X.—H. BENDELACK HEWETSON, M.R.C.S.,

Ophthalmic and Aural Surgeon Leeds General Infirmary.

MR. HEWETSON regretted that he was not able (being detained in another Section) to hear the papers on myopia. The following facts, however, would be interesting to the Section: A medical missionary—Mr. Harrison—whom he had specially trained, had examined a large number of aborigines in Cook's Isles in the South Pacific as to the prevalence of myopia in a set of people only thirty years removed from absolute savagery. Mr. Harrison found a very large proportion of highly-myopic savages. He (the speaker) also noticed myopia in horses and other animals, especially in horses which shied from an inability to see clearly. These were myopic by the ophthalmoscope.

XI.—CHARLES WRAY, F.R.C.S.,

Ophthalmic Surgeon Croydon General Hospital.

MR. WRAY said cases seemed at times to be congenital, for example, a patient came to the hospital when 4 years of age, with myopia over 17 D, and a large myopic crescent. It was difficult to believe that accommodation had any influence. The principal cases for treatment were those occurring in delicate subjects of tender age, in which through the extensile condition of the fibrous tissues it would be legitimate to suppose yielding of the sclera would ensue under prolonged eye strain at near work. Was there any connection between circulatory obstruction and myopia? Any increase in the circulatory mechanism *pari passu* with the increased demand for it by virtue of the estrangement of the globe? It was desirable in hyalitis with high myopia, to use "Franklins's," to take the strain off the ciliary muscle where a good distant vision was indispensable.

MR. PRIESTLEY SMITH'S REPLY.

MR. PRIESTLEY SMITH, in reply, said the errors which accompanied the use of the shadow test in high myopia depended chiefly on the deformity of the back of the eye; the disc stood obliquely, the macula was more distant than the disc, hence it was necessary, for correct estimation, to examine at the macula; but in very high myopia it was practically unimportant to distinguish objectively between, for example, 20 D and 22 D. His own measurements of 1,000 healthy corneæ proved that the size of the cornea bore no constant relation to the refraction; the average diameter was the same in H, E, and M. There was no evidence that high myopia of the ordinary kind was allied to glaucoma; the tonometer showed no excess of tension, the field was not contracted, the disc was not cupped, and the deformity of the globe was different from that which arose from glaucomatous pressure in young eyes; the yielding of the sclera was more localised. With regard to the correction of low degrees of M., the principle of removing inconvenience and preventing deterioration held good. To a schoolboy with M of 1 D, he would say: "Have you any inconvenience? Can you see the black board," etc.? He probably had little or none. Then he would give him no glasses. They would be more plague than profit even for distance, and for reading were unnecessary. But to the parents he would explain the probability of increase, the great importance of strict attention to the amount and mode of study, and would advise that the teacher be communicated with, and that the boy be re-examined in six months. If the M were higher than 2 D, he would give correcting glasses, probably to be worn constantly, certainly to be worn invariably for near work. He would say: "You may wear these all day long; you must wear them whenever you read, or write, or draw, or play the piano." He was very glad to find, from their discussion, that they were agreed as to the national importance of the prevention of myopia. Some recent writers were inclined to minimise it; he believed that such teaching was likely to do serious harm. While resolutions such as that which was about to be proposed to them might in time do good, he was inclined to rely more upon the individual efforts of each one of them in his own neighbourhood.

MR. RICHARDSON CROSS'S REPLY.

AFTER reference to some special points in the relation of the intraocular muscles to the globe and the importance of successive prolonged convergence of the eyes on the production of myopia, Mr. Cross alluded to the necessity, in these days of severe competitive examination, of giving the eyes every opportunity of doing their work in ease and comfort, and strongly advocated the importance of drawing the attention of school teachers and School

Boards to the fact that a large amount of myopia was directly due to the want of proper treatment of the eyes during school work and during home preparation. Everything possible should be done to make the lighting of schoolrooms perfect; to insist on a proper attitude of the students while at the lessons, and to see that properly arranged desks and forms were provided; and to encourage the proper printing of school books. When this had been done it would still be evident that certain eyes could not do the full share of the ordinary school curriculum, and both parents and teachers should realise the importance of seeing that the advice of the oculist was faithfully carried out, whether it were an occasional rest during school hours or the abolition of evening lessons, or absolute rest for six months, or whatever was considered necessary for the eyes. The serious tendency of some short-sighted eyes was also passed in review. But the object of the paper was to try to enforce the interest of the family physician and the public on the whole question, which ophthalmologists had done so much to explain and to evolve.

RESOLUTION.

On the motion of Mr. BELL, seconded by Mr. WRAY, the following resolution was adopted: "That the officers of the Ophthalmological Section communicate with the officers of the Ophthalmological Society of Great Britain and Ireland with a view of making recommendations to the Education Department for the control of the increase of myopia in Board Schools."

AN ADDRESS

ON THE

PRESENT ASPECT OF ANTISEPTIC SURGERY.

(A CRITICISM ON SIR JOSEPH LISTER'S ADDRESS
AT THE BERLIN MEDICAL CONGRESS, AUGUST, 1890.)

*Delivered at a meeting of the East Anglian Branch
of the British Medical Association, at Cromer, September 25th,*

By LAWSON TAIT, F.R.C.S.,

President of Mason College, Birmingham.

IN 1556 the learned Eusebius, seeing with pain that the French surgeons talked nothing but the jargon of the schools, wrote a book entitled *La Philosophie Nationale: apprenant la dialectique pour les Chirurgiens François*, by which he hoped to make easier for the surgeons the study of philosophy, or rather what we would now term logic. I think that an imitator of Eusebius is just as much wanted now as he was three hundred years ago, and that if the jargon of the schools were now less in vogue, and we had in its place some more logical methods, we should never have heard of the so-called antiseptic theory and practice as applied to the art of surgery. Now, perhaps, it is too late seriously to deal with this question, for the tone and attitude adopted by Sir Joseph Lister, at Berlin, clearly shows that the whole sad business is on its last legs, since its author has to lug in its two most active opponents and most successful critics as witnesses for his faith.

Looking back in the writings of our predecessors—and no matter how far we go in the research—we find it continually true of the theories which have been advanced to explain the phenomenon of fever and inflammation, and the proposals adopted for their treatment, that they have all been based upon the last scientific discovery of the day—no matter what that might have been—and it was quite immaterial whether the discovery was a real or an imaginary one. To begin with comparatively modern times, and take the false discovery of Stahl of the well-known phlogiston, a supposed inflammatory principle upon which the ignition of all bodies depended, it was clear that Stahl came very near the truth, but just missed it. It was perfectly certain from his experiments that there was a something upon which ignition depended, and from this conclusion another was advanced, that the exaltation of temperature, which was a leading character of all inflammation or fever, must depend upon the same thing, and the phlogistic theory of these pathological conditions became the fashion at the end of the last century. The inflammatory principle had entered into the body, was corrupting its juices and altering its solids, and

the fever was a burning, the result of the action of the phlogiston, and all remedies directed towards the treatment of the fever had to be of the so-called antiphlogistic type, a phrase which survived in lectures or *matéria medica*, and even in pure surgery, down to our own time. It was an inevitable result of the chemical theories of disease, which grew out of the great advances in chemistry achieved by Stahl, Lavoisier, and Priestley. So late as 1790 it was asserted that "all objections to the admission of phlogiston as an element are now removed, and phlogiston in the abstract is found to be no new principle capable of lighting our researches, but one very common and easily met with, being no other than common charcoal, in proof of the existence of which phenomenon, made evident by our success, we were presented by several experiments, one, the most conclusive of which [by Priestley] we shall recite, of the loss of one grain of charcoal of copper [which we, of course, now designate carbonate of copper] formed by the union of spirit of wine and copper, and which, like common charcoal, was consumed without leaving any residuum. He reduced four-ounce measures of dephlogisticated air till only one-ninth remained unabsorbed by water, and, again, with the loss of one grain and a half of charcoal, six measures and a half were pure fixed air. It is an absolute and undeniable fact that fixed air is composed of dephlogisticated air and charcoal (or phlogiston) and elementary fire."

Priestley's discovery of oxygen as an element destroyed all these theories, and early in the century the phlogiston theory was pretty well abandoned, but it was displaced only for another and equally unsubstantial theory to take its place, and, as anatomical and microscopical research advanced, theory after theory was brought into existence and abandoned in its turn. The strongest and most popular, perhaps, in its short-lived existence, was the gastro-enteric theory of Broussais. But the history of the phlogiston theory is the most potent, because it is the most perfect of its kind, for my purpose, and shows how far astray a theory may lead sober-minded men, when based upon incomplete and misunderstood facts. It shows better than any proofs that I could advance how facts were driven into the theory to bolster it up and explain it which are now known to be perfectly inconsistent with its initial conception—facts which, had any one of them been completely understood, would have been enough to destroy it. In the history of this strange and still fascinating doctrine we have a perfect parallel to the septic theory of inflammation and of fever, which is the favourite hobbyhorse of our own day. Everything at present has a septic origin and a septic inception, yet I venture to say that before the present generation has run out the word "antiseptic" will be all that is left to represent this strange structure, just as "antiphlogistic" was the only word left to represent the phlogistic theory in the middle of the present century. The only tangible survival of the phlogiston theory at present on our shelves is a quack remedy known as "James's powder." The only survival of our present antiseptic practice, I venture to prophesy, in another generation will be that debased and abominable member of the alcohol series misnamed carbolic acid. The fault which lies at the bottom of all this theorising—the construction of systems which do not on an average last for more than ten years—is a want of logic, as Eusebius pointed out; a want, in fact, of the initial basis of all logical systems—a strict definition. Just as in the sixteenth century, so in the nineteenth, a simple fact has been dubbed a theory, and, instead of the septic or antiseptic fact, Lister and his still more illogical disciples talk of the septic or antiseptic theory, whereas there is no theory about it at all, but an absolute and ludicrous logical error. Not content with this blunder, Lister, in the address which is my present object of criticism, speaks of Metschnikoff's relation of a fact as a phagocyte theory. It is not a theory at all, but a simple, still all-important, fact; and the folly of the misrepresentation consists simply in the manufacture of a theory out of an isolated yet important discovery.

Let us come to a real understanding of what we are talking about, and lay down the definition of a theory to be the collection of a series of concordant facts, upon which is based a "working hypothesis." Upon this, as a temporary structure, is collected a still larger series of facts, some positive, many negative, and a few actively opposing, and every conceivable point which bears upon the question is investigated till it becomes evident that the larger series of facts are strictly concordant, whilst those in opposition become steadily reduced; and in this way there is built up a theory. This theory finally becomes a law when all known facts are shown not only to be strictly in harmony with it, but

when formulated as a law it leads to the correlation of facts which up to that time had been supposed to be altogether outside the area, first, of the hypothesis and afterwards of the theory. I need not say that until a very recent time—till, indeed, the time almost within the life and experience of all those present—biological laws had hardly any existence, and to understand what they really are there is only one master in whose lines we must follow. Anyone who will read and understand any one of Darwin's books—say, for instance, that most marvellous piece of reasoning, his work on *Insectivorous Plants*—will be able to comprehend the difference between the position of any one isolated and simple fact and the construction of a theory, and he will never have to alter his views about his facts and the statements of his theory at frequent intervals. Sir Joseph Lister has now to make the confession that he is ashamed of ever having introduced the spray, yet in that point he seemed to me to approach the dignity of logical completeness.

I lately had an opportunity of saying that I doubt all surgical theories. I shall continue to doubt all surgical theories till some surgical Darwin shall arise. I have never known a surgical theory which has led to anything, but I did not suspect that I was so near to my great opportunity, for you know that humble surgical practitioners like myself, when we utter our little protests against great scientific authorities, are sneered at as merely empirical people devoid of science. Quite lately I had my sneer in turn at the scientists, and I received such a storm of applause alike from the critics and from the body of my profession that I warn these eminent scientific people against whom I am now raising my voice, that we do not propose to let them from this time forward have it their own way so completely as they have done.

It is, however, worth while, *en passant*, to listen to Sir Joseph Lister's views of what we may term real science and see how he treats an ancient, respectable, and thoroughly well-established law such as the law of gravity. Thereby we shall see what crude notions of logical definition he possesses. Speaking of the process of cure of an empyema and the closure of the affected pleural cavity, he says: "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." Shades of Newton and Torricelli, can this be true? Is there a new law in physics for the weight of the atmosphere? I am content to be a humble surgical practitioner, a perfectly unscientific person, and I am content to believe with the bulk of humanity that, given a membrane to which the atmosphere has free access on both sides, on both of these sides the pressure of the atmosphere will be exactly the same—about sixteen pounds to the square inch at the ordinary sea level, and with certain ascertained conditions of temperature. I am perfectly content to believe what I see—that the closure of the pleural cavity is brought about by well-known pathological processes, and that when the closure is completed the natural dimensions of the affected lung are always remarkably diminished—often to complete obliteration—for which there occurs later on the well-known and wondrous compensatory enlargement of the other lung—what we call the voluminous lung. Who does not know the twisted spine of the old empyema?

Let us, however, return to our sheep as we find them in Lister's Berlin address. He begins his argument by reasserting that Koch has picked out with unerring precision the cholera microbe with as much definiteness as if it were a rose or a cabbage. I have followed this question with great interest and close attention, and I am quite indisposed to admit any part of this statement except that Koch and others have discovered additional media on which minute organisms can be, as they say, cultivated, but there is nothing new about this. When I was a boy we found we could cultivate the old-fashioned yeast plant in two forms—in sugar solution and on raw potato; and we could alter in this way, vert and revert at will, the whole life history of the plant; and this plant forms the best example and the most familiar one of a "specific" germ—that is to say, a micro-organism having specific peculiarities and constant properties, yielding results which are practically unvarying.

That an indefinite cultivation of these germs in Koch's media will breed them true, or enable us to alter them at will, as we can the rose or the cabbage, has yet to be proved; and until it is proved, Koch's experiments have only the interest of an extension of exact knowledge, having a definite value, as all such extensions have, but not as yet of the slightest practical importance. Even

supposing he has really isolated the cholera bacillus, this again is a mere extension of exact knowledge. We all knew perfectly well that some definite poison was introduced in every case of cholera, and we know that it must be a specific entity, having specific peculiarities and constant properties, yielding results which are practically unvarying.

Thirty years ago, when our microscopes were so insufficient that we could only see and identify cells, we were deluged with a scientific cellular pathology as unintelligible as anything which preceded it, and there exists a great Government Blue Book in which the cholera cell is set forth with perfect accuracy. Do we know now that we are at the end of our tether? Is it the torula cervisiae that produces intoxication, or is it the alcohol which is split out from the sugar by the yeast plant? We know, of course, that it is the alcohol, because the agencies here are so gross that we have got to a finality on the whole question, in all probability. But we are not yet at the end of the germ question, nor have we, in all probability, reached finality in our methods of research; and this bacillus, if we admit to the full its potency for specific production and reproduction, may be, and probably is, only a step, and not a very great one, on the road of our research. We may, in fact, be here repeating our foolish illogical method in that the moment we have a new fact, like the cell and its nucleus, we go building it up into a theory. One fact alone shows that our discovery is not final, and that our theory had better lie quiet. It is this. If a thousand people drink the same germ-infected water, only a hundred or so will be affected. If the bacillus theory is a completion of the facts it is a law, and the whole thousand victims ought not only to suffer alike, but they ought all to die. But then they do not all die, for the majority of the hundred recover. The facts, therefore, about germs in the human body do not coincide with the facts of the germs in the gelatine flasks, and it is clear that there is something more we do not yet understand—something of the utmost and final importance in the story of disease, and which the flasks do not touch. These facts, as we have them, cannot even yet stand as the basis of a working hypothesis, far less of a theory.

Another of Lister's logical blunders is his application of Metschnikoff's research on the phagocyte. Here, again, there is nothing really new beyond extended and accurate observation. In my boyish biological studies we saw the "all-devouring amoeba," as our poetical Professor of Natural History loved to call him, under many different circumstances. We saw him, for instance, collar a paramoecium and destroy him, eat him thoroughly. At that time we thought that the paramoecium was the cause of decomposition. We know now that he lives on the products of decomposition, but in our days it was believed that the paramoecium was the agent of decomposition. We saw that the amoeba was his natural enemy. The struggle of the two seemed to us in its result to determine whether a particular pond in which we found them both should remain fresh or become putrid. If the amoeba triumphed the pond remained sweet; if the paramoecium triumphed the pond became putrid. We were also taught Cohnheim's views by Hughes Bennett, but we had not got so far as to put two and two together and realise four from it. We did not imagine that there could be varieties of still lower forms of life furnishing food for the amoeba in the forms of bacilli, because our lenses did not at that time enable us to see the lower forms save as bright scintillating points. Metschnikoff had more powerful lenses, and he did push to the conclusion, and thereby has extended our knowledge of the facts to an extent which is of great importance, but even this extension is a mightily different thing from the manufacture of a phagocyte theory. It is really a very pretty theory, but, unfortunately, it is not consistent with facts, and that in wholesale quantity—facts under the observation of humble and unscientific persons like myself. Even if the phagocytes are the means by which the tissues resist the threats of the omnipotent and ever-present germs, it is the phagocytes and their conditions which must constitute the really important elements of the question. Keep your phagocytes up to the mark, and you need never bother about germs; for it has ever been recognised that there was something in the body which resisted the power and attacks of disease. This has been a recognised part of every theory that has been presented for the explanation of the phenomena of fever and inflammation. What this was no one ever pretended to know exactly, but for it each had a name. There was no disguising the fact that something in the tissues protected the body against the agency for the introduction of disease, no matter under what theory this might have been discussed. The phlogiston theory

may be, as I have said, taken as a type of all such. Its supporters asserted that phlogiston had power to enter, but that the tissues had certain powers of resistance and much power of endurance, and finally they even conquered the phlogiston. Further, their theories of course led them to the establishment of antiphlogistic remedies.

With microscopic facts Lister alters his nomenclature, and that is all. He has bacilli and phagocytes and antiseptic remedies, but the whole thing is our old friend phlogiston over again. It is almost as inconclusive, and certainly as far as ever from being a finality. The illustration of cholera already given proves this, and many ingenious examples of it are given in the old books. We know that typhus germs in a city like Edinburgh or Dundee (at least as it used to be in my boyhood) must have blown about the town as thick as March dust, but they took effect only upon certain persons and under certain well known conditions, and we know now that typhus will flourish only when the density of the population passes the danger point.

Travellers pass from the haunts of typhus in various centres of overcrowded population into the city in which I live by hundreds and by thousands every week, and they must bring with them lots of these germs, but the germ of typhus has never taken root in Birmingham. Never in the history of our city, with its teeming artisan population of nearly 600,000 people, amongst whom dirt and hunger and misery and disease are as well known as in every large centre, has a case of typhus originated; never has a case of typhus died. Our Midland phagocytes are up to their work as far as typhus is concerned, and the same is true of cholera, for it again has never entered our walls. But the poor phagocytes are helpless against the bacillus of tubercle, and the microbe of syphilis and the micrococci of gonorrhoea.

These are facts which the phagocyte theorists must meet and bring within their fold before we can accept, not their facts, but their so-called theories—before we can allow them to arrange their so-called facts, even in the dignity of a working hypothesis. Further, we must ask them a funny question, Is there any one army of phagocytes for all kinds of bacilli, or are there different classes of these protective regiments, one kind of battalion for each separate kind of specific germ? If not, how do we explain the rhythmic progress of typhus, scarlet fever, measles, and small-pox—the rhythm of each differing so strangely—and the unrhythmic progress of typhoid, diphtheria, poisoned wounds, and syphilis? How do we explain that the phagocytes in small-pox cannot protect the cornea, and how in scarlet fever they do not prevent destruction of the middle ear? How is it that these strangely illogical lesions occur in diseases in which the germs run riot all over the body, and when all over the body there are phagocytes? How is it that two men are wounded at the same operation or *post mortem*, and the one is dead in forty-eight hours and the other fights on with an axillary abscess for months and then recovers? Something more is clearly required before we can establish a logical and complete theory.

Metschnikoff's facts are partially new and wholly valuable. Koch's researches are such as deserve to be encouraged and carried out to their utmost extent; but it is the height of obstruction to garner a few facts into our *répertoire*, facts obtained though they be from the labour of the scientists, and to formulate from them or from any one of them a theory which is to be conclusive, but which never can be complete. The mischief of this kind of proceeding is well seen in the history of geology. A few isolated facts in the history of rock formations had been properly resolved upon the hypothesis of fire action, and therefore these facts were elevated into a theory which governed the whole scientific world concerning the history of the earth's crust. A few dissatisfied people complained that many facts within their knowledge were incompatible with the fire theory; but they were awed into silence by the Wernerian answer, "So much the worse for your facts." Similarly my facts have for twelve years been treated by Sir Joseph Lister. He simply ignored them. He made no inquiries as to their truth; he tried no means of reconciling the difficulty between his theory and my facts; he simply went on with his settled theory and his constantly settled and as soon unsettled details of practice. This has made it extremely difficult to discuss the antiseptic theory as applied by Lister and his followers to surgical practice, for even in their theory they never bound themselves to any distinct expression, and as to their details of practice, they shifted so perpetually that it finally amounted to this: that their claim was for everything that was or seemed to be successful, and that meant not one single detail introduced by

Lister. Their cry was and is, in fact, the old schoolboy joke, "heads I win, tails you lose."

But recently, in one of the best papers on the subject which I have read, a real logical definition alike of the theory and the practice has been given, upon which I can say what I have to say. The theory, principles, and practice of Listerism consist, according to this definition, in "any method of treatment which aims at and succeeds in excluding the causes (germs) of decomposition from wounds." Here at last and at least is something tangible. With such a simple theory the details of its application ought to have been easily ascertained and certainly established; but the fact is that hardly one of Lister's details has not been abandoned. If the entrance of germs into a wound was the immediate and real cause of suppuration and of consequent poisoning, the device of the spray was the most completely logical appliance that could have occurred to the mind of man. Indeed, I was so impressed with its consistency and completeness that I was shaken in my disbelief and gave the method a protracted trial, extending over three years. Nay, more; I invented by far the best spray-kettle ever used, and I out-Listered Lister in its application. I read before the Royal Medical and Chirurgical Society the results of a deliberate and logical experiment upon one hundred cases of ovariectomy, fifty of which were performed with the most complete Listerian details up to date and fifty without any such. I not only showed that the mortality within the series was increased by the Listerian method, but that the other cases recovered better, more easily, and with less fever. Then I finally threw ridicule over the whole thing by going through all the exhausting ceremonies with steam and cold water, and getting better results as I went on. Lister still ignored me and my facts. But I went on publishing better and better results and laughing at Lister, his doctrines and disciples, and challenging them to produce their results. At last ridicule effected what argument could not do, and Sir Joseph condescends to notice my facts.

Having condemned surgical theories, I must not attempt to frame them; but a working hypothesis under which to arrange one's facts is permissible even with a small number of facts, but with nearly three thousand facts (abdominal sections and a still larger number of minor facts or operations), such an aid is perfectly legitimate. In such a case it is no more than an attempt to give general conclusions from the facts, and, as I have already said, till it is found to be wholly inconsistent with a strong series or even one strong fact it may be regarded as a working hypothesis.

I go back to my yeast plant for my analogy, and I say that that specific germ splits alcohol out of sugar, and leaves thus a constant, specific, and permanent result. The poisonous effects of taking a mixture of yeast plant and alcohol is due to the alcohol and not to the yeast plant. Such bacilli as cause decomposition (and such as have specific properties) split out from the dead organic matter some horrid things, just as alcohol is split out—the smells of all decomposing matter prove this; and such fluids retain their smells even when filtered under pressure through wood, impervious to even the minutest bacilli. This filtered fluid under suitable circumstances is fatal to animal life, and causes local sloughing when hypodermically applied to man. There is clearly, therefore, some result of the action of ordinary decomposition which is produced like alcohol, and, like it, poisonous in small doses, not deadly, but in large doses very deadly. Here is a possible explanation of one of the great clinical facts of disease, and personal idiosyncrasies of disease—as known to everybody—would immediately supply another. The yeast plant, by itself, is quite harmless, and in a solution of dextrose or sugar at a low temperature it will not produce alcohol. The ordinary bacilli of decomposition will not attack—at least will not produce these ordinary phenomena in living tissue, but they do so in dead tissue. Enclose some dead tissue with the necessary germs in living tissue, and you get a disturbance very fairly proportional to the dose given. If the dose is small or the tissue not very favourable for decomposition, the constitutional disturbance is slight. Thus a piece of dead beef as large as a walnut introduced into the calf of a man's leg would speedily excite a tremendous disturbance, but a piece the size of a millet seed would probably give no trouble. An ivory peg thrust into a bone rarely gives trouble, and leaden bullets lie quietly even in the brain for years without as much as indicating their presence. That is because, though such tissue is dead, it is not prone to decomposition.

Under the term tissue I include, of course, blood clot and serum.

Such tissues, when effused subcutaneously, may be either maintained in a really living condition, or they may become dead; on this most important question we really have no knowledge, but we know the fact. Whether living or dead, if protected from the access of germs, they do not decompose. The familiar example of a broad ligament hæmatocele proves this up to the hilt. Leave it alone, and the chances are fifty to one that it will slowly disappear without giving trouble. Open it or tap it—that is, admit the ordinary germs of decomposition—and you will secure abundant suppuration without fail. This is exactly the same thing as Lister's famous clot experiment, which Nature herself has been showing us in black eyes and other contusions for centuries.

I have said that part of my working hypothesis is that a dosage of the decomposing substances or of the poison resulting from the decomposition must have a great influence on the results. This, with our everyday clinical experience, amounts to a truism, but look how the Listerians regard it. With them one germ is as good as a thousand. This is not so with specific germs. One torula bulb will take an indefinite time to act on a large quantity of sugar, just as one Scotch fir would take an indefinite time to cover a mountain by seedlings. You may overdose your mash with yeast and make it useless. You seize the moment when your ferment is resting, and rush off your clear beer. Indeed, there is not a fact in the life history of the yeast plant and its specific action that is not full of suggestions which the unscientific empirical practitioner can see at the bedside.

Dosage of typhus fever germs tells a story like the yeast plant. I was six months clerk in Dr. Warburton Begbie's typhus ward at the top of the old Edinburgh Infirmary. I never stayed in that ward more than three-quarters of an hour without coming away with a headache, and an hour's stay produced loss of appetite and vomiting. A bigger dose would have given me an attack of fever, but I got into the way of timing myself to half an hour and taking a long walk round Arthur's Seat every evening, so that I escaped the dire disease, whilst six of my colleagues took it and five of them died. Concerning this argument, Lister unconsciously and indirectly admits its validity when he asks the question, "Surely no one would feel justified in leaving in the peritoneal cavity an unsterilised cord of silk as thick as a finger?" I answer that, if circumstances demanded such a proceeding, I should unhesitatingly do it, provided all the dirt and gum had been carefully washed out of the silk, and that it was pure silk. The gum in silk dissolves freely in water at 150°, and if pure silk is well washed in such water it is incapable of decomposition, no matter what germs are about. Doubtless, as Lister suggests, there are germs in the interstices of the threads, which the phagocytes may attack and enjoy—I don't know and I don't care—and if it would gratify the phagocytes in any way, I would stuff the abdomen like pudding with the germs or bacilli of decomposition, provided there was nothing present for them to feed upon. The great merit of silk is that it will not decompose, yet in time it disappears, whether eaten by the phagocytes or digested by the peritoneum and its juices I don't know, and I really don't care in the least.

I come now to more practical questions in Sir Joseph's address, and I take, first, the sentence which opens up his discussion on abdominal surgery. "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." The introduction of the phrase "it is said" is really too much. Either Sir Joseph has been living in the clouds of his spray for the last twelve years, ignorant of what has been passing round him, or he deliberately mistrusts what Bantock and I have been telling the world in our everyday practice—and a perfectly credible portion of the world has been to see and test the facts. "But in truth," he continues, "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." I for one distinctly wish it to be understood that I adopt none of Lister's so-called antiseptic precautions, because I have no fear of germs and never had. I have been trying to get Lister to believe for years past that all along he has been putting the cart before the horse, but he has never read my views. He tells us that we are antisepticians because we are very particular to clean our sponges. But the Listerian doctrine used to be that a dip in a 5 per cent. of carbolic acid would make any sponge safe. I always knew that it would not make them safe, for sponges are dead animal matter, most prone to rapid decomposition of the foulest kind imaginable. A damp sponge kept at a temperature of 100° will be a mass of stinking putridity beyond all imagin-

tion in twelve hours. A sponge tent after twelve hours in the uterus is the most stinking thing I know, and the most deadly if there has been a wound in the uterus. Sponges are the perfect type of the tissue I dread—dead animal matter specially ready and apt to decompose.

Hamilton's beautiful experiments with sponge grafting, all of which I have followed with fair success, were the first light which came to me explaining why Lister was wrong. I knew Lister was wrong before that, but I did not know why; but Hamilton's conclusions clearly show it, and they likewise bring into correlation a number of facts which formerly were discordant and apparently contradictory, as, for instance, why we can leave a small fragment of sponge in the abdomen, as we often do, without any harm; but if we leave a big bit the patient dies rapidly of suppurative peritonitis, no matter what Listerian precautions have been taken.

Speaking again of Bantock and myself, Lister says: "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." Before speaking of our position Sir Joseph really ought to have been to see it, and then he would never have written thus. The cleanliness adopted in my operations is strict enough, but even he would have been impressed with the fact that its details are directed towards dead tissue, and not against germs. But the above sentence is not interesting so much in the manner of Sir Joseph's reference to his critics, as in the complete falling away from his own faith. Were we not told for years that if our antiseptic measures failed it must be because one germ at least had got in? Sir Joseph now admits my second argument against his theory, that the facts of dosage are against it.

To continue. Lister says: "Both those 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 leaving in residual clots." If Sir Joseph Lister would witness the facts of the case, as they are in my practice daily, he will see that I care not a straw for injuring the peritoneum; that in the great bulk of my operations it is already so damaged that further injury is and must be a matter of utter indifference; and the only fact in his whole statement concerning me which is correct is that I wash away clots (pus, serum, and a great deal more) to avoid the risk of sepsis in the residuum. That is precisely what I have been teaching for the last twelve years. Lister's view was, "Keep out the germ and you may leave blood-clot (and other matters) to take care of themselves." My view was and is, "Get out all decomposable matter and you can let the germs in freely." Lister has now come round to my view, so where is Listerism now? As I said a few months ago at the debate at the Medical Society of London, it is as dead as Julius Caesar, after a short life of twenty-four years.

The mischief probably is not at an end, for we hear that the last phase of this astonishing craze is that wounds are to be dressed with still a new contrivance, one as deadly and dangerous as anything that can well be imagined—a double cyanide.

Lister first admits, as I have said, that I wash away clots to avoid the risk of sepsis, and in the very next paragraph resuscitates his old dead theory in the words, "But now that all are agreed that microbes are the evil with which we have to contend." There are two factors in the trouble, and it can be shown conclusively that one, the germs, are wholly inconsiderable without pabulum on which to feed; whilst the other, the pabulum, is sure to breed trouble, because it is practically and mechanically impossible to keep the germs out; they exist already in the blood and elsewhere, and are ever present, according to the best authorities.

Finally, Sir Joseph Lister claims the draining of the peritoneum as an antiseptic measure. It is not many months since we were very vigorously told by an eminent authority on abdominal surgery that, if the Listerian precautions were properly carried out, drainage was wholly unnecessary, indeed pernicious. When Keberlé first taught me drainage in 1873, he told me its use was to prevent the collection and retention of material capable of decomposition. In Chassaignac's writings may be found the most minute and detailed directions for the same purpose, and the most perfect reasoning on the subject. There is hardly a possible point in which Chassaignac does not

meet the whole requirements, save in abdominal surgery, which was, of course, not then invented.

It is a matter of ever-increasing wonder to me how Chassaignac's logical common sense and practical proposals have been neglected, whilst antiseptics has driven the surgical world wild with a wholly misdirected enthusiasm.

There are three modern details the adoption of which has certainly contributed above all others to make our work in abdominal surgery successful. They are the cleansing of the peritoneum by water, the use of the drainage tube, and the use of saline purgatives in the early stage of peritonitis following operations. I give credit to the first two for a very large contribution towards our success, but they are not so important as the third. Even when we have cleansed the peritoneum thoroughly of all dirt in which germs are likely to multiply and to produce their poisonous effects, and used a drainage tube properly placed, we may yet have in thirty-six or forty-eight hours indubitable evidence of the onset of peritonitis in a large proportion of cases. The prompt use of the turpentine enema and the seidlitz powder will arrest the peritonitis and divert the case from its fatal issue with as great certainty as anything I know in the practice of surgery.

In conclusion, I throw once more a challenge down to Sir Joseph, that he should publish his results in detail. He says: "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 have more than ever justified the performance of operations once quite unwarrantable." It is not easy to give the meaning of the last phrases of this sentence—they may be read in so many ways—but in any case we cannot accept without challenge his statements about the results. All that we want is a published statement of Sir Joseph Lister's operations in consecutive order of date, with satisfactory defining titles and accurate statements of results. I never saw any satisfactory statement of results of operations performed at King's College Hospital; and, as I said in my recent Address in Surgery, institutions which did not publish satisfactory statements of results were beginning to be regarded with doubt. Hence the repetition of my challenge to Sir Joseph Lister.

MEMORANDA:

MEDICAL, SURGICAL, OBSTETRICAL, THERAPEUTICAL, PATHOLOGICAL, ETC.

REINFECTION, RECRUDESCENCE, OR WHAT?

IN June last scarlatina broke out in the Idiot Children's Block adjoining Berrywood asylum, and among those attacked was one of the nurses. The eruption appeared on the second day of June; the patient went through the usual course of a mild attack, the cuticle desquamating considerably, but not profusely. The temperature did not rise above 103°. Convalescence was rapid; by the 20th June desquamation had entirely ceased, and the nurse was, to all appearances, perfectly well.

On July 2nd, however, the disease returned in an intensified form. The throat was more markedly affected; the eruption was more profuse; the temperature rose to 106°, and desquamation of the epidermis unusually abundant.

In neither instance could there be any doubt about the diagnosis, and although similar cases of recrudescence or reinfection have been mentioned by medical writers, I am not aware that they are of sufficiently common occurrence to prevent the above note being of interest to some of your readers.

Northampton.

RICHARD GREENE.

COCAINE POISONING.

A FEW days ago I received a most urgent summons, in the absence of my principal, to attend a lady at the local dentist's, who had been seized with alarming symptoms soon after the injection of $\frac{1}{2}$ grain of cocaine into the gum before the drilling of a tooth. I found her in a very prostrate condition, lying in the dentist's chair, with rapid feeble pulse and most urgent dyspnoea. Mr. Jones, the dentist, had already administered a little brandy, as well as applied nitrite of amyl to the nostrils, and under the influence of some more stimulant, which I ordered, together with the ap-