PRESIDENTIAL ADDRESS ON SOME PENDING QUESTIONS IN GYNÆCOLOGY.

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TWELVE months ago, I opened the second session of this Society with a short introductory address, in which I pleaded the difficulties of that apparently easy task. I said that one of its difficulties certainly was not lack of material; and now that I have to close my year of office, I find a difficulty in keeping my words within bounds, when I review the work of the past session, the work of the Society immediately, and the progress of the department of medicine which it represents. I confess when I was asked to take part in the formation of this Society about thirty months ago, I had little idea that we should ever be more than two or three gathered together, struggling in a long uphill fight for mere existence. But during the past year we have added sixty-five Fellows to our list, and only eight have left us by death or resignation, and we number now, all told, four hundred and sixty Fellows of the Society. Financially, our affairs are not only in a prosperous condition, but we are positively affluent, and have already quite a large sum permanently invested, so that we are more than paying our way in spite of the heavy costs of our *Journal*. But it is the heavy costs of that *Journal* which have, to a very large extent, given us our prosperity; and no words of mine, no expressions of thanks on your part, can adequately represent our indebtedness to the work of its editor. Not only is the *Journal* a faithful record of what is done in our Society, printed and corrected without the speakers or readers being troubled with proofs, but, so far as my own productions are concerned, without any but the most trifling literal errors being sparsely discernible. Further, the *Journal* is brought up to the very last hour of its issue with all that is being done in our department, and its reviews and critical notices are characterised by a fairness and a perfect regard to the opinions of those who are in opposition. Both of these features redound greatly to the credit of the editor and the welfare of the Society he represents. For myself, I desire to express a hope that it will be a long time before the present editorial management of the *Journal* requires any change.

Of the general work of the Society, it is quite impossible to speak in terms other than those of the highest praise. Not a specimen has been shown which has not materially added to our information, and the results of the careful reports upon those submitted to investigation by a subcommittee have been specially valuable. Of the papers, it is difficult to speak without a selection which must appear invidious, because it is impossible for me to speak of them all, and I am tempted to mention only those specially where points are brought out affecting questions now under general discussion. First amongst these I mention—not only because the paper in itself was a model of exact and careful observation, just such a paper as a man in general practice can contribute with great effect and benefit to the Society—but I mention Dr. Chalmers's paper on Puerperal Septicæmia because it raised the question of operative treatment in puerperal as in other forms of peritonitis. I have now come deliberately to the conclusion that it is an act almost of criminal omission to allow a case of peritonitis to die without abdominal section. This is a stronger view of the question than I am prepared to find ready for general adoption, but I was sustained in my view to a great extent when I heard Dr. Barnes say, in the discussion of Dr. Chalmers's paper, "Whenever pus formed and collected in the abdomen and pelvis, he believed the indication was strong to make an incision into the abdomen so as to give it exit." This is the surgical rule in the finger, the eye, the perinæum—in every region of the body—why should the abdomen be exempt? Some tacitly admit their inability to diagnose the conditions. If they cannot improve their knowledge, they should give up practice, or, at least, refrain from criticising others whose powers of discrimination are greater than their own. The very grave question of surgical infanticide, under the name of craniotomy, was raised by Dr. Meadows, and I cannot help feeling that the immense impression which anything from his pen on such a question must leave, must have succeeded already in making thoughtful men pause. It is very sad to hear men speak at random, and wildly gesticulate on the question of removal of

ovaries who, with a light heart, stab a child in the head with a sharp instrument, and deliberately destroy a life under the plea of an obstetric operation. Dr. Meadows has, however, sounded the death-knell of such persons, and it is our duty as disciples of the great beliefs of humanity as well as honest practitioners of our art to aid him in his crusade. Dr. Bedford Fenwick's paper on Intra-Abdominal Tumours as a cause of Cardiac Degeneration is one deserving far more than a mere passing notice, because it is one of the first efforts, if not the very first, to justify and explain the empirical conclusions to which Dr. Bantock and I arrived simultaneously some ten years ago, that all abdominal tumours ought to have early surgical interference, not only on account of the intrinsically better results thereby obtained, but by reason of the far more extended, though less apparent benefits in other directions. We have ceaselessly warred on this subject ever since, and we have brought over to our side every one save those who have arrived at that age when, according to Professor Huxley, all men of science ought to be painlessly destroyed. That time may be taken to have arrived when they give evidence that they can no longer be taught. The scholarly paper of Dr. Robert Barnes, flanked as it was by the masterly criticism of Dr. Samuel Wilks, formed a very remarkable feature of our year's work. If I say that even that paper has not shaken my sceptical position on this subject, I shall not in any way detract from the merits of the paper, for it only means what I said at the time, that the evidence hitherto collected on this subject is almost entirely, if indeed it is not wholly, on the authority of men who have been completely impressed with the authoritative decisions of tradition, that such a thing does occur. I want some new evidence collected if possible, by men who are open to the same doubt as that by which I am myself affected.

The question of normal menstruation naturally suggests itself here; first, because it is the subject of a standing committee of the Society, from which I hope for much good work, although little has as yet been done. We have had in the Society a few scattered discussions on this subject, but nothing definite has resulted. To my own name have been attributed theories of menstruation, as, for example, lately by Martin, of Berlin, of which I am as ignorant as I am innocent. All I plead for is a wholesome scepticism; our text-books and our teaching are infiltrated with the belief that ovulation and menstruation stand as cause and effect, every one so believing being forgetful of two great facts, that menstruation is known amongst the primates only; and that in women it comes long after, and goes long before ovulation.

It is most amusing to hear Sir Spencer Wells and Mr. Knowles Thornton insist that for the arrest of menstruation by operation every scrap of ovarian tissue must be removed. Of all living men, these two should have been the last to forget that two men of the name of Ritchie, father and son, wrote a remarkable book, every word of which has been confirmed by the writers who have discussed the subject since, and whose conclusions are absolutely proved by Drs. de Sinéty and Malassez. If the observations recorded in that book are accurate, the ovary can have no relation whatever to menstruation as a cause. The papers of the two Ritchies were collected and published in 1865, and the book is dedicated to "Thomas Spencer Wells."

The second reason why, in my opinion, normal menstruation must occupy a prominent place in what I have to say regarding the work of the Society during the past year is, that it has been the occasion of two of the most remarkable papers which have yet appeared on the subject; and I repeat here what I said when they were read, that they conferred marked distinction on our Society. I refer to the papers of Mr. Bland Sutton and Dr. Arthur Johnstone. It was a most remarkable thing that these two men, working upon different materials, in different ways, and approaching their subject from altogether different standpoints, and without the faintest kind of association, should come to conclusions which seem to me, on revising them, to be absolutely identical. The latest work on menstruation which had taken possession of the mind of the profession was the theory of Dr. John Williams, and by each of our authors that theory has been knocked "higher than a kite," and in the same way, for both Mr. Bland Sutton and Dr. Johnstone show conclusively that Dr. Williams's methods were faulty, and his preparations badly made. Processes which he regarded as normal were therefore shown to be only the results of inexperience on the part of the observer. Mr. Bland Sutton's conclusions are as follows: Macaque monkeys and baboons suffer a periodical loss of blood from the uterus. Unlike the human female, in them there is no shedding of the epithelial lining of the mucous membrane of the uterus and utricular glands. The amount of blood which escapes is very small in quantity. In examining badly-preserved specimens, all the remarkable appearances described as occurring in the uterus of the human female by Dr. John Williams could be pro-

duced. Dr. Johnstone's paper is equally conclusive, and I am specially proud of it, because he is a typical example of the splendid race which the implantation of Britons on a new soil has produced, and I have had the privilege of ranking him amongst my pupils. He comes forward with a new theory, that menstruation is the result of a glandular function, and he proves that the menstrual organ is the endometrium. First of all, his observations exactly tally with those of Mr. Bland Sutton to this effect, that Dr. John Williams's views are entirely based on the inaccuracies of imperfect preparations, and from the employment of too low powers. Dr. Johnstone's paper is illustrated by some beautiful drawings, for the accuracy of which I can vouch, as I saw the preparations from which they were made; and these, almost without a word of explanation, establish *a priori* probability for his new doctrine which is most remarkable. A section of the uterus of a girl eleven years of age shows a mere coating of columnar epithelium, without corpuscular development. A section of the endometrium of a girl aged 13, who had menstruated twice, shows more elaborately developed columnar epithelium, and the beginning of a corpuscular layer. The menstruating uterus of a woman of 20 shows abundant corpuscular development, constituting a thick endometrium, with its epithelium in process of casting; whilst a senile uterus at 60 shows merely the skeleton of the endometric structure, with almost complete exhaustion of its corpuscular elements, and a total absence of epithelium. Both Sutton and Johnstone agree that in menstruation the epithelium of the tubes is not shed.

Here, then, and at last, we have the explanation of a long series of most intricate physiological and pathological difficulties, the riddles of which seemed to be wholly beyond our grasp.

First of all, we have an explanation of the familiar fact that impregnation and menstruation seem to have a clear relation as to coincidence, the plain fact being that an impregnated ovum adheres only to a surface denuded of epithelium. When desquamative salpingitis has destroyed the tubal epithelium, the ovum may be impregnated in the tube, and may adhere to the exposed corpuscular tissue, and the dreadful issue of tubal pregnancy will result. The writings of the Ritchies, De Sinety and Malassez, and many others, and my own observations from operative experience, show that ovulation is going on constantly from infancy, long antecedent to puberty, and, in old age, long after the climacteric. For pregnancy not only an ovum is required, but it is necessary that the ovum, when fertilised, should pass over the endometrium when it is denuded of its epithelium, and in the condition of turgescence fit for the subsequent processes. But for one ovum thus secured, there are probably scores or hundreds which perish, either dropping into the peritoneal cavity or passing out through the uterus. The menstrual process is necessary, or, at least, such parts of it as involve the preparation of the endometrium are necessary for impregnation, but menstruation has nothing to do with ovulation further than being a means to the end of gestation.

Many curious clinical experiences and pathological facts are connected with—are explained by, and in their turn support—these simple views. Chronic endometritis involves sterility in a large number of its victims. The curette cures the disease and removes the obstruction. The condition of "infantile uterus" means absolute sterility by reason of the arrest in development of the glandular tissue. Cancer of the uterus between 15 and 50 always produces abundant endometric cell-development, because it has its favourite glandular material to work upon. On the contrary, in senile women the process is ulcerative, slow, and free from the immense tumour-development characteristic of its progress in the earlier period.

The singular discrepancy between the uterus and ovaries in their pathological tendencies is clearly due to the two facts, the first discovered by Ritchie, that from infancy to death the glandular function of the ovary is never quite at rest, so that ovarian tumours are met with at all ages; the second, displayed for the first time by Johnstone, that the truly glandular function of the uterus begins with puberty and ends with the climacteric, therefore we have myoma practically limited—indeed in origin the limitation is absolute—to the time between these two incidents of woman's life.

The analogies which go to establish Johnstone's theory are numerous. He gives the thymus gland and the hair follicles, and he might have quoted the testicle. Briefly stated, and in his own words, this theory is "that the endometrium, above the internal os, is not a 'mucous membrane,' but belongs to the so-called 'adenoid' tissues, and that menstruation is for it, exactly what the lymph-stream is to the lymph gland, or the blood-current to the spleen." But how, it will at once be asked, can we explain this curious phenomenon occurring in women only, and in some of the higher apes in a partial sort of way?

Johnstone's observations, though not yet complete, give already almost a complete answer. He says, "In two of the ruminants I have shown that nature has supplied this tissue with an abundant lymph-stream, which in the unimpregnated state washes away the ripe material to the general circulation exactly as it does any other lymph corpuscle. But in woman, where, on account of its erect position, the uterus has to depend on the tenacity of its own fibres for the preservation of its shape, no such thing as loose tissue of a lymphatic network can be depended upon. So, to preserve the integrity of the uterine wall, the emulgent stream is poured into the cavity of the body and got rid of through the vagina. The sow does not menstruate for the same reason that the child does not. The corpuscles are so slightly developed that they do not need rapid removal. Whether there are more than these three conditions in the animal kingdom I am now unable to say; but by this negative kind of proof I am ready to corroborate Ercolani's statement that the change necessary to the formation of the placenta must be much greater in some than in other animals." The Fallopian tubes and their ciliated epithelium, the endometrium and its menstrual discharge, are, therefore, all the result of the upright position.

Much else I might say about the work of the Society during the past year, but I feel I cannot pass over without notice a struggle which Gynaecology is now passing through, which seems to have reached its bitterest phase during the year just past, and in which I have been called upon as your President, along with several other and more distinguished members of your Council, to take a somewhat prominent part.

Struggles such as this are by no means unknown in the history of surgery, and they have been as bitter, but never so short nor so decisive. When Charles Clay, Frederick Bird, and Baker Brown were doing their best to advance their art, and relieve suffering women by their intrepid efforts to establish abdominal surgery, they were persecuted through a long series of years. But they triumphed in the long run, and Clay at least has lived to know that he is recognised as the Father of Ovariectomy in England. His share of credit is certainly larger than McDowell's, because he had to bear the brunt of a malicious misrepresentation, from which McDowell's relative obscurity and retirement in a Kentucky village protected him. Clay and his colleagues made mistakes—fatal mistakes we know them now to be—but the very man who chiefly persecuted all of them has made blunders of a far more serious kind. No kind of human progress is free from error—error sometimes so great that, when we look back on it, we may begin to doubt whether it would not have been better to have forfeited the progress than have gone through what was necessary to secure it, and we should in many instances come to this conclusion but that the advance is permanent, whilst the errors inherent to its progress are transitory. I am sure it is absolutely certain that no progress has ever been made in medicine without awful blunders, and I know of no reason which should make us expect abdominal surgery to be an exception to this rule. That every kind of effort should be made to diminish these mistakes to the utmost must be admitted on all hands, but persecution is not a legitimate weapon to use under any circumstances; and if the surgeons of the Liverpool Hospital for Women had been guilty of everything laid to their charge, the persecution to which one of them has been subjected could not have been more bitter, nor the punishment more severe. His health has been shattered, his practice—for a time at least—ruined, and an enormous fine in costs has been levied upon him. In fact, a seven years' sentence of penal servitude would have been a mild alternative to what he has gone through.

Some of those who have been instrumental in bringing about this result have been all along crying out that what they were doing was for the honour of their profession and the public interest. I remember that a native of a small Scotch village was complimented on the evidence of the high moral and religious tendencies of his community, as evidenced by three large, elegant churches in the village, one Established Church, one Free Church, and one a United Presbyterian, each large enough to hold more than the entire population of the place. He put the matter right by saying that their prominence was due not so much to religion as to spite.

For nearly twelve months this burning question has been before the law courts, and has been discussed in journals both lay and professional; it has been whispered that even the opinion of the highest personage in the land has been solicited upon it. Further information is wanted, however, before our side of the story is complete. We want to know who provided the funds for the prosecution of Dr. Imlach, and who are the anonymous writers in two medical papers who have so unfairly and unjustifiably misrepresented the questions at issue, and the grounds upon which they have been

decided. These men are either familiar with the questions upon which they write, or they are not. If they are not so, then they have no business to use the enormous influence of a certain journal to misrepresent a great question, and place themselves under the shield of the anonymous leader-writer; and for this reason, that such articles as have appeared in that journal must inevitably deter a large number of timid men from doing work concerning the necessity of which there can be no doubt. I think it infinitely more harmful that the thousands of women in this country suffering from the risks and agonies of pyosalpinx should go unrelieved, than that a few uterine appendages should have been unnecessarily removed.

[Mr. Tait here protested against the action of the Editor of the *Lancet* in refusing to report in any form the proceedings of the British Gynaecological Society.]

The questions raised against the staff of the Liverpool Hospital for Women were mainly three: 1, that operations were there performed without consultations being properly summoned; 2, that they were done without the patients being aware of the nature of the operation, and its possible results; and 3, that such operations were done in cases where they were unnecessary.

These questions have all been decided in favour of the staff by evidence on oath, subjected to most rigorous cross-examination in a court of law; by evidence obtained by a full inquiry by the committee of the hospital; and partially, if not wholly, by an independent Inquiry Committee appointed by the Liverpool Medical Institution. But it seems to me my duty here to remark on these three questions, by reason of the intimate relations they bear to the work of this Society, and from the fact that, as your President, I was asked to give evidence at the trial, and also asked by the committee of the hospital to make an independent inquiry.

The trouble was started by the consulting medical officer making a complaint to an outside body that he was not consulted for such operations, and that they had risen in number from 86 in 1884 to 166 in 1885. He concealed the fact, or overlooked it, that the number in 1884 was confined practically to little more than the last six months of the year, during which the work of the hospital really began, so that the rise was far more apparent than real. Up to that time the results of abdominal surgery in Liverpool, at least as represented by that done in the special ward of the Royal Infirmary, were so awful, that it could be no matter of surprise that the opening of a special hospital, where the mortality was small, was followed by the usual rush of patients to the more successful workers. This was the story of the Samaritan Hospital in London, and has been abundantly illustrated elsewhere. Further, this consulting medical officer gave us his views on oath on abdominal surgery. All I can say is that there are, and have been, six consulting officers of eminence attached to my own hospital, whose services have been sought for freely, and given loyally and abundantly. Had there been one of them who held the views enunciated by Dr. Grimsdale, I should have disregarded him; I should have plainly intimated my opinion that he was a dangerous and obstructive person, and I believe that unanimously his resignation would have been asked for. At a recent meeting of the Medical Society of London I raised the question most pointedly, and had the passive support of the meeting, whilst Mr. Knowsley Thornton gave me his active support in saying that men who went to consultations prejudiced against such operations rendered such consultations merely farcical, and were men who did a great deal of harm. We have then, and for the first time, a pronounced professional verdict on this point.

That operations were done at Liverpool without the details of the proceedings being made sufficiently clear to the patients, is a very much more difficult question to deal with. To inflict such a rule is a new departure in professional practice, but I think a very wholesome one. I have always followed it, at least, since it dawned upon me how easy it was to be misrepresented. But what the writer in the *Lancet* wants us to do, is to tell the patients roughly that they are going to be "spayed." For one, I shall do no such thing; for they would go away with an altogether erroneous impression. I should lead them more astray if I told them that than if I told them nothing at all.

Sir Spencer Wells adopts the phrase of "spaying," and actually has gone so far as to say that "modern oöphorectomists touch hands with the spayers of New Zealand." But Sir Spencer Wells has given abundant evidence himself that removal of the appendages does not bring about the terrible evils which have been anticipated from it. In his books he has laid special stress upon it. Sir Spencer has "spayed" his thousands, for what difference can result in physiological effect if Sir Spencer removes a large

cystic ovary or a small one with an abscess in it?—two large cystic ovaries or two ovaries involved in the troubles and adhesions of double pyosalpinx? The remote effects on the system, as the growth of hair, in consequent and unseemly obesity, or the change of voice and the loss of sexual appetite, must be exactly the same. Sir Spencer Wells stands up now as the most wholesale "spayer" alive, and he tells us that his "spaying operations" have had no such ill-effects as were anticipated. I accept his evidence because it wholly agrees with my own experience.

On the evidence of Sir Spencer Wells, the *Lancet* writer makes a most curious slip in his issue of last Saturday. He says: "Hospital patients in Liverpool differ but little from those in London and elsewhere; they suffer from the same diseases, and the proportion of those requiring abdominal section is much the same in every large town. Sir Spencer Wells is intimately acquainted with this class of patients in a hospital for women in London, and his criticism must be considered to have much weight." But Sir Spencer Wells says exactly the reverse. In his article in the *International Medical Journal*, he asks: "Who can diagnosticate with certainty the presence of irreparable disease in these out-of-the-way organs?" plainly indicating that he at least cannot. But in the same hospital, and from the same *clientèle*, his successors, Dr. Bantock, Mr. Thornton, and Mr. Meredith, are picking out lots of such cases and operating on them successfully. It is very funny to hear Sir Spencer speaking of the organs upon which he has spent the energies of his life as "out-of-the-way organs," when the younger generation can get at them with the greatest ease.

The Committee of the Liverpool Hospital for Women selected a number of specialists, and wrote officially to them all for opinions, and with one exception, arising out of Dr. Angus McDonald's death, they obtained answers. What Dr. Angus McDonald would have said I know, for on his death-bed he consigned to my care one of his best and favourite patients, to have the operation done, and in the last paper he wrote occur these words: "I therefore confess myself a convert to the opinions of Mr. Lawson Tait on this subject, 'impugn it whoso listeth.' I have seen enough to satisfy me that in a considerable number of otherwise incurable and excessively distressing cases, complete relief from suffering and return to usefulness is afforded by the removal of the uterine appendages."

The list, besides Sir Spencer Wells, included Dr. Thomas Savage, Dr. Thomas Keith, Mr. Knowsley Thornton, Dr. Bantock, Dr. Aveling, Dr. E. Garrett Anderson, Professor Simpson, Dr. Fabricius, Professor Hegar, and myself.

From all they received answers entirely confirming the views of the medical staff of the Liverpool Hospital for Women, with the exception of Sir Spencer Wells, who, curiously enough, forwarded his reply to Dr. Grimsdale, who in turn published it in the Liverpool daily papers, before it reached the secretary of the hospital. The only criticism favoured by Sir Spencer was, that the statement that one-third of the in-patients of the hospital were subjected to abdominal section was so shocking, as to be almost incredible. But I have examined the records of the Samaritan Hospital, as supplied to me by Sir Spencer Wells himself, and I find that he used to "spay" about 40 per cent. of his patients in that institution. In spite of Sir Spencer Wells, the report of the Hospital Committee is eminently satisfactory, so much so that I think it ought to be placed permanently on record in the pages of our *Journal*. Otherwise it will be a lost document.

There also, and as a contrast, should appear the report of the Inquiry Committee of the Liverpool Medical Institution—a document as impotent and colourless where it ought to have been effective, and as wrong where it ought to have been right, as it is possible for it to be. You have already dealt with its most salient features; let me, in conclusion, deal briefly with one other point, for it brings that report, Sir Spencer Wells, and Dr. John Williams, all into the same current of condemnation. In the report of the Council, just adopted, it is pointed out most clearly and most properly that the period over which the inquiry extended was far too short to be conclusive. If the Committee had included one specialist, or if they had asked the advice of someone experienced in diseases of women, they would have been kept out of the ludicrous blunder here apparent.

When both ovaries are removed for cystoma, the pedicle is secured so as always to embrace the tubes, and therefore the woman is completely "spayed,"—to use Sir Spencer Wells's own expression. She thereupon, in the great bulk of cases, undergoes an artificial climacteric, and this is equally the case in the removal of both appendages no matter for what causes. It is one of the peculiarities of this discussion, that removal of the appendages for cystoma has been made to appear something quite different, far more barbarous than their

removal in other instances. But it is only fair to Sir Spencer Wells to insist that this is a mistake; the results are entirely the same. Menstruation is arrested immediately in the great majority of instances, and a premature climacteric is gone through, with all the disturbances, mental, moral, and physical, which characterise nature's own change. If the Liverpool Committee had known this, and had taken the trouble to read up the symptoms of the change of life, they would have made their report more precise and of some value.

Some ten or eleven years ago I removed the appendages of two young women for cystoma; that is, in the language of that time, I did double ovariectomy on them, and ever since I have remained on terms of close friendship with them, so that I had no hesitation in asking the permission of their husbands to obtain from them a detailed account of their subsequent history.

The first, aged 22, was engaged to be married when I operated, and was married a few weeks after her convalescence was completed; she has never menstruated since then. At the time of her marriage the curious phenomena of the change had begun. She was greatly troubled by the sudden flushes and chills which most women have at the climacteric. Her temperament altered very much; she became morose and fretful, had great aversion to marital intercourse, and derived nothing in the way of pleasure from it; in fact, she told me frankly, she had almost made up her mind to leave her husband, and blamed her friends and myself for permitting her marriage. Her husband was a good fellow; he had been warned of the possible troubles, and he bore himself well in the trial. He waited patiently, and in about thirty months he had his reward. The climacteric troubles ended, and his wife began to reciprocate his caresses, and they have since lead a perfectly satisfactory married life.

The second patient had been married two years, and had one child, when I removed both appendages for cystoma. The wound was a long time in healing, because the pedicles were very short, and she menstruated regularly through the wound for nearly a year, during the whole of which time she suffered intensely from flushes and chills, and severe nervous headaches. Indeed, for some months it really seemed a grave question whether we had done any good by saving her from death by removing the tumours. Menstruation ceased, and she became morose, and had such a tendency to religious melancholia, that we sent her away. She took to drinking, and we were in despair, for she had lost all interest in her husband, her child, and her home. We kept her moving about amongst strange people and places for nearly two years, when improvement set in, but it was nearly four years before she returned to the healthy tone of body and mind in which she had lived before she grew her tumours. Now, her husband confides to me, her sexual appetite is almost aggressive.

If similar inquiries are made amongst women who have gone through the normal climacteric—that is, the change of nature's own making—similar experiences will be found in large numbers. Very many women at that time lose all sexual desire, even acquire absolute antipathy to their husbands, take to drinking and the opium habit, and make their surroundings as miserable as they are themselves. This distress is not the lot of every woman; some pass through the change hardly knowing more than occasional discomfort, but I have known many to whom this miserable stage of existence has been extended over four, five, and even six years. Can anyone imagine that, in imitating nature, which after all is the limit of the art of physicians and surgeons alike, the surgeon's knife can work miracles? When a woman goes through this climacteric change as the result of an artificial process, and before her time, she has at least this comfort, that it has not to be repeated. We may assume that, if her misery is more than usually protracted, it would have been as bad if she had not been the victim of disease requiring an artificial process. In fact, in the case of myoma, we know with certainty that the artificial troubles are far less than those associated with the indefinitely delayed senility of an intra-mural myoma.

For the Liverpool Inquiry Committee to have overlooked all these important facts is the most absurd thing I have ever known a collective body of men to be guilty of. Before the Imlach trial I made some occasional and quite casual inquiries amongst married patients from whom I had removed the appendages for various reasons, and I did not find one who did not give a perfectly satisfactory account of her sexual life after the climacteric troubles were over. Since the trial I have made a far more extensive and systematic inquiry—a very objectionable occupation, but one necessitated by the filthy statements made then and since, and I have not found a single instance in which an appetite which existed before had been in any way impaired. I found also, what I suspected to be the case before, that a large proportion of women have no sexual appetites at all, but do their best to gratify the aggressive requirements of men they love. In a majority

of instances sexual powers have been restored by the operation which had been placed completely in abeyance by the diseases for which they had been operated upon. Further than this, and most surprising of all, I have found seven instances where I removed the appendages in women before they had ever engaged in sexual intercourse, who have married since and in whom perfect sexual reciprocity seems to exist.

More wonderful still, I have two cases of women who have married after having hysterectomy done, and who were certainly *virgines intactæ* at the time of the operation. In one there is an occasional reciprocation in the act, but in the other there is an aggressive sexual activity not usual amongst women. My inquiries are still proceeding, and it may be necessary to place some details on record, though there are manifest objections to such a course of action. I cannot but regard the whole investigation as brutal, though not a single patient has resented the inquiry, when informed as to the cause which has driven me to it; and I have had but little difficulty in getting full replies from private patients of an intelligent kind. I did not pursue my inquiries far amongst hospital patients, as the Liverpool Inquiry Committee did, for I soon found that they were somewhat difficult to make understand that I had a serious object in making it, and I further found, what I knew beforehand, that hospital out-patients of the female persuasion will readily say what they think you want them to do. I have, by accident, come across one of the women examined by the Liverpool Committee, and I am perfectly certain that the method of investigation pursued by that committee was not the way to arrive at the truth about them, or any other matter in issue.

Gentlemen, I have taken up much of your time on this subject; but, whether you agree with my views on it or not, you cannot but admit that it possesses an interest so great for us individually, as well as an importance so overwhelming for the department of surgery which we practise, as not only to excuse, but to demand, the fullest discussion. That I have had to descend to quagmire—that I have had to use rough words in the row which has gone on for now nearly a year—is not my fault. We have had to follow the enemy, and I think we have had a very satisfactory issue so far. We have been fighting not only the battle of a man who has been grievously wronged, but we have been fighting the battle of surgical progress, and, in accordance with that Hippocratic oath to which we have all subscribed, sustaining the interests of those suffering creatures whom we have sworn to aid as best we can.

A more pleasing duty I have now to perform—to congratulate the Society on the choice it has made of Dr. Granville Bantock as my successor. To follow Meadows was an honour which greatly enhanced the distinction you conferred on me; to be followed by Bantock increases, if it may be, my appreciation of your favour. I have tried to recollect how long I have known him, and I cannot remember. It seems as if it were since the dawn of abdominal surgery that we have gone on as friendly rivals, eager to outstrip each other, but never showing other than a friendly and brotherly interest in the mutual progress. It would be useless to record the various fights of the last ten years which have occurred over the many questions of abdominal surgery, but in all, I think without the most trifling exception, we have always been side by side. I cannot wish better for myself—I hope I may say that it is no ill I wish for him—than that our relations may so continue.

REQUESTS AND DONATIONS.—An anonymous donor has given £1,000 towards the endowment fund of the Jaffray Hospital, Birmingham.—Mr. George Fielder, of West Horsley Place, Leatherhead, bequeathed (after the falling in of two life interests) £500 Consols each to the Charing Cross Hospital, the Middlesex Hospital, the Surrey County Hospital, and the Seamen's Hospital (late "Dreadnought"), Greenwich.—The Royal Albert Asylum for Idiots and Imbeciles of the Northern Counties, Lancaster, has received £500 under the will of Miss Alice Lowe, of Blackpool, and £284 4s. 1d. under that of Mrs. Mary Kitson, of Rochdale.—Mrs. Mary Ann Garston, of Aigburth, near Liverpool, bequeathed £100 to the Liverpool Royal Infirmary.—Mr. John Rawlins bequeathed £100 to the Yeovil Hospital.—Mr. Charles J. Schofield has given £100 to the Royal Hospital for Women and Children.—Mr. Thomas Berry, of Grove House, Worksop, has given £100 to the Sheffield Public Hospital and Dispensary.—"F. R." has given £100, "in memory of dear sister," to the National Hospital for Consumption, Ventnor.—"W. L. T. R." has given £50 each to the London Fever Hospital, the Hospital for Consumption and Diseases of the Chest, the East London Hospital for Children, and the National Hospital for Consumption at Ventnor, and £20 to the Chelsea Hospital for Women.—The Mercers' Company have given £52 10s. (in addition to previous donations) to the Central London Throat and Ear Hospital.