while he draws his head back, and turns it aside. After a brief interval the attempt may be renewed, but with the same result, when a few words have been read. There is no dimness of sight, defect of accommodation, or pain in the eyes or head to account for the symptom. It usually comes on suddenly, and is the first sign of serious cerebral disease, being soon followed by other symptoms, such as headache, giddiness, aphasia, hemianopsia, hemiplegia, and so on. All of these cases have ended fatally. The details of the necropsies, in those cases where they were obtained, leave much to be desired. In every instance the lesion was on the left side of the brain, the patients being all right-handed. The disease seems to have occupied chiefly the inferior parietal lobule, extending sometimes as far forwards as the inferior frontal convolution, and sometimes as far backwards as the angular gyrus. It is evident, however, that we must be content to wait for more definite information with regard to the usual seat of the lesion, before this symptom can be utilised in practice for the purposes of localisation.

In crossed amblyopia—in which the eye on the side away from the cerebral lesion is almost blind, with very contracted field, while the field of the other eye is also contracted, but in a less degree—the lesion has been found in the lower and hinder part of the inferior parietal lobule.

I must mention that so distinguished an author as Nothnagel, writing about a year ago,⁵¹ expressed his doubt as to the occurrence of crossed amblyopia. He does not think the perimetric examinations have been made in these cases with care sufficient to exclude the possibility of their being examples of incomplete hemianopsia. Nothnagel does not offer any experience of his own, and I am slow to think that observers like Ferrier, Gowers, and Sharkey could fall into such error.

In conclusion, with reference to optic neuritis, it is merely necessary to state that it has practically no localising value; nor should I think it is necessary even to do this much but that, in some recent cases of brain surgery, it seems to have been tried to utilise the optic neuritis for localisation. Optic neuritis occurs in most cases of intra-cerebral tumour, irrespective of the seat of the disease.

And now, gentlemen, I have come to the end of this lecture, and have not as yet made any reference to that distinguished man in whose honour I have the honour to address you. But, I do not believe you could wish that we should separate this evening, without your having heard from me some expression of the esteem in which we hold Sir William Bowman, nor will I be guilty of such an omission. And here I might dwell upon Bowman's scientific attainments, upon the good work he did in years gone by—not alone in ophthalmology, but also in physiology—upon all this Society owes to his prestige and to his generosity, upon his clinical knowledge and his operative skill, and upon many another topic which his name suggests. But I prefer to remind you of the high standard of professional life he has shown us, a standard which we of a younger generation must endeavour to maintain. Conscientious in his relations with his patients, honourable in his relations with his professional brethren, careful not to put himself forward in any unrecognised manner, not seeking notoriety, simple, kind, courteous, dignified, William Bowman is presented to our mind's eye as the personification of the best of those qualities which go to make an English gentleman. We are indeed privileged in being permitted during his lifetime to offer some tribute to his distinguished scientific merit; but, while recognising how much we owe to him scientifically, I think, and I believe you will admit, we should not forget how much his every-day professional life has tended to give to the noble speciality he adorns that high tone which belongs to it. Our sincere hope is that Sir William Bowman may long live to enjoy the honour conferred on him by his Queen, the love of those who are dearest to him, and the warm, heart-felt esteem of this Society.

⁵¹ Congress für Innere Medicin, Wiesbaden, 1887.

REGULATION OF THE SALE OF CARBOLIC ACID.—From the report of the meeting of the Council of the Pharmaceutical Society of Ireland, it is seen that the subject of accidental poisoning by carbolic acid has again been under consideration by that body, and that, in pursuance of the view taken with regard to it on a previous occasion, it has been resolved to urge again on the Privy Council the desirability of adding carbolic acid to the poison schedule. The proposal to do so met with the approval of the King and Queen's College of Physicians in Ireland in 1882.

GENERAL SUMMARY OF CONCLUSIONS FROM A SECOND SERIES OF ONE THOUSAND CON- SECUTIVE CASES OF ABDOMINAL SECTION.

Read before the Birmingham and Midland Counties Branch.

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

Professor of Gynæcology in Queen's College, Birmingham; late President of the British Gynæcological Society; Honorary Consulting Surgeon to the Brooklyn Hospital for Women; etc.

WHEN I had the privilege some four years ago of communicating to this Society the results of my first series of 1,000 consecutive cases of abdominal section, I said that I did not know whether the general death-rate, 9.3 (the figures ought to have been 9.2, the fractional increase being an error), was high or low, for no such series had up to that time been published, nor have I seen such a series since. I thought the figure must be high, and I expressed my conviction that if I lived to complete another such series the death-rate would be very much lower, and it is with no small satisfaction that I can record a diminution from 9.2 in the first series to 5.3 in the second. I pointed out that as my first series included my early work, which had been so heavily tainted by the mortality of clamp and other blunders arising from the traditions of the elders, the mere freedom from these alone ought to give me far better results. That this prediction has been completely verified is certain, from the mere fact that my mortality in the treatment of ovarian and parovarian cystoma has fallen from 8.1 to 3.3. But that by no means represents the full gain which I can point out, for in this class of cases I had, in my previous series, a large group of uncompleted operations with a mortality of at least 50 per cent., and these have now practically disappeared. They were cases chiefly of sessile ovarian tumours and imbedded cysts of the broad ligament, the removal of which I always now complete, and thus at once diminish my mortality together with that greatest of all opprobria, imperfect work. I am now in a position to say that no cystic tumour of the abdomen exists which cannot be removed, and this is quite as true of solid tumours of a non-malignant character associated with any organs in the abdomen except the liver and spleen, and I lay far less stress upon my diminished death-rate as evidence of the improvement of my work, than upon the fact that in my second series I have only to plead guilty to 6 uncompleted operations in 1,000, whereas in my first series the number was 30, with the same mortality in each—50 per cent. Even in these six I know now, for *post-mortem* examination revealed the fact, that I could have finished the operations in at least two, perhaps in three, if I had screwed my courage up to a still harder strain, and I blame myself accordingly.

I therefore lay it down as an axiom that a man who has a large proportion of unfinished operations is a bad surgeon, worse than one with a high mortality. I may here again assert that I never in any way select my cases or refuse to operate in such as look unfavourable for any statistical results. This diminution of uncompleted operations may, I think, be fairly and entirely put down to increased experience, giving greater readiness of resource, better judgment, and increased manual dexterity; and to some extent the diminution of the death-rate may be credited in the same direction. But I am proud to be able to say that there is certainly another factor which enters largely into this increased success, and that is the fact that the mind of the general profession is now rapidly freeing itself from the shackles of the ancient authorities of abdominal surgery, and is opening itself to the new rules of progress. The most important of these are two: first, that the principles which govern our actions in general surgery must apply to the abdomen; and the second is that no doubt or difficulty of diagnosis should stand in the way of adopting that

means by which the diagnosis is at once made clear, and successful treatment rendered possible by means of abdominal section.

In our Birmingham and Midland district these rules have received an almost universal acceptance, and it is only from outside places now that we get cases which are a disgrace to our art. The day has now come for very plain speaking on this subject for very many reasons, some of which I have repeatedly urged on former occasions, and a few others which are new. Of the former I may cite one. Do we not invariably urge a woman who comes to us with an abscess of the breast to have it opened without delay? If so, why should we not deal on the same principle with a woman who has an abscess in the pelvis? We might just as well treat the former with a splint as the latter with a pessary, and this kind of nonsense is one I could illustrate afresh every week from my practice.

A rule was introduced by Sir Spencer Wells some thirty years ago, and is maintained stoutly yet by his followers, that an ovarian tumour should not be removed as long as the woman can go about. On the contrary, the rule which I have advocated for the last ten years is that the time for the removal of an ovarian tumour is as soon after the time of its discovery as is possible.

What would be thought of us if we told our patients with enchondromata of the thigh, or sarcomata of the jaw, or lipomata of the neck, or aneurysm of the femoral, to nurse and treasure their troubles as long as they could go about? What would we say of a physician who, when he discovered an incipient pneumonia, advised his patient to go about as long as he could, and, when he could do so no longer, this physician would come in to treat the case? I ask again and again why the rules of general surgery and medicine should be excluded from abdominal surgery; and I can get no one to whisper an answer; and I show in every series I publish, with increasing emphasis, that early operations mean successful operations, and I am glad to say that nineteen out of twenty of my neighbours agree with me. But what about the others? Let me give two of my most recent experiences on this subject; they both occurred last week. A gentleman who is very amiable and good, sent me a woman in the last stage of exhaustion and emaciation, and he sent me a notice to the effect that he had been attending her for three months, with a lump in her pelvis, that he could not make out what it was, and that, therefore, he sent her to me. I opened the abdomen, and removed a huge putrid foetal sac, the result of a ruptured tubal pregnancy, a thing that ought to have been removed when this gentleman first saw her. Of course she had no chance of recovery, and died a few hours after the operation. Now, this gentleman's conduct cannot be considered a credit to our profession, and I have in all probability sacrificed his friendship for ever by frankly telling him so. For twelve weeks he went on physicking this poor wretch; and when he sent her to me, she had nothing left to pay for an operation, and was an object of my charity—a subsidiary, but still certainly an element in the case. But, worse still, he secured for me a fatal result, which I do not deserve; and, worst of all, a crime in my eyes, for which there is no forgiveness, he secured for surgery a disaster instead of a triumph. I am speaking strongly, gentlemen, but not more so than I feel; for the advancement of our art depends not more on those who practise such a department as this, than on those who are in the position to give us reasonable chances of success, and that success is essential for all of us concerned in dealing with sick people.

A worse case than this, however, is the second; for the gentleman who sinned as I have described, sinned by an act of omission, and in ignorance. But the second was a sin of commission, based upon that assumed certainty of diagnosis which the inexperienced mostly are guilty of.

My friend and former pupil, Dr. Gowans, of Perth, sent me last week a woman from that city, who brought with her a note dated March 22nd, 1888, and signed "Skene Keith," to the effect—

"The case is probably sarcomatous. I told the husband that he must not hope for a recovery, and told the patient herself that, when the weather got better, she should go out and see if she got stronger. She would be better, I think, with a bottle, perhaps a tonic, or stomach mixture."

The case seemed to me rather one of large ascitic effusion, due to an ovarian tumour jammed in the pelvis, and probably fixed there by papilloma. As she had been tapped twice nothing further was to be got in that direction. With a sort of half hope that I might get the tumour out, I opened the abdomen, and, to my disappointment, found that the omentum, mesentery, spleen,

and liver were all fully infected by advanced growths of papilloma, the centre of origin of which was a small cystic tumour of the right ovary, covered by dendritic masses. The tumour could of course, have been removed, and a recovery for a short time would probably have resulted, but no permanent good would have been done, and I might have had to record a death which, had it happened, would have been more fairly credited to Dr. Skene Keith, and therefore I contented myself with an exploratory incision. From the history of the case I think it is perfectly certain that in March there was no papillomatous infection, possibly no papilloma at all. Dr. Keith's diagnosis of sarcoma is merely an illustration of how completely mistaken we all may be, even the youngest. The blunder consists in his not admitting the advisability of adopting the practice of exploring. Dr. Skene Keith has signalled himself by sneering at some people who open the abdomen on all sorts of pretexts, and his father has bewailed the "restless spirit of abdominal surgery which has lately been let loose," and they both have laid claim to a freedom from errors in diagnosis to which no others have a right. But such a case as this, and it is by no means my first experience, walking in the footsteps of these two conservatives, makes me satisfied in my restlessness. Had this woman's abdomen been opened last March in Edinburgh, as it ought to have been, she would have been another life saved.

It is far better that we should even open the abdomen a few times unnecessarily than that we should deliberately limit the powers of our art in this foolish way.

In my former series there were ninety-four simple exploratory incisions; of these I said that "ninety-four is a large number, but it must be remembered that years ago I advocated exploratory incisions in preference to firstappings, and that I have persistently carried out the practice. They serve the purpose of completeappings; and as the patients almost uniformly recover, they do not harm at all." The same is true of the present series, and I never tap as a first proceeding, I always explore. In the present series there are only fifty-three of such operations, and this diminution is chiefly, if not entirely, due to the fact that I get the cases earlier than I used to do, so that I arrive at results exactly the opposite of the disaster just narrated in Dr. Skene Keith's case. There are at least thirty cases in this series in which I have removed tumours successfully, and these, had they been delayed, as in his case, would have resulted as in that instance; an argument for early operations and for exploratory incisions which is perfectly unanswerable.

Against the fifty-three exploratory incisions there are recorded two deaths, but these two deaths might be eliminated from the table with perfect fairness, for the operation neither delayed death nor accelerated it by an hour.

I have also to repeat what I said before on this subject, that "occasionally the exploration is followed by drainage, and in some of these cases results of the most marvellous kind have followed, the ascitic effusion has not appeared again," and tumours of spleen, liver, and of other kinds, the nature of which were not ascertained, have disappeared, and the patients have been permanently cured.

I am as far off as ever from any explanation of this extraordinary fact, but as it has received abundant confirmation since I first announced it, it is beyond dispute.

The next group of my series is a lot of 263 cases of operations for chronic inflammatory disease of the uterine appendages, with a mortality of 3.4 per cent., as against a mortality of 5 per cent. in my first series, a diminution due largely to increased experience. Here, too, the absence of mere exploratory incisions and incomplete operations is very marked, for there is not a single case of incomplete operation to be credited to this group. These operations are far more difficult in every way than removal of tumours of any kind, and the deaths are almost entirely confined to cases of broken-down women, the "too-late" cases, as Mr. Bryant well designates them. Since my last paper this subject has been threshed out in numerous discussions, and has even appeared in the law courts, but the confirmatory verdict of the lay, as well as that of the professional, jury, has been in absolute harmony with all I have said on the subject. Useless and diseased organs, incurable by ordinary means, must be removed from the pelvis, just as from other approachable parts of the body; even the lungs are no longer sacred to the "restless spirit of modern surgery." The route of my practice is not governed solely by my own wishes and inclinations. It is far more completely influenced by the judgment of my professional brethren; and the fact that

from all parts of the world these cases have been sent to me in the increased proportion of 263 in this 1,000, to that of 201 in my first series is a convincing proof that the benefits of this proceeding are being fully recognised. I am not concerned in the present paper with the ultimate results of such cases, but I may mention incidentally that these have been materially augmented, and the period of convalescence shortened, since the discovery by Dr. Arthur Johnstone, of Danville, Kentucky—and it is one of my greatest honours to be able to say that he was one of my pupils—that there is a large nerve trunk entering (or leaving) the cornu of the uterus, in the angle between the round ligament and the tube, which seems to have a powerful agency in the process of menstruation. When this trunk is secured in the ligature close to the uterus, the results of the operation are more speedily effected, and are more complete.

I have just received a very strange pamphlet with a very attractive title, *Electricity versus Tait*, surely a subject large enough to attract universal attention. Of the manner in which the author speaks of the defendant in this action, I have no reason to complain, in fact quite the reverse. It is very flattering, and I hope altogether true; but when he speaks of the immediate subject, which is really that of chronic inflammatory disease of the uterine appendages, I feel sure that every skilled reader will come but to one conclusion, that the author has never seen the disease which he professes to cure by electricity. I showed the pamphlet to a skilled electrician a few days ago, and his verdict was that the author did not know what electricity was, or anything about it; yet the following is a quotation from its pages: "In the use of electricity we subdue the local inflammation, promote absorption of hyperplasia and hypertrophy, establish the general health, and produce proper nutrition. In this manner of electrification we have a means brought to bear upon nutrition, local and general, of such pronounced influence that the result is all that could be desired. By it I have been enabled to carry the class of cases instanced above over the apparent limit of repair; observed thickened tubes and adhesions of long standing disappear, which had resisted every means to overcome them." By such writing as this we are strikingly reminded of Bishop Berkeley and his book on coal-tar water, from which I might reclaim parallel passages; but the famous metaphysician was floored by the concrete example of a sailor's wooden leg. Let our electrolytists try a living limb of flesh. Gonorrhoeal inflammations of the uterine appendages are very common, and similar diseases of the large joints are not unknown; they are almost equally intractable. Fortunately in the case of the joints the recovery can be greatly assisted by rest in splints; but we cannot put the ovaries and Fallopian tubes in splints; we have no means of giving them physiological rest short of destroying them. Therefore they are in a far worse position than the joints I speak of. Asking amongst my friends who practise general surgery, I find they say that gonorrhoeal inflammation of joints is a most intractable disease, rarely getting well under the most favourable conditions in less than six months, and often extending over years; finally, that a serious proportion of the cases never get well at all, or get well only with stiff, useless joints. Here, then, is Dr. Berkeley's wooden leg. Let us ask our electrolytic friends to take some cases of gonorrhoeal elbows and knees, and let us see what they can do. If they cure them, as they say they cure the same disease in the uterine appendages, then we can believe them about the pelvis. But let them remark the contrast: the joints are outside, visible, and easy of observation, whereas the favourite hunting ground of quackery is an obscure, mysterious mucous canal.

One other word about this *Electricity versus Tait*, and I am done with the subject. He assumes, like many other critics, that those of us who deal surgically with the abdomen and pelvis are like Mr. Wemmick, "Here's a church, let's have a wedding." "Here's a something, let's have an operation." So far as I am concerned, this is not so. I have always said that the great bulk of cases of inflammatory disease of the uterine appendages get well without operation, though by getting well we do not necessarily mean that they are cured. All I contend for is that the residuum should not be condemned to live on, hopelessly suffering, whilst there is a *dernier ressort* at our command. When that should be called in is a matter for determination in each particular case, and depends upon conditions which cannot be formulated into laws, varying as they do in importance, even according to the social position of the patient. Electricity may yet prove the means of still further diminishing the number of cases to be submitted to

the surgical method, but it does not promise much in the hands of those who have taken it up. Ever since the year 1827, which date indicates, so far as I can discover, the first serious application of the electrical current to the treatment of disease, it has been a source of disappointment to the honest, earnest practitioner, with an exceedingly limited area of exception, whilst it has, on the other hand, yielded an abundant harvest to the pouch of the charlatan.

Naturally enough, this brings me to speak of the treatment of myoma, concerning the electrolytic treatment of which much discussion has taken place within the last two years, since Dr. Apostoli made assertions concerning it which startled the surgical world. To form a judgment on such a subject as this, there are two methods, both of which must be applied: the first, open, it is true, to much fallacy, and the second still fallacious, but much more trustworthy; but they must both be tried to satisfy a mind that is logical. The first method is that of *a priori* reasoning; and when Dr. Apostoli's assertions were subjected to this process, it was found that there were grave initial objections to his statements. In the first place, there was nothing new about his processes whatever. It is true he had a new nomenclature, but that was all; and in not a single essential detail were his methods new. Years before they had been tried, had failed, and had been found to be dangerous. Then it was remembered that Dr. Apostoli had made precisely similar assertions about the process of involution of the uterus after labour. He had asserted that all the evils of subinvolution might be obviated by the electric current, his proposals had received a fair trial, and had been uniformly condemned. Cognisant of all this, when in Paris eighteen months ago, I made very extensive inquiries, and I could find no evidence which induced me even to visit Dr. Apostoli's clinic; for merely to see electrodes applied to patients by a man who had had no experience in gynaecology, who had received most of his training as an army surgeon, could be of no service. I inquired as to what was known to others concerning the ultimate results of patients who had been treated by his methods, and I heard quite enough to make me determine that I should leave this kind of treatment on this side of the Channel to others. I had no desire to complicate my work with such difficulties and dangers as I heard of on all sides, and I was perfectly sure that I should before long find that there were others ready enough to try them. Speedily we had accounts in the medical journals of visits to Dr. Apostoli—Dr. Burton, of Liverpool, and Dr. Elder, of Nottingham, wrote such papers. I have heard of two fatal cases in Liverpool occurring in the practice of Dr. Grimsdale, and in which the treatment was carried out by Dr. Burton. I wrote to Dr. Grimsdale, asking if the statement were correct, but he has not even answered my letter. Dr. Elder has written to me within the last few days with regard to electrolysis: "I am convinced it will not do as much as some of us hoped. In the majority of hæmorrhagic cases, it certainly is an efficient hæmostatic, although the effect is not always permanent. Now and again, in the same class of cases, it signally fails to modify the flow. Rarely under its use a tumour disappears; in a larger number of cases it causes marked shrinking; whilst in some, even with symptomatic improvement, no diminution in size follows. Pain it usually relieves, and in most cases an improvement takes place in the general nutrition. As it evidently will not abolish hysterectomy, I have invented an 'Elder's Clamp,' which I hope you will try." But all that Dr. Elder admits can be secured by easier and safer methods; and, therefore, electrolysis gains no support from his evidence. My colleague, Dr. Savage, also went to see Dr. Apostoli, and wrote a paper on what he was told he was seeing; but he does not try the methods, at least not in hospital practice; and when, at consultations, I have urged him to do so in what seemed to me suitable cases, he merely shook his head with much gravity, and proceeded to remove the uterine appendages.

I have, therefore, received no more encouragement from the second or empirical method of investigation than I did from *a priori* argument, and I have left electrolysis severely alone. My own negative experience has quite justified this action, for it is as follows. Since May, 1887, there have been fifteen patients who have passed through my hands, upon whom the electrolytic treatment has been tried, as a substitute for surgical interference. Three of these have died, of what I do not know. Nine others have returned to me for operation, and have all got well. The remaining three are as yet undecided. In addition to these, I have performed hysterectomy on three patients, who had just before been subjected to treatment by electrolysis; and I have removed

the appendages in four cases which have been similarly treated.¹ I conclude, therefore, that, up to the present, we are still in want of evidence in favour of Dr. Apostoli's claims in this country; and if we take a recent record in Germany, we find all that I urged against it on *a priori* grounds completely confirmed.

It is published by Dr. Brüse, in the *Zeitschrift für Geburts- und Gynäk.*, Band xv, Heft i.

CASE I.—The patient was 50 years old, a multipara, suffering from multiple uterine myomata. Uterus, 12 centimetres in length; the tumour reached to within two fingers width of the umbilicus. She suffered much from pain and irregular hæmorrhage. After a few sittings the pain was eased; the size of tumour and uterus diminished. After thirty-five sittings, uterus 6½ centimetres; all pain and irregular hæmorrhage had disappeared. Fifteen months later she came again; the pain and tumour had returned, but again disappeared on applying the constant current. After sixteen sittings, uterus 5 centimetres long, and the highest point of the tumour 7½ centimetres from the umbilicus; the patient suffered no more pain.

CASE II.—Patient aged 43, had aborted twice, and suffered from a large uterine myoma, which filled up the pelvic cavity and projected 15 to 16 centimetres above the symphysis. The sound passed 11½ centimetres into the uterus. She suffered from irregular hæmorrhages. After a few sittings the pain was eased, the size of tumour and uterus diminished. After thirty-five sittings, uterus 6½ centimetres. All pain and irregular hæmorrhage had disappeared. Fifteen months later, she came again; the pain and tumour had returned, but again disappeared on applying the constant current. After sixteen sittings, uterus 5 centimetres long, and the highest point of the tumour 7½ centimetres from the umbilicus; the patient suffered no more pain.

CASE III.—Patient aged 43, had aborted twice, and suffered from a large uterine myoma, which filled up the pelvic cavity and projected 15 to 16 centimetres above the symphysis. The sound passed 11½ centimetres into the uterus. She suffered considerable pain and from pressure symptoms: menstruation regular. During treatment the pain ceased, and the tumour reached 13 centimetres above the symphysis. The patient absented herself for nine months, and came back worse than before. The tumour had grown again, and the uterus measured 12½ centimetres. On using the constant current, the symptoms again abated. After eleven sittings, the uterus was 11 centimetres long; the patient then left my care.

CASE IV.—Patient, aged 29, one child, submucous myoma. The uterus was as large as a four months' pregnancy. There was much dysmenorrhœa and menorrhagia. After one sitting (1½ milliamperes), slight pains like labour pains; second sitting (20 milliamperes), strong contractions of the uterus, with labour pains, which lasted for two days continuously till the next sitting. Bloody discharge; pieces of tissue (myoma) were passed. A badly smelling discharge came on; the myoma had necrosed and was being cast out. The constant current was applied twice again; the necrotic mass removed with spoon; the uterus drained. Death five days later from septic peritonitis.

CASE V.—Aged 36, nullipara, uterine myoma. Great pain and much dysmenorrhœa. No result after twelve sittings. Hysterectomy was successfully performed.

As a contrast, let me submit a group of 148 cases of removal of the uterine appendages for myoma, with 3 deaths, giving a mortality percentage of 2.03, as against my 99 cases in the former series with a mortality of 7. I have already published in detail (*BRITISH MEDICAL JOURNAL*, September, 1885) proof of the satisfactory and permanent results obtained from this operation, and have only, in addition, to say that these are maintained. I have, in the two series, to include 247 cases with 10 deaths. Of the 237,

I know that 3 have since died of cancer, and that 3 have had hysterectomy performed, owing to the subsequent growth of the tumour, and I know of no other failures.

The next group is that of 88 hysterectomies, with a mortality of 11.3 per cent.—a striking contrast to my previous series of 54 cases with a mortality of 35.7, a result so distressing that I utterly condemned the operation. But, as Dr. Elder says, we are driven to it, no matter what Dr. Keith says! and, if Dr. Keith had not refused to let me see him do the operation, my mortality in this series would have been less than it is now, for he had a method of operating which I did not know, and the knowledge of which at once reduced my mortality enormously. The point was a very simple one. He tied the broad ligaments separately, and stripped them off the uterus. He does not seem to have appreciated the reason of the success of this method, but the explanation is very simple. It prevents occlusion of the rectum by taking off strain. The fact that I have gone so far as to have thirty-one consecutive recoveries from hysterectomy satisfies me that, before long, I shall bring its mortality down to something approaching that of ovariectomy.

I have already said in previous writings all that need be said concerning such operations as cholecystotomy, nephrectomy, nephrotomy, opening and draining pelvic abscesses, and operations for tubal pregnancy. I have nothing to alter and nothing to add.

For the relief of intestinal obstruction, I still prefer to perform enterotomy, rather than make prolonged searches for the seat of obstruction. I have put a group of these cases together; but they have so many divergent features, that no efficient conclusion can be derived from mere statistical display. I propose to make this subject the basis of a special paper to the Midland Medical Society during the ensuing session. Of suppurating peritonitis treated by abdominal section, washing out, and draining, I have to speak in terms of unqualified satisfaction. I have lumped together all the cases, save those that were puerperal, in the same group, for it really is impossible to draw hard-and-fast lines between the acute cases and the chronic. I have operated, in this series, on 26 cases and have saved 22. I say "saved" advisedly, for, without the opening of the peritoneal abscess, they must all have died. The mortality is 15.3 per cent., and in my first series I had 9 cases with a mortality of 22.2 per cent. Here, again, I must plead with my brethren for assistance, and urge that cases of purulent effusion in the peritoneum should be treated by early and efficient drainage, just as purulent effusion in the pleura is dealt with.

In London this operation has been received with much favour, and has had very fair success. In a communication on the subject, Mr. Treves expresses a view which seems to imply that in my practice the operation arose, in a sort of blundering way, in a case which I did not know what I was about; and subsequent writers are, therefore, disposed to give Mr. Treves the credit of introducing this practice. Mr. Treves is quite mistaken about my introduction of the operation, which took place in a case sent to me by Dr. Arthur Underhill in 1879. Dr. Underhill had diagnosed the case completely, and I quite concurred in his view of it; I therefore opened that patient's abdomen, not only knowing quite well what I was about, but with the full intention of relieving my patient, and I was successful. The operation was performed on October 30th, 1879.

Of puerperal peritonitis, I have to speak still with bated breath. I have operated four times, and had only one recovery. But I nearly had two others; and, I believe, if I had obtained twelve hours' start in these two cases, the mortality of my little group would have been 25 instead of 75 per cent.

Puerperal peritonitis—puerperal fever, as it is practically called—is a deadly disease. The recoveries from it are quite fractional. The argument, therefore, for attempting to save such cases by opening or cleansing is surely a strong one, but it must be fairly tried; that is, it must not be left merely to close the scene with. I am strongly disposed to think that, in severe cases of puerperal inflammatory mischief, the chief trouble is in the womb itself, and I am inclined to think removal of the organ in its inflamed condition may be requisite. In any case, the disease is so hopeless when left alone, that any kind of effort seems legitimate.

One small group of cases remains for discussion, as it is full of interest; I mean those in which the pregnant uterus was amputated because it was impossible to remove the child by the natural passages. In my life I have three times performed the

¹ The only case in which I have seen pronounced benefit has been derived from the treatment of electrolysis, and is one which occurred at the hands of Dr. Apostoli himself. It was a case of subinvolution and chronic metritis, with the invariable retroversion of the uterus, accompanied by chronic catarrh of the endometrium. This lady went over to Paris, and had, according to her own account, thirty-one applications of the electric current, several of which were conducted under an anæsthetic. Of course she does not know exactly what was done, but she tells me that the treatment was so severe that nothing would induce her to undergo it again. It occupied five months, and cost a very great deal of money. Now such a case as this could be, and certainly in my practice is, invariably cured by the simple process of dilating and curetting the uterine cavity, and applying Paquelin's cautery freely to the inside. This proceeding is absolutely devoid of risk; nothing in the least approaching anxiety has ever been experienced in an enormous number of cases I have successfully treated. The pain is infinitesimal, and only one sitting is required. The cost is a mere bagatelle. I do not see, therefore, that the electrolytic treatment can be accounted in any way an advance upon the treatment by curette and actual cautery; in fact, it is entirely the contrary.

old Cæsarean section, and the mother died in every case. In a fourth case, seven years ago, I did exactly what Sanger claims as a new operation, and I removed the appendages to prevent future impregnation, but the patient died. I put all these deaths down to the fatal influence of the "puerperal condition," but it is clear that that explanation is nonsense, for I have now done four operations on the principle laid down by Professor Porro, and all the mothers have recovered, and the children have all lived. The operation I perform is certainly not Porro's operation, for I have altered nearly every detail; but the discovery of the new principle—that of removing the uterus to save the mother from the fatal effects of suppurative metritis, and the possibility of having the same trouble over again—is the credit of my friend Professor Porro. I prefer to give the operation the name of no particular person, because men's names attached to operations are always productive of misunderstandings, quarrels, and confusion. I indicate the principle by calling it "amputation of the pregnant uterus," every word of this term being fact, and not theory.

Analysis of Series.	Cases.	Deaths.	Mortality.	Mortality of previous Series.
			per cent.	per cent.
Exploratory incisions ...	53	2	3.7	2.1
Cystoma parovarian ...	24	1	3.3	8.1
" of one ovary ...	158	6		
" of both ovaries ...	78	2	2.03	7.0
Enucleation of cysts of broad ligament...	12	0		
Removal of appendages for myoma ...	148	3	3.42	5.0
Removal of appendages for inflammatory diseases ...	283	9		
Removal of appendages for deformity ...	2	0	0.0	0.0
Hysterectomy ...	88	10	11.3	35.7
Opening and draining pelvic abscesses ...	6	0	0.0	0.0
Hepatotomy ...	5	1	20.0	0.0
Cholecystotomy ...	28	2	7.1	0.0
Nephrectomy ...	12	2	16.6	0.0
Nephrotomy ...	24	1	4.16	0.0
Ruptured tubal pregnancy ...	28	1	3.57	9.0
Hydatids of peritoneum ...	4	0	0.0	0.0
Tumours of omentum and mesentery ...	2	0	0.0	0.0
Enterotomy ...	11	2	18.18	12.5
Supp. peritonitis, opened and drained ...	26	4	15.34	22.2
Puerperal peritonitis ...	4	3	75.0	—
Radical cure of hernia ...	9	0	0.0	0.0
Amputation of pregnant uterus ...	3	0	0.0	—
Tumour of abdominal wall ...	1	0	0.0	—
Resection of cæcum for cancer ...	1	1	100.0	—
Perityphlitis, removal of vermiform appendix ...	2	0	0.0	—
Incomplete operations ...	6	3	50.0	50.0
Cholelithotrixy ...	2	0	0.0	—
	1,000	53	5.3	9.2

This operation, I venture to predict, will revolutionise the obstetric art, and in two years we shall hear no more of craniotomy² and evisceration, for this new method will save more lives than these proceedings do, and it is far easier of performance. It is the easiest operation in abdominal surgery, and every country practitioner ought to be able and always prepared to perform it. No special instruments are required—nothing but a knife, some artery forceps, a piece of rubber drainage-tube, two or three knitting-needles, and a little perchloride of iron.

My method of operating is to make an incision through the middle line large enough to admit my hand, and then I pass a piece of rubber drainage-tube (without any holes in it) as a loop over the fundus uteri, and bring it down so as to encircle the cervix, taking care that it does not include a loop of intestine. I then make a single hitch and draw it tight round the cervix, so as to completely stop the circulation. I give the ends of the tube to an assistant, who keeps them well on the strain, so as to prevent the loose knot from slipping, the reason of this being that should there be any bleeding and any necessity for further constriction, I could secure this in a moment, without undoing any knot, and the simplicity of this method greatly commends it. I then make a small opening in the uterus, and enlarge it by tearing with my two forefingers, seize the child by a foot and remove it. I then remove the placenta, and by that time the uterus has completely contracted, and is easily drawn through the wound in the abdominal wall. The constricting tube will now probably require to be tightened, and the second hitch of the knot may be put on at the same time and the work is practically done. Stuff

² Save for hydrocephalus.

a few sponges into the wound to keep the cavity clear of blood, and pass the knitting needles through the flattened tube and through the cervix, and in this simple way a clamp of the most efficient kind is at once made. The uterus is removed about three-quarters of an inch above the rubber tube. The usual stitches are put in, the wound closed round the stump, which, of course, is brought to the lower part of the opening, and then the stump is dressed with perchloride of iron in the usual way.

The operation takes far less time to perform than it takes to describe, and as there is hardly any possibility of complications, it is one of the simplest operations that can be undertaken, and must always be pretty much the same; for this reason no one need be in any fear about undertaking it, for in the absence of variation in the difficulties to be encountered, it differs entirely from any other operation in abdominal surgery. If performed before the patient has been mauled about by ineffectual attempts to deliver, its mortality will be no greater than that of ovariectomy, and the arguments in its favour against all alternative proceedings are, first, that it cannot be more dangerous to the mother than most of these are, that it saves the life of the child, that it prevents the unfortunate mother from again being placed in a similar condition, and it certainly has the great advantage over alternative proceedings having a similar object, that its great simplicity, as contrasted for instance with operations proposed by Thomas, Müller, and Sanger, will make it possible for the country doctor, less experienced in surgery, to perform it without hesitation. These complicated and difficult proceedings may have their advantages, though I confess I do not see them, but they will be left for the hands of experienced specialists. The operation I have described will be the operation of emergency, when only the resources of general practice are at hand.

✓ PRESIDENTIAL ADDRESS ON THE PROGRESS OF LARYNGOLOGY.

Delivered at the First Meeting of the British Laryngological Society, on November 14th, 1888.

By SIR MORELL MACKENZIE, M.D.LOND.,

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GENTLEMEN,—My first duty on taking possession of the presidential chair to which your suffrages have called me is to thank you for the high honour which you have conferred on me. I consider it much more than a mere honorary distinction to be chosen to preside over such an assembly as this. To be the first president of a Society numbering among its members the foremost workers in the field of medical science to the cultivation of which my own professional life has been devoted, is a dignity and a privilege peculiarly gratifying to me. It will be my endeavour to justify your selection by striving to be something more than a figure-head—ornamental, or the reverse—as presidents have been sometimes known to be.

In discharging the duties of my office, I have to think, not like certain great monarchs, that the eyes of my predecessors are on me, but that I am setting an example and establishing precedents for my successors. The Society whose formal entrance on life we are met here to-night to celebrate is, I feel, destined to play a most important part in the furtherance of our knowledge of diseases of the upper air-passages, and of our ability to prevent and cure them. But the scientific work which it will perform, valuable as that cannot fail to be, is not its only title to independent existence. Our Society embodies in concrete form the development and tendency, the efforts and aspirations, of laryngology. It gives a "local habitation and a name" to the speciality such as it has never yet had before the public eye in this country. The Laryngological Association supplies a bond of professional union among throat specialists in the United Kingdom which has hitherto been wanting. It has, therefore, a double function—the advance of laryngology, and the promotion of proper *esprit de corps* among its professors. Under both these aspects the Society will, there can be no doubt, be of the greatest use to the younger generation of specialists; and I, as representing