

TAIT. (L.)

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A DISCUSSION OF THE GENERAL PRINCIPLES
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REMOVAL OF THE UTERINE APPENDAGES.*

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PRESIDENT OF THE BRITISH GYNÆCOLOGICAL SOCIETY, ETC.

I AM induced to raise a discussion upon the general principles involved in the operation of removal of the uterine appendages before the Medical Society of London for two reasons. In the first place, it appears to me that the dignified position occupied by this society, and its perfect freedom, so far as freedom can be obtained in such a matter, from anything like prejudice in favor of old views on the one hand, or a desire for improper innovation on the other, give a guarantee that such a discussion as this may be carried on before its members with a greater likelihood of a judicial consideration, and a greater certainty of arriving at a reliable and accurate verdict. Certainly we may expect this to be the case rather than if the discussion were taking place either in a society formed of men whose views became crystallized at a time when this recent innovation had no clear hold on the profession, or, on the other hand, before a society which has enrolled among its members leading men in all parts of this country who have been concerned in the development of this new enterprise.

The second reason for my appearing here is that we have had a paper from the pen of Sir Spencer Wells in the "International Journal of the Medical Sciences" for the past month, which indicates a much nearer approach to a common understanding upon

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this most important question than has yet appeared possible between the two schools of gynæcology, the old and the new, and I really am almost tempted to confine what I have to say to the points raised by Sir Spencer Wells in that paper, and to differ but little from them, for, in spite of the many passages of arms which have occurred upon this subject, and in spite of the hard words which have been used on both sides concerning our various views, no one is more anxious than I am to arrive at a conclusion which shall be satisfactory and honorable to my profession and safe to the patients committed to our care. I therefore desire to speak in terms of the strongest commendation concerning the paper which has been contributed by Sir Spencer Wells, and if I make a single qualification in that praise it will be confined to its peroration, where I think Sir Spencer has forgotten two things. The first is that part of his own history in which, until some twelve or thirteen years ago, he, being the only operator for ovarian tumors who had anything like a reasonable measure of success, performed an enormous number of operations which only eighteen or twenty years ago were looked upon by men in the very front ranks of our profession as little short of butcheries. He gathered these large numbers to his operating-table by reason of his relative success, and he must not forget that the same principle may possibly and charitably be extended to other operations in the hands of younger men, and that the numbers of these operations, which seem to him to be open to some question by their largeness, may be due to exactly the same cause which drew to him his own great clientele.

The second is that the number of these operations may seem to him to be actually very great, but in any such operation the numbers can be only relative; and it is hardly worth while to discuss at first whether the numbers be too great or too small. Let us settle the principles upon which the operations are to be performed, and then each operator will be called upon in his turn to justify the tables which he creates upon these admitted principles. In these discussions the uniform line of argument has been rather the reverse, quarreling with the numbers and not listening to one word that was to be said in favor of the general principles upon which these numbers rested. Sir Spencer Wells is clearly at fault in such a line of criticism.

Under such conditions let me raise the first general principle concerned in these operations, namely, the one upon which their nomenclature is to be determined. The writers of various leading articles in medical newspapers within the last few months have made an endeavor to confer upon these various operations the objectionable term of "spaying," and this choice was made clearly not for any purpose of scientific assistance in the settlement of any one of the numerous questions which arise concerning these operations, or for any other purpose whatever than to prove, if by any possibility it could prove, personally offensive to the men engaged in this kind of practice, and not only personally offensive, but personally hurtful to them.

One well-known leader-writer upon this subject in the most prominent medical journal of our country told us that all operations must have their nomenclature decided by their anatomical relations alone, and that no kind of intention on the part of the operator or method of his, and no kind of motive on which the operation was based, could in the least degree influence the name by which it was to be called. But the moment it was pointed out that, if I am to be called a "spayer" because I remove the uterine appendages, then an obstetric physician—my friend, for example, Dr. John Williams—must be called an "abortion monger," because he occasionally induces premature labor. The offensive incorrectness of the word "spaying" was in this way easily settled, and I hope we have heard the last of it.

There comes now the introduction into the English literature of the word "castration," as copied from the German and French, and it forms the title of the articles contributed to the "International Journal" by Sir Spencer Wells, Dr. Hegar, and Dr. Battey. This phrase is just as much open to objection as the other, although its use can hardly be regarded as being burdened with the same intentional affront. But let any one take down any volume of surgery upon which he can lay his hands, and turn to the definition of the operation of castration, and he will find that it is retained exclusively for the male. Now, an operation so simple as castration in the male must not, for strong anatomical reasons, be confounded with the operation of a far more serious and far more difficult kind when it is intended to remove the essential organs of a female patient. The anatomical relations of the removal of

the human testicle and the human ovary are as opposite as things can well be. Further, it is quite open to doubt that the ovary is the analogue to the testicle. There is no such operation that I have ever heard of in the male corresponding to the operation for removing the Fallopian tubes in the female, and as castration is confined absolutely, in the English language, to the simple removal of the testicle, if it is to be extended to the female it must be confined to the operation of removing an ovary, and to the removal of an ovary which is perfectly healthy. If it is not to be confined to the removal of an ovary which is perfectly healthy, then it must become the generic term for the removal of the ovary and ovaries under all conceivable circumstances—for this simple reason, that if it is not so, then you will have to draw such an artificial and absurd line as Hegar attempts when he says that “we understand by the term castration the removal of normal or degenerated ovaries, not, however, including those which have developed into large tumors.” Where are we to draw a line as to what is a large tumor not to be called castration, and a small tumor which is to be so called? Some of the most terrible operations of the class generally “called ovariectomies” are for small dermoid tumors not much bigger than an orange, which give rise to intolerable pain and abscesses throughout the pelvis. Is such an operation as this—one of the most serious abdominal sections which we may be called upon to perform—is this to be called castration? The thing is too absurd to be listened to for a moment. Again, an overwhelmingly large number of my own operations are performed for removal of a diseased tube, in which the ovary may be so little affected, or, even if considerably affected, would be so rapidly cured by arrest of the periodic congestion of the pelvic organs by arrest of the function of menstruation, that its removal is a matter of the utmost indifference. If I remove one suppurating Fallopian tube or both tubes so affected, and leave both ovaries, as I very often do, how can it be called a case of castration when I do not comply with the prime fact which is expressed in the use of this word? Also let me further point out that the word castration had its origin in the mutilation of the lower animals for the purpose of our food supply, and I think it is a matter of regret that it ever has been introduced into human surgery. As applied to animals, it means removal of both testi-

cles ; as applied to a man with a diseased testicle, it probably, in the vast majority of instances, refers only to the removal of one. If applied to the female patient in the case of bleeding myomata, it must of necessity mean the double operation. If it refers to a case of pyosalpinx, it may mean either the single or double. Now the nomenclature which I have suggested and for which I must plead for a favorable consideration is the use of the term "removal of the uterine appendages" to cover every kind of operation of that nature. I would yield a question upon which I hold some views for the destruction of the word "ovariotomy," and almost for the destruction of the word "oophorectomy," but I almost think that neither of these words would be necessary if the generic term "removal of the uterine appendages" were adopted, for you have only to use this phrase and to specify the disease for which the operation is done, and in one or two instances to specify the extent to which the removal has been carried, and the classification becomes a matter of the greatest ease. Thus I am habituated in my published tables to speak of removal of the uterine appendages "for myoma." That means the double operation, for a single operation would be an absurdity unless there was a congenital absence of the appendages on one side. Then I speak of removal of the uterine appendages for "chronic inflammatory disease," that may be either unilateral or bilateral, or it may mean removal of one tube or both tubes, as I have said, without the ovaries. Then, again, we speak of removal of the uterine appendages for "reflex conditions," and they of course again necessarily involve the double operation ; then we have the separate groups of removal of one ovary for cystoma, removal of both ovaries for cystoma, the removal of parovarian cysts, and hysterectomy, and in this way we get something like a common logical nomenclature under which by far the greater number of abdominal sections can be ranged, and operations in connection with both ovaries and tubes can be counted in either group. There are, of course, as in every kind of classification in every subject of human study, instances where it is with the utmost difficulty that you can say into which group they really ought to go. Thus, for example, you may have a large myoma, and you begin your operation for the purpose of removing the uterine appendages to arrest the bleeding of that myoma. Then when you

get inside you may find two large cystic ovaries, larger even than the myoma, and you are puzzled to say whether you have removed two cystic ovaries or whether you have removed the uterine appendages for a myoma, and some kind of latitude must be allowed to each tabulator as to how he groups such borderland cases. To show how difficult classification by nomenclature really is, Dr. Meadows has raised a difficulty by alluding to the fact, which really can not be disputed if we examine the question by the strictest canon, that the uterus is an appendage to the ovary, and not the ovary an appendage to the uterus. But this is carrying the matter unnecessarily far. For generations we have spoken of the uterus and its appendages, and we may continue to do so without harm. If we reversed the nomenclature in this particular we should have to construct a theoretically bifid uterus and regard the half of a woman, and the corresponding half of some man, as the proper annexa to every human ovary.

The next general question in connection with this subject upon which I want to speak is raised by Sir Francis Wells in the first paragraph of his paper, where it is noted as "Castration in Mental and Nervous Diseases." Although I have a very large share in Sir Spencer Wells's conclusion, I must say I regret that he has said so very little upon removal of the uterine appendages in mental and nervous diseases, and has said such a very great deal upon their removal for other conditions. Speaking of its application *without distinction of cases*, he tells us that "this operation has an import which attaches to no other surgical operation. It not only puts in jeopardy the life of the individual on whom it is performed, but it involves the certainty of the non-production of the whole series of beings that might result from man's obedience to the first command of his Creator, 'be fruitful and multiply.' Its potential fatality as regards the subject sinks into insignificance when compared with the absolute extinguishment of one line of the species." Sir Spencer seems to regard this injunction as a "twelfth commandment." With the eleven others mankind has not been remarkable for compliance. In this, however, he has erred on the side of excess of obedience.

Here, also, I regret to say that I think Sir Spencer Wells has given a very insufficient consideration to the one great fact which has been laid down by almost every writer without exception upon

this subject, that in the great majority—I think in 95 per cent.—of the cases of various kinds which are suitable for this operation sterility has already been secured by the disease for which the operation is performed. He says himself concerning myomata, for example, that “they interfere with fecundity.” I think it must be admitted by any one who knows anything about these diseases that in the great majority of cases they absolutely destroy the possibility of maternity. I have a great belief in the opinions of women upon all matters concerning their own sex. Here is the opinion of a very clever woman on this subject, Dr. Mary Dixon Jones, of Brooklyn. She has operated successfully in a number of cases by removal of the appendages, and says: “But lately there is a great hue and cry about the possible future baby. They do not stop to think of the countless number of women who are barren and childless for years from various forms of uterine disease—‘a drop may stop a dynasty.’ When women are suffering from hopelessly diseased tubes and ovaries, they must not be ‘unsexed’; they must continue years in torment and misery and inability for any kind of employment or avocation, because perhaps in the diseased ovary there may be a healthy follicle, which may contain a healthy ovum, which may find its way through a possibly diseased tube, and *possibly* find other favorable conditions—like Mrs. Toodles, who purchased a door-plate on which was cut the name of Thompson, because she might have a daughter, who might grow up and might marry a man by that name. Removing diseased uterine appendages is not unsexing a woman; it is restoring her from helpless invalidism to all the possibilities and opportunities of life and labor. It is not taking away the possibility of her having children—that has already been done by disease; it is only removing a cause of suffering” (New York “Medical Record,” August 21, 1886).

Many writers say that myoma is not in any way a fatal disease. Dr. Keith persists in his belief that it has no risk to life at all; but, if so, why does he publish such long tables of hysterectomies with a mortality of 12 or 13 per cent.? The sufferings and risks of myoma have been, in my opinion, greatly overlooked, and when we see a patient suffering intense pain and profuse loss at each menstrual period, with a tumor growing and occurring at such an age that it is likely to grow, why should we hesitate to

grant her relief? We can secure that relief for the patient and secure the diminution or disappearance of the growth by the arrest of that bleeding, with a certainty as great as anything of which we can speak in surgery, by means of an operation which now has its mortality reduced to less than 2 per cent. It is a matter for the patient's judgment to decide as to whether she will or will not adopt that line of treatment, and it is not to be wondered at that, under such circumstances, the patients accept the treatment, and that large numbers of them come from all parts of the world to submit to it. Therefore I hold with the belief contained in the general conclusions at the end of Sir Spencer Wells's article, that "the operation, which I shall call removal of the uterine appendages, is one which should be advised in some cases of uterine fibroids, and in uncontrollable uterine hæmorrhage." On this principle Sir Spencer and I are in accord.

I accept again with equal pleasure his second conclusion, that "it is to be resorted to in certain malformations of the genital organs, deformities of the pelvis, and accidental obstructions of the vagina." Of course, here again sterility is absolutely involved, and we clear away the great bulk of the argument against the operation which Sir Spencer Wells asserts in the first sentence of his paper.

Sir Spencer Wells's third conclusion is one upon which I think a very great deal might be said. It is that "the right to use this operation is very limited in cases of ovarian dysmenorrhœa or neuralgia, and only when they have resisted all other treatment, and life or reason is endangered." I, for one, have become extremely skeptical that there is such a thing as ovarian dysmenorrhœa, because when ovaries are bound down by adhesions due to old perimetritis the uterus is nearly always similarly bound down. I am now disposed to believe that, although much pain will be given during the development of a follicle, its rupture, and the dehiscence of the ovum when the ovaries are so bound down, yet that the intense pain is not ovarian, but uterine and tubal. Such cases I believe there are where life is not threatened, but they do resist all other kinds of treatment, and reason is often endangered by the sufferings which the patients undergo. Therefore, as the removal of the uterine appendages and arrest of menstruation is the only permanent and complete cure for such patients, the only

means of securing physiological rest and complete rest for them, I am disposed to accept Sir Spencer Wells's conclusion, but to extend it largely to certain cases which seem to have escaped his notice. These are precisely the kind of cases which occur with greater frequency in the lower ranks of life. It will happen frequently to have such a case sent one in a servant girl, or a governess, or a girl who has been kept standing the best days of her young life behind a counter, with the story of intense menstrual suffering and absolute inability in many of them to keep any one situation more than a few months by reason of the fact that their employers soon get tired of their recurrent invalidism. Remove the uterine appendages and stop the suffering, and you give such a patient a new life, because previously it was impossible for her to earn her living and do her work in her ordinary avocation. If the whole question is put to her, as it ought to be with perfect sincerity and plainness, if her sufferings are genuine, she will accept the operation as her last resource. I entirely agree with Sir Spencer Wells, and it has been my guiding rule and practice in such cases to do the operation only when they have resisted all other treatment. But the treatment by rest and methods of luxury, which is really the only treatment likely to be successful, is an absolute impossibility in the cases of any save those who are well endowed with the good things of this life. For the poorer classes there is nothing but the operation which will give them permanent security in the obstinate class of cases.

Sir Spencer Wells's fourth conclusion is that "in nearly all cases of nervous excitement and madness it is inadmissible." There I am, for the present at least, disposed to agree with him. I have tried it, as I have said in my publications on this subject, in six cases of pronounced menstrual epilepsy—that is to say, in cases of epilepsy in which the attacks were confined to the menstrual periods. The patients were all benefited most undoubtedly, but none of them were cured. They were all improved in health, and I think that, if I were to judge for myself as a patient of that kind, I should elect to have the operation done; but I could not, for one, continue to act upon Battley's principles, and therefore I have given up the practice of performing operations in nervous cases entirely, and I shall only resume it when I have had the ground made perfectly and completely clear upon

the numerous other and far more important questions involved in it.

The fifth conclusion recorded by Sir Spencer Wells is that "it should never be done without the consent of a sane patient, to whom its consequences have been explained." I agree with the utmost emphasis that no operation of this nor of any other kind ought to be done without the most complete explanation of it to the patient—that is to say, that it should be made as clear as it possibly can be according to his or her degree of intelligence, and no such words as "spaying" or "castration" ought to be used to the public, because, if the intention is to convey the true nature and intention of the operation, such words as those convey meanings to the ignorant which are absolutely contrary to the intentions and purposes of this operation.

Finally, the conclusion, number six, put on record by Sir Spencer Wells, that "the incision of morbid ovaries and appendages should be distinguished from oophorectomy, and it should not be done without the authority of consultation, as in most other cases of abdominal section," is one which I most clearly emphasize. But I want to draw Sir Spencer Wells's attention to this fact, that he has argued absolutely contrary to the chief force of his own conclusion, that the excision of morbid ovaries and appendages should be distinguished from oophorectomy, and that chiefly in connection with my own work. He is kind enough to allude to me in the following way, the first time my work has attracted even the most passing notice from Sir Spencer Wells's pen, and I must protest against the complete misrepresentation to which he subjects me—in fact, he makes me advocate and practice what I have unceasingly raised my voice against. He is speaking all through about what he calls "oophorectomy," or "castration," and it is clear he means what Battey most unfortunately called "normal ovariectomy."

"On the 1st of August, 1872, a few days after Hegar's operation, Tait, of Birmingham, is reported to have also removed two ovaries from a woman who was sinking from irrepressible hæmorrhages due to uterine enlargement or tumor. She recovered, and was better two years afterward. In the course of the next year it is also recorded that he did three more similar operations. In two of these cases he took away only one ovary. That was imper-

fect castration—not the complete operation of Hegar. The want of appreciation of Hegar's motive for the operation is evident." Here Sir Spencer forcibly illustrates my argument against the introduction of the word "castration," for it has led him in this case into a most absurd mistake. Had he turned to the table of cases from which he has quoted—apparently from memory—he would have seen that the first date was February 11, 1872, nearly six months before Hegar's case. He would also have seen that in the three cases in which he says I did not appreciate Hegar's motive I removed one ovary because it was diseased, and left the other because it was healthy; and in all three I cured my patients. Curiously enough, Sir Spencer saw one of these cases with me. The principle involved was wholly different from that independently arrived at and acted upon by Hegar and myself—within five days of each other—concerning the treatment of bleeding myoma. The principle in these three cases mistaken by Sir Spencer was the primary new departure of removing an ovary for pain or distress independently of the size of the diseased organ, and independently of life being threatened. In the first of these cases the ovary was only as big as a hen's egg, and was the subject of chronic inflammation and abscess. Its removal entirely relieved the patient of pain. In any case I must protest against these three cases being called "oophorectomies," or "castrations," or "normal ovariectomies," or "Battey's operations," or any other name which does not mean that they were cases of removal of ovaries just as much diseased as if they were the biggest cystomata which could be found in our lists.

Still speaking of "oophorectomy," Sir Spencer says: "Tait, of Birmingham, has been identified with it from the beginning. He has modified it and extended its application. Many others have followed in his steps. Some have tried to outstrip him. The ovaries and their appendages now go the same way; and the meshes of the physical, mental, and moral network of reasons why the operation should be done are so closely woven that few cases of a perplexing nature, that can anyhow be connected with the generative organs or functions, have a chance of escaping laparotomy or something more. . . . The oophorectomists of civilization touch hands with the aboriginal spayers of New Zealand."

This kind of writing reminds me of nothing so much as Dean

Ramsay's Scotch laird, who, when in a rage, went out into the street and swore "at large." It has no other intent. But I object altogether to have my name mixed up with such nonsense when I have, over and over again, protested against the doctrines and practices here, by implication, fathered upon me. Sir Spencer Wells's confusion arises out of his erroneous nomenclature, and a want of precision on his part in recognizing clearly the logical effect of principles which he himself, to a very large extent, admits. To follow these principles carefully is a much more difficult matter than Sir Spencer imagines, and nearly every new writer on the subject agrees that "removal of the appendages," "oophorectomy," "castration," "normal ovariectomy," or whatever it may be called, is a uniform proceeding, simple alike in its character and its performance, and for its performance requiring unskilled hands and a slipshod understanding of it. Therefore I have been continually crying out warnings which have not been listened to. Perhaps those of Sir Spencer Wells may have more heed paid to them, but I object to him directing them at me.

In defense of this protest let me quote from my original paper which Sir Spencer heard at the International Congress in 1881, and of which I gave him at the time a printed copy. Speaking of the cases operated upon for chronic irreparable inflammatory disease, I said that "in four out of thirty-two cases it was impossible to complete the operation, and that operations of this kind were far more difficult than operations for cystoma." All my subsequent experience confirms this. Speaking at Edinburgh, in February last, in answer to several writers who had made the same confusion as that perpetrated by Sir Spencer Wells, I said: "*Normal ovariectomy*" is an operation requiring no skill, little experience, and hardly any judgment, and therefore has been extensively and, I fear, somewhat indiscriminately practiced. I have protested again and again against it, yet many whose voices are no louder against it than my own blame me for it, accuse me of doing it, and generally get confused over the whole subject. I desire once more to say that, save when the seat of such organic disease as will explain genuine suffering, the uterine appendages ought not to be removed; and that those who attribute all the pelvic aches and ails of women to the ovaries and tubes, and rush in to remove them, are dangerous people. I don't say they are

dishonest, but I say they are misguided. This kind of laparotomy epidemic is no worse, however, and certainly not more harmful, than the tenotomy epidemic which spread all over the world when Dieffenbach first introduced his brilliant and serviceable operations. Every oblique eye was made more oblique on another axis, and many club-feet were hopelessly destroyed—results to be deplored, but common enough in all instances of human progress. New things, especially new drugs, are always done to death, and I greatly fear that indiscretion with such a new drug as chloral has done more harm than all the surgical indiscretions collectively.”

Again, writing in answer to Dr. Henry C. Coe, of New York, who also was guilty of the same mixing up of cases which I do not approve of with those in which I advocate operative proceedings, I wrote as follows in the “American Journal of the Medical Sciences” of September last: “I think I have great reason to complain of the confusion into which Dr. Henry C. Coe has fallen—a confusion which he summarizes in the seventh deduction at the end of his paper, and which he regards, he says, as legitimate. It is as follows: ‘The present enthusiasm in this country in favor of Tait’s operation will not endure, because it will eventually be discovered that the number of permanent cures is entirely out of proportion to the number of operations.’ I wish to say that what he has described throughout his paper, with, so far as I can see, quite a small number of exceptions, is not ‘Tait’s operation’ at all, but is an operation upon which Tait desires now, for at least the twentieth time, to enter a most earnest protest.”

“I have again to protest against the use of the word ‘oophorectomy,’ as employed by Dr. Coe, because there has grown up associated with that name a number of vague ideas which are misleading from their very vagueness and the impossibility, which is evident everywhere, of separating and clearly defining them. Thus it is clear from Dr. Coe’s paper, which is written by a pathologist ambitious of pronouncing from a pathological standpoint a decision upon a surgical question (a feat which is wholly impossible), that he has not yet achieved a complete idea of the fact that ‘oophorectomy,’ as he calls it, includes a lot of perfectly different things. Thus it may be an operation for a uterine myoma, or for a case of reflex trouble, as designed by Battey, and again an

operation for chronic inflammatory pelvic trouble; and all these are absolutely different in every conceivable way. The pathology of the three classes is different, the theory upon which the operation is performed in each case is widely divergent from each of the other two, and, finally, the clinical histories of the patients, and the technique by which their diseases are to be relieved, present irreconcilable differences.

"Until, therefore, Dr. Coe has got this idea into his mind and drops the use of the word 'oophorectomy,' it is perfectly impossible for him to really understand the bearings of the discussion."

In a rejoinder to my letter, Dr. Coe has frankly admitted the completeness of my argument, and probably in America, at least, for the future we shall have a closer adhesion to logical statement, and I shall not be blamed for what I protest most loudly against.

At page 466 of the "International Journal" Sir Spencer Wells asks, "Who can diagnosticate the presence of irreparable disease in these out-of-the-way organs?" I answer that I did it in October, 1871, in the instance of this patient, and that I have done it hundreds of times since, have taught dozens of other men to do it, and I could teach Sir Spencer himself if he would come to Birmingham for the purpose. Some people seem to be able to teach themselves from my writings or other sources, for I am constantly getting papers sent to me with accounts of successful operations in all parts of the world, with the diagnosis previously and correctly made. Let Sir Spencer Wells read Dr. Mary Dixon Jones's article in the New York "Medical Record," and he will see how a woman can understand, recognize, and successfully treat the troubles of these out-of-the-way organs when the subject of irreparable disease. I hold in my hand the last number of the "Columbus Medical Journal," and I find there conclusive evidence from Professor Reed, of Cincinnati, that pyosalpinx exists, can be recognized, and treated in Ohio. In Germany they diagnosticate these cases. In France, India, Australia, Japan, Spain, Canada, and everywhere throughout the States the cases are found in numbers and successfully treated. How is it that Sir Spencer Wells alone confesses his inability to recognize them?

Dr. Mary Dixon Jones ends her testimony with this remarkable sentence, which I give even at the risk of a charge of egotism. "There is no advance made in modern surgery that will do more

good, save more lives, or relieve more suffering, or add more to the sum of human life or human happiness, than this one operation known as 'Tait's operation.' It will save more lives than ovariectomy, because more need it."

We now come to the final point for discussion—not one of general principle, but one of detail, and therefore one which ought not to be introduced here. Its introduction is, however, inevitable, for it is the *ὑστερον* which has been misplaced for the *πρότερον*. I mean the number of cases of removal of the appendages for all sorts of motives which are performed. On this let me repeat what I have already said in Mundé's "Journal" (September, 1886):

"Upon the whole of this question I do not pretend to say that unnecessary and therefore improper operations are not being performed; unfortunately, I know they are, but it is due not to the principles of the operation, nor to anything concerning the operation itself, but simply to the inherent tendency to error which prevails in everything that is human. Everybody now seems to be desirous (especially on your side of the Atlantic) of opening the abdomen, and so long as this is the case the production of specimens which do not justify their removal will be inevitable. But when an operation is put in the hands of responsible people, whose reputation and personal existence will be made to depend upon their thoroughly understanding the principles upon which the operation should be performed, and which should not be departed from, this tendency to human error will be diminished. That it ever can be removed entirely is impossible, because unjustifiable and improper operations are just as common upon the operating table of the general hospital as they are upon that of the gynæcological department."

Of Dr. Coe's paper let me say that its title, "Is Disease of the Uterine Appendages as frequent as it has been Represented?" "reminds me very much of the characteristic statement, 'that the thing is about as big as a lump of chalk.' I do not know who has made any representation as to the frequency of disease of the uterine appendages, neither do I know in what population any statistics on the subject can be obtained. The varying frequency of operations for disease of the uterine appendages must, of course, be in the practice of different men entirely relative. In my own

practice these operations are extremely frequent, because I tap the clientele of the whole world; they come, and have come, from almost every country under the sun. I can easily imagine therefore that, compared with the practice of some men who have not given so much attention to this particular department of surgery, they are enormously frequent in my practice. But even I would not venture for a moment to make any kind of statement as to what their absolute as compared with their relative frequency really is. Indeed, I have not the faintest notion. But, taking it even from the purely pathological standpoint, which is evidently that occupied by Dr. Coe, it is proved beyond doubt that their frequency is great; and not only is their frequency great, but their mortality is terrible.

“Let us take the only two investigations which have up to the present time been made from anything but a surgical standpoint. Dr. Kingston Fowler in three years found fifteen cases of pyosalpinx, leaving out of the question altogether the minor troubles which do not and can not, save by the merest accident, appear on the post-mortem table in Middlesex Hospital, and, of these, eight had been fatal from peritonitis due to rupture of the pyosalpinx. Still more recently, and still more forcibly, comes the argument propounded by Dr. Grigg, who out of five deaths, which occurred within a certain period in the practice of the Queen Charlotte Lying-in Hospital, and these five were all the deaths that occurred in that period, found that four were due to chronic lesion of the uterine appendages. But for the careful examination made at Dr. Grigg's special request by Dr. Allchin, every one of these four cases would have been set down to ordinary puerperal fever; and how can we tell, unless more frequent post-mortem investigations are made in puerperal cases, that these murderous diseases of the appendages are not of infinitely more common occurrence than we imagine? No sooner does a woman get a tympanitic abdomen and feverish symptoms after a labor than it is the practice to immediately pronounce it a case of septicæmia, whereas my belief is, and the belief is sustained absolutely by Dr. Grigg's experience, that, if the abdomen were promptly opened and causes searched for, not only would the word septicæmia be to a large extent banished, but we might be able to save lives which up to the present moment have been sacrificed.”

I remember very well when Lawrence, Tyler Smith, Syme, and Miller united in saying that all abdominal surgery was abominable surgery, and even harder things than that. Then there came a time when Wells was doing his hundreds of abdominal sections, and nobody else touching the work in anything but a tentative way. Suppose in 1868 some one asserted it was not the principle of ovariectomy that was wrong, but the fact that Mr. Spencer Wells did so many. Mr. Wells's answer would have been, Come and see the cases done. The objector would have had two courses—either to accept the invitation, or refuse it. If he had accepted, he would have been converted; if he had refused, he would have been disregarded. I have offered the same challenge to all my objectors. Those who accept the challenge go away and do likewise. Those who decline I disregard.

It is pleasant to find, after all this, that, upon the general question that such operations should not be done without consultation, I am perfectly agreed with Sir Spencer Wells. But then, I must plead that consultations in some cases are eminently farcical, because the eminent persons summoned to the interview arrive there prejudiced against such operations as we are now discussing. In order that there may be no mistake as to my meaning, I shall give cases and names. Some months ago I was called to see an American lady at one of the hotels in London. She had come over specially for the purpose of having my opinion on the proposal that her appendages should be removed on account of intolerable suffering, for which she had been under the care of all the gynaecologists of Europe and America, from Marion Sims downward, for twenty-five years. In order that everything should be done that caution could suggest, Dr. Matthews Duncan and Dr. John Williams as physicians, and Dr. Bantock and myself as surgeons, met in conjunction with Dr. Freeman and another gentleman who was concerned in the case. I satisfied myself very soon that the operation ought to be done, but all I could get out of Dr. Duncan was that there was nothing the matter with the patient, that she should be put under the care of some good doctor, and made to dance for twelve months. Dr. Williams was more cautious, but not more lucid. The operation was accepted by the patient and friends, and was performed by Dr. Keith, who, in a letter to

me, abundantly justified its performance by the conditions found in the pelvis.

All I can say is, that, in such a case as this, there is not a practitioner in the Midland district known to me who would have withheld his sanction to the operation proposed.

Again, Sir Spencer Wells seems to me similarly prejudiced. I have in my pocket a letter from him to the family doctor of a lady who has a myoma as big as a baby's head. Six months ago it was a small thing in the pelvis; eighteen months ago there were no symptoms at all. She bleeds profusely at her periods and suffers greatly, and I agreed with her doctor in advising removal of the appendages. By her friends' advice she went to see Sir Spencer, who expressed his opinion that "at her age—forty-two—it is not likely to increase much, and, after the cessation of the catamenia, will undergo senile atrophy. So I would certainly not operate at present." Sir Spencer Wells, in such an opinion as this, is clearly imbued with the old and deterrent influence of his high ovariectomy mortality. If, in order to obtain relief from her symptoms, this patient had to run the risk of a 25-per-cent. mortality, no human eloquence could justify it, nor even, I think, with a 12½ percentage. But when I can show her that the risk is little more than one per cent., I show her relief at a price so favorable that I can only look upon her as a fool if she does not accept it. In my own district men send me such cases, with deliberate requests for operation, by the dozen. They see by past experience how much is gained at infinitely small cost. When I find Dr. Matthews Duncan and Sir Spencer Wells interfering with what I believe to be the steps proper to be taken alike for the relief of our patients and the advance of our art, I utter fervent prayers that they may soon be brought into a better light; but I also tell my patients to avoid them, and consult more reasonable men. In his paper in the "International Journal" Sir Spencer Wells says the mortality of removal of the appendages is 14·6 per cent. I know not where he gets his figures, but my own published results up to date give three deaths in about two hundred cases.

The editor of the "Medical Press and Circular" has had the rashness to say that these operations are done for the sake of the fees and fame to be obtained by them. In a proper sense this is true of everything we do, but in the improper sense, which

Mr. Norton clearly intended, the insinuation is too vile for discussion.

An attempt, dictated by what I regard as the worst of all motives, which has been made in Liverpool to prejudice this question and to settle the problems of surgery in a court of law, met with an ignominious defeat. What I think of it is best given in a letter of singular power and eloquence, received by me from one of the most promising young hospital surgeons of our country.

"The other day," he writes, "I saw a small vegetable cart presided over by an old woman, and bearing on one of its panels, in large, white letters, the owner's name, followed by the legend, 'APRUVED CASTRATER AND SPAYER.' By some damnable perversion of motive a few men are seeking to represent human surgery as wandering about the country in a sort of quack caravan, doing for the profit of its practitioner what this gelder is doing for his and the farmer's profit. You know what I feel about the operation, and how carefully I would hedge it around. But this outcry, and particularly Wells's paper in the 'International Journal,' so handicaps a juvenile like myself that I feel like saying to every suffering or bleeding woman who wants relief, 'Bleed, suffer, and die if you like; I won't touch you. I know I can cure you, but men you have trusted and enriched and be-titled say the diseased organs are sacred, and that the man who removes them is merely a profane seeker of gain. Go to these men and be cured, or bleed, suffer, and die unaided by a newer and better surgery than they can give you.'

"It is hard to avoid some such feeling as this in the case of a juvenile like myself," he continues. "Why are so many men with capacities for greatness so unspeakably little? Since the Liverpool business I have not done an operation of this sort. There is little satisfaction in doing an operation which one may be prosecuted for like a common swindler."

My friend has far too much courage to remain long in this state of mind, and I know he will soon return to work in which he has already achieved brilliant results, but it is this state of mind into which timid men may easily be driven and from which they may never emerge. There are, however, others of a different type, and I for one—supported as I am by the confidence of hundreds, I think I may say thousands, of my professional brethren

who trust me with the lives of their most valued patients, with the lives of their own mothers and sisters and wives—I for one shall not deviate from the path I have cut out, and not all the outcry of men whom I regard as wholly prejudiced and to a large extent willfully ignorant, nor the terrors of actions for damage, nor abusive articles, in either medical or daily papers, can make me swerve from what I believe my duty alike to my profession and my race.

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