THE ANTISEPTIC THEORY

TESTED BY THE STATISTICS OF

ONE HUNDRED CASES OF SUCCESSFUL OVARIOTOMY.

BY

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THERE is hardly any department of medical practice in which the reasoning used to support any particular proposition is raised above the ordinary level of the statement of convictions or impressions of a purely empirical character, based solely upon the personal observation of the arguer. This is certainly the case in surgical practice, though here, perhaps, if anywhere, we might expect it to be possible to argue upon a purely physical basis. As soon as this does become possible we shall have made a gigantic stride in establishing rules of surgical proceeding, and shall thereby obviate much of the useless discussion with which we now occupy our time.

With these views, for which I hope a charitable consideration will be granted, I venture to indicate a line of research in which results may be obtained of a more definite and conclusive character than any yet laid before us, and if I have ventured to apply it to the discussion of

the antiseptic system it is because I think that this particular instance affords the best ground for arguments of a purely physical kind, based as it is upon a theory now so well supported that it may practically be taken to be one of the great laws of biology.

To those who have followed closely the elaborate researches upon the phenomena of putrefaction, which have occupied some of the greatest minds of our time during the last fifteen years, it must, I think, be admitted as an established fact that these phenomena arise from the presence of minute living organisms in the air which can be removed from it by a variety of physical means, by which the air is rendered absolutely harmless to the substances which are experimented upon. Further, that so far as we know, no phenomena of putrefaction do occur without the admission of these so-called germs to the substance putrefying, that the putrefactive processes depend entirely upon them and the organisms to which they give rise, and that the origin of such organisms within the putrefying fluid, independently of a sowing of seed in the fluid, though by no means to be regarded as an impossibility, is not yet proved as an actual occurrence. To any who take a general view of the biological scheme from the aspect of the evolutionary philosophy I think it must be clear that the so-called "spontaneous" generation of the early forms of life is a necessary corollary, but it is by no means certain that it is a part of the present process, nor is it likely, in my opinion, that we now have upon the earth such conditions as would render biogenesis possible.

For my present purpose, therefore, it is enough for me to assume, as I do most fully, that the germ theory has been completely substantiated, and that no known process of putrefaction does occur save by the admission of resting spores or swarm spores of some of the many minute living organisms which are invariably associated with putrefactive changes. But concerning this there is another constant position associated with these phenomena. The

materials upon which the experiments have been made, of infinite variety of kind and constitution, have all been dead, and no one has yet pretended that, by the admission of germs to living matter, he has produced the phenomena of the putrefactive changes which constantly result in matter which is dead. To quote the apt illustration given by Dr. Wm. Roberts in his masterly exposition of this most difficult subject, the ordinary hypodermic morphia syringe will inoculate inevitably a sterilised solution of dead organic matter, but amongst the hundreds and thousands of hypodermic injections which are made daily, no one has yet declared a single instance of putrefactive changes resulting from it in the healthy, or even in the diseased, human body.

It will, therefore, be seen that the application of the facts of the germ theory of putrefaction to the phenomena of diseases of living tissue is met at once by an overwhelming difficulty, to the removal of which none of the adapters, so far as I have seen, have as yet applied themselves. Granting that the same germs which would inevitably produce putrefaction in a dead infusion of beef are constantly admitted to wounds, there is not the slightest particle of evidence that they do produce any change whatever upon living tissue, still less is there any evidence that the changes which occur in the numerous varieties of what we call blood poisonings, even when they are of an undoubtedly local origin, have the slightest analogy to those seen in a putrefying dead infusion. The mere presence of bacteria in the fluids of wounds, or in fluids enclosed in cavities, whilst offering many difficulties to the adapters of the germ theory, prove nothing for their position until they have shown that these organisms ever do occur in fluids or tissues which are truly living.

The difficulty, therefore, is this, that what we call vital action, for want of a name based upon a better understanding of what it is, places living tissue in an altogether different category from tissue in which the phenomena of life are no longer present.

Now, this is consonant with every-day experience. If a decaying hyacinth bulb or a rotting apple be examined, the presence of the minute forms of life is found to be absolutely confined to those parts where the changes have been effected, whilst those parts to which the rot has not extended are found absolutely free from them, and the difficulty of the adaptation of the germ theory is simply this, that its advocates have assumed that the invasion of the germs is the cause of the decadence of the vital phenomena and the ultimate death, whilst there is the alternative still undiscussed and certainly undismissed—that the decadence of the vital powers, due to some cause possibly yet unknown, is that which gives the germs their protential ascendancy, and enables them to do what, during full vital action, they were wholly unable to effect.

If the views of the germ theorists were correct we ought to expect that no operation could be done successfully without rigid antiseptic precautions. The slightest cut of the skin ought to be followed by septic poisoning. There ought to be no difference in the mortality of operations in small and in large hospitals, in town and in country. In fact, if germs could have had the unbounded influence which is claimed for them by many antisepticists surgery must long ago have been an extinct art, if, indeed, it ever could have struggled into existence.

The uniform experience of operating surgeons has taught them that the success of their work will depend upon three factors—the condition of the patient, the condition of his surroundings, and the nature and extent of the operation performed.

Of these three, most undoubtedly the most uncertain factor is the first. What condition of the system it is which is favorable to operations is almost unknown. I must base my conclusions chiefly upon my own work, and in my special operation of ovariotomy I am perfectly certain that apparent perfect health is by no means a certain indication of a power of resistance to those conditions, whatever they be, which result in so-called septic poisoning.

The second of the factors, the condition of the surroundings of the patient, contains elements of far greater certainty. It has approached the position of a statistical law that the death rate is in constant harmony with the density of the population, and that when the density exceeds a certain minimum of safety there are introduced specific septic diseases, as typhus fever, which are wholly unknown under other conditions, and which, even after the danger density has been reached, attack certain individuals only, and not all, for reasons which can be expressed only by saying, as I have already said, that the living tissues of those affected could not, and did not, resist the septic influence.

Every advance we make in sanitation shows that this factor, the condition of the surroundings of the patient, is of extreme importance.

The third factor which influences surgical success is the extent and importance of the operation performed. Everybody knows that while amputation of a finger is probably fatal in not more than one in ten thousand cases, nearly one half of all amputations of the thigh die. Now, if the adaptation of the germ theory to surgical practice were as promising and as legitimate as some of its supporters allege, we should have had the remarkable result, previous to its application, that amputations of the fingers and of the thigh ought to have approached one another in mortality to an infinitely larger extent than they have done.

If the contact of a bacterium germ upon a wound could be the source of blood-poisoning then the size of the wound and the nature of the operation could make but small difference in the result, and a wound into the theca of a finger tendon, and one of similar size into the peritoneum of another patient in the same ward, ought to have had very similar risks. But, as a matter of fact, they did not, and we are forced to the conclusion that, even if bacterium germs lighting on wounds were the cause of much surgical mortality, that the power of vital

resistance by the tissues or the condition of the patient, and the extent and nature of the operation, are of infinitely greater importance as factors in the general result. This logical difficulty has evidently occurred to many of those who carry out Mr. Lister's adaptation of the germ theory to surgical practice. I have seen a rigid antiseptic occupy an hour and twenty minutes in making incisions a fraction of an inch in measurement, and barely skin deep, for the purpose of laying bare the tympanic membranes of an infant in whom they had been congenitally covered, the protraction of the operation being due solely to the antiseptic precautions. Such a proceeding produced in my mind a variety of emotions, chief of which were admiration for the enthusiastic consistency of the operator and sympathy for his evidently wearied audience. The just criticism of such a proceeding is, Has any one ever seen such a trivial operation result fatally from septic poisoning, unless in some such hospital, as is described by John Howard in 1780, as the Hotel Dieu, with three patients in each bed? I certainly never have, and I have performed some thousands of them; and if it were necessary to take one hundred minutes to do what I could do in three I, for one, should seek my livelihood in some employment other than that of an operating surgeon.

The logical conclusion to be made from the facts is, therefore, that in minor operations germs have never, or at least hardly ever, any influence at all, and that in major operations the condition of the patient is of immense importance in enabling him to resist the influences, whatever they may be, which result in what we call the septic condition.

In any examination of the question there will of course be the primary difficulty, that it is by no means agreed as to what constitutes a major operation, and that between different operations which are admitted as major, there are known to be very different rates of mortality. Thus, amputation of the leg is proved, in my book on 'Hospital

Mortality,' to be more than twice as fatal when performed for accident as when performed for disease. It must be perfectly evident, therefore, that any examination of this question must be conducted upon the usual rules of statistical investigation, the chief of which is that similar and not dissimilar accidents should be grouped together. Any mere statement, therefore, of the general percentage of deaths from septic diseases on the general hospital or other population are absolutely worthless unless they be most carefully analysed, and they are, of course, open to the still further objection that what constitutes a death from septic disease is by no means a perfectly accepted definition.

There is a popular belief that statistics can be made to prove anything, than which there is no popular belief more erroneous. Statistics alone seldom prove anything, certainly they never explain anything. Thus, the Registrar-General's tables tell us that there are certain death quantities which are perfectly constant, and they establish the fact that half of all our human mortality occurs before the fifth year of life. But this neither proves nor explains the cause of this mortality, nor does it even explain its factors, until a more careful analysis of individual cases is made. Therefore, nothing whatever can be proved for or against the adaptation of the germ theory to surgical practice by mere statistical statements. But in spite of this, statistics may be made to show exactly in what direction analysis of individual instances should be made, and, therefore, they alone are capable of forming the first step of accurate inquiry. First, let us ascertain as fully as possible what the facts are, and then analytical arrangements of them will certainly afford a more or less complete explanation of their method of production.

Some of the warmest supporters of the antiseptic system uphold it, on the ground that under its protecting influence operations can now be undertaken successfully which formerly were impossible, such as laying open joints, &c. Before I proceed to the immediate subject of

my paper, I desire to point out that this is an argument which cuts both ways, and which seems to me to form one of the great dangers of antiseptic surgery. The immense favour with which the antiseptic system has been so widely received, is most undoubtedly due to the fulness of its promise as a royal road to surgical success, as a something which puts the skilled and the competent upon a level with the inexperienced and incompetent; and I know that there have been abundant instances of bitter lessons already, that even an antiseptic spray will not condone the want of manipulative dexterity or the absence of readiness in emergency.

There is, further, an inevitable result in the full acceptance of this germ-theory adaption, that the other factors —the condition of the patient and his surroundings—will be relegated to unimportant positions, and we shall have a great risk of inducing an inattention to general hygiene and the incursion of rash experiment, which will do more harm than antisepticism will do good, even if everything claimed for it is true. That this is no fancy sketch, is proved by what Mr. Spencer Wells narrated in his lectures at the College of Surgeons on abdominal surgery.

Even if it were true that joints can be opened now as they could not be before, the question must first be answered before the fact can be credited to the antiseptic system—Is this more than was to be expected from general surgical advance, seeing that in 1866 ovariotomy was opposed as an operation of an altogether unjustifiable character, and yet *before* the antiseptic system was applied to it, it had become more justifiable than lithotomy or amputation below the knee?

It is, therefore, evident that for the proper estimation of all results a most careful analysis must be made of all known or possible factors contributing to them. I have already said that no rough aggregate of operative results would be of the slightest use for any purpose whatever. The estimation of the results in one particular operation, such as ovariotomy are, on the contrary, of immense import-

ance, not as proving or explaining anything, but as pointing to the directions in which research may be made with profitable result. Thus, it must be evident to every one that a large group of one hundred ovariotomies must present features more similar to those of another set of a hundred than can probably be got in any other surgical comparison which is possible; and it is a probably correct assumption that if the same surgical skill and patience, the same attention to minute details, and the same state of the surroundings were common to the two groups, their resulting mortality would be identical or nearly so. But if there is one thing we value more than another, as being likely to contribute to success in surgical operations, it is personal experience; and we, therefore, may fairly expect that with each succeeding hundred ovariotomies the mortality will diminish, owing to the increasing skill of the operator. And this is the case notably in the practice of Dr. Keith who, beginning with 11 per cent., went successively down to 8 and 6, before he began to use antiseptics; and of my own experience I can only say that whilst I had nineteen deaths in my first fifty operations, I had only three in my second fifty, and I fully expect that in my third group these good results will be at least maintained.

Now, can such a method of examination, that is, by mortality results, indicate anything more than the mere increase of general skill? Can it be made to show for some individual proceeding an advantage over some rival practice? Most certainly not. A recovery after an ovariotomy is the sum of a number of details, all of which were efficient. A death, on the contrary, may be the failure of one only, and that may be or may not be under the control of the surgeon. Thus, of my three fatal cases in my second fifty, two were deaths due to details wholly beyond my control, and having no relation whatever to either the antiseptic system or any other of the operative details. The third death was due, as far as I could determine, to the irritative effects of thymol used with full antiseptic details. Two of these deaths were antiseptic out

of twenty-nine cases treated antiseptically, whilst of twenty-one cases treated without antiseptic precautions, I had only one death, and as she died within three hours after the operation, the want of antiseptic precautions could have had nothing to do with her death. From this group of cases, therefore, the argument would be wholly against the antiseptic system, and though my impression is that the conclusion would be a just one, yet the argument is absolutely fallacious, as all such are.

In the discussion of this question, which occurred a few months ago, the only statistical argument of the slightest importance was given by Mr. Spencer Wells, who said that a very marked improvement had occurred in his results since he had used antiseptic precautions. But nearly concurrently with his adoption of germicides, he adopted the intra-peritoneal method of dealing with the pedicle, a method which has been superlatively successful in the hands of Dr. Keith, and to which chiefly I attribute my own rapidly increasing success. Thus, Mr. Wells' mortality improvement argues nothing in favour of antiseptics, but probably far more, in my opinion, for the short ligature.

Here, for instance, are my own results, which point conclusively in this direction.

		Per cent. mortality.
(17 cases).	Ligature, non-antiseptic .	5·9.
(29 cases).	Ligature, antiseptic . .	3·45.
(36 cases).	Clamp, non-antiseptic . .	25·
(26 cases).	Clamp, antiseptic . . .	27·

Dr. Keith, in the record of his cases, does not give completely such details as afford a perfect statement of his results based upon the various methods of treating the pedicle, but he tells us that in his first 50 cases he used the clamp 48 times with 9 deaths. In his second 50 he merely indicates that his confidence in the cautery is returning. In his third 50, the clamp was used 34 times, with 7 deaths, and the cautery and short ligature 15 times without a death; and now I understand from himself he

has entirely abandoned the extra-peritoneal method of dealing with the pedicle, as indeed has everybody else, by reason of Dr. Keith's unprecedented success with the intra-peritoneal method, even before he adopted antiseptics.

It is made very probable by this, as far as my practice is concerned at least, that the improvement is due chiefly to the introduction of the intra-peritoneal treatment of the pedicle, and as far as can be seen there is nothing to be credited to antiseptic precautions, for the difference in result between the ligature used under antiseptic precautions and without them is no more than can be safely referred to increased personal experience, as the non-antiseptic cases were of course of earlier date. However, I am not prepared to say the antiseptic system is absolutely without result, for it certainly must be admitted that occasionally an operation will be done under some unfortunate circumstances, so that immediate and direct poisoning of the wound may take place; and that any substance having the properties of carbolic acid, and used according to Mr. Lister's plan, may have the effect of preventing disaster. My own belief is that if this were effectual only once in a hundred operations we should adopt these precautions in the whole hundred, unless it could be shown that they were productive of greater harm in other directions, and I am bound to say I do not think they are.

In a previous communication to the Society I have already said that some of the details of the antiseptic method prevent the healing of wounds, and this is an absolutely uniform experience in my practice. I have never yet got an ovariotomy incision to heal with Lister's dressings, as I nearly always did with Mr. Wells' dressing of plain dry lint, and to the dry lint I intend to go back. Some of the antiseptic disciples have told me that my failure in this direction is due to the inefficacy of my methods, and it is said of others who fail equally that it is due to their want of belief. Now, such arguments are really childish. In my own case the want of belief is not

a difficulty, for I have the most profound belief in the germ theory of putrefaction, but I utterly fail to appreciate its application by Mr. Lister. The statement that my methods are faulty is, of course, a charge against either my intelligence or my honesty, and I reply that both are quite equal to the average, that I as earnestly desire to cure my patients as can any of my confrères, and that, in spite of the failure of the method, my patients get well in a proportion which is greater than has yet occurred in the parallel experience of any other ovariotomist, that is to say, in Dr. Keith's second 50 he lost 8, and in Mr. Wells' he lost 17, whilst I have lost 3.

But this statistical research points out that there is a method which, so far as I know, is yet a wholly untried one, and which may give very important indications of the value of any individual detail, such as the antiseptic system, especially one like it, based on a theory of a purely physical kind, and having a claim for preventing febrile complications.

This method is based on an examination of the method of recovery of those in whom the sum of the details is such as to lead to that satisfactory result.

The basis of the antiseptic claim is that the system prevents septic poisoning, that is, septic or surgical fever. Every one who has watched a number of ovariotomies knows that by far the larger number of deaths occur from the incidence of fever, and that the pulse and temperature rise progressively, though perhaps with intermissions, till they reach the fatal vanishing points. With a single exception this is true of all the deaths I have had. If, therefore, the antiseptic system favours a larger number of recoveries by preventing septic fever, it is an absolute certainty that the recoveries will be uniformly and correspondingly facilitated, inasmuch as in non-antiseptic cases the germs will enter every peritoneum and will theoretically produce fever in every case, and only in those cases where there is a sufficiency of an unknown something which counteracts the septic poison will recovery be obtained.

Equally according to the theory will the germs destroyed by the antiseptic precautions enter the peritoneal cavity harmlessly, dead and unfit to produce septic fever.

Another step in the syllogism is that as the temperature and pulse curve are uniformly admitted to represent the course of any case involving febrile action, if the antiseptic system makes its claims justly, ovariotomies performed under its precautions ought to indicate a more even and less febrile course of recovery than the non-antiseptic cases, and this should occur independently of all other details of the operation.

I would put the possible conclusion briefly thus:— If we find a marked difference between the curves of cases treated antiseptically and those not so in favour of the former, then I think I may say that more has been done to establish Mr. Lister's view than anything I have yet seen. If there be no difference, then the question is just where it was; but if there be a difference on the other side, then I think the application of the germ theory to surgical practice will be certain to fade away from professional and popular acceptance just as many as fair-looking visions have done before.

Further, a just grouping of cases may display wherein consists the real road to surgical success.

The line of research which I have taken is already sufficiently indicated, but I may further say that, like Mr. Spencer Wells, I have a separate note-book for every case of abdominal section, in which every particular of the slightest importance is entered. The pulse and temperature observations are taken by trained nurses, and they are constantly subject to my personal testing, so that I think it hardly possible that error of any appreciable extent can have been introduced. If there has been, it is of course quite as likely to have occurred on one side as on the other; and if on any certainly on both. At any rate, the observations were made without the slightest reference to any purpose other than as a periodical record of the progress of the cases, and therefore any conclusions

they may point to can have no prejudice about them. I may say further, that the remarkable regularity of the curves is extremely suggestive of their accuracy, and I ought to add that in three instances I was able to detect errors by a re-examination of the data constituting points wherein a remarkable deviation from uniformity occurred. My experience in this is so very striking that I feel persuaded that if I could have dealt with hundreds of cases, instead of scores, I should have been able to present curves of absolute standard uniformity. As they are, I believe their value to be great, but not absolute, merely by reason of the smallness of the figures from which they are constructed, and it is almost unnecessary to point out that, under such circumstances, any error or accidental variation has a decreasing importance just in proportion to the amount of material employed.

The plan used is of course very simple. Taking the morning and evening observations of the temperature and pulse for each case during a period of ten days, I constructed for the morning and evening of each day an average of the total observations under discussion, and marked this upon the graphic paper. I took ten days as the limit, because I believed that this exceeded by at least three days the average period of stable recovery in cases of ovariotomy, and because it was the limit to which the observations could be extended with full material. My general impression was that a successful ovariotomy was practically well on the sixth day, but it will be seen from the charts that, like other general impressions, this is quite a mistake, for convalescence is not fully established till the eighth day, and is certainly not complete on the tenth. Therefore, probably my conclusions would have been better with more extended observations. I also see now that my statements would have been more perfect if I had carried out my figures to two, or even three places, but this would have involved an amount of labour for which I may frankly say I was not prepared.

Concerning the mere duration of recovery, some inter-

esting remarks may be made. Taking the curves of Fig. 11, which are constructed from the whole of the 100 cases, it is quite evident from the pulse curve, still more from the temperature curve, that recovery takes a sudden progress forwards on the eighth day, but that it is not then complete. On the sixth and seventh days the temperature gives distinct indications of exaltation, especially nocturnal, and this is clearly seen, on examination of the constituent curves, to be due to the suppuration consequent on the separation of the clamp, and probably, also, on the formation of stitch-hole abscesses.

The consideration of this curve (Fig. 11) leads me to say that I attach less value to the temperature curves than to the pulse curves, for the reason that the temperature during the course of recovery from ovariotomy is liable to extraordinary explosions. I have repeatedly seen a patient's temperature rise three or four degrees, and in one recent case six degrees centigrade, without the slightest apparent reason, the exaltation lasting from half an hour to three or four hours, and then the temperature would fall quite as rapidly, leaving the patient without any appearance of effect, or any record of it, save on the chart. This is not the case with the pulse curve, for if that rises the general appearance of the patient, and other signs and symptoms, amply prove that something is wrong, and the changes of the curve do not occur or give way with rapidity, but always gradually. Therefore, temperature readings require to be far more numerous than pulse readings to give the same uniformity of result. Pulse readings are also not subject to such influence by limited suppuration as temperature readings, and this is shown by the marked difference in the temperature and pulse curves (Fig. 11) on the sixth and seventh days. Further, the temperature rises almost uniformly at night during the progress of recovery, whilst the pulse does not do so after the fourth night, and this confirms my general impression that the fourth night is the critical night of the course of an ovariotomy. My conclusion is

finally confirmed by the fact that, whilst I have seen a case end badly without the temperature rising to any remarkable height, I have invariably found the pulse rise continuously till it disappeared.

The constituents of the curves on Fig. 11 are broken up into minor curves in the preceding ten figures, each such curve consisting of an average constructed from a varying number of cases having some pronounced feature of their treatment in common. Fig. 1 includes 27 cases in which the pedicle was dealt with by the clamp, without any antiseptic precautions having been employed. Fig. 2 includes 19 cases where the clamp was used with full and rigid antiseptic precautions throughout the case. Between these two groups there is really very little difference. The non-antiseptic cases had slightly higher temperatures during the second, third, and fourth days, but during the same days the antiseptic cases had a more pronounced difference between their morning and evening temperatures, and my impression is that this is a more serious condition than a more uniform range, even if fractionally higher. The non-antiseptic cases have a decidedly better pulse curve than the antiseptic cases, and, as far as this indication goes, their recovery was one day shorter than the antiseptic cases.

The curves on Fig. 6 are constructed from 26 cases in which the pedicle was dealt with by three intra-peritoneal methods without antiseptic precautions, these three methods being given in sub-groups in Figs. 3 and 4, and one case in which I used the cautery, but of which I have not thought it necessary to give a special figure. Fig. 6 makes it perfectly certain that in cases where the intra-peritoneal method is employed, recovery is more even and speedy and much less anxious than when the clamp is used, either with or without antiseptic precautions, and this is absolutely in harmony with the corresponding death rates. Judging from both temperature and pulse curves, the non-antiseptic and short ligature method gives a better and shorter recovery than any other; and if

we compare Figs. 4 and 5, where the short ligature intra-peritoneal method is contrasted in 15 cases where it was employed without antiseptic precautions, with 28 cases where they were rigidly carried out, the non-antiseptic cases, we find, make somewhat, though not very much, better recoveries than the antiseptic cases, the difference consisting chiefly in the nocturnal acceleration of temperature on and after the fifth day. This I believe to be due to the uniform suppuration of the wounds which has occurred in the antiseptic cases, and which I believe to be entirely due to the irritating effects of the gauze dressings. This irritation was more perceptible when protective was used than when it was omitted, and I think the whole effect was due to the gauze preventing the drying of the wounds.

I have gone back to the dry lint dressings in my most recent practice with infinitely more satisfactory results in the wounds, and I have just read a paper of Prof. Hegar, in which he says that after having performed oophorectomy a large number of times with antiseptic precautions, he has given them up, as he got better results without them. Here it occurs to me to mention a very singular fact, that after the sixth or seventh day, or at most the eighth, I always gave up the antiseptic dressings and used the familiar red lotion, or oxide of zinc ointments, to facilitate the healing of the wounds. Under these old-fashioned circumstances, germs, of course, were allowed to do as they liked upon the wound, but not a single case died. Why was this? I cannot answer the question, further than by repeating the fact in another form, and supposing that in six or seven days after an ovariotomy the patient gets into some condition which enables her tissues to resist septic or germ influence.

It is impossible not to be struck with the remarkable regularity in the recovery progress of the 15 cases where the short ligature was employed without any antiseptic precautions, forming, as this group does, the most uniform of all the curves. The highest temperature is the re-

actionary rise within twelve hours after the shock of the operation, and that rise is limited to half a degree. After that the temperature is practically normal. The highest pulse rise is at the same time, and amounts only to 110; then from the morning of the third day there is such a rapid and uniform fall of the pulse curve that it becomes practically normal on the fifth day. Contrasting these curves with those of the clamp cases, whether used with antiseptic precautions or without, it is absolutely impossible to avoid the conclusion that the difference in recovery is due to the extra-peritoneal method of dealing with the pedicle, and there is an entire absence of any evidence of influence on the part of Listerian antisepticism.

Again, I have constructed curves in Figs. 7 and 8 for the purpose of contrasting the intra- and extra-peritoneal method of dealing with the pedicle, without reference to antiseptic precautions, and the result is certainly very remarkable. In the intra-peritoneal cases the important temperature exaltations are over on the third day, whilst in the extra-peritoneal cases the high temperature is continued till the seventh day, the evening temperature of that day being exactly the same (37·6°) as the highest temperature of the intra-peritoneal cases, this latter being practically the reactionary rise. The pulse curves of the two groups show still more startling contrast. The highest pulse of the intra-peritoneal cases is 104 on the evening of the second day, the reactionary rise; and from the evening of the third day till the morning of the fifth, it falls uniformly from 102 to 90. On the second day of the extra-peritoneal cases the pulse is 107, and it does not fall below 100 till the morning of the eighth day.

Finally, I have constructed Figs. 9 and 10 with regard purely to the employment of the Listerian method of performing operations, regardless of the methods of dealing with the pedicle, and I must point out that if the claims for this system which are made can really be substantiated in such an operation as this, its influence should be paramount, and should over rule all other details in its effect

upon recovery. But it is perfectly clear that it does not do so. The high temperature on the sixth and seventh evenings, visible in both figures, is derived clearly from the group of clamp cases. The general curve of temperature is decidedly higher for the antiseptic cases than for the non-antiseptic cases, and in the pulse curve the recovery of the non-antiseptic cases is pronouncedly more even and shorter than in the antiseptic cases.

It is not necessary to attribute this delayed and more difficult recovery to the use of Mr. Lister's various processes. It is quite enough to show, as these curves clearly do, that the so-called antiseptic processes of Prof. Lister have not facilitated the recovery of my patients, as is asserted to be the case in the practice of others. These assertions so far have been confined to the statement of general impressions, whilst I have given a physical basis for my impressions, which is capable of accurate estimation, incomplete it may be, but more complete than any attempt yet made, and for any suggestions of improvement in the method I shall be very grateful. In fairness to myself, I must conclude by saying that I have followed closely upon all the published details of those who advocate the antiseptic system, that at first I was strongly impressed by its influence, but that it was only after a large experience of it that I began to suspect that I was attributing to it effects due to other causes. My suspicions are fully confirmed by the facts I have given above, and though they will cause me to modify my conduct of the details, especially in the dressings, yet I shall continue to employ all the antiseptic precautions during operations, as I have done hitherto, until a wider experience decides whether they may not all be given up. I must once again admit the great lesson that the antiseptic system has taught me, that there is no detail in the performance of an ovariotomy, or in the preparation for it, so insignificant that it may be trusted to a deputy; and there is no circumstance so unimportant that a neglect of it may not lead to an important result.

DESCRIPTION OF CHART

OF

TEMPERATURE AND PULSE CURVES

IN

MR. LAWSON TAIT'S ANTISEPTIC AND NON-ANTISEPTIC CASES OF RECOVERY AFTER OVARIOTOMY.

Diagrams I, II.—46 clamp cases.
I.—27 non-antiseptic.
II.—19 antiseptic.

„ III—V.—54 cases (including 1 cautery case), in which the pedicle was treated by the intra-peritoneal method.
III.—10 écraseur cases.
IV.—15 ligature cases, non-antiseptic.
V.—28 ligature cases, antiseptic.

„ VI.—26 intra-peritoneal cases, non-antiseptic (1 cautery, 10 écraseur, 15 ligature).

„ VII.—54 intra-peritoneal cases, antiseptic and non-antiseptic (1 cautery, 10 écraseur, 43 ligature).

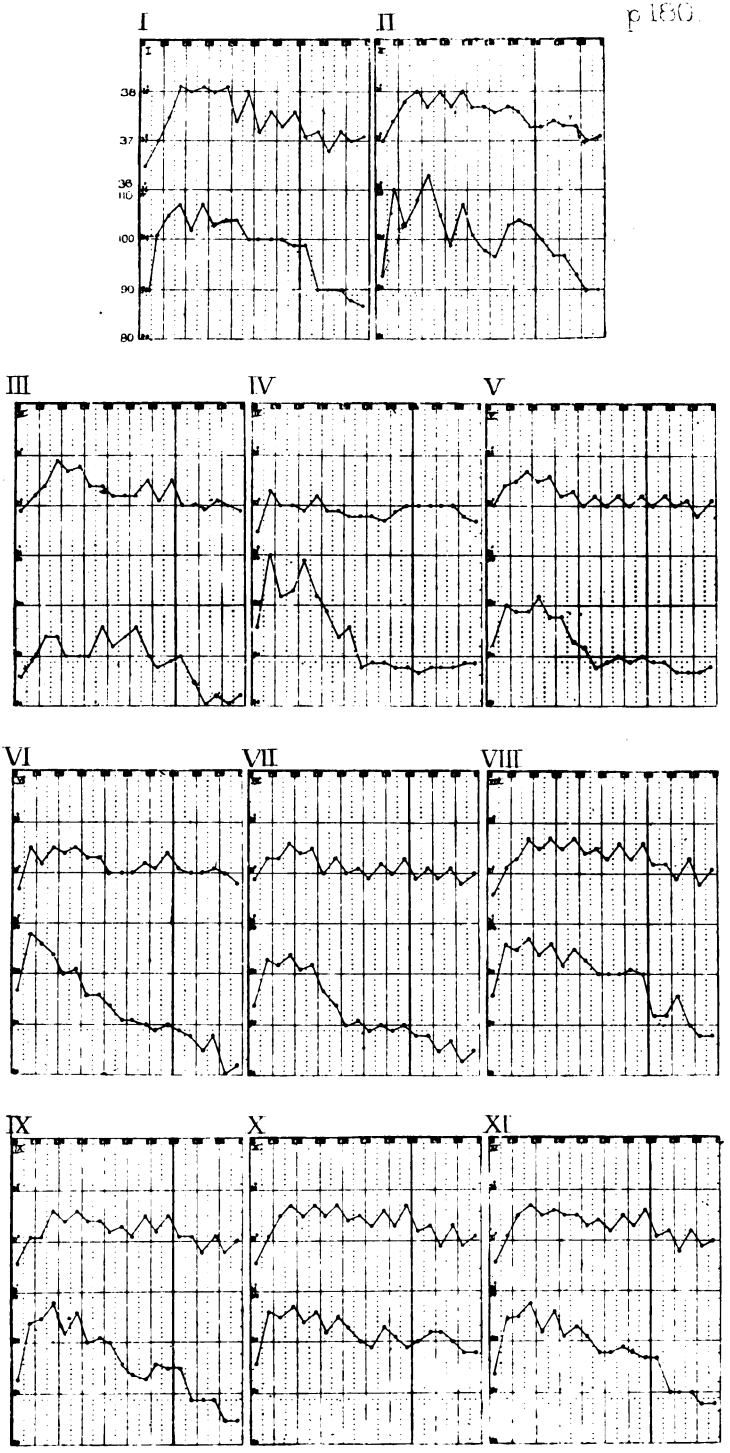
„ VIII.—46 extra-peritoneal cases, antiseptic and non-antiseptic (all clamp).

„ IX.—53 non-antiseptic cases.

„ X.—47 antiseptic cases.

„ XI.—Average of 100 cases.

Med. Chir. Trans. Vol. LXIII.
p. 180.



Muntern Bros. lith.