General summary of conclusions from one thousand cases of abdominal section / by Lawson Tait.

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FROM

ONE THOUSAND CASES

OF

ABDOMINAL SECTION.

BY

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

SURGEON TO THE BIRMINGHAM HOSPITAL FOR WOMEN, &c., &c.
VICE-PRESIDENT OF THE GYNÆCOLOGICAL SOCIETY
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A SERIES OF ONE THOUSAND

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The conclusion of such a series affords a suitable time for a retrospect and a critical examination of the results of the work and a consideration of what general conclusions may be drawn from them.

The question of the best anaesthetic for use in abdominal surgery is one to which of course I have given a very large amount of attention, and it is very singular that in the class of drugs, the action of which there can be the least doubt about, we are as yet certainly very unsettled in our views. Like all pupils of Simpson, I began my professional life with a most profound belief in the advantages of chloroform over all other anaesthetics. I have never seen an accident from chloroform, but partly by reason of the fear of inquests and partly by the example and teaching of Dr. Keith, a belief grew in my mind that ether was preferable to chloroform, and at first I had the impression that the sickness after ether was less marked than after the use of its rivals. I was not, however, very long in discovering that ether has special risks for people with a tendency to bronchitis; and later on I discovered, and have already published the fact,

that during the administration of ether the secretion of urine is completely arrested. It was subsequently very forcibly impressed on me that for patients with damaged kidneys ether is a dangerous anaesthetic, and although I cannot say that I have seen any fatal results arising from this peculiarity of its action, I certainly have had abundant cause to fear it. My first alteration, therefore, in my views concerning ether, was to limit its application to patients under forty, but even after this I found my confidence in its safety greatly diminished by the fatal occurrence of bronchitis in a case of hysterectomy in a woman aged thirty. In this case the patient's breathing was embarrassed from the moment she recovered from the anaesthetic, her urine was scanty and became ultimately albuminous, and she died on the fourth day from suffocative catarrh, the post mortem showing that so far as the operation was concerned everything was perfectly satisfactory.

Amongst my other experiences of anaesthetics was the employment of bichloride of methylene and of Richardson's mixture, generally known as methylene ether. The latter is by far the most easy of employment of all the anaesthetics I have seen tried. Patients recover more rapidly from it and seem to be less affected by it than any other anaesthetic; but I had a death from it, and on that account promptly gave up its use. The bichloride of methylene has many advantages, but it has one practical disadvantage that its application requires a special apparatus and an amount of skill in the employment of that which cannot always be had. I have also found that the sickness after it is more persistent and troublesome than after ether or chloroform, and there are some patients who are kept under its influence with very great difficulty. A further objection to it is that it does not keep well, and it certainly is not commonly supplied in a pure condition, so

that my employment of it was not very extensive. The last stage of my experience in the employment of anaesthetics is the use of a mixture of two parts of ether and one part of chloroform, given by means of Clover's apparatus. This mixture is rapid in its action, is not at all unpleasant to the patient, and certainly the sickness after it is far less than with anything else I have ever used. I cannot say that I think it more safe than any other anaesthetic, for we may be quite certain that with none of these drugs will there ever be absolute security against accidents, but I have seen no bronchitis after its use; it has no effect in arresting the secretion of urine, and on the whole I am far better pleased with it than any other, and I may say that Dr. Annie Clarke, who has now been my administrator through a very extensive experience, is quite in agreement with this conclusion.

The next point for consideration is an accurate determination of the meaning and application of the term abdominal section, and this is by no means so easy a matter as might at first sight be imagined. I have gathered from the writings of others that there is by no means a complete agreement on this point, and there are several of my own cases upon whose classification I have considerable doubt. The commonly accepted view is that an operation is not an abdominal section when the peritoneum is not opened, yet there are many operations where this is done that are not, at least so far have not, been classified as abdominal sections. Thus herniotomy, when the sac is opened, would be an abdominal section if this definition were logically followed. The removal of a kidney or the opening of the colon might or might not be an abdominal section by the accidental peculiarities of any particular case. Indeed numerous difficulties may be raised. Any definition can be objected to, and no hard and fast line

can be logically followed, but it seems to me that less confusion will arise upon the discussion of many points if we take the definition I have indicated, and therefore I follow it. The only changes effected by it upon series already published are that a few kidney operations, all successful, are excluded, as are also two cases of removal of extraperitoneal cysts, both fatal operations. The difficulty mentioned in connection with herniotomy does not occur in my practice, as I do not perform such operations save in the umbilical variety, and that operation no one can object to being classed as an abdominal section.

I desire it to be understood, therefore, that in all of the cases in the following analysis of the series, the peritoneum has been involved.

ANALYSIS.

	Cases.	Deaths.	Per centage, Mortality.	
Exploratory Incisions	94	2	2·1	
Cystoma — Parovarian	65	2	3·07	8·1
Cystoma — One Ovary	239	26	11·	
Cystoma — Both Ovaries	101	5	5·	
Removal of Appendages for Myoma	99	7	7·	
,, ,, ,, ,, Inflammatory Disease	201	10	5·	
,, ,, ,, ,, Epilepsy	6	0	0	
,, ,, ,, ,, Deformity	1	0	0	
Hysterectomy	54	19	35·7	
Opening for Draining Pelvic Abscesses	30	0	0	
,, ,, Incomplete Operations	30	15	50·	
,, ,, Cholecystotomy	13	0	0	
,, ,, Nephrectomy	3	0	0	
,, ,, Nephrotomy	9	0	0	
Extra-uterine Pregnancy	11	2	18·	
Hepatotomy for Abscess and Hydatids	10	0	0	
Hydatids of Peritoneum	2	0	0	
Tumours of Omentum and Mesentery	5	0	0	
Enterotomy	8	1	12·5	
Adhesion of Intestines relieved	2	0	0	
Chronic Peritonitis opened and drained	7	1	14·3	
Acute Peritonitis	2	1	50·	
Umbilical Hernia	4	0	0	
Caesarian Section	1	1	100·	
Scirrhus Tumour of Abdominal Wall	1	0	0	
Suprapubic Lithotomy	1	0	0	
Enucleation of Myoma	1	1	100·	
	1,000	93	9·3	

The first point to attract attention is the general death-rate, 9·3 per cent. Whether this is high or low I cannot say, for no such series has before been published, and, therefore, I cannot discuss it relatively. I think it high, and am perfectly certain that if I live to complete another such series it will have a very much lower mortality, and for two reasons. In the first place, the present series contains my early work, where the want of experience told heavily. A contrast of the results in my first series of fifty ovariotomies (thirty-eight per cent. mortality) with my last three hundred and thirteen (4·80 per cent. mortality), is enough of itself to indicate the terrible influence of want of experience; and though other factors enter into the explanation of this great difference, the influence of inexperience is everywhere visible. In hysterectomy it is very evident, and also in the removal of diseased appendages, but it is most apparent in the marked preponderance of incomplete operations in my early practice. Many operations were then left unfinished which would not be so now, and thereby a heavy item of mortality would be removed.

This quite justifies what I said in my first contribution to abdominal surgery, that in this branch of our art "more I think than in any other in the whole field of surgery, does the personal experience of the operator, gained by failures and hair-breadth escapes, serve him in good stead for his subsequent work."

The second reason from which I predict that my second series of a thousand cases will have a much lower death-rate is that important causes of failure have been absolutely removed by the complete discontinuance of the clamp in ovariotomy and of the ligature in hysterectomy, and cases of all kinds are now operated upon at earlier stages of these diseases than they were when I first began my work, all of which points will be discussed in their proper places.

The first group of cases of which I have to speak is formed of ninety-four exploratory incisions, made for the purpose of ascertaining exactly the state of matters inside the abdomen and settling any doubt as to the diagnosis and prognosis in each case. Ninety-four is a large number, but it must be remembered that years ago I advocated an exploratory incision in preference to a first tapping, and that I have persistently carried out the practice. As I have said many times before, so I say again now, I make exploratory incisions to make sure that I am not wrong, whereas formerly I used to make them only to find I was entirely mistaken. They serve the purpose of complete tappings, and, as the patients almost uniformly recover, they do no harm at all. There are two deaths recorded in this group, but I might have eliminated these two cases with perfect fairness, for one died really of the prolonged sickness after the anaesthetic, a broken-down old woman of sixty, and in the other I passed a trocar into the irremovable tumour, so that the operation was not strictly limited to mere exploration.

In nearly fifty of the cases the exploration has amounted merely to a tapping. Occasionally it was followed by drainage, and in some of these cases results of the most marvellous kind have followed—results which have completely justified the practice. In four cases I have explored the abdomen to see if I could remove the spleen, and in two of these the enlargement of the organ has completely disappeared, and the patients have now perfect health. In one case, resident in London, I opened the abdomen and found a large tumour of the liver, which seemed to me to be certainly malignant. The ascitic effusion has not occurred again, and the tumour now, nearly a year after the operation, is certainly less than half of its original size, and the patient is rapidly getting well. In five cases, where there was a large effusion due to papilloma, mere

exploration has completely cured the patients, a singular fact that I have already drawn attention to. It is perfectly impossible to understand this, just as it is perfectly impossible to understand how papilloma in two cases, presenting in every detail identically the same characters, is a curable disease in one and a rapidly malignant disease in the other. But of this there can be no doubt now, for my original observation has been fully confirmed by Mr. Alban Doran. Several cases have also been published where pelvic pain has been relieved in a most inexplicable way by mere exploration.

The great advantage, however, of this extended application of abdominal section is most apparent in other groups where tumours have been removed and diseases cured under circumstances apparently hopeless, and when the operation was begun under the belief that nothing satisfactory could be accomplished.

The next group includes four hundred and five cases of cystomata. Of these I have nothing new to say, there is nothing that I have said of recent years that I have to unsay, unless it be that I have an impression that in some previous paper I have said that I never lost a case of removal of a parovarian cyst. I cannot find the statement, but I have a strong impression that I did make it, and now I display a group of sixty-five cases of removal of parovarian cysts, with two deaths. These two cases have been dissected out of my earlier lists, published before I recognized the importance of separating parovarian from ovarian cystomata. The explanation of the fatalities is that they were clamp cases.

Upon the subject of the clamp, as on the subject of Listerism, I have nothing more to add to what I have already said, and the following analysis of the four hundred and five cases proves as clearly as any figures can indicate a conclusion, that the clamp was the chief

cause of my early mortality, and that Listerism had little or no influence one way or the other.

	Cases.	Deaths.	Mortality, per cent.
Clamp (Listerian) - - - -	36 ..	9 ..	25
Clamp (non-Listerian) - - -	26 ..	7 ..	27
Ligature (Listerian) - - -	30 ..	2 ..	6·6
Ligature (non-Listerian) - - -	313 ..	15 ..	4·80
Total - - - -	405 ..	33 ..	8·15
Extra Peritoneal Cases - - -	62 ..	16 ..	25·7
Intra Peritoneal Cases - - -	343 ..	17 ..	4·95

The diminution of mortality, about two per cent., in the group of cases (313) where I used the intraperitoneal method without Listerian details, when compared with the group (30) where I used the same method with Listerism is in all probability due to increased personal experience. I have only further to say that so far as I am concerned this statement ends all discussion of this much debated subject, unless something should happen to alter my opinions about it. For the present I am tired of wrangling over it.

The third group contains ninety-nine cases of removal of the appendages for the arrest of the growth of uterine myoma and for the hemorrhage due to that disease; and in connection with these cases it will be convenient to discuss the group of hysterectomies, fifty-four in number.

Nothing could be more startling than the different mortalities of these two operations, and it is only fair to the first of the two to say that five out of the seven deaths occurred in the early part of my practice, and that the other two might very fairly be reckoned on a list of what Mr. Bryant calls "too late" cases. My belief is now firmly established that if this operation were performed as soon as symptoms drew attention to the disease its mortality would be hardly appreciable, and it would restrict the necessity of the terrible operation of hysterectomy to a very small group of cases. I am glad to know that in this view I am

supported by Mr. Knowsley Thornton. The operation is very often not an easy one, and in the earlier part of my practice it happened three times that I was unable to complete the operation, one of these cases died. In another, I could neither remove the appendages nor the tumour; it went on growing, and was removed (hysterectomy) some years after by Mr. Knowsley Thornton, with a fatal result. The third incomplete case died, I am informed, about two years after my attempt, from protracted hemorrhage.

The ultimate results of about a third of these cases I have already published, and the whole of them will form the subject of a detailed paper when a sufficient time has elapsed to enable me to obtain fair conclusions.

In a recent paper in the *Medical Times and Gazette* (July 5th, 1884), Sir Spencer Wells said that more evidence was wanting on this subject, and that mere vague statements make no impression on a thoughtful and sceptical profession. It is a very remarkable contradiction of Sir Spencer Wells own words, which I have just quoted, that he himself has contrived three separate and wholly contradictory explanations of the fact that the removal of the uterine appendages arrests menstruation, and surely he would not have devised these explanations unless he had accepted the fact. In 1880, he advanced the theory that the arrest was due to the inclusion of the ovarian artery (whatever that may be) in the ligature. In 1881, he produced a dissection at the International Medical Congress, which proved clearly that no human ingenuity could tie this so-called ovarian artery. Finally, in his recent address on "Modern Surgery," (*British Medical Journal*, Nov. 15th, 1884) he tells us that "we have learned that if the ovaries be not completely cleared away—if, having been adherent, they have been twisted or scraped away from their connections and some small portions left—menstruation may afterwards recur quite regularly,

even though both tubes have been totally removed." Birmingham certainly was a curious selection for the enunciation of doctrines so old-fashioned as this, displaying as it does a belief in the ovular theory of menstruation, now happily exploded. It is perfectly impossible to argue with anyone in a state of mind like this. I can only quote the words written about it in a pamphlet recently received by me from its author, Professor Reeves Jackson, of Chicago. "Thus it has been urged as a possible explanation that some portion of the ovarian tissues may have been left behind, and that this remaining portion has been sufficient to continue the work of maturing and expelling ovules. Surely the mind which could rest content with such an explanation would be easily satisfied. In refutation of its force it is only necessary to call attention to the obvious anatomical and physiological difficulties involved in the supposition and to the fact that no such condition has been shown to exist in any of the cases under consideration." I can only say that in eleven of my cases there has been complete arrest of menstruation, although I have left either one or both ovaries, being satisfied for one reason or another to remove the tubes only. But on the other hand, there are occasional cases that go on menstruating regularly for some months after both tubes and ovaries are removed. In one case a woman goes on menstruating still in whom both tubes and both ovaries together with three-fourths of the uterus have been absent from her for nearly fifteen months. It is perfectly clear that so far as we have gone we have obtained no clear explanation, indeed we have no trace of any explanation of such cases as these. My conclusions so far, already published more than once, with detailed evidence in support of them, are that the ovaries have nothing whatever to do with menstruation, and my experience confirms the views first published by Ritchie in 1842, and republished by him and his

son Charles in 1865. It is most singular that these views should be so absolutely ignored by Sir Spencer Wells, seeing that the volume in which they are published is dedicated to himself, and that Charles Ritchie was his assistant for years, and unfortunately died in his service.

It is perfectly certain that the Fallopian tubes have some most especial connection with the function of menstruation, for in the great majority of cases their removal either with or without the ovaries promptly arrests it. In some patients the process is not arrested for many months, as we might expect from the ordinary phenomena of the menopause, and it yet remains to be seen whether there are any cases in which the operation absolutely fails in arresting menstruation. It is perfectly certain that there are some cases in which even when removal of the uterine appendages arrests the function at once and completely, it does not arrest the growth of the tumour. In such an instance I believe the disease belongs to the variety of soft œdematous myoma first described by myself as a special form. I am now watching three cases where the tumours go on growing after complete arrest of the periods by operation, and I greatly fear all three of them will come to hysterectomy. I have already published cases in which this soft œdematous myoma has continued to grow after the normal climacteric change.

I now come to speak of hysterectomy, concerning which I may say at once, it is an operation which I detest. Its mortality is fearful. Sir Spencer Wells has had over fifty per cent. of deaths, and my own mortality has been 35·7 per cent. Bantock has recently had a run of bad luck; the mortality of other operators is not fully displayed, and Keith alone has had brilliant results. My own heavy bill is chiefly due to deaths from hemorrhage, which resulted from trying the plans

recommended by Spencer Wells with the intra-peritoneal method. All the eight cases in which I tried this plan died, and they all died in the same way, shrinkage of the pedicle, from serous oozing and subsequent hemorrhage. I tried every device which I could invent and everything I had seen recommended to prevent this accident but in vain. Four deaths occurred in seven cases where I used Wells' clamp; in two cases I used the cautery with one success. In thirty-seven cases where I adopted the extra-peritoneal method with the principle of circular constriction of the pedicle, I have had six deaths, and this is far more satisfactory and promising. There can be no doubt that for uterine tumours the extra-peritoneal method of dealing with the pedicle is the only one admissible. The principle of circular constriction, originally devised by Kœberle and modified by myself, is a most important addition to the details of the operation. The details and methods of operation elaborated by Bantock are, on the whole, the best I have seen; but there is something wanting yet to complete success. Every patient, or at least nearly every one, who recovers from hysterectomy does so, as it were, by the skin of her teeth. These cases never go on straightforwardly to recovery as ovariotomies do. The amount of worry which is given me by every case of hysterectomy, even when successful, is such as to be almost beyond the recompense of any fee, and the disappointment inflicted by every death is quite indescribable.

Of one group I think I may speak with a certain amount of satisfaction, that is, the group which includes thirty cases of incomplete operations, even if all that I can say is that three per cent. of incomplete operations is not a large proportion, and I have the satisfaction of knowing that it is still on the decrease, as my experience grows. It may be well here to give an analysis of this group, in order that the results of the dissection may

be applied, if necessary, to the various groups already indicated in the general analysis.

INCOMPLETE OPERATIONS.

	Cases.	Deaths.
Ovarian Cystomata - - - - - - - -	6 ..	3
Uterine Sarcomata - - - - - - - -	7 ..	4
Removal of Appendages for Myoma - - -	3 ..	1
,, ,, ,, Inflammation - -	7 ..	3
Tumours of various or uncertain origin - - -	7 ..	4
	30	15

The mortality is of course heavy, fifty per cent., and the results in the great majority of those who survived the operation were very unsatisfactory, though in some the disease has been arrested apparently for an indefinite time. I have no doubt now that in many of those cases I might have finished the operation, in fact, I know I could, but I always had a horror of a patient dying on the operating table, and from that distressing incident I have hitherto been entirely free. I now think that it would have been better even to have had such a disaster, and to have finished a larger number of these operations.

Concerning the cases of the remaining groups, the diseases treated, and the details of the treatment, I have already published so much in detail that I have very little left further to tell you. Of these, there are a number of small groups, which altogether amount to ninety-seven operations, performed for mortal diseases, with five deaths, a mortality of almost exactly five per cent., and everyone of these operations, ten years ago, were either undreamt of or but faintly indicated. I do not think I am entitled to say less than that I have a very large amount of pride in the fact that everyone of these operations was either originated in this town or was reared from its state of struggling infancy into full adolescent life within its fostering boundaries, but the orator who addressed the Midland Medical Society in

November, entirely ignored the fact that from 1842 till now abdominal surgery has grown and advanced, not in London, but in the large provincial towns of Great Britain.

In the address to which I have just alluded, Sir Spencer Wells has written the following sentence:—"I have more than once warned the profession against the capricious extirpation of the ovaries, because they are supposed to be the source of all womanly ills; and I repeat my caution, seeing to what widespreading evils its neglect may lead. I think it a charity to lay a checking hand on an erring judgment before it has gone too far; and I would fain save our profession from the public odium which must, sooner or later, be the penalty suffered by all, if a few indiscreet members of our body act without general sanction or disregard general disapproval." But Sir Spencer Wells' warnings are of no use unless he will discuss and prove his statement, because numerous discussions have been invited and taken place, and any kind of disapproval challenged, but nowhere has the latter been shown. It is perfectly true that a few anonymous critics have made some ill-defined and indefinite charges against something or somebody in general, and a few irresponsible utterances are on record, but as soon as specimens are produced, as soon as papers are read, the discussion and the verdict is one of entire approval.

I wish, therefore, again to draw attention to the fact that a mere reiteration of unsupported, inaccurate, and exaggerated statements after repeated challenges is absurd. By the admission of his silence, Sir Spencer Wells is unable to justify his language. It is not true that anyone "removes ovaries capriciously because they are supposed to be the source of all womanly ills," but it is true that Sir Spencer Wells and others refuse to remove uterine appendages in the most capricious fashion, or that they are unable to recognise them

when they are the morbid cause of intense womanly agony.

A few years ago, some correspondence passed between Mr. Spencer Wells and myself on this subject, and I desired a public discussion of the subject.

Mr. Spencer Wells did not accept the challenge, and I have now to repeat what I said then, and more, for this spirit of capricious refusal to remove the uterine appendages and to defer the performance of operations which ought to be done, adds very materially to the terrible list of "too late" cases, which is now the greatest hindrance to increased success. The very last fatal case of hysterectomy which I had is an instance in point. She had been under the sole care of a very distinguished surgeon for nine years; in 1878 he talked to the patient and her friends of removing the appendages, but capriciously enough he did not carry out the proposal; she came to me, worn out with pain and hemorrhage, her face and legs œdematous, her abdomen full of red serum, and a tumour that weighed forty-eight pounds. The operation was simplicity itself, not a single point of difficulty occurred, though the surgeon in question had given as one reason against the operation that the tumour was very adherent. There were no adhesions. The patient died merely from exhaustion and shock. Had the operation been done before the onset of œdema, I feel certain she would have recovered. Had the appendages been removed, as they ought to have been, four or five years before, hysterectomy might never have been necessary.

I have grouped together all my cases of pyosalpinx, hydro- and haematosalpinx and chronic ovaritis into a group of cases of "Removal of the uterine appendages for chronic inflammatory disease," for the very evident reason that the trouble is the same in all cases, varying in detail and variety, but without any facts upon which

definite distinctions exist for the establishment of a hard and fast nomenclature. From the large, soft and broken-down ovary, affected by acute inflammation, to the small cirrhotic mass firmly glued to the pelvic wall or to the back of the uterus, there are infinite gradations of change through which no line can be drawn. From the large sausage-like tube containing some ounces of pus to the small occluded organ glued to its cirrhotic ovary, the transitions are equally imperceptible and the trouble as great in all of them. Sometimes on one side there is pus in the tube, whilst on the other side there is blood or serum. Sometimes the tube is most affected on one side, and on the other the gland is the seat of the disease. In future I shall attempt no such distinctions, for they are illogical, and, in many cases, impossible.

The group includes two hundred and one cases, with a mortality of five per cent., and here again the influence of inexperience is very apparent, for seven out of the ten deaths occurred in the first hundred; and out of the seven incomplete operations, five occurred in the first hundred. I do not think that for the future the mortality will be more than three per cent., though even five per cent. as a primary result abundantly justifies these operations. The indications which I had already obtained from several of the more striking cases, that the diseases dealt with by these operations are really of a very fatal kind, has been completely established by the remarkable observations of Dr. Kingston Fowler, who has displayed beyond a doubt that the rupture of a suppurating tube is a very frequent cause of fatal peritonitis, whilst all these diseases are productive of intolerable suffering from which it is perfectly impossible to relieve the sufferer in any other way. All this has been discussed over and over again, and I have nothing new to say upon the subject. I cannot of course speak here precisely of the ultimate

results in the whole of the one hundred and ninety-one cases, because a great many of them have not yet passed a sufficiently long time since the operation to enable me to judge of the absolute effects of the operation; but this I can say of them without any hesitation, that in the great majority the relief obtained is immediate and complete. In some, the relief is complete for a few months and then they seem to go back to some of their old sufferings. In a few, there seems to be no relief whatever for a very long time after the operation, and I have now seen a sufficient number of these cases to be able, I think, to arrive at an explanation of this. I have before me a list of thirteen cases in which, for a time varying from six months to two years, little or no relief was obtained by the removal of the uterine appendages. In seven of these cases sufficient time has now elapsed for the establishment of complete success, but all the others are just as ill, or nearly so, as they were before the operation was performed, with this exception, that in five out of the six menstruation has been completely arrested. In the sixth case it is going on still with perfect regularity, though with less pain than there was before the operation was performed, now fifteen months ago.

In every one of these thirteen cases an accident occurred in the effusion of a large quantity of blood into the periuterine tissues some time after the operation, generally within a week; in some of the cases it was clearly localised on one side, whilst in others it seemed to be a uniform periuterine effusion. The result in every case was the same, the pulse and temperature went up, the patient suffered a great deal of pain, and the condition of invalidism remained after the operation for various periods. In one patient, having been perfectly satisfied that the effusion had suppurated, I re-opened the abdomen, about four months after the original operation, and cleared out a

small quantity of pus, with the result that the patient was at once and completely relieved of all distress. I think there are at least two other patients out of the six, who still remain unrelieved, in whom this proceeding will also have to be resorted to, but as for the other three, I think that time will bring them right without it. The occurrence of these haematoceles is a great puzzle to me, because I cannot remember any peculiarity in the cases in which I have noticed it which would enable me to classify them, but there can be no doubt it exercises a most pernicious influence in delaying the satisfactory results of the operation.

Whilst stating briefly the facts of these qualified failures, I can say on the other hand that I might fill a large volume with descriptions of cases in which the most brilliant and satisfactory results have been obtained, women who were prolonging a miserable and useless existence lifted at once into a condition of healthy and active life, and many others clearly saved from impending death. Only one illustration need be given in addition to the number I have already published, and I give it mainly because it has a certain dramatic and personal interest attached to it, combined with a certain amount of absurdity in that the patient should actually have been in my own house for nearly eighteen months without my being aware of her state.

M. J., aged 28, entered my service as a domestic, in August, 1878. She had been married for some years to a soldier who had not obtained an official permission to marry, and who, therefore, could not take her with him. She remained in my service till the beginning of 1880, and during that time I became aware incidentally that she suffered very intensely during her menstrual periods and had profuse floodings. She did not express any desire for my professional assistance, but unfortunately for herself spent the whole of her earnings in wandering about from one

practitioner to another without any benefit from the numerous pessaries with which she was treated, until she was obliged to give up her work and enter a hospital. After she left my service I heard something more about the details of her case, and that combined with the information which I subsequently got from herself, was to the effect that she had received a gonorrhœal infection very soon after marriage and had never been well since that time. After she had been treated, without any good effect, in various institutions, she came to me, as an out-patient, in March of this year, when I obtained from her a complete statement of her history, and on making an examination was easily able to diagnose chronic inflammation of the tubes. I operated, on April 2nd, 1884, and removed both tubes occluded and distended with pus. The ovaries did not come out with the tubes, and I did not think it necessary to remove them. The patient was immediately relieved from distress, has not menstruated since, and has remained for now eight months perfectly free from pain, and is earning her living with perfect satisfaction as a domestic servant.

If I had exercised a little more curiosity about the ailment of this poor woman whilst she was in my own service, I would have rendered her a distinct benefit in shortening her sufferings by at least three and a half years, and I regret very much that I allowed her to waste her money and continue her sufferings for the want of a little judicious enquiry. The operations themselves in these cases are far more difficult than any operations for the removal of cystomata, indeed one distinguished author has gone so far as to say that they are so difficult that the performance of them is quite unjustifiable, an opinion in which, of course, I do not coincide. It is to me extremely satisfactory to learn that after a discussion of this subject for the comparatively short period of eight years, every fact that I have stated and

every conclusion that I have made, has been already completely confirmed by independent observations, and that no matter what be the vigour of the denunciations levelled against this advance in abdominal surgery by one or two of the passing generation, the result is that we have a conclusive professional verdict in favour of relieving the enormous amount of suffering endured by women from chronic inflammatory disease of the uterine appendages.

One word more, in conclusion, I have to say concerning a cause of mortality still very potent, and one which is capable of being entirely eliminated. In the recent Harveian lectures, Mr. Bryant shows a good example by publishing a list of what he terms aptly the "too late cases." If I could eliminate this class from my list I would very materially diminish the mortality percentage. The great majority of the incomplete operations belong to this class, certainly more than two-thirds of my deaths after hysterectomy belong to it, for had the appendages been removed in due time, the more terrible operation never would have been necessary.

It is now very exceptional to have ovarian tumours sent to us "too late," though it does sometimes happen. The profession is now almost completely educated to the security of early operations. I wish it were so in other cases, particularly in the case of hemorrhagic myomata and chronic inflammatory diseases of the appendages. This will doubtless come in time, for the literature of the subject is now teeming with striking examples of too long delay. Witness the terrible list recently published by Dr. Alexander, of Liverpool, in the *Medical Press and Circular*, of August 27th, 1884. Let us exercise as much precaution as ingenuity can suggest against premature and unnecessary operations, but on the other hand let us get free of the terrible stigma of being so often "too late."

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