corpuscles. The ordinary solid, therefore, in determining coagulation, does not cause the deposition of fibrine already formed, but so influences the corpuscles as to make them give up an ingredient necessary for the formation of that insoluble body.[2]

With this further light upon the subject, the conclusions derived from the experiments to which I have referred seem to explain the special coagulability of the exudation in intense inflammation. Under intense irritation the capillary walls will naturally be affected by the noxious agency as the veins of the sheep's

[1] Vide the Croonian Lecture, 'On the Coagulation of the Blood,' *Proceedings of the Royal Society*, 1863 (reprinted in vol. i, p. 109).

[2] Regarding the corpuscular elements of the blood which are concerned in supplying to the plasma the materials necessary for the formation of the fibrine and the chemical interactions of those substances, various important researches have since been conducted, in which I have had no share.

foot were by the constricting cord, and, like them, will act upon their contained blood as if they were ordinary solids. The plasma of that blood will therefore receive from the blood-corpuscles the material requisite for forming fibrine, and, passing through the pores of the capillaries with that addition, will constitute a coagulable exudation.[1]

On the other hand, if irritation is less severe, although the corpuscles acquire more or less adhesiveness, involving corresponding obstruction to the flow through the capillaries and consequent undue passage of liquor sanguinis through their walls, the constituent tissues of the vessels are not reduced to the condition in which they act like ordinary solids in relation to coagulation. This seems to follow from the uncoagulable character of the effused fluid. For we know that what used to be termed the serum of oedema or hydrocele is simply the normal plasma.

Adhesiveness of corpuscles and coagulation are both brought about by the operation of noxious agents upon the tissues of the part concerned. But it by no means follows that they are in all respects analogous phenomena. We have seen that normal blood has no innate tendency to coagulate, and needs no action of the tissues upon it to ensure its fluidity. But the blood-corpuscles may be naturally adhesive bodies, possessing a viscosity only kept in abeyance by some influence exerted upon them by the living tissues in their vicinity; and such appears to be really the case.

A very interesting observation which I made long ago, but to which I have not before directed attention in this point of view, shows that an extreme degree of adhesiveness of the red discs may exist within a blood-vessel, the walls of which are in perfect health with reference to coagulation. If a horse's jugular vein, obtained in the manner I have described, is suspended vertically, the blood in it remains fluid for an indefinite period, but the red corpuscles soon fall from the upper parts of the fluid, leaving a buffy layer of plasma, readily seen through the translucent wall of the vessel. And this behaviour of horse's blood implies, as we have seen, a high degree of adhesiveness of the red discs.

If we compare this with the perfect absence of grouping of the red corpuscles which was observed within a vein of the bat's wing, in spite of their extreme adhesiveness in the same animal in blood shed from the body, we cannot but be greatly struck with the contrast. As regards the circumstances of the two vessels, we see that in the bat's wing the vein was of small calibre, and was in its natural relations to surrounding structures; whereas the horse's jugular was of very large dimensions and isolated from the rest of the body.

It seems impossible that the adhesiveness of the corpuscles in the jugular

[1] I once ascertained the coagulability of a drop of clear fluid which had exuded from a recent contused wound, by drawing the point of a needle through it, to which it yielded threads of fibrine.

vein was the result of isolation of the vessel from other structures. For adhesiveness of corpuscles is not occasioned in the frog's web by amputation of the limb ; nor is it produced in the human subject by complete detachment of a portion of tissue ; as is clearly shown by the persistent healthiness of a piece of skin entirely transplanted in skin-grafting. We are therefore forced to the conclusion that the adhesiveness of the red discs in the horse's jugular, as contrasted with its complete absence in the vein of the bat's wing, was due to the larger size of the vessel in the former case. And the only way in which it seems possible to interpret this difference of behaviour of the corpuscles in the two cases is to suppose that they possess an innate and normal viscosity which is kept in abeyance by some action of the healthy tissues ; this action having a limited range of operation, so that, while effective for vessels of small size, it fails to influence the mass of blood in a large venous trunk. And I may remark, in passing, that it is only in the smaller vessels that absence of adhesiveness of the corpuscles is essential for the free transmission of the blood.

The mobility of the black pigment-granules of the frog has often struck me as extremely remarkable. Perfect absence of any tendency to aggregate on their part must be fully as essential to the freedom with which they move through the exquisitely delicate ramifications of their containing cells as want of adhesiveness of the blood-corpuscles is to their free transit through the capillaries, and I cannot but think that the two phenomena must be analogous. It may be, for aught we know to the contrary, that the pigment-granules may be themselves living entities. Their uniformity in size is in favour of such an idea. Our fathers would have been greatly astonished to learn that the chlorophyll grains of vegetables were, as has been shown in recent years, living organisms, multiplying by division like the nuclei of their containing cells ; and though the pigment-granules are much smaller, they must be greatly surpassed in minuteness by many microbes which, though hitherto invisible to us, we believe from analogy to be the causes of some infective diseases. But however this may be, the perfect mobility of the pigment-granules seems to me a special property which they possess as constituents of the healthy living body ; in other words, to use once more the expression which in the present state of our knowledge is indispensable, a vital property.

If this be so, we understand what would otherwise be very unintelligible, viz. that when the pigment-cells have their functions temporarily suspended by a noxious agent, the granules do not become diffused as they do when simply withdrawn from the influence of the nervous centres, but remain exactly as they were before the irritant was applied, whether fully concentrated, completely diffused, or in any intermediate state. If we suppose that the pigment-

granules, like the blood-corpuscles, acquire under irritation a tendency to mutual aggregation which they do not possess in health, it follows, as a matter of course, that when vital energy is suspended by the noxious agency, they will adhere together and retain their relative positions.

After being appointed to the Chair of Surgery in the University of Glasgow, I became one of the surgeons to the Royal Infirmary of that city. Here I had too ample opportunity for studying hospital diseases, of which the most fearful was pyaemia. About this time I saw the opinion expressed by a high authority in pathology that the pus in a pyaemic vein was probably an accumulation of leucocytes. Facts such as those which I mentioned as having aroused my interest in my student days in a case of pyaemia, made such a view to me incredible, and I determined to ascertain, if possible, the real state of things by experiment. I introduced into a vein of a living horse a short glass tube open at both ends, containing a piece of silver wire in which was mounted a little bit of calico, which I thought likely to give rise slowly to putrefactive change ; shutting off the portion of vein concerned from the general circulation by means of ligatures. After the lapse of some days I removed the venous compartment and found that the blood in it had undergone very remarkable changes. The limits of this lecture (which have been already too widely extended) make it impossible for me to enter into details, as I had hoped to have done, regarding the researches of which this was the commencement. I must content myself with stating the conclusion to which I was led at the time I am speaking of, and which was confirmed by later investigation, viz. that the introduction of septic material into a vein may give rise to the rapid development of large nucleated cells which, growing at the expense of the original constituents of the coagulum, convert it entirely into a thick yellow liquid. The pus so formed contains corpuscles which, like those which I sketched in the early case at University College, are not pus corpuscles in the ordinary sense or leucocytes, but the variously sized, more or less granular nuclei of the large cells, the pellucid bodies of which constitute the so-called liquor puris. Into the question of the origin of these rapidly proliferating cells I must not enter. This process of genuine suppuration of the blood-clot removed all the difficulties I had felt in interpreting the post mortem appearances in pyaemia, and also its clinical features.

Having become familiar with the appearances of these cells in suppurating coagula, I was able to recognise them in acute abscesses in the human subject, and to demonstrate them to others by mixing carmine with the pus, so as to render clearly defined the limits of the pellucid bodies of the cells, which otherwise would have been regarded as liquor puris.

I am, of course, aware of the great importance of the emigration of leucocytes, discovered by Cohnheim, and rendered immeasurably more interesting by Metchnikoff's observation of their phagocytic powers ; and I know that collections of pus have often such an origin. But I am quite satisfied that this is not the exclusive mode of pus-formation, and that it is often produced by the proliferation of cells, as was first taught by my illustrious predecessor in this chair of two years ago (Professor Virchow), in the *Cellular Pathologie.*

While these investigations into the nature of pyaemia were proceeding, I was doing my utmost against that deadly scourge. Professor Polli, of Milan, having recommended the internal administration of sulphite of potash on account of its anti-putrescent properties, I gave that drug a very full trial as a prophylactic. I have notes of a case in 1864, in which, after amputating the thigh for disease of the knee-joint, I gave ten grains of the sulphite every two hours from the time of the amputation ; and when, on the sixth day, an ominous rigor occurred, I doubled the frequency of the administration. Death, however, took place nevertheless, and this was by no means my only experience of such disappointment.

At the same time, I did my best by local measures to diminish the risk of communicating contagion from one wound to another. I freely used antiseptic washes, and I had on the tables of my wards piles of clean towels to be used for drying my hands and those of my assistants after washing them, as I insisted should invariably be done in passing from one dressing to another. But all my efforts proved abortive, as I could hardly wonder when I believed, with chemists generally, that putrefaction was caused by the oxygen of the air.

It will thus be seen that I was prepared to welcome Pasteur's demonstration that putrefaction, like other true fermentations, is caused by microbes growing in the putrescible substance. Thus was presented a new problem : not to exclude oxygen from wounds, which was impossible, but to protect them from the living causes of decomposition by means which should disturb the tissues as little as is consistent with the attainment of the essential object.

It has been since shown that putrefaction, though a most serious cause of mischief in wounds, is not its only cause. In other words, it has been proved that there are microbes which produce septic effects without occasioning unpleasant smell. But the principle that first guided me, still retains, I believe, its full value, and the endeavour to apply that principle so as to ensure the greatest safety with the least attendant disadvantage has been my chief life-work.

OBITUARY NOTICE OF THE LATE JOSEPH JACKSON LISTER, F.R.S., Z.S.

WITH SPECIAL REFERENCE TO HIS LABOURS IN THE IMPROVEMENT OF THE ACHROMATIC MICROSCOPE

Contributed in a Letter to the President of the Royal Microscopical Society.

[*Monthly Microscopical Journal*, March 1, 1870.]

Communicated by the President at the Anniversary, February 9, 1870.

Edinburgh, *February* 8, 1870.

My dear Sir.—In compliance with your request, I proceed to furnish you with some particulars regarding my late dear and honoured father.

He was born in London on the 11th of January, 1786, his parents being highly respected members of the Society of Friends. At fourteen years of age he left school to assist his father in the wine trade: but though he was for many years closely occupied in business, he contrived, by early rising and otherwise, to supplement largely the plain, though good, school education he had received, and he was in many respects a self-taught man. Such was the case as regards his mathematical knowledge, which he turned to such excellent account in his labours for the improvement of the microscope.

His predilection for optics manifested itself very early. He used to tell how, when a little child, he enjoyed looking at the prospect through air-bubbles in the window-pane, which improved the vision of the then myopic eye and enabled him to see distant objects with distinctness. This fact afterwards led him to think it probable that in very young children the eye is generally myopic. The same taste was indicated when he was a boy at school by the circumstance that he alone of all the boys possessed a telescope.

The achromatic microscope was early an object of interest to him; but it was not till the year 1824, when he was thirty-eight years old, that he did anything to improve the object-glass. His first work of this kind is recorded in a note, dated 1825, to the following effect: 'The $\frac{4}{10}$ and $\frac{2}{10}$ achromatic object-glasses, made by W. Tulley at Dr. Goring's suggestion, delighted me by their beautiful performance, but they appeared to me to have a great disadvantage in consequence of the thickness in proportion to their focal length, which W. T.

thought could not be avoided. I therefore induced him to make for me one of $\frac{9}{10}$ much thinner in proportion, and had the satisfaction to find its performance *very nearly* equal to his best $\frac{2}{10}$. In one respect, indeed, it is superior; showing when in good adjustment the reflection from a minute ball of mercury a bright point in any part of the field, while in the $\frac{2}{10}$ and $\frac{4}{10}$ it is so shown only in a small portion of the field near the centre, and in the rest has a bur shooting outwards.' This bur, of which a sketch is given, is the first mention of the 'coma' which afterwards formed so important a subject of his investigations. The note goes on to describe a suggestion for another combination, illustrated by drawings of magnified views of the curves of the glasses, executed with his usual extreme neatness and accuracy; and it concludes with the words, 'tried many experiments to ascertain the best means of correcting small errors in aberration.'

The note from which these quotations are made is the first of a long series of accounts of experiments, with remarks upon them, indicating an amount of labour of which, as I never saw the papers before, and as the work was for the most part done either before my birth or during my early childhood, I had previously had no idea. The notes are beautifully arranged, and might well be published just as he left them. I must, however, content myself with mentioning, in chronological order, some of the most interesting of their contents.

In 1826, after a description of Amici's reflecting microscope and an account of its performance, I find further projections of object-glasses for Mr. Tulley, followed by a drawing for the engraver to illustrate a description of Tulley's microscope, published by that optician. A copy of this pamphlet has been preserved, and the first page begins with this acknowledgement: 'Before commencing the description of the microscope it will be proper to state that the construction of the instrument and its apparatus was suggested and made from original drawings by my friend, J. J. Lister, Esq., whose ingenuity and skill in these matters are very generally acknowledged.' The chief novelties in this instrument, besides the improved object-glasses, were the following:—

Graduated lengthening tube to the body. The stage-fitting for clamping and rotating the object. A subsidiary stage. A dark well. A large disc, which would incline and rotate for opaque objects. A ground-glass moderator. A glass trough. A live-box made with flat plate. A combination of lenses to act as condenser under the object (apparently the first approach to the present achromatic condenser). The erecting-glass; and the adaptation of Wollaston's camera lucida to the eye-piece.

The value of the erecting-glass for facilitating dissections under low powers is, perhaps, even yet not sufficiently appreciated.

The camera lucida had long been a favourite instrument with my father

for drawing landscapes ; and I may add that the tripod which he invented for supporting the drawing and the camera is that which is now universally used by photographers.

In December of the same year occurs an account of an examination of a set of four plano-convex lenses, each consisting of a bi-convex of plate glass and a plano-concave of flint glass cemented to it by varnish, constructed by Chevalier, of Paris. Various interesting observations are here met with. He found that the maker had done injustice to his own instrument by shutting out a needlessly large portion of circumferential rays ; and that when the apertures had been enlarged by increasing the holes in the stops, the glasses performed much better, so as to ' give him strong doubts of the figure of these small achromatics being injured by varnish ' (for in Tulley's glasses the constituents of the compound lenses were not cemented together), and he remarks on the great advantage that would be derived from cementing, if unobjectionable otherwise, in facilitating the manufacture.

He made various trials with these glasses in combination, and remarks : ' I will put down my trials of the glasses as they were made. Some of them have surprised me ; and they will show, I think, remarkably, the advantage to science and art of collating the detached labours on the same subject, of distant individuals. The French optician knows nothing of the value of aperture, but he has shown us that fine performance is not confined to *triple* object-glasses ' (Tulley's were triples) ; ' and in successfully combining *two* achromatics he has given an important hint, probably without being himself acquainted with its worth, that I hope will lead to the acquisition of a *penetrating power* greater than could ever be reached with one alone.' In the light of subsequent events this reads almost like a prophecy.

With respect to a combination of one of Chevalier's glasses with one of Tulley's, he writes : ' The performance of this compound is the finest I have ever seen produced by achromatic glasses, and furnishes, I think, a very important fact. Its virtual focus is ·52 inch, while W. Tulley's $\frac{3}{10}$ is but ·33 inch, and Chevalier's combination only ·26 inch ; yet it goes beyond them both in clear positive power of defining.'

But the most interesting parts of this note are those which record, for the first time, some puzzling appearances in combinations of compound lenses, which ultimately led him to his great discovery of the two aplanatic foci. Each of Chevalier's compound plano-convex lenses when used singly presented a bur or coma outwards, but when two of them were combined, this coma, instead of being exaggerated, as might have been expected, was '*less than with any single glass*', while the performance in other respects was satisfactory. ' Observing

the advantages resulting from this combination,' he 'tried some others', among the rest two of Tulley's triple glasses, each of which taken singly was of fine performance. But, instead of unmixed improvement resulting, we find it noted: '*N.B.*—Each glass *separately* shows a bright object all over the field without bur, and is *not far from being achromatic*. But, *combined*, the objects not in the centre have a strong bur INWARDS, the colour is much under-corrected, and the spherical aberration is not right.'

In the following year we find similar anomalous appearances recorded. Thus, on one occasion, on using in combination a triple glass of Tulley's free from coma and otherwise excellent, and a double plano-convex in which, when used alone, the spherical aberration was rather under-corrected and an outward coma presented itself, the combination proved to have the spherical aberration rather over-corrected and showed an inward coma. Again, a bi-convex glass of Herschel's construction, consisting of a bi-convex of plate with a flint meniscus, when used alone with the flint surface foremost had little or no coma, but when combined with a triple $\frac{9}{10}$ free from coma, showed a 'bur much inwards'. The same glass used alone with the plate side foremost showed a 'bur inwards', but when it was combined with the triple, which had before had the effect of inducing an inward coma, the bur inwards was changed to a 'bur slightly outwards'.

Such are samples of the perplexing and seemingly inconsistent observations recorded at this period. To a less accurate observer and a less acute mind they must have proved utterly bewildering. But he did not despair of finding an explanation of the appearances, and the last note on the subject in that year alludes to the angle formed by the rays of light with the concave lens as affecting the direction of the coma.

He was afterwards occupied for a while with planning triple glasses to be used in front of the previous triples of Tulley, and with general arrangements for the instrument. But, in November 1829, a set of five plano-convex glasses manufactured by Utzschneider and Fraünhofer, very similar to those of Chevalier but uncemented, having been placed freely at his disposal by Mr. Robert Brown, the botanist, he set to work in good earnest to strive to solve the difficult problem. The experiments made with this object are recorded in a series of tables, the first of which gives an accurate description of each of the five new glasses and also of those of Chevalier, and of their performance when used singly. The others give the effects of various combinations of those glasses upon the chromatic and spherical aberrations and upon coma. He had previously observed, as mentioned in a note in 1827, that in a particular combination of two glasses the coma was diminished by separating the glasses. And we find in these

tables that the performance of each combination is given, both when the glasses are close and when they are separated a certain distance from each other. As we look down the tables we seem for a while to find confusion worse confounded. We see, indeed, abundant evidence of the great effect produced both upon coma and upon spherical aberration by the distance between the glasses; but the effects appear altogether inconsistent, if not contradictory. Thus, as regards coma, two of Fraünhofer's glasses which, if used singly, gave slight outward coma, gave when combined and near together a great deal of coma rather outward, but when separated by 1·2 inches an almost entire absence of coma, and what there was rather inwards. But, farther down, three glasses which each gave outward coma when single, are seen to present in combination an inward coma when close, and an outward coma when separated. With respect to spherical aberration we seem for a while to meet with something like a law. We find that two glasses which, if used alone, are free from spherical error, when combined and close have that error over-corrected, but this over-correction is removed by separating the glasses. And the same thing occurs with several other combinations. But looking down the table we come to a case where the excess of spherical correction caused by a combination of three glasses placed close, cannot be removed by separating them, and then follows a combination of three, in which 'the excess of spherical correction is *increased* by separating for the short distance we can go'. And, again, a little lower occurs a combination, also of three, in which 'the excess of spherical correction is diminished but not conquered' by separation of the glasses.

Yet out of this apparent confusion he educed a principle which reconciled all the conflicting appearances, and formed the basis upon which all fine combinations for high powers of the microscope have rested. He found that in a plano-convex lens, constructed like those above described, in which a double convex of plate has its colour corrected for a moderate aperture by a plano-concave of flint, the effect of the flint lens upon the spherical error caused by the plate varies remarkably according to the distance of the luminous point from the glass. If the radiant is at a considerable distance, the rays proceeding from it have their spherical error under-corrected; but as the source of light is brought nearer to the glass, the flint lens produces greater proportionate effect, and the under-correction diminishes till at length a point is reached where it disappears entirely, the rays being all brought to one point at the conjugate focus of the lens. This, then, is an aplanatic focus. If the luminous point is brought still nearer to the glass, the influence of the flint lens continues for a while to increase, and the opposite condition, of over-correction, shows itself; but on still further approximation of the radiant, in consequence apparently

of a reversal of the relations to each other of the angles at which the rays of light meet the different curves of the lens, the flint glass comes to operate with less effect, the excess of correction diminishes, and at a point somewhat nearer to the glass vanishes, and a second aplanatic focus appears, and from this point onwards under-correction takes the place of over-correction, and increases till the object touches the surface of the glass. Such a lens, then, has two aplanatic foci : for all points between these foci it is over-corrected, but under-corrected for points either nearer than the shorter or more distant than the longer focus. A knowledge of these facts enables the optician to combine a pair of such lenses with perfect security against spherical error. In order to do this, to quote from my father's paper in the *Philosophical Transactions*, read on the 21st of January, 1830, ' the rays have only to be received by the front glass from its shorter aplanatic focus, and transmitted in the direction of the longer correct pencil of the other glass.' The light then proceeding through each glass, as if from one of its aplanatic foci, is brought correctly to a focus by the combination. Supposing two glasses to have been so arranged, if the front glass is carried nearer to the back one, light proceeding from the shorter aplanatic focus of the front glass will reach the back glass as if from a point nearer than its longer aplanatic focus, that is to say, from a point between the foci, and therefore the spherical error will be over-corrected. On the other hand, separation of the glasses beyond their original interval produces under-correction. Thus, by merely varying the distance between two such lenses, the correction of the spherical error may be either increased or diminished at pleasure according to a definite rule, and slight defects in the glasses can be remedied by simply altering their relative position, the achromatism of the combination being meanwhile happily little affected.

Another beautiful circumstance connected with the aplanatic foci is that of their relation to the coma. At the shorter focus the coma is inwards, at the longer focus outwards ; and in a combination of two lenses arranged as above described, the inward coma from the shorter focus of the front glass destroys the outward coma from the longer focus of the back glass, and ' the whole field is rendered beautifully flat and distinct '.

The same principle applies when the lenses are of different form, and when more than two are combined. Thus the manufacture of the achromatic object-glass was reduced from a matter of uncertainty and empiricism to a scientific system, and has become susceptible of a degree of perfection that would otherwise have been impossible.

But though he had thus discovered the principle of construction, his own abours were far from being concluded. The next section of his notes is labelled

'Memoranda on object-glasses made for experiment, December 1829 to May 1830'. These include a great number of interesting observations, such as trials of lenses of different forms; descriptions of the 'colours of over, under, and right correction', as seen when the object is out of focus, illustrated by coloured sketches; experiments on the effects of varnish; proof that a compound lens has more effect on spherical and chromatic aberration when placed behind in a combination than when in front, &c.

Then follow a set of notes of peculiar interest, describing the effects of glasses made by his own hands. These are referred to in a letter to Sir John Herschel, of which he preserved a copy, together with Sir John Herschel's reply. The letter is dated London, 24th of 2nd month (February), 1831. In it the following passages occur: 'Finding, however, that W. Tulley was too busy to pursue for me the experiments I wished for ascertaining how compound object-glasses could be combined to the greatest advantage, I determined in November last to make a trial myself. The result was, I acknowledge, beyond my expectations; for without having ever before cut brass or ground more than a single surface of a piece of glass, I managed to make the tools and to manufacture a combination of three double object-glasses, without spoiling a lens or altering a curve, which fulfilled all the conditions I had proposed for a pencil of thirty-six degrees. . . . Long illness among my children afterwards absorbed all my leisure till about three weeks ago, when I made a second and more complicated trial, projected for obtaining the same effect with a much larger pencil. This is just finished, but not without altering one of the original curves; and its plan might be improved if I could spare time to make another set. Still, I flatter myself these attempts would interest thee, as showing how easily the principle I mastered may enable an utter novice in glass-working to produce vision which I have not yet seen exceeded.' In the second of these trials he deviated from the plano-convex form of the lenses, employing a combination of three, of which the front was a double meniscus, the middle a triple, and the back one a double plano-convex. The reasons for preferring these forms are given in full detail in his notes, among which occurs the ingenious idea of regarding the triple with the middle of flint glass as divided by an imaginary line through the flint into two double achromatic glasses, each of which may be considered separately as having two aplanatic foci. The object he proposed to himself was 'a construction fitted to obtain the largest pencil with good front space and without coma'; and after describing the mode by which this was arrived at, he says, 'This combination proves most satisfactorily the advantage of keeping the angles of the rays at all the different curves moderate, the vision being singularly definite and easy. . . . Indeed, taking all

together, I think I have met with nothing to equal it—the distance of the front glass from the object being 0·11 full.'

Having now completely satisfied himself of the applicability of his principle, he devoted much of his leisure for several years to various investigations by aid of the instrument which he had so greatly improved. Some of the results are well known to the public. Selections from his observations on zoophytes and ascidians, beautifully illustrated by sketches from life by the camera lucida, form a classical paper in the *Philosophical Transactions*. But a laborious inquiry, chiefly conducted by means of the microscope, into the limits of human vision, as determined by the nature of light and of the eye, has never been published. He had at one time almost prepared an account of it for the press, when the illness of his eldest son, which ended fatally, threw such a cloud over his spirits that for several years he had not the heart to complete the work. And when at last he did resume it, and was on the eve of publication, he learned that the Astronomer Royal, Professor Airy, had reached the same conclusions, though by a different road, and so abandoned the idea, a circumstance in my opinion deeply to be regretted.

But to return from this digression. The next note in order of date regarding the construction of the microscope is one made in 1837, headed 'Remarks on A. Ross's suggestion for three glasses to admit a large pencil, which J. J. L. thought would not answer. A. R. tried it, and found it a failure, before trying J. J. L.'s suggestion below.' Then follows a drawing of a proposed combination of three glasses 'for the same object', giving the dimensions of the lenses and the curves of the various surfaces, with a statement of the effect proposed to be produced by each glass upon spherical aberration and coma. This resulted in Ross's celebrated $\frac{1}{8}$–inch object-glass, the construction of which was afterwards adopted by the other principal London makers.

A statement in his handwriting found among his papers gives, in a few words, his relations to the British microscope :—

'I had been from early life fond of the compound microscope, but had not thought of improving its object-glass till about the year 1824, when I saw at W. Tulley's an achromatic combination made by him at Dr. Goring's suggestion, of two convex lenses of plate glass, with a concave of flint glass between them, on the plan of the telescopic objective. They were very thick and clumsy. I showed him this by a tracing with a camera lucida, which I had attached to my microscope, and the suggestions resulted in "Tulley's $\frac{9}{10}$", which became *the* microscopic object-glass of the time. But the subject continued to engage my thoughts, and resulted in the paper *On the Improvement of Compound Microscopes*, read before the Royal Society, on the 21st of January, 1830, announcing

the discovery of the existence of two aplanatic foci in a double achromatic object-glass. This has formed a basis for subsequent important improvements, the object of which has always been to obtain sharpness and achromatism over the field in the picture from a larger and larger pencil; this being an essential to obtaining higher and higher defining power.

'After succeeding fairly in a trial combination with this view, I left the subject for a while, hoping it would be pursued by opticians. But the glasses produced by the makers continued to be on the first simple construction of two or three plano-convex compound lenses till the beginning of 1837. At that time I called on Andrew Ross regarding some object-glasses he had made to a microscope for Richard Owen; when he told me he had been long engaged in unsuccessful trials for a new construction. And at his request I gave him a projection for a $\frac{1}{8}$-inch objective of three compound lenses, the front one a triple, which he soon worked out successfully, and it became the standard for high power for many years.

'For lower powers I suggested at the same time a double combination, and, borrowing of him a lens from among his former failures, and applying it in front of one of my own at home, obtained at once the performance required.

'It was natural that A. Ross should regard these as trade secrets; and accordingly, in his article on the microscope in the *Penny Cyclopaedia* he does not mention them, giving only the earlier construction of my article in the *Transactions*. The same is given afterwards in the treatise which J. Quekett asked at the point of its publication to dedicate to me! And I did not feel required to disclose A. Ross's secrets. After a while, with his consent, I instructed James Smith, 1840, to execute the same construction for inch and half-inch glasses. Even in 1843 it was with the understanding that he should not go to deeper powers than $\frac{1}{4}$-inch, and "Smith's quarters" were long in repute. In these projections the endeavour was to keep the angle of pencil at each surface of the glasses as moderate as was consistent with the other essentials; and by degrees the pencil admitted has been enlarged beyond my expectations. Some variations too have been since made in the construction in which I have had no part; but for all, the principle of the two aplanatic foci has furnished the clue.'

I believe I am correct when I state that in foreign microscopes also, object-glasses of high powers and fine performance are constructed on the same principle. And thus it seems not too much to say, as has been lately said by a Professor in one of our Universities—the son of one who was formerly associated with my father through a common love of science—that he was 'the pillar and source of all the microscopy of the age'.

Although in this notice I have confined myself chiefly to matters connected with the microscope, it is right that I should add that these were far from forming the exclusive occupation of his leisure hours. The comprehensive grasp of his intellect and the extent and variety of his attainments were as remarkable as the accuracy and originality which characterized his microscopical work. Indeed there were few subjects in literature, science, or art with which he did not show himself more or less familiar. His clear, calm judgement and strict integrity made his opinion highly valued among his friends in matters of difficulty or dispute. He was most unselfish, and scrupulously tender of hurting the feelings of others, and extremely generous in the pecuniary support of public philanthropic objects, as well as in secret acts of charity. Though warmly attached to the religious Society of Friends, to which he belonged, he was a man of very liberal views and catholic sympathies. But the crowning grace of this beautiful character, though it might veil his rich gifts from those not intimate with him, was a most rare modesty and Christian humility.

Living to an advanced age, he retained his activity of body and mind to the last. But while to his friends this appeared remarkably the case, he was himself keenly alive to the gradual effects of years upon him, and his sensitive nature shrank from the idea of the helpless state to which he might be brought if his life should be prolonged like his father's, who lived to ninety-eight ; and he often expressed his desire that he ' might not outlive his powers '. His wish was granted. He had only just returned from a stay at the sea-side, where he had enjoyed long rambles and excursions, when a feverish attack rapidly but almost painlessly prostrated his strength. Fully aware that his end was approaching, his loving interest in others was conspicuous to the last, while for himself he showed no anxiety, except the earnest desire for a speedy dismissal. He died at Upton House, in Essex, on the 24th of October, 1869, in the eighty-fourth year of his age.

Believe me,

My dear Sir,

Yours very sincerely,

Joseph Lister.

To the Rev. J. B. Reade, F.R.S., P.R.M.S.

INDEX TO VOLUME II