

similar 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 analyzed, 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.

Thus, it must be evident to every one that a large group of one hundred ovariectomies 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 ovariectomies 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 eleven per cent., went successively down to eight and six before he began to use antiseptics; and of my own experience I can only say that, while I had nineteen deaths in my first fifty operations, I had only three in my second fifty, and in all my subsequent practice these good results have been fully maintained; indeed, have been excelled.

A recovery after an ovariectomy 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 the 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; while of twenty-one cases treated without antiseptic precautions, I had only one death, and as the patient 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 some 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 favor of antiseptics, but far more, in my opinion, for the short ligature.

The greater part of what I have just said upon this most interesting and important subject, is taken from a paper which I read before the Royal Medical and Chirurgical Society of London, in February, 1880, and which is published in vol. lxiii. of the "Transactions," and its general conclusions are quite in harmony with the opinion expressed by Mr. Spencer Wells, in his lectures delivered before the Royal College of Surgeons. I give his opinions as they are reported in the medical journals at length, for they are very important. He says: "If (say the supporters of Listerism) in one of the most serious operations ever performed upon the human body, where the largest serous cavity is opened and the freest access is afforded to any infective organisms which may be near the patient, Mr. Wells can obtain eighty recoveries out of one hundred operations; and as on two occasions he has had long series of successful cases—one of twenty and two of twenty-seven—without a death to break the run of success:

if in the last two years of his practice at the Samaritan Hospital, in 1876 and 1877, he had only seven deaths out of seventy-one cases—not one in ten—and all this without any of the special precautions supposed to be necessary by the advocates of the antiseptic system—never using the spray nor any carbolyzed solutions for sponges or instruments, no catgut sutures, and no protective carbolyzed gauze for dressing; if such results as these, in such an operation, have been obtained, is it possible that the germ theory can be true or the practice founded upon it necessary? It is troublesome; may it not be injurious? Cases of poisoning by carbolic acid—fatal poisoning—have been recorded, and, in many cases where recovery has followed, the patients have been exposed to great suffering and inconvenience.”

“They claim the successes as proofs of the value of the [antiseptic] system; they explain the failures by some alleged neglect of some petty detail. In one case I thoughtlessly used a clamp which had not been carbolyzed, and was warned of probable failure; but the case appears, in spite of my negligence, on the list of successful antiseptic operations, and was one of the cases in which there was no fever after the operation. If there had been any fever, no doubt it would have been explained to the entire satisfaction of many people by the presence of some infective germs on the clamp. Of course, this explanation might possibly be true; and I freely admit that, in an experimental inquiry, no such carelessness should be permitted, especially as it gives rise to the suggestion, from zealous or enthusiastic partisans, that no one but themselves can be safely trusted to perform an operation antiseptically. “If,” they say, “you do not believe in the presence and omnipotence of germs—infective germs—all about the patient and surgeon, nurses, bedding, instruments, sponges, dressings—indeed, always and everywhere—you will be sure to leave some loophole unprotected against the entrance of one or more of these dreaded enemies. You must accept the theory, or act as if you did, or your practice will certainly be faulty.” My answer to this would be that I have watched the performance of a good many operations by surgeons who have complete faith in the system, and who believe they carry it out accurately, but I have never yet seen one case where a good many germs might not have escaped the action of the carbolic acid; and I believe that I or any surgeon present who wishes conscientiously to protect his patient from any of the atmospheric or other impurities which may possibly affect her injuriously, whether he uses carbolic acid or any other antiseptic, may, by careful attention, do so as completely and thoroughly as the most accurate experimenter can desire. Feeling all this, and feeling,

also, that undue faith in the system may lead to rash practice and the attempt to do things which had better be left alone, and knowing, also, how difficult it is to obtain any trustworthy, statistical, comparative facts to determine the relative success of any operation well performed, with and without antiseptic precautions—all other things being equal—I think what I have said of my own observations, although they only amount to twenty-two cases, may have some little value.” What Mr. Wells complains of here is, I think, a very just object of criticism. The argument of many of the supporters of Mr. Lister’s theories, and the promulgators of his practice, use an argument very like the school-boy catch, “Heads you lose, tails I win.” If there is success, then it is Listerism; if there is failure, then it is not Listerism; some important detail, like Mr. Wells’ clamp, has been improperly managed, and the failure is no fault of the system. Another method of their argument is to say that their opponents, having no faith in the system, can have neither honesty nor intelligence sufficient to carry it out. To such a method of discussion it is impossible to reply with courtesy; and my only answer is that, as it is my best interest to cure my patient, I am ready and, I believe, capable of applying any practice for that purpose, but that I do not care willingly to approach the land of quackery.

Since Mr. Wells delivered his lectures he has seen reason to change his opinions, and he now attributes his diminished mortality to the introduction of antiseptics. There is this difficulty, however, left for him to explain: his mortality now is double that which Dr. Keith had secured before he used antiseptics at all, and at a time when Dr. Keith’s experience was little more than a fifth of what Mr. Wells’ is at present.

I think it very likely that if my own results with the Listerian details had been bad, it would have been said that I did not understand the system, and never could, and I think the charge, probably, would have been correct. But my results were good, and therefore they were allowed to pass. But I really believe that very few of my cases would have been admitted by an enthusiastic Listerian, and I am quite sure that for the last two years not a single case would have passed muster. Whether or not that be so, here are the results of the whole of my practice up to the date of my writing this:

	Per cent. Mortality.
Ligature, non-antiseptic (187 cases)	3.74
Ligature, antiseptic (52 cases)	3.84
Clamps, non-antiseptic (36 cases)	25.00
Clamps, antiseptic (26 cases)	27.00

Can any reasonable being doubt that here the improvement lies in the giving up of the clamp? In the figures for the "antiseptic ligature" I give credit for twenty-two consecutive recoveries in which Listerism was not really complete, as Mr. Lister's dressings were never used, nothing but plain dry cotton wool being put upon the wounds, and if the results had been bad among these, my Listerism would be scouted. If I put these cases to the credit of "non-antiseptic ligature," as I very fairly might, then the account would stand thus:

	Per cent. Mortality.
Non-antiseptic ligature (209 cases)	3.00
Antiseptic ligature (30 cases)	6.6

Thus it is that I am very hard of belief about the merits of the Listerian details as applied to ovariectomy, the more so as I have carried out all the processes with plain cold water, and got quite good results, and I find that the chief advocates of the "antiseptic system," those who attribute their success to it, never get their mortality lower than ten or twelve per cent., whereas without it I keep under five per cent. I can regard this sudden love for the antiseptic system as nothing but a shield to cover the retreat from the use of the clamp.

Mr. Spencer Wells' last utterances on the question of Listerism are very important. In his recently published work on "Ovarian and Uterine Tumours" he says: "Four of my last sixteen deaths were caused by septicæmia, so that antisepticism has not abolished this plague of abdominal surgery. Lister's antiseptic plans have not brought me to the point of seeing no deaths from septicæmia, as promised by some of their enthusiastic promoters, nor have they advanced my success in operating beyond what was obtained without it." These words seem to me to contain a practical and complete admission of what I have always contended, that Mr. Wells' greater success was due to his giving up the clamp. The mortality of his first 800 cases was 25.5 per cent., while in the subsequent 200 it was only 14.5.

My views received very valuable support from Dr. Granville Bantock, Senior Surgeon to the London Samaritan Hospital, in a letter he published in the *British Medical Journal* for January 8, 1881, from which I take the following extract:

"I quite concur with Mr. Lawson Tait, when he says that 'the method of recovery' is of far more value in estimating the merits of any particular system than 'the mere death-rate,' which is often a matter of luck, and especially when the difference in the latter is measured by units. I further agree with him in attributing to the present mode of treating the pedicle

much of the success which has recently been achieved. If there is one thing more than another, in the matter of ovariectomy, to which I look back with satisfaction, it is to the persistency with which I held to and urged the systematic use of the ligature at a time when (in 1875), owing to the powerful and sustained advocacy of Mr. Spencer Wells, the clamp was at the zenith of its fame. Notwithstanding the excellent results obtained by Dr. Tyler Smith, the ligature had fallen into unmerited oblivion, and was at that time used only as a *dernier ressort*, and in the most desperate cases. Thus, Mr. Wells says (at page 361 of his book), 'you will hardly wonder that I use the clamp whenever I can;' and at page 371, he says, 'the more I see of ovariectomy . . . and the more I am driven to resort to cautery or ligature, the less I am satisfied with the results of these methods, the more reluctant am I to employ them, and the greater is my confidence in the clamp,' etc. The evidence I produced in favor of the ligature, as far back as 1872, before the Obstetrical Society, appears to have missed that notice which is claimed, and which subsequent events have shown it to have deserved."

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 that 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.

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 ovariectomies 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 few exceptions this is true of all the deaths I have had. If, therefore, the antiseptic system favors a larger number of recoveries by preventing the so-called 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, being 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, ovariectomies 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 favor 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 fair-looking visions have done before.

In order to test this point as far as I could I took the morning and evening observations of the temperature and pulse for each case during a period of ten days, and constructed for the morning and evening of each day an average of the total observations under discussion, and marked this upon 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 ovariectomy, and because it was the limit to which the observations could be extended with full material. My general impression was that a successful ovariectomy was practically well on the sixth day; but it will be seen from the figures 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 a great amount of labor.

Concerning the mere duration of recovery, some interesting conclusions were indicated. From the curves which I constructed from the whole of the one hundred cases, it is quite evident from the pulse curve, still more from the temperature

curve, that recovery takes a sudden progress forward 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 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 ovariectomy 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 on the sixth and seventh days. Further, the temperature rises almost uniformly at night during the progress of recovery, while 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 ovariectomy. My conclusion is finally confirmed by the fact that, while 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.

It is made almost certain by all that I have seen, 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 I can discover 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 not worth discussing, and depends really upon one death in one hundred and thirty-nine cases, and that death was not due in any either to septics or antiseptics, but would have occurred in any case. I have seen a case killed by thymol, and

this agent has been, by common consent, quite abandoned. Perhaps I have never seen carbolic acid kill a patient, as it is used now, but I have seen it produce very serious symptoms, and, therefore, I have entirely discontinued Mr. Lister's practice in abdominal surgery save in such cases as those conjointly responsible with myself desire that it should be employed. The only detail I have retained is the practice of having my instruments in a water-bath, which I find to be an easy and effective method of keeping them clean.

Dr. Keith has quite recently come to somewhat similar conclusions, and he has given up Listerism for abdominal surgery, and he told us at the recent International Congress, in London, that Listerism would add two or three per cent. to the mortality of ovariectomy. I am certain he is right.

For ovariectomy I generally have the patient anæsthetized on the bed and then lifted upon the table, the object of this being that she is saved the distress of seeing the preparations which have been made for the operation, as the instruments and other appliances are not brought into the room until she is unconscious. When upon the table, the arms and legs are secured by belts, and two clean towels are arranged so as to leave the abdomen exposed between them. I prefer this method to the rubber cloth with a hole cut in it, originally used by the late Sir William Ferguson, for it entails difficulty in securing the latter to the patient's skin, and, as a rule, I have not found that it saves very much mess. The bladder has previously been emptied by the nurse, and I have personally inspected all the arrangements of instruments, sponges, etc., to see that nothing has been omitted.

I begin the incision midway between the umbilicus and the pubes and cut downward, going completely through the skin and subcutaneous fat at one cut, for a distance of about two and a half inches. I then look for the white line and divide it to an equal extent, and after that I cautiously divide the fat and transversalis fascia until the peritoneum is exposed. A pause is then made, and every bleeding point is secured by a pair of scissor-forceps. I never, in the first instance, make an incision any longer than two and a half inches, because I am very averse to making the wound any larger than it need be, and, as a rule, three inches is enough. As soon as all the bleeding points have been secured, I open the peritoneum so as to admit my forefinger, and with that I make a brief preliminary exploration. The peritoneum is then laid open to an extent corresponding with the wound in the other tissues and the cyst is exposed, any further bleeding being immediately arrested. In this stage of the operation, indeed throughout the entire proceeding, I never use

any other director than my finger, for upon that I can place most reliance. If there should be no adhesions between the tumor and the peritoneum in front, this part of the operation is very simple; but if there be adhesions it is often no easy matter to determine the point of union between the two membranes, and it is in such a state of matters that inexperienced operators are apt to commit a grievous error. I have twice known instances, and I have heard of several others, where the glistening peritoneum has been mistaken for the wall of the cyst, and the operators have industriously set to work to separate the former membrane from the transversalis fascia. Sometimes, even when there is no adhesion, the peritoneum is so greatly altered in appearance, having become so thick, leathery, and gelatinous, as to deceive any but the most experienced operator. When the point of union between the cyst and peritoneum cannot be discovered, the best way is to cut cautiously inward until the cyst be opened, because then it can be emptied, the whole wall carefully taken up and examined, and the several parts accurately determined. This, however, wants great caution, for it may be that a piece of intestine lies in the way and may be opened by mistake; but the experiences of a few cases will enable an observant surgeon easily to determine when he is cutting through muscular fibre. When the cyst is reached, it should be tapped by a large-sized syphon trocar (Fig. 31, p. 261) and emptied as quickly as possible. Sometimes, however, the contents of an ovarian cystoma are glairy or even perfectly gelatinous and will not pass through a trocar, and nothing tries the presence of mind of an inexperienced operator more than this. The peculiar adhesive mass which sometimes fills an ovarian cyst will neither pass through a trocar, nor be seized by the hand nor lifted up by a sponge, and to remove it from the cyst is often a matter of the greatest difficulty. It was upon such a tumor as this that Houston performed the first ovariectomy. In such a case the walls of the cyst are generally thin and extremely fragile, so that they cannot be held by forceps. Such tumors are also often very adherent, the adhesions bleeding very freely when broken down, and many of these cases are lost on account of the mere extension of time required for the removal of the cyst contents.¹ In a case like this there is nothing for it but to enlarge the incision to the extent of four or five inches, or even more, and to remove the mass by the hands as rapidly as possible, the assistant aid-

¹ In a pretty table (III.) in his book *Resultats Statistiques de l'Ovariectomie*, M. Koeberle shows conclusively how fatal protracted operations are. It follows, therefore, that in cases of difficulty—and these are the only fatal ones—an experienced, skilful, and rapid operator is sure to have by far the best results.

ing in the process by squeezing upon the flanks. If, however, the cyst contents are fluid and the tumor consists mainly of one cyst without adhesions, nothing would seem more simple than an ovariectomy, and the removal of a parovarian cyst is usually a proceeding which takes a shorter time to perform than to describe.

When the cyst has been emptied, it should be drawn gently out through the wound; and if non-adherent, no obstacle will be met with unless it be the presence of some secondary cysts. The trocar having been maintained in its position by means of the spring catches at its sides, its point should be made to enter these cysts, and thus empty them; but very often the contents of these secondary cysts are much thicker than the fluid contained in the major ones, and sometimes they are so numerous and of such small size that they constitute a solid mass at the base of the tumor. In such a case I lay open the major cyst, and, passing my hand within it, I break up into its cavity as much as I can of the secondary cyst mass, in order to save any further enlargement of the abdominal wound. During the whole of these proceedings as much care as possible should be taken to prevent any escape of the cyst contents into the abdominal cavity.

Adhesions are mediate or immediate, the former being generally parietal or omental, existing in the shape of round or flattened bands of peritoneum. They seem to be formed by isolated patches of adhesion, which have dragged off the peritoneum from the abdominal wall, or by pieces of adherent omentum. They are seldom large, and are only of any moment when traversed by an important blood-vessel, which must always be secured by torsion or ligature, or by the cautery. The immediate adhesions require great patience to overcome them; but even when they unite the tumors to the most important organs, they may be separated, or a piece of the cyst may be detached and left. One of the dangers of the removal of an ovarian tumor with extensive pelvic adhesions—and still more of a uterine tumor—is damage to one or both ureters. It will be remembered that they run down obliquely from the kidney over the brim of the pelvis down to the bladder, lying over on each side of the cervix, and close to it. I have often laid them bare, but have never been unfortunate enough to injure them. In the celebrated case published by Simon (*Chirurgie der Nieren.*, 1870), the ureter had been dragged into the wound and clamped, and the patient recovered, with a urinary fistula of the left ureter. Simon heroically removed the left kidney and cured his patient, and I saw her alive some years after the operation, and at the

same time I saw her tumor in one bottle and her left kidney in another.

Nussbaum had a similar accident, and he cured his patient by making an artificial continuation of the ureter between its divided points. When dividing the pedicle of a large tumor of old standing, I have several times seen hugely dilated lymphatic trunks, which looked very much like ureters—a resemblance which more than once made me very uncomfortable for a few days.

Many curious and exceptional forms of adhesion occur, one of the most remarkable of which is occasional adhesion of the tip of the vermiform appendix of the cæcum to the tumor. I have seen this three times, twice in my own practice. The first time I saw it it was unfortunately divided by the scissors of the operating surgeon, with a fatal result; and it was only the discovery of this misfortune at the post-mortem which enabled me to recognize a similar condition, and avoid a like disaster in my own cases. In dealing with pelvic adhesions it must be borne in mind that the large venous trunks in that region lie immediately under the peritoneum, and that they must be carefully avoided. In the first operation I ever saw, one of these trunks was torn across by the rough handling of an inexperienced operator, and I need hardly say that death ensued in a very few hours, because it was quite impossible to secure the bleeding point, or even to find it. Immediate death was only staved off by stuffing the pelvis with a towel. This event made such a strong impression upon me that I thought it hardly possible I ever could take part myself in an operation fraught with such terrible risks. Hemorrhage from adhesions has always a very strong tendency to arrest itself, and most of the bleeding points will stop of their own accord, or can be stopped by simple pressure with a sponge; each point of importance should be seized by a pair of scissor-forceps, or touched with a cautery, or with a piece of solid perchloride of iron. For the arrest of a general oozing from a large surface, the pressure of two or three dried sponges is generally enough, and it is, therefore, my practice always to separate adhesions as rapidly as possible, and to follow their separation by the immediate application of sponges, leaving these *in situ* until I have finished the separation and removal of the tumor.¹

¹ A few days ago I removed a tumor universally adherent, full of pus, and quite rotten. I tore across the pedicle before I had recognized it, and its vessels bled profusely. Like the rest of the tissue, it was quite rotten, and the more I tied the vessels the more they bled. The cautery was equally useless. I therefore secured all the points by means of forceps, and left them *in situ* in the abdomen, closing the