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

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BY

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LECTURE I.

MR. PRESIDENT AND GENTLEMEN,—The views I am about to put before you in this series of lectures are views which I should propound very timidly were it not for the fact that, although new, they are sustained by a number of facts. Although I am very reluctant to admit anything new, I am forced, I may say, to consider that the views that I now propound are more than demonstrated. When I say so, I do not mean that every one of the views that will be mentioned here is fully demonstrated, but certainly a good many of these views are already amply demonstrated, while the others are sustained by the facts; and I do not know of any facts that seem to be in opposition to them.

But there is another part in the views of which I will have to speak—that which relates to what is admitted generally, and of which, of course, I ought to speak first; and before establishing any new doctrine, it is essential to put down those views, if they must be put down.

As regards the demonstration that those old views must be put aside, there cannot be the least doubt that the facts in contradiction to them are so plentiful and decisive, they are so common, they occur so constantly in our practice, that I wonder, I must say, that the views which they show to be so false, to be so untrue, to be so highly unacceptable, I wonder that those views still, if not accepted, at least are taken because nothing better exists.

There are two points especially as regards the production of paralysis which are absurdly inconsistent with facts. The one which dominates through all historical paralysis is, that it depends on the cessation of the power of the part to work where the disease exists. If a disease exist in what is now considered as a psycho-motor centre, the disease is looked upon as destroying the function of the part where it exists. If the disease exist in the conductors, establishing a communication between those psycho-motor centres and the muscles, then it is considered that the function of those conductors is destroyed, and that paralysis arises from that destruction. In the two cases, it is the same doctrine that prevails. In the two cases, there is the loss of function of the part where disease is found. This is the first great point among those which are admitted which is absolutely in opposition to facts.

This course of lectures that I have now the honour of commencing will show amply how facts are constantly in contradiction to such a view. I am now talking generally of the subject, saying that it is impossible to explain in this way a paralysis such as that which we see so constantly, almost every day, among you; a paralysis in which there is a slight diminution of the power of the leg, a great diminution with complete loss of power of the arm, of slight diminution of the power of the muscles of the face, and a slight diminution of the power of motion of the tongue. These four points are the characteristics of the common form of cerebral hemiplegia. We find in that common form pretty much the same features. There may be other symptoms added to them: these are the essential characteristics. Now, if such a paralysis issue from the destruction of conductors, or from the loss of the action of a nerve-centre, how can it be that this very same identical form of hemiplegia will be found in every man who has the disease? We cannot admit that every small part of the brain is a whole centre in itself; we cannot admit that every part of the brain contains also all the conductors conducting the muscles of the arms, the leg, the tongue, the face: there must be certainly some other explanation of paralysis than that. Any how, I will revert in the course of these lectures to this point.

The second point is, that when paralysis does not come from a disease in the brain, either the conductors serving for the transmission of the will-power of the muscles, or one of the centres which has been lately discovered, and some of the older ones—which, I must

say, stand their ground better than the new ones—one of those parts has not been attacked by disease, and that the disease is in other parts of the brain, which are considered as not endowed with special functions as regards voluntary action. Well, this view also does not stand criticism, as we find; and we shall have plenty of facts to demonstrate it in the course of these lectures. We find that a disease in the very heart—if I may call it so—of those parts which serve for the transmission of the order or the will to the muscles, or in those very centres, old and new, is there destroying the part; not a disease simply like that which comes from a pressure or a tumour, however large—because nervous tissue may certainly be compressed without losing its function—but in diseases where there has been actual destruction of tissue, whether it has been taken away by any cause, by the knife, by a ball or bullet, or some other cause purely organic, there are many cases which clearly prove that from the medulla oblongata up to the psycho-motor centres, any part can be destroyed without any loss of function; so that, on the one hand, we find that paralysis can appear wherever the disease is located in the brain, whether that part is a part considered as being a centre for voluntary action, or a conductor starting from the centre going into the muscles; and, on the other hand, we find that when disease is located in those parts which are considered as absolutely essential to the transmission of the order of the will, or correction of the order, those parts can be destroyed without any paralysis—at least without any marked paralysis, if we take the great mass of cases, and certainly without any paralysis, if we take only this case, which shows how few are absolutely decided.

This criticism will be carried out during the lectures I am now beginning; and I think that the amount of proof, if I am not absolutely mistaken, will convince—I hope, at any rate, some among you, especially the younger members of the profession, who will be able to work out the subject, and carry it to a fuller demonstration than I can at my advanced period of life.

There are admitted points which are so well known that I do not think it is at all necessary to speak of them. I will merely mention, with some details, what relates to new facts that have been put forward lately since the great advance of M. Charcot in his diagnosis, at least in locating paralysis in certain parts of the brain.

I may say that although I am entirely opposed to the conclusions that we have to draw from the facts which we observe so frequently as regards the function of paralysis, and although I will now establish, or try to establish, new views with regard to the mode of origin of paralysis, I must say that the diagnostic value of a great many facts remains, notwithstanding the change of our conclusions. When a paralysis is absolutely complete without symptoms indicating considerable hæmorrhage, and striking only motor power without affecting the senses or sensibility, M. Charcot was of opinion that the internal capsule in that part which does not pass through the great ganglia in the base of the brain was attacked. As regards diagnosis, I believe this is perfectly true in many cases; but it has not, however, the value that my friend thought it had, and that other people had thought also. It does not prove that the channel for the transmission of the order of the will to the muscles is there, and it does not prove also that these fibres are essential in any way to the transmission of the order of the will to the muscles. On the one hand, as I shall show by-and-by, there may be a destruction of that part without a diminution of the transmission of the order of the will to the muscles; and, on the other hand, there may be, and there are very often, indeed, cases of complete and lasting paralysis that will not diminish at all so long as the man lives, and there are many such cases in which that internal capsule is in the least degree affected.

The view that the transmission of the order of the will takes place chiefly through those fibres of the internal capsule is founded on facts which are not many, but which, I think, show that, when that part is diseased, there is generally a complete paralysis on the opposite side. This is grounded chiefly on the fact that, in cases of descending degeneration from the brain down to the base of that organ, those fibres of the internal capsule are diseased. Suppose, for instance, there is an alteration of either of the two convolutions, which are in front and behind the fissure of Rolando in those cases, as M. Charcot was one of the first to ascertain, there is a degeneration of nerve-fibres which takes place not only in the brain itself through the internal capsule, but through the crura cerebri and the medulla oblongata, and then passes into the spinal cord into the opposite side. It has been considered that that degeneration shows the line of communication between the centre of the will-power and the muscles, and, indeed, that the line of communication can be carried further. It can be carried down the nerves as the nerves themselves become diseased. Sometimes the motor nerves in the paralysed side and the muscles themselves become diseased, and diseased very rapidly in some of those cases, so that all along from the upper parts of the brain down to the muscles there is a

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continuous alteration, attacking the periphery and making considerable alteration everywhere. But, is that the channel through which the will-power acts on the muscle? Certainly not. If we admit for a moment that such is the channel, we are led to conclusions which are altogether in opposition to facts. We find among other things—not to dwell too long on this subject—that the part which becomes altered in the spinal cord is the posterior part of the lateral column. The posterior part of the lateral column of the spinal cord certainly is not the channel through which the will acts on the muscles. That posterior part of the lateral column, as I have found and demonstrated by vivisections, and also by a great many facts given by my friend M. Charcot himself, is endowed with the power, when irritated, of producing either permanently rigid muscles or convulsions.

I will not insist on the insufficiency of the proofs drawn from the history of that degeneration going on from the brain towards the muscles, as time presses. The most important theory, of course, is drawn from facts showing that disease in certain parts of the base of the brain which are considered as the channel of the will to the muscles most frequently produces paralysis. There is no doubt as regards the frequency of the coming on of paralysis when there is disease in the anterior part of the medulla oblongata, when there is disease in the crura cerebri, or even in the pons Varolii; but, on the other hand, as I will show in the next lecture, by facts which are absolutely certain owing to various circumstances, there are cases, and clear decisive ones, showing that those parts had been destroyed without any marked paralysis or any paralysis at all.

As regards the anterior pyramids especially, my friend M. Vulpian has shown that in two cases they were next to gone, that is, that there was hardly a fibre left in them that was not destroyed, not simply atrophied, but almost entirely destroyed as regards a good part of them. There was no trace of paralysis in those cases. One of the patients walked through the ward of the hospital the day before death; and there had been nothing found to show any diminution of power. But there are a good many other cases of which I shall have to speak by-and-bye. I now, in this general view of the subject, will only mention that there cannot be a doubt that those pretended necessary charges between the brain and the muscles are not so essential as is supposed. What, then, is the meaning of destruction in paralysis, if it is not in cases where there is a destruction of tissue in parts which are considered essential for the action of the will on the muscles? What, then, is paralysis of motor action? This is the point which, as regards the new view I have to bring forward, is the most delicate and the most difficult to establish. But, to begin about these views, I would state at once what is a necessary conclusion from fact—it is not theory—but simply what the facts clearly show—it is, in fact, the mind of facts, if I may say so—their spirit. There is no doubt that very few fibres, very few channels of communication, are sufficient to perform almost all the movements that our will may require—very few conductors. I repeat that this conclusion in this sense comes from the facts; it is no view of mine, it is the speech of facts; they show clearly that a small number of fibres may be sufficient to establish a communication between the will-centres and the muscles. The second conclusion is that one-half of the brain, including its base, is quite sufficient for the communication between the will-power and muscles on the two sides of the body. Now, if we admit those two facts—those two views if you like—we have a question which certainly comes out immediately, and is, I have no doubt, present in your minds. It is: Why, then, does paralysis so often appear, even when there is such a slight disease—as in many cases—a slight alteration in those parts which are considered as the channels of the will to the muscles? There lies the part where theory must come in. In the third of these lectures I shall have to dwell at length on that point.

I will now say simply that, when disease exists anywhere in the nervous centres, it is exactly and absolutely to every purpose the very same thing as if it existed in the mucous membrane, as if it existed on the top of any nerve or the periphery of any nerve in our own body. It produces there an irritation, as in every part which is excitable, if there be disease—and especially disease of certain kinds—in it or around it. Any part excitable is necessarily excited if there be disease near it or in it. If there be irritation, then there is a conveyance of the irritation through all the possible channels existing in the neighbourhood. If those channels be multiple, as is the case with the brain, then a greater chance of the coming on of paralysis exists than when the disease is existing in the bowels, or when the disease is simply an irritation of the mucous membrane, or of the nerves of the lungs, the nerves of the kidneys, the nerves of the liver, and so on. It may be that this is not the only reason why disease in the brain will have more power than disease elsewhere in producing paralysis; but that is certainly one of the great reasons of the difference between the brain itself and the periphery. There are a great many facts, experimental and

clinical, which establish that the brain is exceedingly excitable, and by far more so than the peripheric parts of the nervous system. This, indeed, is quite clearly established now; and the reading of clinical facts without any preconceived view, simply to ascertain whether the brain is excitable, would show it very clearly and very decisively, in demonstrating that the greatest variety of effects will occur from a disease located in a very small part of the brain, never mind where the part is located, as all parts of the brain seem to be considerably excited. But the experimental facts are more decisive about that: the experiments made by a young friend of mine, Dr. Rochefontaine, in showing what a variety of effects will come from the galvanisation of a limited part of the brain; not merely those movements which are of use for the irritation of the very same part, but also an immense variety of other phenomena dealing with the action of the heart, relating to the action of the breathing, relating to the action of the motor nerves, and the action of the spleen, the action of the liver, the action of the kidneys, the action of the muscular organs in the abdomen. Another experiment, which was not so cruel as it might seem at first, because it was made under the influence of ether, was cauterisation of the brain; which, indeed, had the animal been perfectly sensitive, would have caused no trouble as regards feeling, because the brain is certainly not sensitive, although it is semi-excited. Excitable of course does not mean sensible; it means that a reaction at any part will take place; it means that there is power of transmission to other parts; but it does not show at all that the part is sensitive. The brain, then, not being sensitive, I might have dispensed with the ether. However, the facts were ascertained under the influence of ether, and were perfectly characteristic of the great variety of facts ascertained from irritation. In some cases, a paralysis was produced of the face on one side; in other cases, there was paralysis of the two lower limbs from irritation of one side of the brain; in other cases, and that more frequently when the irritation was large, there was more paralysis on the opposite side, and considerable irritation as regards the extent of surface. But then, once more, there was a change produced in the spinal cord so great that I found the phenomena which we know to exist entirely different. There is, therefore, a considerable difference in a case of section of the spinal cord when the brain was burnt and in the case where it was not burnt. We therefore find that an injury to the brain, whether in vivisection or from irritation much greater than a burn, will produce a change, and a very considerable one, in the spinal cord. I will add, and that will be the object in a great measure of my third lecture, that I cannot but consider—and there are very good reasons indeed for the view, although it is not clearly proved—that paralysis appears anywhere from a disease in the brain, in the medulla oblongata, the cerebellum, or any other part wherever disease appears, on account of the restraining and arresting of the suspensory action exerted on the muscles and nerve-cells at a distance from the part where the disease is. Supposing, for instance, that the cells in the spinal cord are affected at once by disease in the brain, as in those experiments of mine—supposing an action takes place similar to that which we know to take place on the nerve-cells—then we have, I must say, too easy an explanation of the phenomena of paralysis. In fact, admitting that view, there is no difficulty whatever, as we know irritation in our system can produce effects in our system sometimes in one place and sometimes in another, according to the disease in various parts of the system. The fault of that view is, that it may lead to satisfaction, and thereby to the prevention of the progress of science. I myself dread to be carried away by the easy facility with which everything can be explained.

Another demonstration that I wish to make is this, that every part of the brain can give rise to paralysis; sometimes to one form of paralysis, and sometimes to another. I have notes of a vast number of cases; I cannot read them or mention them at length, but I will give you the purport of them. In the first place, I spoke of a typical form of paralysis, which we find frequently. I said that it may come from any part of the brain; there is no question as regards that. I will now say that hemiplegia proper, when it is complete, never exists alone, and that certainly is quite in conjunction with what is admitted as regards the use of the two halves of the brain. There is not a single case that has been put into opposition to this view; complete hemiplegia of one side is always accompanied by a certain degree of paralysis on the other side. If the old view were correct, there would be complete paralysis sometimes on one side, and not at all on the other. I will not say more on that point. There are a great many cases—indeed, twenty of these on paper—which show that a disease existing in the two halves of the brain occupying the whole of the middle line, and the same parts of the two sides of it, will produce hemiplegia only on one side. If we admit the old view, of course such a fact is impossible; if we admit, on the contrary, that paralysis depends upon irri-

tation, and that the excitability of the parts is not the same in the two sides of the brain as in the rest of the body, then it is very strange that hemiplegia will appear on the one side or the other, according to the mere excitability of the parts. Next to this, there is a fact that I have dwelt upon in a long lecture which I published in the beginning of the year, that a disease located on one side of the brain can produce paralysis on the same side. It may be in the medulla oblongata itself, or in the cerebellum, or in the pons Varolii, or in any other part of the brain. Well, with the old views, it is impossible to understand this unless you admit what has not yet been proved, except in one case to my knowledge, that there is no discussion of the pyramids; but if we prove that the pyramids are not the channels of the transmission of the will to the muscles, it matters not whether there is discussion on the one part or the other, so that in one case there is a paralysis in one side corresponding to the disease on the same side. It is contrary to the old views that paralysis occurs, not because of the destruction of the conductor, or will-centre, but because of the irritation existing where the disease is, acting sometimes on the one side, and sometimes on the other side, of the cells of the spinal cord and the medulla oblongata, with a sort of involuntary action, so as to stop their activity. There are a variety of facts, besides these, that I dealt with in my lecture, there are a great many cases in which there is a disease of the two sides, but paralysis in those cases appeared on the side where the greatest irritation existed. Of course, if paralysis may appear on the side where there is disease, it may appear where there is a disease on the two sides, but on the side where the greatest irritation exists. There are also cases in which hemiplegia exists on one side of the body, and then some time afterwards it appears on the other side, the first hemiplegia being cured, and nothing is found but the disease on one side of the brain. Was there some other disease that was not known? I do not know. There are many cases which support the view I maintain, which is not in accordance with the old views. Then there may be disease existing in one side of the brain, producing at one time paralysis of the arm on one side, and paralysis of the leg on the other side; or a disease which produces at the same time paralysis in one arm, and paralysis in the other. But here now comes a kind of case, and I have a good many of them, which is extremely demonstrative. In those cases there is paralysis of the body on one side, and paralysis of the face on the other side. These cases are very well known, and very well explained, perhaps, when the disease is in the pons Varolii, through the disease striking at the same time the roots of the facial nerves, or the trigeminal nerve on one side where it exists, producing paralysis of these nerves. This is perfectly plain; and of course when I have put forward the views I entertain as regards paralysis, I deal not only with a paralysis not depending upon the existence of the nerve, but its entrance into the base of the brain, as in those cases it is a disease of the nerve that exists and not a disease of the nerve-centre. In these cases, the disease may sometimes be in the cerebellum, sometimes in the ganglia at the base of the brain, sometimes in the posterior lobe, sometimes in the anterior lobe, and so on, so that the greatest variety of seat has been observed in those cases in which there was this paralysis. And what do we see in these cases? the paralysis on the same side as the disease in some of these cases, and paralysis in the opposite side of the body in some of these cases also. There are two cases also in which paralysis appears on the corresponding side of the face to the disease, and the paralysis in the body appears in the corresponding side; there are but two of these cases, and they are very interesting; but all the details are missing in one of them, so that I would not say that we should attach much importance to them. There are forms of paralysis which, although depending on the brain proper, have all the characteristics of paralysis depending on the cerebellum or on the spinal cord itself. There are a number of cases in which hyperæsthesia appears in the paralysed limb, while the same degree of hyperæsthesia in a few of these cases was said to exist in the opposite side; and, in those cases where hyperæsthesia existed on the side of the paralysis in the brain-disease, physicians have not paid attention to the sensibility of the supposed healthy limbs. But in two cases that I have seen, and in many cases published by Dr. Priestley, hyperæsthesia existed on the paralysed side, and anæsthesia on the opposite. There are many cases in which, with the lesion on one side of the brain, paralysis has existed in both sides. Of course it may be said that there was another lesion which had not been found. That is quite possible; but in cases of hæmorrhage of the brain, in cases of sudden paralysis, it is very likely, if the patient die soon, we may find that there was no other lesion, and that we are to consider paralysis as having been produced on the two sides by the irritation starting on the one side. There are many cases of paralysis of the lower limbs, and a number not so large by far of paralysis of the two upper limbs, occurring from disease in one part of the brain. I used to fight against the

admission of such a thing; I could not believe it. I could not realise that it was possible that a disease located anywhere in the anterior, the middle, or the posterior lobes, not to speak of the pons Varolii, medulla oblongata, or cerebellum—that any disease located in the cerebral lobes can produce paralysis of the lower limbs without acting at all on the face and the upper limbs. I have, however, been convinced that beyond doubt there are such facts; and, in the publication of facts, which I shall make fully, you will find details which will leave no doubt on this point. There is one kind of paralysis which is very much in opposition to the old views, and that is paralysis limited almost entirely to one limb. If disease exist in any part of the brain, and produce paralysis in one arm, you know perfectly well it is chiefly the arm which is affected in these cases. It is considered, so far as certain facts go, a demonstration of the view of Soissart and others that the brain in certain parts contains the will-power of the arm, and in other parts the will-power of the leg; that the anterior part of the brain serves to move the leg, and the posterior part serves to move the arm. There are, however, a great number of facts which give the lie to this conclusion. There are cases of paralysis occurring throughout the upper and the lower limbs indifferently, with the lesion located anywhere, and not in one particular spot with reference to the paralysis in each part of the body; so that there is no doubt whatever that paralysis in the leg and paralysis in the arm may appear, no matter where the lesion may be. I find the time has gone, so that I can only say one word more; but I will return to this in the next lecture. It is that facial paralysis may be caused by disease in a limited part of the brain, without any paralysis anywhere else; such cases are extremely common, judging from the number of facts I have collected.

THE PROGRESS OF FEVER PATHOLOGY.

Extracts from an Address delivered at the Annual Meeting of the Bath and Bristol Branch.

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[RECOGNISING the advantage of making these annual addresses fulfil a sort of *compte rendu* purpose, Dr. Goodridge selected for his subject, the progress of fever-pathology. There were at least three reasons, he thought, why the subject of fever should possess especial attractions to the medical mind. 1. The frequency of its occurrence. 2. The gravity of its import, and 3. The occult nature of the process. He then continued]: "Occult in times past it most certainly was, unsurpassed in this respect by any other disorder of the nosology; occult, it is to be feared, we must confess, the febrile process, in not a few nor unimportant particulars, still is. But has not the light of modern research which has illumined so many dark corners of medical science at length penetrated the thick darkness here? Have not the improved means and methods of investigation—the introduction of the various so-called instruments of precision into clinical and pathological use—nay, the great and important department of experimental physiology, which has of late so rapidly developed, borne some fruit in this quarter? It is just to these questions that we would now address ourselves; and I think we shall find that if the results achieved are not so successful, so brilliant, as are witnessed elsewhere, it is at least not because of any neglect of the advantages referred to, nor of any lack of able and zealous labourers in the field or of solid and enduring work done, but rather because of the intricacy and recondite character of the subject to be dealt with, the exceeding difficulty and complication of the many problems to be solved, and the manifold sources of fallacy which continually start up on the right hand and on the left, while prosecuting the several branches of the inquiry.

Deriving, as we do, the word fever from *febris*, and that from *fervere*, it might be supposed that the recognition of excess of heat as the most characteristic feature of the disorder was as old as the Latin noun itself, and co-extensive with its use. Yet this would be not quite correct. For, firstly, since the days of Hippocrates until quite recent times, authorities had frequently maintained that fever was a favourable circumstance, a salutary process, and they found that their etymology and their pathology coincided, or at any rate they made them coincide, for they derived the word from *februa*, which, as we well know from the name of our second month, at one time the final one of the Roman year, means to make pure. And, secondly, Boerhaave and his followers held that the increased frequency of the pulse was the great characteristic of the febrile state. The opinions entertained by the ancients upon the subject of fever will always form a curious and interesting chapter in the history of medicine. We cannot, however,