LECTURES ON PARALYSIS AS AN EFFECT OF BRAIN-DISEASE.*

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BY

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LECTURE II.

MR. PRESIDENT AND GENTLEMEN,—In the first lecture of this course, I gave a broad view concerning the origin of the symptoms of brain-disease, and especially as it relates to paralysis. I perhaps committed the fault of not giving at full length, as I had not time enough for it, more information concerning the view that is to take the place of those that I consider as wrong. I will not to-day enter into that part of the subject fully, as it is to be the main purpose of the last lecture. But I can say, for the benefit of those among you who do not know the views that I have already published, not at great length, but with sufficient detail, that I consider there are nerve-cells endowed with certain properties distinct from the properties existing in other cells. But I dissent absolutely from all my friends, the localisers, in admitting that the cells that serve one function, or that are endowed with the same vital property—for instance, the cells which serve to the expression of ideas by speech—are congregated in one part of the brain; in other words, that they form there a kind of cluster, and can be considered as forming a kind of organ in the great organ, the brain. I do not agree with them in that point. I think that there is no definite place, and certainly not a cluster of cells, where a certain function has its seat. My view is, that the cells used in one function, or the cells endowed with the same vital property, are scattered in the brain; that they are diffused through the whole mass of it; so that if any experiment be made, such as that which Flourens performed long ago—that is, dividing layer after layer of the brain, from the front part towards the back—it is found that every property of the brain persists, every function persists, until at last, reaching to the hinder part of that organ, when very little is left at all, then everything disappears at once. I will not, however, say that I have grounded the view to which I have come on that fact, either solely or chiefly. I consider the fact as being valuable, of course, and of great importance for the establishment of the view that cells are scattered in the way that I have stated; but there are other proofs which I cannot, unfortunately, as time is not sufficient, dwell upon in this course of lectures. There are clinical facts which are of great importance also, and perhaps of greater importance than the purely experimental facts. I will not say more in this lecture on the views that I have to put forward, as I expect I shall have to dwell at length on them in the third lecture.

But as I have spoken of facts which are observed in animals after experiments, and of facts observed in man at the bedside, and have stated that one class of facts has less value than the other class, I may perhaps answer some questions that have been raised in the minds of a number of you, at the time of my first lecture, by stating that it would be absolutely wrong to come to the conclusion that I, a vivisectionist, after having passed all my scientific life without a week in which I have not performed some experiments on animals, look upon that means of obtaining knowledge as inferior, and still more as of very little value, as compared with clinical observation. I must say, and I state it fully, that I do not think it is possible to compare the two kinds of things. The value that comes from experimental researches at times, is infinitely greater than that which comes, and may come, from clinical observation. At other times, and perhaps more frequently, the results of clinical observation are infinitely superior; so that, what was concluded from something that fell from my lips the other day was quite wrong. I have stated that certain conclusions, obtained by experimenting upon animals, seem to me not to be right; but that has nothing to do with the value of the means. Everybody knows that medical men will sometimes make mistakes from observations in pathological anatomy. Everybody knows that medical men will publish

papers that are altogether wrong in their conclusions, although the paper is the result of clinical observation. That does not show anything against clinical observation as a means of progress; it shows that we are men, and that we are unfortunately able to commit mistakes; therefore, when I stated that there had been mistakes, as I consider them, and I may be wrong myself—that there have been mistakes committed by vivisectionists—I did not mean in the least to say that vivisection is a bad means, an useless means, for the progress of science. Having said this, I will proceed with my lecture.

I left off, in the last lecture, after having begun to speak of facial paralysis in cases of brain-disease. In the field of observation that relates to facial paralysis, we find most valuable arguments against the views that are held by everybody, so far as I know. I already mentioned that disease, in parts of the brain which are considered as being parts of the channels between the centre of voluntary movement and the muscles, or disease existing in what is considered as a centre for voluntary movement, instead of producing the ordinary form of paralysis, can produce only a paralysis in the face; while the number of cases is now very large showing that disease in the corpora striata, disease in the optic thalami, disease, in other words, in any part of the brain above and in front of the pons Varolii, can produce only paralysis of the face. And this paralysis of the face can be of two kinds; it can be a paralysis only of those muscles which are usually paralysed in cases of cerebral disease above the pons, and that is a disease in which the orbicularis oculi is hardly affected, or, if affected, only for a few days; and there is also that other form of paralysis of the face, resembling that which takes place when the facial nerve is divided, in which every muscle animated by that nerve is paralysed. There are not many cases of this last kind where the disease was not in the pons Varolii; and when it exists in the pons Varolii, as you well know, it is very natural that there should be a complete paralysis of the facial nerve, or a paralysis of all the branches of that nerve, more or less complete, on account of the injury to the nerve itself, which starts from that organ. But in cases in which the disease was in other parts of the brain, there are, to my knowledge, at least five instances in which the aural nerve was paralysed. One of these instances was published by a very good observer indeed, Dr. Diday of Lyons, and others by a Dr. Duprès, who was a most excellent observer. The other cases also were published by men of merit, so that there is no possibility of doubting that, in cases where certain symptoms seemed to show that there was hæmorrhage in the brain, as in four of these cases it was hæmorrhage that brought the trouble on, at the same time that the symptoms of hæmorrhage in the brain occurred, the facial paralysis occurred, although there was no paralysis elsewhere. Facial paralysis can occur either on the side of the disease or on the opposite side; and in that respect it does not differ from the other forms of paralysis caused by brain-disease, which, as you may remember, I stated in my last lecture to occur either on the same side or on the opposite side. The number of cases, however, of paralysis on the side corresponding with the disease and occupying the limbs, is relatively small, compared with the number of cases of paralysis on the opposite side to the disease in the brain; taking all the cases I know, there are but about two hundred and fifty in which the paralysis existed on the same side as the lesion in the brain. As regards facial paralysis, the relative frequency of its appearance on the side of the disease is greater; but it is not in those features that facial paralysis is important to show how wrong all the views that I contradict are. It is chiefly on account of what I will now explain. We know that in the ordinary forms of hemiplegia, due to brain-disease, there is, according to circumstances with which we are not quite familiar, only a small number of muscles in the face that are paralysed—usually there are but three. Those three muscles, at any rate, are always paralysed, if there be others when the cause of the disease is above the pons Varolii. Now, as the disease that we find may occupy any part of the brain that has a name—and I have such a number of cases to show this, that certainly there cannot be a doubt about it—as the disease which produces this paralysis of three muscles can be found anywhere in the brain; if we conclude that it is because the centre serving to the movements of those muscles, or the fibres establishing a communication between those muscles and the brain, are diseased—if we conclude that it is because, in other words, there is a loss of function either of the centre moving those muscles, or of conductors going from that centre to the muscles—then we must admit that that centre is everywhere, and we must admit also that these conductors are found everywhere. This conclusion is so absurd that we must give up the explanation which leads to it. Now, we must the more give it up if we take the other side of the question. As we find that there is no part of the brain which, in some cases, has not been destroyed without any facial paralysis at all, we would have to conclude, from the second series of facts, that the facial nerve does not

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send fibres anywhere in the brain; that it has no centre anywhere in the brain. The second conclusion, which is just as well founded as the first, is in direct contradiction to it, and each one of the two conclusions would destroy the other. I will not insist more on this point.

I will pass now to what relates to paralysis of the tongue in brain-disease. As you well know, it is not rare indeed that the whole of the tongue is paralysed in its muscles, when there is a disease in the left side of the brain. It is paralysed, not because the patient does not speak, as most aphasiacs can move their tongue pretty well, and can utter sounds very distinctly; but without aphasia or with it, that is, whether it exists or not, in cases of disease of the left side of the brain, it is not very rare at all. I have myself seen what I mention more than ten times; and on record there are a great many cases that I have analysed, in which what I describe exists. The patients in this case cannot put out the tongue at all. They may be perfectly conscious, they may be without any aphasia, they can move their tongue enough inside to utter sounds, but they cannot put it out. That kind of paralysis which belongs not exclusively to the left side of the brain, but almost exclusively to it, implies an influence exerted by the disease in one side of the brain acting on the nuclei of the nerves of the tongue—certain parts of them, at least—those that move the tongue forward. It implies an influence exerted on those nuclei on the two sides of the base of the brain. Thus it is one of those facts, of which I spoke in the last lecture, in which a disease in one side of the brain produces paralysis on the two sides of the body.

Now, as time presses, I will only mention one fact more about the hypoglossal nerve. If that nerve were affected in the way it is believed to be in cases of brain-disease; if, in other words, the fibres of that nerve went up to the brain, to some centre either in the corpus striatum or in any other part that you like, having no place of passage except through the pons Varolii, it is extremely remarkable that there are on record a good many cases of disease occupying the whole of one-half of the pons Varolii without having produced a paralysis of the tongue, without having produced any change either in speech or in the faculty of moving the tongue while eating, or in the power of putting the tongue straight out.

I will not insist on other kinds of local paralysis; but I will say, in a general way, that they are on record; and I will publish cases, showing that every muscle in our system may be thus affected by groups generally, but individually sometimes—the muscles of the trunk, the neck, the pharynx, the abdomen. There is a case of paralysis of the right anterior muscle of the abdomen; and as regards every muscle, in fact, which you could name, there is a case to show that that muscle was paralysed when the brain was diseased, the paralysis existing alone in some few cases, but generally with paralysis more extensive, attacking the limbs generally, and other parts also.

After having dwelt so far to-day, and in the last lecture, on the appearance of any kind, any form, and any degree of intensity of paralysis anywhere; and having shown, as I hope I have done, that those various kinds and forms and degrees of paralysis can appear, wherever is the seat of the disease in the brain, I have to come to the question which is just the one to complete the demonstration that I have in view—that is, cases in which disease has existed in the brain without producing paralysis. I have here an immense number of very short notes of cases, amounting to not far from a thousand, in which disease, and generally considerable disease, existed without producing a marked paralysis, or paralysis at all anywhere. Those cases, strange as it may seem to many of you, are chiefly cases of disease attacking those parts of the brain which are considered as being the channels between the organ of will and the muscles. Most of these cases of disease are cases of disease in the pons Varolii, in the medulla oblongata, in the crura cerebri, in the internal capsule, and the great ganglia round the internal capsule, that is, the optic thalami, both the internal and outside corpora striata; so that it is more common by far to find a case of disease having destroyed tissue in those parts which are supposed to be the path of the will towards the muscles. There are by far more cases showing that disease has existed in those parts, without producing paralysis, than cases of disease in other parts of the brain considered, on the contrary, as having little or very little power, either as conductors or as a centre for the action of the will on muscles. One cause for that is, perhaps, that the frequency of disease on that track is by far greater; still the proportion of those cases of absence of paralysis in cases of disease on the track itself, seems to be greater than the proportion of cases of non-appearance of paralysis in cases of disease in other parts of the brain than that track. This is certainly quite contrary to what we might be inclined to believe. It is bad enough for the old view that there is a single clear case of destruction, for instance, of one-half of the pons Varolii, without any marked paralysis, or any paralysis at all; it is bad enough that there is but one of these cases,

without having, as I have said, an immense number of such instances.

I will now pass to a demonstration which, I think, will complete what I have said against admitting that paralysis appears from the destruction of the conductors of the will to the muscles, or from the destruction of centres that act on muscles. There is here a drawing which will serve for the demonstration. You see that there are fibres upon the pons Varolii and the medulla oblongata. You well know that some physicians admit that the decussation of the conductors of the will to muscles take place, not only in the pyramids, but also in the pons Varolii. Admit that it is proved, and let a disease exist in the pons Varolii alone; that disease in the pons Varolii, admitting the decussation that takes place under it, should produce a complete paralysis in all cases on the opposite side of the body, and should produce also an incomplete paralysis on the same side. It would attack, at the same time, fibres that are decussating; but, supposing the disease to be in the left side of the pons Varolii, it would attack fibres that are coming down there, and pass into the opposite side of the body; and it would attack fibres that make their decussation below. It would produce, therefore, a double hemiplegia in all cases, necessarily, as it would strike fibres that are decussating there, and it would attack fibres that decussate below that place. There would be necessarily, then, if the view were right, a paralysis on both sides of the body, greater on the one side or the other, according to the number of fibres, be they greater or smaller. Should the disease exist in one-half of the medulla oblongata, there again we would find, if the disease exist just below the pons Varolii, that it strikes some of the fibres that decussate and some of the fibres that do not decussate. There are some that have made their decussation above that would escape. Thus the disease in the pons Varolii and the lower part of the medulla oblongata would produce a paralysis on the same side, and also paralysis on the opposite side. There would be the incomplete paralysis in all these cases. What do we see? That the paralysis is sometimes complete, whether it exists in the pons or in the medulla oblongata, and that on the opposite side or on the corresponding side; but it is extremely rare, though there are such cases, that it will produce an incomplete paralysis on the two sides of the body. I may say, if the disease be extensive and occupy nearly the whole of the pons Varolii, it produces an almost complete paralysis on the opposite side, but very little, if any, on the corresponding side. There are other cases, and I have already given details about some of them, in which a disease there has produced a paralysis only on one side. There is one most important case, published by Mr. Stanley, in which paralysis existed on one side of the body, both in the face and in the limbs. The paralysis in the face was very well explained by the fact that the nerves of the face were destroyed, and it was very natural that there should be a paralysis on that side; but, as regards the body, there was no appearance of paralysis on the opposite side, and there was an almost complete paralysis on the corresponding side. Hence, facts are entirely in disagreement with the view that there is a double decussation, one in the pons Varolii and the other in the medulla oblongata.

Now, suppose you try any other way of explaining the phenomena. Suppose you admit, as I did, and I may say I published a paper to prove it—I confess it very willingly, indeed, and I think it is only due to truth that I should—I published that paper to establish that the decussation of voluntary motor conductors takes place only at the lower part of the medulla oblongata, and not at all in the pons Varolii. Facts, that were then apparently quite decisive, showed that disease, beginning from the crura cerebri, and going down to the lower part of the medulla oblongata, occupying almost the entire lateral path of those parts, caused paralysis, no matter where the disease, was on the opposite side, and only on the opposite side. Since then, I have come to know that there are other cases, and that we must give up that view; and among the facts there are especially a number of cases, like the one that I mentioned the other day, of disease occupying the two anterior pyramids without any paralysis, the patient walking in the wards, and that a few days before death; and the disease in the anterior pyramid was one of slow growth, so that it had not been produced in a few days. There are also a great many other cases. There was a case that I translated and published in an American journal, by a Professor of Utrecht, in which the whole half of the medulla oblongata was diseased, the disease affecting at the same time the anterior pyramid on one side, and also the anterior pyramid of the other side as it extended into the spinal cord, so that the fibres on the two sides were diseased. In that case there was paralysis, it is true; but it was on the wrong side, according to the theory—it was on the side of the disease, and not on the opposite side. I myself maintained for a long time, and I stated in my lectures at the College of Physicians in 1858, that the conductors of sensation decussate all along the spinal cord; and that

disease in the medulla oblongata, if it occupies one side, strikes fibres that have made their decussation all along the cord, so that anæsthesia always exists in a case of disease of the medulla oblongata on the opposite side, and only on the opposite side; and it is perhaps known by a good many among you, that some medical men have called a certain kind of spinal hemiplegia by my name, owing to the fact that, by help of experiments on animals, and by help of a good many clinical cases of disease of the spinal cord, I had, as I thought, established that a disease on one side of the cord produces complete paralysis, if the disease be considerable, on the same side in the limb, and complete anæsthesia on the opposite side. The case of which I have spoken was certainly a death-blow to the conclusion that I had drawn from the facts I had observed. I must say I have found a great many other cases which, if I have a chance of lecturing elsewhere on anæsthesia, will show that, as regards the conductors of the sensitive impressions, as well as the conductors for voluntary movements, the views previously entertained must be put aside; and that anæsthesia, as well as paralysis, amaurosis, aphasia, loss of consciousness, or any other loss of function, occurring from brain-disease, comes, not from the destruction of the function of the organ of the brain diseased, but from an influence exerted by that organ on nerve-cells at a distance, endowed themselves with the power that is lost. I will try, in the last lecture, to suggest what space is occupied by these nerve-cells, the function of which is lost, and to dwell on the production of that loss of function; but I repeat that all those symptoms consisting of a loss of function appear, not from the destruction of the organ we find diseased in the brain or the part of the brain we find diseased, but from an influence exerted at a distance. For instance, with regard to amaurosis, the facts are very striking. I can produce amaurosis in the right eye or in the left eye at will, and that by a very slight lesion. If I prick a medulla oblongata on the corpus restiforme, immediately the eye on the same side loses its power of sight. If, on the contrary, I injure the brain in any part of the middle lobe, and also in certain parts of the posterior lobe, without reaching the base of the organ at all, amaurosis appears immediately on the opposite side of the eye.

There are a great many other facts that I could mention, to show that, for amaurosis, as well as for anæsthesia, paralysis, aphasia, and all the symptoms consisting in the loss of a function, the production is through an irritation at the place where there is disease, acting at a distance on nerve-cells endowed with the function, so as to destroy their activity.

I now pass to some other phenomena. There is very frequently, in cases of brain-disease, an appearance of loss of motion, which is quite deceptive; and that appearance has led experimenters lately to draw conclusions from taking away certain parts of the top of the brain, which I conceive to be wrong. If we take away those convolutions of the brain, which have been considered by Dr. Ferrier and others as the centre for the movement of the arm—for instance, if we take away that centre, very frequently there is an appearance of paralysis, existing for a few days. That paralysis always disappears, and indeed it does not exist, if, instead of taking away the organ itself, you cut the brain at a distance from that organ, and take away by far more matter than if you take away the organ itself. If you take away the organ without irritating its neighbourhood, that is, without cutting nerves that have a power to produce an arrest of activity on the cells at a distance, then you find that there is either no trace of paralysis, or very much less than if you take away simply the organ, as the experimenters I have named have done. But, coming to what I meant to say, it is not the interference with voluntary action that causes this paralysis of the muscles; at least, if there be any paralysis of voluntary action, it is so much increased in appearance by deficiency of the muscular sense, that certainly the muscular sense is the part generally affected. That alteration of the muscular sense in animals, from various injuries to the brain, is by far more frequent than any other paralysis. In man, it is not so common, but it exists, and particularly in cases of disease of the convolutions in the beginning of the general paralysis of the insane. In the beginning of the inflammation of the grey matter of the brain, that leads to the phenomena of the paralysis of the insane, it is chiefly the muscular sense which is affected. The seat of the muscular sense, if I had time to speak of that, is not to be looked upon as being in the cerebellum, where many persons have placed it. Facts would seem to show that the convolutions of the brain are the centres for the muscular sense; but, as I have said already, there is an intermediate element between the injury we make and the phenomena we observe; there is something which takes place between these two extremes; there is some alteration which is the real cause of the phenomena.

I now pass to another series of facts. Paralysis may appear just as an attack of epilepsy can appear, although there is an organic disease in those parts of the brain which are considered as being either centres

or conductors for the voluntary action. I have collected a good many of these cases, in which paralysis comes and goes, although the cause is persistent—that is, the supposed cause of disease—in some parts of the brain, such as the corpora striata or the optic thalami. Lately, a friend of mine, Dr. Raymond, has published a very striking case, in which the ascending frontal convolution, the one just in front of the fissure of Rolando, was diseased. In that case, which seemed to him to be decisive in showing that that part is a centre for the movement of the arm, there was considerable alteration of the part, and the patient was attacked with paralysis for a few hours, and then the paralysis disappeared, just as convulsions may come on, or a fit of coughing or any kind of fit; and although he concluded that the fact proved the correctness of the view generally admitted, that there lies the psychomotor centre for movements of the arm, the fact itself was quite contrary to the view. It is clear that, if the destruction of that part had destroyed the organ which is the psycho-motor power of the muscles in the arm, the paralysis would have appeared from the destruction and would have remained; it would not have come and gone. Hence facts of that kind are decisive to show that paralysis, as well as other phenomena which show an irritation—that is, either convulsive movements or any other kind of proof of an irritation existing in the brain—may come and go in a very similar manner to what occurs in the production of convulsions. There is much to be said also against the views that I contradict in this, that paralysis due to an organic cause of disease in the brain may disappear suddenly; and indeed there is a very striking case which, perhaps, may prove to have more value than many of you may think, in which it was found by mere accident that pressing on the dorsum of the foot, the patient being completely hemiplegic or very nearly so on that side, produced a cessation of the paralysis so long as it was made. Legros mentions seventeen cases in which he obtained the same result; and many of you are acquainted with the peculiar form of myelitis where there is a spasmodic state of the lower limbs, the limbs being generally quite rigid, and at times having rapid movements, in which, as Legros shewed me first, by merely pressing on the big toe of the patient, everything disappears; there is an absolute relaxation; the convulsions of stiffness cease altogether, and it disappears in all cases; so that there is a cessation for a time of these attacks. In these cases there is just the same thing, after all, that we see in cases of epileptic attacks in which the patient, before losing consciousness, has his head drawn towards one shoulder; if that take place, almost invariably, if the head be pushed firmly over to the opposite shoulder, the fit is averted, and there is no attack. It is the same as we see in cases of pretended aura. The ligature, which is applied in those cases, does not act in preventing something from forming, but, on the contrary, is sending something to the brain which produces an arrest of the morbid activity of the cells. Thus there may be some chance of rescuing, for a time at least, certain hemiplegics by acting on the skin in certain parts of their body—I would not say the foot only—and of producing a temporary change in their paralysis; and if the temporary change be produced, indeed I do not know if it would be going too far to be hopeful that, perhaps, more would be obtained than a temporary arrest of the trouble. This leads me to say that, if I am right in the views that I am now trying to put forward, it will be necessary to change a great deal of our treatment in paralysis due to an organic disease in the brain. Evidently, if we are to look upon paralysis as a mere effect of an arrest of activity of certain cells, which of course does not imply an organic disease, and does not imply an alteration of structure in those parts; if that view is right, the field of treatment ought to be somewhat enlarged, if not utterly changed. Although our means of diagnosis may be less than we thought they were, we can usually come to a diagnosis when an affection is in the base of the brain. Finding where the disease is, we usually try to apply means in agreement with our knowledge of the place of the disease, and I do not think that that is the proper treatment—whether the parts which are affected, and which really are the seat of the lesion producing paralysis—whether those parts are in the spinal cord, in the base of the brain, or in the whole of the brain itself, as I believe it chiefly exists, makes no difference. It is quite certain that we have to look upon another seat of disease, or alteration of nutrition, than the one we knew; and, therefore, our means of treatment must be changed. There is no possibility, so far, of knowing exactly where are the alterations which really give rise to paralysis. I have said, in the beginning of this lecture, that I believe that the cells that serve to voluntary action are scattered through the brain; but although I have said that, it is not certain that paralysis does not appear from the influence exerted in a direct way on the cells of the anterior cornua of the spinal cord; and there are cases of disease which really show that these cells are affected by disease in the brain. We can produce a change in the cord immediately by certain injury to the brain; but there are also cases of disease in men in

which, with the descending alteration along the lateral column of the cord, there is also a disease of the cells of the anterior cornua with the cord; and there is certainly a kind of paralysis, of which I have already put together some five or six clear cases, in which the paralysis is developed very slowly indeed, progresses very slowly, and, at the same time that it progresses, phenomena appear showing that the spinal cord itself is being injured, so that the disease in the cord seems to be the real cause of the paralysis. In the next lecture I will conclude this subject.

AN ADDRESS

DELIVERED AT THE OPENING OF

THE SECTION OF OBSTETRIC MEDICINE.

*At the Annual Meeting of the British Medical Association,
in Sheffield, August 1876.*

By LOMBE ATTHILL, M.D.,

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I THINK, gentlemen, I may safely assert that the proceedings of this section of the British Medical Association, which is devoted to the consideration of the subjects comprised in the term "Obstetric Medicine", attract, on the whole, more general attention from the great body of our profession, than do those of any other section. The reason for this is sufficiently obvious, for while the busy practitioner may be wholly unable to devote time or attention to the study of the important subjects included under such heads as those of "Physiology" and "State Medicine", or find that in practice cases of operative surgery are comparatively rare, he is certain to discover that the conditions and affections brought under discussion here, are of daily occurrence amongst his patients; hence he seeks to improve his acquaintance with the nature of the conditions, and to learn the best means of successfully treating those affections, which are peculiar to women; the more so, as the study of these diseases has probably been neglected, possibly entirely overlooked by him, during his student's career.

Another reason for the interest evinced in the proceedings of this section is this, that marked and rapid progress has of late years been, and still is being, made in the department of obstetric medicine. The very name of the section proves this. A few years ago the term "obstetric medicine", if used at all, would hardly have been understood. This section of the British Medical Association was until very recently termed that of "midwifery". Consider for a moment what this change of nomenclature implies—it implies this, that the study of the process and phenomena of parturition, important though they be, is by no means all that is now required of the obstetric practitioner; that is, not of those alone who make obstetrics their special study; but of all, and their name is legion, who are called upon to treat the diseases of women.

This section then includes subjects of a most varied and extended nature; it includes midwifery proper, the diseases of the puerperal state, and those incidental to pregnancy, the considerations of disease of the vagina, bladder and uterus, of the breasts, and last and surely not least, of the ovaries. Diseases of these latter organs are doubtless, in one of their aspects, within the domain of surgery proper; but that condition which demands the performance of the capital operation of ovariectomy, is by no means the most common of those requiring treatment, and, moreover, not a few able and successful ovariectomists are to be found amongst the ranks of obstetric surgeons. Some such I have the pleasure of seeing around me here to-day.

Gentlemen, we deem the practice of midwifery to be in no way derogatory. It is our honourable function to succour woman in her hour of trial, to shorten or relieve her sufferings, often to save her life or that of her offspring; to meet with promptness and decision the numerous dangers and difficulties which frequently and unexpectedly occur during labour, and which tax to the utmost our courage and endurance and skill; but these duties, though most important, form but a small portion of those which now devolve on us daily. The affections I have already indicated as coming within the province of obstetric medicine are so numerous and of such constant occurrence, that the right treatment of them is all important, as well for the sake of the sufferer as for the reputation of the practitioner.

The truth of this is now on all sides admitted, and the study of ute-

rine disease, in its protean forms, is consequently steadily becoming more general; but unfortunately our knowledge of the pathology of these important affections is as yet imperfect, and our treatment consequently in many respects empiric and unsatisfactory. Still great strides in advance are steadily being made, and we may look forward hopefully to a time not far distant when phenomena and symptoms at present overlooked or misinterpreted will be explained, and our treatment consequently become more scientific and efficient.

To the late Sir James Simpson, without doubt, is due the credit of inaugurating an era which has been marked by great and rapid progress in the department of obstetric medicine. His master mind perceived how vast an amount of unrecognised disease, and what an extensive field for pathological investigation, existed with reference to the reproductive organs of women; before his day little was known of uterine disease, and as to treatment, it consisted of little more than in exposing the cervix uteri, and applying to its vaginal surface, if it happened to be abraded, a solution of nitrate of silver, or of some other mild caustic. Of disease of the body of the uterus almost nothing, of its interior absolutely nothing was known. A morbid, and as we now know, an unfounded dread existed of attempting to interfere with, or to investigate the condition of the cavity of the uterus. All this is now changed. We know that disease of the cervix uteri is of less frequent occurrence, and of less serious import than that of the body, and that its cavity may with impunity be trespassed on, and disease occurring within it successfully combated. Without doubt the most important practical result of the teachings of Sir James Simpson is this, that we do not now hesitate to dilate the uterus and investigate the condition of its interior, when symptoms indicative of serious mischief within the organ require us to do so.

I am well aware that by some practitioners the dilatation of the uterus is still looked on with dread, and that the attempt, if made at all, is undertaken with the greatest hesitation. I can only say that I believe these fears to be groundless, and that, if due care be taken to select suitable cases, and proper methods of carrying out the process be adopted, the treatment is a safe as well as a justifiable one. My own experience in the dilatation of the uterus has been great. I have practised it very frequently indeed during the last ten years, and as yet in no single instance has a bad symptom followed, nor have I even once been compelled to abandon the attempt. But I am far from throwing doubt on the accuracy of the statements made by others, who have recorded the occurrence of alarming symptoms, or even of death, as consequent on the attempt to dilate the cervix uteri; and I am quite prepared for the possible occurrence of such, for all are aware that cases must occur in which the most trifling exciting cause will be followed by serious symptoms, though no grounds existed beforehand for anticipating the occurrence of such. But these are exceptional, and I believe, as a rule, that when serious symptoms arise, either during the process or in consequence of dilatation of the cervix uteri, they do so either because an unsuitable subject has been selected in whom to practise the treatment, or an unwise method adopted for carrying it out. On examining the records of the cases in which serious or unpleasant symptoms followed the attempt to dilate the uterus, I find they have generally occurred when practised,

1st. Either for the relief of dysmenorrhœa depending on the existence of a narrow cervical canal;

2nd. When the cervical canal is encroached on by a fibroid of large size and unyielding structure;

3rd. When the process has been attempted to be carried out rapidly by means of metallic dilators, or,

4th. When it has been protracted over several days.

I have therefore, in order to guard as far as possible against the serious results recorded by others as following attempts to dilate the uterus, laid down for myself the following rules, which I can recommend with confidence to others.

1. Never to dilate the cervix uteri for the cure of dysmenorrhœa or sterility depending on a narrow cervical canal or conical cervix.

2. Never to dilate in cases in which a large and dense intramural fibroid presses on and partially obliterates the cervical canal.

3. Never to use metallic dilators of any kind, but to choose for the purpose either sponge- or sea-tangle-tents, which expand slowly and gradually.

4. Never to continue the process of dilatation for more than forty-eight hours. I prefer, in the few cases I have met with in which, after the lapse of that time, the cervix was not sufficiently opened to suit the purposes I had in view, to postpone all operative interference for some weeks, rather than risk the result by prolonging the dilating process.

With respect to the first of these rules, I look upon the treatment of what is termed "mechanical dysmenorrhœa" by dilatation as being