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

Delivered at the Royal College of Physicians of London.

BY

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LECTURE III.

MR. PRESIDENT AND GENTLEMEN,—I have, in the last two lectures, accumulated facts, or at least conclusions from facts, to show that the views that are held as regards the production of paralysis cannot be any longer kept. I have now to say but very few words in addition to what I have stated, before I try to establish by what mechanism, according to the views we have now, paralysis appears.

Among the few series of facts that I have not mentioned at all, there is this one, which is capital: that those affections of very slow growth, such as tumours, for instance, aneurisms, and so on, very frequently will produce a paralysis quite as suddenly as any sudden lesion can do it. Of course, if paralysis depended upon the destruction of tissue, it would take place in these cases as slowly as the growth of the tumour itself, and proceed according to the rapid degree or progress of that morbid growth. We see the reverse, also, very frequently. We find very frequently with affections that are sudden, such as hæmorrhage and softening, that, although the cause of paralysis is sudden, it may not appear at the time of the change in the brain, and of its pressure by the blood, if it be blood which is effused; and that it will come some time afterwards, and proceed slowly. Hence there is no relation, in a great many cases at least, between the rapidity of the production of the disease that causes paralysis and the rapidity or suddenness of paralysis itself. There is another fact, or series of facts, which is of value. It is, that the left side of the body is usually more completely attacked with paralysis than the right side for similar lesions, and also it is more frequently attacked; so that the frequency, and rapidity, and extent of paralysis will be greater in the left side of the body than in the right, for similar lesions in the opposite sides of the brain.

Another fact, or rather a series of facts, as I have said, of great interest, is that the upper limbs will be affected with paralysis more frequently when disease is located in front of the pons Varolii, in the cerebral lobes, either in their base or in the convolutions; while paralysis of the lower limbs is somewhat more frequent than paralysis of the upper limbs in cases of disease of what the French have called the isthmus of the encephalon, that is, the pons Varolii, the medulla oblongata, and the cerebellum. Lesions of the spinal cord, in its upper part, in the cervical region, will also give rise much more readily to paralysis of the upper limbs than paralysis of the lower limbs, the lesion existing above the origin of the nerves of the upper limbs. The spinal cord, therefore, in that respect resembles, as regards its morbid properties, the cerebrum proper, the corpora striata, and optic thalami; while lesions of the other parts of the brain, which exist between the parts of which I have spoken and that part of the cord I have mentioned—those parts of the brain, on the contrary, produce paralysis of the lower limbs. In this variety you see pretty much the same thing that I have mentioned all along; you see that, although there are perhaps generally rules, there is one great thing which dominates everything. It is that, according to the excitability of parts, the paralytic effects that will appear will vary considerably; and this really is the great point in the lectures I have delivered, that, according to the excitability of parts, effects excessively varied will occur.

There have been great efforts made, and I am sure very remarkable ones, by a physician of this city, Dr. Hughlings Jackson, to reconcile some of the facts I have mentioned with the old views which are taught in books, and which, I may say, most of us here have taught. He has tried to show, for instance, as regards the corpus striatum, which is often found diseased without any paralysis at all, that a small part of that organ is a whole corpus striatum—if I may say so—that is, each small part possesses the functions of the whole organ; so that, if a destruction take place there, only taking away three-fourths or more, but leaving some of the organ, there is yet a power persisting. This view certainly answers perfectly

well for those cases in which a part of the organ is found healthy, but it stops there. Let disease destroy one of the corpora striata, and then the supposition vanishes if paralysis do not appear; and there are such cases. Still further, there are cases in which the two corpora striata have been found diseased, without any marked paralysis; and, in one remarkable case, there was no paralysis at all. But, against the view of Dr. Jackson, there is this, which is more decisive—that, if we admit the correctness of the view for a nervous centre, that is, for a cluster of nerve-cells, we should not admit it for the crura cerebri, we should not admit it for parts where there are fibres only; and we see the same thing for parts where there are fibres that we see for the corpora striata, that is, we may find disease destroying the conductors of one-half of the base of the brain, and destroying a great part also of the conductors of the other side, without any marked paralysis. Still more, if Dr. Jackson's view were accepted on the data that he has given, there would remain this great fact against it, Why is it that paralysis will occur at all? If you carry the disease to all parts of the brain, how is it that an excessively small lesion will produce paralysis? Another supposition that he and others have made is, that there are two sets of conductors, coming from each half of the brain, so that each half of the brain would be good for all the functions of the whole brain. He supposes that there two sets of conductors for each half; and if paralysis appear sometimes in one-half of the body, sometimes in the other half, from a lesion in the base of the brain, he considers that disease has in some cases destroyed those conductors that go to the corresponding side, and, in other cases, the conductors that go to the opposite side. It is very well so far as this goes, but why is there a complete paralysis, if it be so, in any instance whatever, in cases in which there is disease limited to one-half of the brain, if the two halves of the brain have fibres that may transmit the orders of the will to muscles to each side of the body? And if paralysis appear only from the destruction of conductors, there ought not to be a paralysis in any case whatever when disease is limited to one side of the brain. Hence another supposition must be made to explain paralysis, than to admit that it proceeds from the destruction of conductors or from the destruction of a nerve-centre.

A third supposition has been put forward by Dr. Jackson, and by a good many of my friends in France, Dr. Charcot among others, viz., that there are supplementary organs in the brain. My friend Dr. Broca has been compelled, by facts, to come to that supposition for that third frontal convolution on the left side, where he has located the force of expressing ideas by speech—he has been compelled to admit that, for this function, there are other parts of the brain which are in a dormant state as regards their activity, and which may assume full activity when the third frontal convolution on the left side has been destroyed; so that aphasia, which has appeared at first from a lesion of that place, can disappear completely, from the education and development of power in the part which naturally has been endowed with that faculty, but where it had not been developed. As regards his supplementary action, of course much can be explained by its help; but there is a very great deal more that cannot be explained. It is said, for instance, that if a disease in one anterior lobe do not produce the least trace of paralysis, it is that other parts of that lobe serve to the same function, or (and this is the view that is held by a few of my English friends) the other anterior lobe serves there in place of that one which is diseased. But what becomes of those views if we find either an almost complete destruction of one-half of the brain without paralysis, or, which is more common, an affection destroying the tissue at the base of the brain, and destroying it to a considerable extent, so that in some cases the whole half of the base of the brain is gone altogether, and without any paralysis. In those cases we have to have recourse, not to supplementary action coming from the same side, but to the supplementary action coming from the opposite side. But what is the worth of that hypothesis? There are some cases in which not only the two anterior lobes, but a part of the middle lobe and the two sides, have been completely destroyed. What also of those cases in which the two middle lobes have been occupied by a tumour on each side, without any trace of paralysis? or of those cases in which there was a destruction more certain than that which could be produced by a tumour occupying the part under the ventricle?—that is, of those parts which are considered as centres for the action of the will to the muscles, the corpora striata and the optic thalami, if they serve; and, as regards the optic thalami, it is a question whether they serve or not the voluntary action. On the other hand, we can find disease in one of these parts only with complete paralysis, and we may find disease in the two sides without any paralysis. The theory, therefore, that paralysis will cease after the development of a power which existed but which had had no education, cannot hold good; and certainly is quite in opposition to what facts teach in those cases in which there is a considerable disease on the two sides, in the same parts, without any marked paralysis.

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And now, as I have not yet referred to a part of the brain, which lately many physiologists, Professor Carpenter among others, have been led to consider as an organ of intelligence, and which Meynert and others have considered as the centre for sensation—the posterior lobes—I will say about these posterior lobes that, although galvanising them in animals does not produce convulsions, disease there will produce convulsions very frequently, and will also produce paralysis; and paralysis will appear oftener in these cases on the corresponding side than on the other, that is, oftener if we compare the frequency of that kind of paralysis with its frequency when there is disease in the other lobes. I do not mean to say, absolutely speaking, that that kind of paralysis is more common when the disease is there than the other kind of paralysis. These posterior lobes, therefore, it would be very easy to show, have none of the functions that have been attributed to them; that is, they have not exclusively these functions, and they possess the power of producing paralysis and all the other symptoms of brain-disease, as well as in any other part of the brain-centres. Now, can we explain the phenomena that I have mentioned, in admitting the view which I hold, that a few fibres alone are sufficient for all the actions of the will on muscles; and can we admit also, that a few nerve-cells are sufficient to produce all the actions of the will on muscles? I will not try to prove that it is so, as it would carry me far beyond the limit of this lecture; but there is one point of view about this which ought to be mentioned. If some among you have accepted as true, from the facts I have mentioned, that a few fibres can be sufficient for the transmission of the orders to the muscles, it may be said that perhaps, a few fibres being quite sufficient, if there be disease in the base of the brain destroying the tissue, there are a few fibres escaping; and that, therefore, there is no need for admitting that each half of the brain may serve for the movement of each half of the body. The other view might be held true, that it is the right side of the brain that moves the left side of the body, and *vice versa*. This cannot be admitted for this reason, that, in the first place, it does not apply to cases in which paralysis appears as the result of an extremely small lesion; that would have to be explained by something else; but it does not also hold good, on account of this, that in some cases (not a great many unfortunately, because microscopical examinations are things which have not been made very frequently, except during the last twenty years) where there was complete destruction and absence of fibre on one side of the brain, the voluntary actions were yet as perfect as ever. More than that, these few fibres that are essential must have pretty much the same location; you cannot place them indifferently in one place or another. And disease shows that it cannot be so, as there are a great many cases in which, taking the medulla oblongata for instance alone, either the interior pyramid alone, or the lateral column of the cord alone, or the posterior column, the corpus restiforme, have been destroyed completely without paralysis; and when paralysis has appeared in these cases, it has sometimes existed on the same side. There is no possibility, therefore, of explaining the facts that I have mentioned by this supposition of only a few fibres; there must be this addition, that the two halves of the brain have the power of acting in the two sides of the body. There are individuals, evidently, in whom it does not seem to be so at all; but there are some in whom it is so. There are persons who are absolutely right-handed, in the full meaning of the word, and completely unable to make use of the left side of their body. These persons are likely to be completely paralysed in case they have paralysis of one-half of the brain, because they have not educated the two sides of the brain properly to perform the actions of the two sides of the body. If you examine the movements of a child, a year or eighteen months, or two years old, you will find that they are performed equally by the two sides of the body. If any action be performed by one arm, the other is doing it at the same time; there is, evidently, at that age an action in the two halves of the body. I cannot of course assert—although there are very good reasons for it, which I have not time to mention—that it is one-half of the brain only which is acting then; still, I repeat, there are very good reasons for supposing that it is so. But, even admitting that the two halves of the brain are acting, there is a simultaneous action of the two halves of the body. Unfortunately, we are barbarous enough to prevent such a thing from going on; and we destroy, I am afraid, a good deal of the power of one-half of the brain. I have had the opportunity of delivering a number of lectures on the importance of educating equally the two halves of the body; and the facts I brought forward clearly show that there is a great likelihood, at least, that we leave in a dormant state a great deal of the power of our brain, and that we make use, unfortunately, of only a part of the whole organ, and many of us of only one-half of the brain, for most of the actions of the body. You will say, and it is perhaps an objection present in your minds, that if it be so, admitting that the right side of the brain is the one we make use of for certain actions of the body, supposing that it is the organ that we em-

ploy for the movements of the two sides of the body, as I suppose it to be, how, then, is paralysis produced only on the left side of the body generally when there is disease in the right side of the brain? and how is it also that there is paralysis at all in the right side of the body, when there is disease in the left side of the brain? This is no objection, if everything that I have mentioned, and what I will say again in a moment, shows that paralysis appears, not from the destruction of the conductors or of centres for the will-power, but owing to the irritation which puts into play a certain activity of the part where the disease is, from surrounding parts, so that there is a transmission of that irritation to the nerve-cells at a distance, so as to stop their activity and cause paralysis. And, as I have tried to show, paralysis, owing to such a cause, may appear anywhere, never mind where you locate the cause of it in the brain. Hence there is no great objection to the view that we employ only one-half of the brain for certain actions, and the other half for the other actions.

Now, there is another possible explanation of some symptoms of brain-disease and the production of paralysis. We have all more or less been inclined, when we have seen a patient in a state of coma from a considerable effusion of blood in the brain, or from fracture with depression of the bone pressing considerably upon the brain, or from any other cause whatever producing pressure on that organ—we have all been inclined to look upon the cessation of activity of the brain in these cases, that is, of coma, as the effect of pressure. Pressure on the brain will produce a great variety of phenomena, although it is located in the same place, in a great many instances; and if it produces a great variety of phenomena, it is not by a simple cessation of the function, as the function is identical for the same place. The occurrence of a great many various phenomena implies that pressure acts by simple irritation, and not by a destruction of the function. Still more, the brain can bear a degree of pressure which is infinitely greater than many of you know, or are perhaps ready to believe. Experiments have shown that considerable pressure on a healthy brain, in dogs, will sometimes produce no effect at all, while in other cases a much smaller amount of fluid will produce symptoms which will be various in animals as well as in men. Therefore, we cannot look upon pressure as having the extent of power that is supposed. There are cases in which, in men, immense pressure has produced no symptoms at all; and there are cases, and this is very remarkable, where pressure, existing in the two halves of the brain, from effused blood, has produced nearly every symptom. And there lies another fact, namely, that paralysis and other symptoms appear from the irritation acting on other parts, and not from destruction of functions, or the direct putting in play of the special activity of the part pressed upon. If blood be effused on one side of the brain, symptoms are far more likely to appear than when pressure exists in two halves of the brain. And we have something similar to what we observe when we take away parts of the brain of animals; if we take away more on one side than on the other, these phenomena will occur; but if we take away parts of the brain on both sides equally, these symptoms do not appear until we have reached almost the end of the brain. There is an irritation on the two sides, a counter-action which prevents the manifestation of phenomena, and this we know indeed for a great many cases of disease. If we be exposed to a draught on our full face, striking us just in front, that draught will not produce a facial paralysis—it will not produce bad counter-effects; but let us be struck by a cold wind, blowing violently on one side only of the face, there is not that counter-action which comes from the opposite side, and we may be attacked with either paralysis or other morbid effects. It is very remarkable indeed to find that, in a great many cases of considerable hæmorrhage pressing on the two sides, there was no paralysis at all or very little. Pressure, therefore, is to be considered as acting, as indeed might the point of a knife, or a galvanic current, or some cause of excitation brought upon the brain, and not by the destruction of the function of the neighbouring parts, those on which it acts chiefly.

There is another fact which shows that irritation has a great deal to do with the production of paralysis, that is, what takes place in muscles. If we compare what occurs in paralysis upon a limb, especially the left arm, as it is nine times out of ten the left arm that is paralysed, where there is paralysis there, even when that paralysis is absolutely complete, the muscles remain well nourished, and there is no marked alteration of tissue. It is, then, not owing to the lack of action, to the lack of activity of muscles, that these parts become atrophied when they are paralysed. Paralysis from brain-disease will produce alterations in the muscles, sometimes exceedingly fast. There are cases, and I have seen two, in which paralysis from hæmorrhage in the brain was accompanied by considerable atrophy within a fortnight, so that it is clear that an irritation takes place in the brain, or centre, which is propagated so far as the muscles themselves, producing a change in their

nutrition. There are already a great many facts showing that disease in the brain will produce a great variety of changes of nutrition, and of structure, and, of course, of vitality of functional manifestation in the spinal cord. Those changes have much to do with the production of paralysis, I have no doubt. The important point, as time presses, is my stating that, through all irritations taking place in the brain causing paralysis very quickly, we find changes occurring in every part between the brain and the parts paralysed, that is, the muscles, and including them. There is one other great argument, which is, that the same irritation which causes paralysis will often cause also convulsions, that is, an active phenomenon. You know I have stated that the view I hold of this production of paralysis is that, in all cases where it occurs from organic disease in the brain, it is owing to an influence which is similar to that which is exerted on the heart, that is, inhibition, or suspensory action, or arrest, if you wish to call it so. As inhibition cannot exist without effects of irritation, there is something of the same kind as that which causes convulsions in cases of brain-disease; and it is important to establish their origin, to find the very same cause will very frequently produce the two phenomena. The very same cause applied to two parts of the brain will cause on one side paralysis, on the other convulsions; and very frequently, as you know, convulsions begin in cases of disease of the brain, and paralysis follows; and there are a number of cases in which, after each attack of convulsions, the paralysis makes progress, so that there is a great analogy between them.

I pass now to the ultimate part of this course, which is to show, if I can, that paralysis depends, as I have said, on inhibition; but, before doing so, I must pay a compliment to a gentleman of great ability, a Fellow of this College, who has fought against me with great talent, with great energy, and who has tried long ago to show that, in certain cases of paralysis, those which are called reflex paralysis, the cause is inhibition. I have now become a convert to my antagonist's views; and I not only agree fully with him, as regards the production of paralysis in those cases by that mechanism, but I go very much further. I now consider, as I have said frequently, that every paralysis due to an organic disease in the brain is caused (as Dr. Handfield Jones believed to be the case in reflex paralysis) by inhibition. I will not dwell at length on facts which show that it is through inhibition that paralysis appears. I will say, however, that when we study cases of diseases of the brain, there are a great many facts which appear to prove that it is through inhibition that paralysis comes. Among those facts, we find this, that the very same cause which produces paralysis will very frequently cause, at the same time, purely inhibitory phenomena. We find that hæmorrhage in the brain will cause an arrest of the heart's action, more or less complete; we find that it will cause also a diminution in the power of respiration, not that kind of alteration which we know to exist in coma, and which consists rather in a trouble and a difficulty than in a diminution of respiration, but a simple weakness. We find a great many other facts of the same class—among others this peculiar phenomena, worthy the attention of surgeons, and to which I have tried, vainly I must say, to call their attention so far, that is, the peculiar kind of influence exerted on nutrition, and which shows itself by a cessation of those exchanges in many parts of the body; so that the blood will return from those parts quite red, just as it comes from the arteries, and it will also come out with a diminished temperature; so that, after a time, the part will be excessively cold. The cessation of the exchanges between tissue and blood is a phenomenon of arrest, as clearly as the cessation of the activity of the heart; and I must say there are a great many facts which show that disease of the brain, as well as a good many other purely reflex causes, may produce a cessation of the secretory power of any gland, as well as the cessation of the action of the heart. There is, therefore, a variety of clear inhibitory phenomena. Among that class of effects of disease of the brain, there is one extremely remarkable, which has been stated, as you well know, by Russian physicians: it is what takes place in the reflex activity of the spinal cord. When we act so as to stop this reflex activity, in those cases, as well as in many experiments (and that can be done even in tetanus, so that for a time at least the tetanic phenomena may disappear—whether they be due to strychnine or to disease)—that cessation of the reflex activity is clearly also a cessation through inhibitory influence. And so it is in the action of the sphincter of the bladder and the sphincter of the bowels. Those two sphincters, as you know perfectly, are paralysed sometimes in cases of disease of the brain. It is perfectly known now that a disease, occupying the spinal cord in its upper part, may not paralyse those sphincters; on the contrary, they remain so active through the action of their centre, which is in the lower part of the cord, that there is no issue possible of either urine or fæces. There is, therefore, an inhibitory influence exerted on the cells

of the spinal cord that serve to those sphincters, in some cases of disease of the brain. Deglutition is also a mere reflex action, and deglutition is frequently affected by brain-disease. There is in that case also a proof of the same thing, that is, that there may be an arrest of the activity of certain cells, caused by an affection of the brain. But the demonstration can be given by experiment in a very clear way indeed. If we prick the medulla oblongata suddenly, we see symptoms that every kind of activity of the brain is lost, not only the activity by means of which respiration is carried on, not only the activity of the heart, through a reflex action of the medulla oblongata in the cells of that organ, but also the activity of the mind of the animal, the activity of its will, as it falls down dead to every appearance, and dead permanently in most cases, and in such a peculiar way that the body will not decompose after death, for perhaps a month or so, even at a temperature of 50 or 60 in a laboratory. In fact, I had a dog that was so unwilling to become decomposed, which, being shown to the Society week after week for four weeks, annoyed the Society so much that they told me the dog would never become decomposed, and it was quite useless to bring it there. It did not decompose till two months after it had been killed by a mere prick. There is, therefore, a power in an irritation of the brain of acting on cells, not only of the brain, but of other parts—the base of the brain and the medulla oblongata, and probably the spinal cord itself, and certainly on the cells of the heart. There is a power of arresting suddenly the action of those parts.

But now the great difficulty about this view remains: How is it that paralysis will last so long sometimes, when it is caused by a very small lesion of the brain? We know by experimenting that, if we continue to galvanise the par vagum, for instance, after having produced arrest of the heart's action, if we maintain life in the animal, notwithstanding the persistence of the irritation of the par vagum, the heart will beat for a time, in most cases. And that certainly seems to me quite in opposition to the view I maintain, that paralysis may be due to an irritation producing inhibition, and that inhibition may persist in its effect for years sometimes. But the circumstances are quite different; and so many facts show that nerves are absolutely different from nerve-centres in their action, that this objection is certainly not a very good one. Now, on the other hand, we find often that, although disease is extending in the brain (and this is the case particularly when there is an inflammation in that organ), the paralysis, which at first had been produced, will diminish and gradually disappear. We find a series of facts of paralysis appearing and disappearing, by attacks just like convulsions, although the cause of it is a persistent one. Hence, I must say, I do not see that the objections to be made on that ground of persistence of paralysis, and the difficulty of accepting that inhibition can persist, will prove good. There are many cases in which the pulsations may fall to something like forty in a minute, and remain there for weeks and months, from a disease in the brain. Here is an inhibition which is considerable; and how can we tell that a degree of inhibition as great as that is not sufficient, in nerve-cells, to produce a cessation of the power of the will upon them?

There are other arguments which I cannot bring forward. At any rate, I do not consider that this part of the views I hold is fully proved; and one of the great objects I have had in view in propounding it, especially before an assembly like this, is that perhaps some one more able than I am, younger and more active and freer in mind, will be able either to prove or to disprove this view; and, if it is disproved, to establish some other view in its place.

I have now to say a few words on the theory of brain-disease generally. It seems very difficult to admit that a few cells will be sufficient, as I have said they are, for all the functions of the brain. When I say a "few," I do not know what is the number—it may be two hundred or three hundred, or more; but experiments show that a very small part of the brain in animals, and the lower you go in the scale of beings, a very minute part, is quite sufficient for all the functions of the brain. As soon as all is gone, then we fall into automatism, and there is no more will; but, so long as there is a part, there is will-power. In man, we have no fact going so far as that; there is no case in which destruction has been so great, and has allowed the persistence of will-power; but there are many cases showing a considerable destruction of brain-tissue on the two sides, with persistence more or less complete of the brain-faculty. It is clear, therefore, that a number of cells may be quite sufficient; but then, you will say, what can be the use of such an immense deal of cerebral matter, if so much can be destroyed without destroying the functions? Of course, that would not be an objection to facts; but it is something to consider, and I believe it leads to this, that the great mass of brain-tissue is employed chiefly in the production of nerve-force. I have observed carefully patients who had shown that they had some symptoms of disease of the brain, more or less extensive, and which the necropsy showed to exist after a time; and I found that those

patients, although they had no real paralysis, had not the power of continuing long in action—there was certainly a diminution of nerve-force in them. This has been noticed by the celebrated surgeon Larrey in a case in which part of the brain had been carried off in a military man, in whom the mental power had remained pretty good, but could not be employed for a very long time. There seems to be, therefore, a clear use of a great part of the nerve-tissue in the brain in producing nerve-force. There may be also something else; and I will conclude by this, regretting that I have no time to develop the subject. I have had, in several instances, to deliver lectures on what Professor Carpenter has called unconscious cerebration. I have tried to show that there is a power of the nervous system, whatever it may be, which is of a mental order; that is, it has psychological attributes, psychological faculties, mental faculties as our ordinary mind; but it is by far, infinitely I should say, superior to our ordinary mental power. It is that other power which makes discoveries, and not our common power. The discoveries come like a flash to us, just as a tune or a piece of poetry will come to a composer or a poet. There are experiments which certainly show that that mental power makes use of our senses much better than we can—appreciates distances much better than our mind does. There is, therefore, a power besides our own mental power which must be located somewhere. It is very likely that the brain is its location. I would not say that it is located in one part more than in another; in fact, I cannot but believe that it is located in cells and diffused, as cells having special functions are, according to my view, in the brain, but certainly as functions belonging to that kind of power. It would be a most interesting point to ascertain, in men of eminence who have the misfortune to be attacked with disease of the brain, if they lose that power of discovery, that power of policy, that power of composing, that in health they possessed. If so, it would show that a good part of the brain possesses that power through the few fibres of which I have spoken, as being essential for communication between the brain and the body. I cannot conceive that anything else but something like telegraphic messages are sent; and that, of course, implies that in the spinal cord there are other powers of a mental order also, quite inferior of course, and having no spontaneity, but able to receive, to understand a message, and to execute what it states.

I am very sorry to have indulged in these few hypotheses, as I have no time to dwell at length upon them; and it remains for me to thank the President for his kindness in permitting me to deliver this course of lectures, and to thank the gentlemen who have been so kind as to honour me with their presence.

OVARIAN DROPSY: SOME POINTS IN ITS PATHOLOGY AND TREATMENT.

By PROTHIEROE SMITH, M.D.,
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IN bringing the subject of ovarian disease and ovariectomy before the Association, I am not so much actuated by the desire to express my own opinion as to invite discussion, in order to determine what is the most eligible mode of dealing with a malady which has become of frequent occurrence, and which, without the aid of operative assistance, almost invariably proves fatal in its issue. To accomplish my purpose satisfactorily, I shall divide the matter of which I am about to treat into three parts or inquiries; and the replies to these inquiries will, I hope, elicit such remarks from others as shall greatly enrich, if not exhaust, our subject.

The three heads or questions alluded to are:

1. Can any reliable prophylactic means be devised to lessen the frequency of ovarian disease by obviating *in limine* its cause?
2. What is the best manner of preparing patients for ovariectomy, and of performing the operation with a view to success?
3. What treatment after the operation is best calculated to ensure the recovery of the patient?

1. Can any reliable prophylactic means be devised to lessen the frequency of ovarian disease by obviating *in limine* its cause?

During a period of thirteen years, commencing in 1836, whilst engaged there as assistant obstetric physician with the late Dr. Rigby, the result of my observation of the out-patients of St. Bartholomew's Hospital in comparison with those seen subsequently at the Hospital for Women, leads me to the impression that ovarian dropsy has, during the last thirty years, increased in frequency. If such be the case, it would suggest the importance of inquiry into the possible causes of this increase. First, assuming that the ovaries usually become diseased in consequence of a morbid procreative effort—of which the subjects of it

may be and generally are entirely ignorant—we should naturally expect that these maladies would be most frequent during that period of life which is allotted to menstruation and utero-gestation; and, as a rule, such is the case. As I have before elsewhere observed, there are certain developmental stages in our existence when organs receive additional force to perfect that which was incomplete at first—as in dentition, at the termination of the first seven years; and, in puberty, at the end of the second seven years, etc.

From this time of life, in accordance with the unerring wisdom of an all-wise Creator, there is imparted to woman an additional nutritive power for the purposes of procreation. This is shown by a state of hyperæmia; and, if this be not employed according to the divine law, "increase and multiply", or by the counteracting vital expenditure at menstruation, she, at times, realises the curse pronounced against her at the fall, viz., "I will greatly multiply thy sorrow and thy conception". Whilst, however, the laws of God and man necessarily limit marriage life, it follows that this restriction, when menstruation is faulty, may lead to uterine and ovarian engorgement, and so explain the origin at times of ovarian and uterine disease.

This predisposition to disease may be recognised by the flushed countenance, with dysmenorrhœa, and other disorders of menstruation, and a full bursting sensation, with hysterical affections, mental disturbances, contracted and incompressible pulse, and sound, as the twang of a tense string, accompanying the heart's action, and a heightened temperature. Such conditions I often saw, in my early professional career, treated by venesection; a remedy which appeared almost invariably to relieve the patient at once of her distress. At this time, forty years ago, when I was resident in Devonshire, ovarian disease was there very rarely seen; but since bleeding has gone out of fashion, whether *post hoc* or *propter hoc*, the malady has apparently become more frequent. With a view to confirm the idea that the frequency of ovarian and uterine disease might, during the procreative period of life, to a certain extent depend on undue or excessive nutrition, insufficient menstruation or sterility, and the general neglect of bleeding as a remedy, I have in vain sought for statistics at a time—half a century ago—when the lancet was more commonly employed, in order to compare the relative frequency of ovarian disease then with what now obtains. Failing this, I sought for information in a country in which bleeding is still often employed; and the result of such investigation has been to confirm me in the belief that, by judicious dietetic restrictions and cautious use of the lancet, many cases of ovarian disease might be at times prevented, by removing that condition of hyperæmia which, if not relieved naturally by menstruation or utero-gestation, tends to mischief in those organs for whose special use it had been supplied.

I should not find time now to give in full my correspondence with Dr. Saucher, Marquis de Foca, of Madrid; but, in reply to my inquiry, whether he could supply me with any facts, statistical or otherwise, to show whether, as venesection is still commonly practised in Spain, and had been virtually abolished in England during the last half century, the instances of ovarian disease were less numerous in Spain than with us, he states: "Venesection is frequently employed in Spain, especially in hot climates and in the south provinces"; and again, after speaking of the predisposing causes of ovarian disease, and generally tracing them to hyperæmia, hypertrophia, or hypersthenia, he says, "ovarian cysts are very rare among us comparatively with England".

Although I admit that the opinion herein expressed offers insufficient grounds to allow any definite conclusion, still I hope I may have advanced enough to warrant the anticipation that others, with more time and ability, may be led by my remarks to obtain more conclusive evidence as to the cause of the frequent occurrence of ovarian disease. I pass on now to my second inquiry.

2. What is the best manner of preparing patients for ovariectomy, and of performing the operation with a view to success?

It is desirable, when not contraindicated by extreme weakness, that, for some time previous to the operation, the patient should abstain from animal food and stimulants, and be kept quiet; and, if possible, in bed, attention being paid to the state of the bowels, the character of the urine, and the temperature of the body. Should slight attacks of circumscribed peritonitis occur during the development of the cyst, it may be enough only to apply leeches and hot poultices to the abdomen, and to prescribe a light diet without stimulants. As a rule, advanced cystic and other diseases of the ovary, requiring operative treatment, are accompanied more or less with inflammation, partial or general, of the peritoneum, proper to the cyst itself, or covering adjacent organs. The pathognomonic signs of this condition are often indistinct; but where, in addition to abdominal pain, there exist crimson lips, and a hard though small and very incompressible pulse, so that no pressure above by an assistant obliterates the radial pulsation, venesection will