THE GULSTONIAN LECTURES ON EPILEPSY.

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LECTURE III.

MR. PRESIDENT AND GENTLEMEN.—The aid which, during the last twenty years, the microscope has afforded in the investigation of the morbid anatomy of diseases of the nervous system has thrown little, if any, light on the pathology of idiopathic epilepsy. Neither the observations of Schröder van der Kolk on the size of the vessels of the medulla oblongata, nor the changes which have been described by Meynert in the cornu Ammonis, can, I think, be accepted as having any real significance. In the cases which I have had an opportunity of examining, the cornu Ammonis has presented no pathological change; while in two cases in which I have met with disease of this structure, the patients had never suffered from convulsion. In considering, therefore, the pathology of epilepsy, we must seek for other evidence than that afforded by the pathological anatomy of the idiopathic disease, and draw our inferences from the morbid changes in organic disease attended by convulsion, and from the facts furnished by experiment and by the clinical study of the disease.

It may, in the first place, be assumed as not needing proof that the muscular spasm which constitutes the most conspicuous feature of the attack is due to the sudden violent action of nerve-cells—i.e., to the "discharge" of grey matter; and that the sensations which the patient experiences before losing consciousness are due, directly or indirectly, to the same cause—to the commencement of the discharge. To prevent misconception, I may say that the word "discharge" is used in the sense in which it has been employed by Dr. Hughlings Jackson and others, as a short expression for the sudden liberation of nerve-force, without any theory as to the form of physical or vital force thus liberated.

Premising this, the subject of the pathology of epilepsy resolves itself, in the main, into four questions: What is the seat of the discharge which thus produces the symptoms of the fit? Is the seat of the discharge the seat of the disease? How far does such discharge explain all the symptoms of the attacks? What is the nature of the morbid change which causes the discharge? Perhaps to no one of these questions can a conclusive answer be given. Nevertheless, these are facts which indicate the direction in which an answer is to be sought, and, what is equally important, the direction in which we must not seek it too exclusively.

What is the seat of the primary discharge? What grey matter is it which thus suddenly stimulates the nerve-fibres and the muscles? It is hardly necessary to remark that the spinal cord acts only as a conductor, and that the over-acting grey matter is to be sought for above it, within the skull. Regarding the encephalic masses, we have two classes of facts. First, there exist in the medulla oblongata, adjacent to the centres which regulate the respiratory movements and the state of the vessels, structures capable of giving rise by their action to general convulsions, the "convulsive centre" of Nothnagel. On the other hand, of all regional diseases of the brain, lesions of the convolutions stand incomparably first as a cause of convulsions; and experiment also demonstrates that irritation of the cortex, in the motor region, has the same effect. The teaching of experiment, then, is that both the cortex and the medulla may originate convulsions. The teaching of pathology is, as Dr. Wilks long ago insisted, that epileptiform convulsions have, in most cases, their origin at the surface of the brain. It may be doubted, however, whether the pathological facts alone, or in conjunction with experiment, quite warrant the conclusion that epilepsy is a disease of the convolutions. Dr. Burdon Sanderson's researches make it probable that the convulsions which occur when the surface is irritated may depend on the discharge of motor centres more deeply seated, though connected with, and excitable from, the surface-regions. So far as pathology and experiment go, these deeper structures, at the level of the corpus striatum, or perhaps lower still, may be the parts which discharge in epilepsy. The fact that lesions in certain regions of the convolutions cause convulsions beginning in certain parts, as the arm or

the leg, is not incompatible with this view, since discharges beginning in the lower centres may have quite the same definite forms as when they are excited from the surface. Physiological considerations make it probable that the arrangement of represented movements in the lower centres is on the same plan as in the higher. Nor does the affection of consciousness afford valid ground for the assumption that epilepsy is a disease of the cortex, since those who believe that the discharge originates in the medulla find an explanation of the loss of consciousness by assuming an extension of the discharge to the vaso-motor centre, causing arterial spasm throughout the brain. Even if we admit for a moment that there is no proof of arterial spasm, and that the over-action of motor grey matter is the only certain element in the convulsion, such over-action may disturb the centres above as well as the centres below; and, as Dr. Robertson of Glasgow has suggested, the loss of consciousness may be the effect of an upward discharge, just as the muscular spasm is the effect of the downward discharge. Thus, neither pathology nor experiment enables us to exclude the lower encephalic centres as the seat of the primary discharge in epilepsy; and, in the light of all known pathological and experimental facts, the latest writer of a systematic account of the disease, Nothnagel, can see no reason to modify the conclusion that the primary seat of the discharge—the seat of the disease—is the convulsive centre in the medulla oblongata.

Pathology and experiment, then, fail to take us further towards a definite conclusion, but from clinical observation we may gain other important information. At this point, the study of the modes of onset of convulsions becomes of the highest value. The commencement in the limbs helps us but little, because, as I have said, movements in the lower centres are probably differentiated, if not as much, yet on the same plan as in the convolutions. But it is different with the auræ referred to the special senses, or consisting in mental processes. We have seen that, of all the fits which begin so deliberately as to allow the patients to be conscious of the onset, a special-sense warning is present in a fifth—that is, the discharge, as far as we can ascertain, commences in a special-sense centre. But these centres are certainly situated within the hemispheres, above the pons, and far above the "convulsive centre". Hence the conclusion seems inevitable that the discharge in such cases originates in the hemispheres. It is equally clear that an attack which begins with an intellectual aura, an idea, cannot commence with a discharge in the medulla oblongata. Such an attack we can only conceive as commencing in the highest of all the cerebral centres, that which constitutes, to use the phrase made current by Spencer and Jackson, the anatomical substratum of intellectual processes.

We may therefore conclude that the teaching of pathology, if inconclusive alone, is right in its indications, and that, in at least a large number of epileptic attacks, the process of the fit commences in the cerebral hemispheres, and that in many it commences in high centres. If this be true, the question arises, Are we not justified in regarding all fits as due to changes in the hemispheres? Are there any facts to indicate that the lower centres of the pons Varolii and medulla oblongata are ever the primary seat of the discharge in idiopathic epilepsy?

It is certain that convulsions may arise from these lower centres, since they may still be produced in animals after the removal of the cerebral hemispheres. Moreover, in man, convulsions are caused, in rare cases, by diseases of the pons and medulla. But the clinical facts to suggest that they ever commence in this situation in idiopathic epilepsy are, it must be confessed, scanty. Does the study of modes of onset help us in this particular? There is one group of auræ which may be regarded as supporting the view that the fits which they initiate take origin in the medulla. I refer to those which I have grouped as the pneumogastric auræ, consisting in epigastric sensations rising to the throat, choking, dyspnoea, palpitation. We know that the pneumogastric nucleus and respiratory centres are in close proximity to, and connection with, the convulsive centre, and it is readily conceivable that a commencing discharge in this situation may lead to disturbance of the respiratory and pneumogastric centres before it causes general convulsions. But on this point other facts must be taken into consideration. The phenomena of conscious sensation make it certain that the whole body is represented in the cerebral hemispheres. It is conceivable that, when a discharge originates in certain parts of the hemispheres, the central structures connected with the vagus, or representing the respiratory processes, may be the seat of the first discharge, or may, at least, be the most sensitive to the commencing disturbance, and be the channel through which the consciousness is first impressed. We must remember, as we saw in the first lecture, and as Dr. Hughlings Jackson has pointed out, that these central representatives of the vagus are the most readily disturbed in emotion, such as fear. Thus the early affection of these centres, which we have just considered to be possible in a discharge originating in the medulla, is equally possible in

a discharge originating higher up. Much weight cannot, therefore, be placed on the pneumogastric aura, as evidence that the discharge originates in the medulla. This conclusion is supported by cases which show that this aura is sometimes felt in attacks which certainly commence in the hemispheres. As was stated in the last lecture, this pneumogastric aura may be associated with special sense warnings even of high elaboration, and an instance of this is the case which I related at length.

We are probably not justified in concluding that attacks never arise in the medulla. The extremely varied character of auras, referred as they are to every part of the organism, shows that the discharge may commence in any part of a very wide region, perhaps the whole of the brain in which sensori-motor processes are represented. The cases are, however, very few which afford any clinical evidence that the discharge commences in the medulla. It is possible that, in cases of reflex convulsions, the process occurs in this situation. Almost the only example of idiopathic epilepsy which has come under my notice, in which the symptoms suggested that the discharge originated in the medulla, was a patient of my colleague Dr. Ramskill, who kindly permits me to mention the case. A lad, fourteen years of age, had been subject to epileptic attacks for two years, when he gradually lost all power in his arms and legs, and even his neck was weak. In this condition, passive movements of the trunk, slight flexion of the spine, invariably caused a severe epileptic fit, attended by loss of consciousness. Under treatment, especially ice to the neck, he steadily improved, and passive movement would no longer cause a fit, but for a long time slight attacks occurred whenever, after being still, he exerted himself. Although there was no epileptogenic zone, the attacks remind us of Brown-Séquard's epileptic guinea-pigs, in which the convulsions certainly did not originate above the pons.

The conclusion, then, is that the convulsions in idiopathic epilepsy may, as Dr. Hughlings Jackson has urged, be due to the discharge of any of the grey matter of the encephalon, which subserves sensori-motor processes. This brings us, however to the second question, Is the seat of the primary discharge the seat of the disease? If, by such discharge, all the symptoms can be accounted for, we have no need to go further, and the theory that any other pathological process underlies the phenomena must rest upon its independent evidence. To learn, therefore, whether the seat of the discharge can be regarded as the seat of the disease, we must consider, first, whether there are any symptoms for which the discharge of grey matter, which causes the aura and the convulsion, cannot account; and, secondly, whether there is direct evidence of any other pathological process.

It has been supposed that the motor discharge, especially if it occur in a low centre, will not account for the loss of consciousness. We may doubt whether this assumption is justifiable with regard to any part of the brain, even to the medulla oblongata. Any small lesion of the brain, in almost any part, will cause transient loss of consciousness. All parts of the brain are intimately connected; the whole must be sensitive to a change in any one part. The maintenance of consciousness is its highest function, the first to be interfered with by any disturbance, such as results from a discharge. If the discharge originate in some cases in the lower centres, its upward effect may, as already suggested, cause a disturbance in the highest centres with which consciousness is incompatible. For the attacks characterised by loss of consciousness only, the theory held by Dr. Hughlings Jackson, that they consist in a discharge in or affecting the highest centres only, seems an adequate explanation; and it is rendered probable by the fact that such attacks are often attended by sensations of a highly specialised character. In the case of the lower centres, it seems possible that a slight discharge in some situations may affect the centres above, and not the centres below—may impair consciousness, and not cause convulsion.

Thus it seems unnecessary, for the explanation of loss of consciousness, to assume the occurrence of any other pathological process than the discharge which causes the convulsive phenomena. As far as I am aware, the only other symptom which may be held to indicate that there is more than this discharge is the pallor of face which is sometimes conspicuous. But the peripheral vaso-motor nerves are known to be influenced, in a very ready way, by the stimulation of other nerve-structures. That pallor of face may result from cerebral action is proved by the effect of emotion, whatever may be the mechanism by which that effect is produced. An injury to the brain of a frog will cause the arteries even of the foot to contract. Hence it is readily intelligible, and even probable, that pallor of face may be an effect of the discharge which causes convulsion.

If, then, it be unnecessary, in order to explain all the symptoms of attacks of epilepsy, to go beyond the discharge of which we have certain evidence, we must ask what direct evidence there is of the occurrence of any other morbid process. According to current theory, another

element plays an important part in producing the phenomena of attacks—vaso-motor spasm. It has been held that the discharge itself, though originating in the hemispheres, is due to vaso-motor spasm affecting particular arteries, and exciting the action by causing local cerebral anæmia. This view, which regards epilepsy as simply a disease of the vaso-motor centre in the medulla, is now held by few. It is unnecessary to discuss it separately, since the evidence on which it rests will be considered in speaking of the second view, much more widely held, that, although the convulsion depends on the discharge of motor centres, the loss of consciousness is due to arterial spasm. A third theory of vaso-motor influence is that put forward by Dr. Hughlings Jackson, that the local discharge in the brain excites, at the spot, arterial contraction, and that this determines the spread of the discharge.

What, then, is the direct evidence that vaso-motor spasm in the brain causes loss of consciousness, or any of the symptoms of the attacks? The evidence may be put, if somewhat baldly, yet I think accurately, thus. Epileptic attacks are accompanied, at the onset, by pallor of face. Pallor—that is, anæmia—of face is evidence of anæmia of brain. Anæmia of brain will cause loss of consciousness and general convulsions. Therefore, loss of consciousness in epilepsy is due to anæmia of brain, the result of vaso-motor spasm; and, some would add, the convulsions themselves are due to the same cause.

The gaps between several of the steps of this reasoning will doubtless seem to many of you, as they do to me, considerable. But the first two statements are themselves of doubtful truth. I pointed out, in the last lecture, that epileptic attacks are often accompanied by no pallor of face, and this is true most conspicuously of many attacks of *petit mal*. I am convinced of this from my own observation, and I think I may quote in support of this the statement of Dr. Radcliffe, that the initial pallor is "often difficult to catch". Further, the assumption that pallor—i.e., vaso-motor spasm—of the face is necessary evidence of vaso-motor spasm in the brain seems scarcely justifiable. Surface-vessels have no necessary correspondence in their condition with those of deeper structures. We do not infer that the brain is congested whenever the face blushes, and why should we assume that the brain becomes anæmic whenever the face becomes pale? We have seen that it is conceivable, and even probable, that the discharge in the brain may cause a reflex contraction of the peripheral vessels. In cases in which the pallor of face is apparently the result of cardiac failure, it is probable that this is the effect of the commencing discharge, and not its cause. Convulsion is not part of the ordinary phenomena of cardiac syncope. On the other hand, as the discharge often impresses the consciousness first through the pneumogastric centres, it is probable that it may, in such cases, influence the periphery first by the same channel. That such cardiac failure is not the ordinary cause of loss of consciousness in epilepsy, a very small amount of clinical observation will demonstrate, since such loss of consciousness may occur without either failure of the pulse or pallor of the face.

Thus, between the fact that profound anæmia of the brain and medulla oblongata will cause both loss of consciousness and convulsion, and the theory that these are commonly so caused in epilepsy, there is a gulf over which no bridge has yet been placed. No connecting facts have been adduced which are not susceptible of other and better explanations. It is a gulf, moreover, which is widened by every addition to our knowledge of the clinical history of the disease.

One vaso-motor theory remains for discussion—that which assumes that a local discharge on the brain excites, at the spot, reflex vaso-motor spasm, and that this determines the spread of the discharge. This theory, I would venture to suggest, is scarcely warranted by known facts, or needed to explain them. It is unnecessary, because the anatomical relations of known centres already afford us the means of explaining many facts in the progress of auras, and justify us in the belief that the explanation of the others, still mysterious, will be found in the relations of the unknown centres, and especially, as stated in the last lecture, of those which subserve sensation. The theory seems scarcely warranted, because in all organs, as far as we know, functional activity causes reflex dilatation, not contraction, of their vessels; and the experiments of Ferrier have proved that this is as true in the brain as elsewhere.

One other point on the current theory of epilepsy may be briefly noticed—that which refers the final clonic spasm to a cause different from that which excites the initial tonic spasm. The clonic spasm is ascribed to the irritation of the medulla by the venous blood. It was, however, pointed out in the first lecture that there is no abrupt transition from the one form of spasm to the other. The clonic spasm is developed by interruptions in the tonic spasm; and as the fit ceases, the muscular contractions do not become slighter, but less frequent. The clonic stage certainly corresponds to the cyanosis which results from interference with respiration, but if we recognise the continuity of the tonic and clonic spasm, we can hardly attribute the two to totally dif-

ferent causes. Observation of fits suggests, I think, that the venous congestion does produce the clonic spasm, not, however, by direct excitation, but by interrupting and ultimately arresting the spasm which before existed. I think that the fixation of the chest in an epileptic fit simply does in an intense manner that which we do in a slighter manner when we arrest a hysterical convulsion by closing the mouth and nose. There is nothing inconsistent in a certain degree of anoxæmia arresting spasm, and a more profound degree, as in actual asphyxia, exciting it. Analogous difference is seen in the action of many stimuli. The view that the tonic spasm is only clonic spasm compressed, that the clonic spasm is only the tonic spasm spread out, is supported by the fact that in attacks which begin slowly, the slight initial spasm is almost always clonic, and as the discharge spreads and becomes more intense, the spasm becomes tonic. In partial epileptiform seizures, the spasm is usually clonic throughout.

The conclusion, then, is that all the phenomena of the fits of idiopathic epilepsy may be explained by the discharge of grey matter; that the hypothesis of vascular spasm is as unneeded as it is unproved; that there are no facts to warrant us in seeking the seat of the disease elsewhere than in the grey matter in which the discharge commences; that this is in most cases within the cerebral hemispheres, although possibly, in some instances, lower down in the medulla oblongata; that epilepsy is thus a disease of grey matter, and has not any uniform seat. It is a disease of tissue, not of structure.

[To be continued.]

INTRODUCTION

TO A

DISCUSSION ON INTRA-UTERINE MEDICATION.*

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TEN years ago, at the meeting of the Association at Leeds in 1869, I read a paper in this Section "On the Treatment of Chronic Uterine Catarrh". Its object was to show that, in a large class of cases of uterine disease, characterised by those general symptoms which are familiar to the gynaecologist as accompanying morbid conditions of the uterine organs, especially when there was much catarrhal discharge from the os uteri and evidence of inflammation of the interior of the cervix, it was a matter of supreme importance that our treatment should not be limited, as was too often the case, to the vaginal portion of the cervix only, but that topical applications of a suitable character should be made to the deeper seated parts of the uterus beyond the external os, in which the disease was often primarily situated, and of which the inflamed, abraded, and granular cervix was, in a large number of cases, merely the external manifestation. I then described to the Section the method of intra-uterine medication which I practised, and which I more fully detailed in a series of lectures on "Intra-uterine Medication in the treatment of Chronic Uterine Catarrh", published in the *Lancet* in 1873. I venture to mention these facts, since they show that my attention has long been directed to this important and still comparatively unstudied branch of gynaecological practice, and afford, perhaps, some explanation of the honour that has been done me by entrusting to me the opening of a discussion on "Intra-uterine Medication".

There are few subjects connected with the diseases peculiar to women of more interest and importance; and this for two reasons. First, there is no kind of treatment which, properly conducted, is of more value. This is a statement of the truth of which but little experience is required to satisfy ourselves of, since all who have successfully carried it out must know of many patients who have by this means been effectually cured of sufferings which may have lasted unrelieved for years in spite of long-continued general treatment, and which rendered life a burden and a misery. Secondly (and this is even of more consequence), like all methods of powerful efficiency, it is rather a double-edged weapon. Capable of infinite good, it may also, when injudiciously, improperly,

and unskillfully used, do infinite harm. Amongst the various cycles of fashion in uterine disease, there was a time, which many here doubtless remember, when practitioners talked of little else than ulceration of the neck of the womb, and when the speculum and the stick of caustic had a wide-spread use, happily now only a tradition. If, as I fear must be admitted, large numbers of practitioners, in the laudable desire of at least finding out something wrong, and using some remedy, applied nitrate of silver to the exterior of the cervix in hundreds of cases in which it was not needed, although they did not do any good, at least they probably did no great harm. Strong advocate though I be of intra-uterine medication, I earnestly hope it may never become a fashionable treatment, or that any will adopt it unless they have made an earnest study of its indications and mode of application. I shudder to think of the consequences to that long-suffering organ the uterus, if it ever become a routine practice to medicate its cavity indiscriminately. I would, therefore, seriously impress on my hearers the importance of not resorting to the practice we are about to discuss, unless they have fully satisfied themselves that the case is suitable for it, and until they have acquired a certain amount of manipulative skill in the treatment of uterine disease; and I can, at the same time, assure them, for such is my strong conviction from no small experience in intra-uterine medication, that, when these essentials have been seen to, there is no class of case which will give more satisfaction both to them and to their patients.

The limited time allotted to me preclude any attempt at discussing in detail either the symptoms or the pathology of the morbid states in which intra-uterine medication is indicated. To do so in any satisfactory way would involve a long dissertation which, were I even permitted to make the attempt, I dare not inflict on my hearers. To make the matter a little more clear, however, to those who have not made a special study of the subject, I must beg leave to mention, in the simplest and most elementary way, the conditions of the uterus which call for or admit of intra-uterine medication. If I appear to make dogmatic assertions on disputed points of either pathology or practice, I will beg you to believe that it is only because time will not allow me either to enter into arguments in their favour, or to bring forward proofs of their correctness.

In order to understand the reasons why, as I believe, topical applications in various morbid states of the uterus require, to be efficient, to be applied to the deeper seated parts beyond the external os uteri, it is necessary to bear in mind the anatomical structure of the lining membrane of the uterus and cervix. You will remember that the mucous membrane of the uterus is thickly studded with minute orifices, not more than one-thirtieth of a line in width, the openings of the utricular glands, which are small *cysts-de-sac* of structureless membrane lined with epithelium, having their blind extremities in the submucous tissue. These complex structures are always more or less in a state of active change, being thrown off in the form of *débris* at each menstrual epoch, and re-formed by the proliferation of the cells of the muscular and connective tissues. The mucous membrane lining the cervix is even more complex in its arrangements. It is covered with a series of prominent folds or ridges, which collectively are known by the name of the *arbor vite*; and these, more especially on the lower half of the cervix and on its external surface, are studded with innumerable white papillæ, each containing a vascular loop. All over the interior of the cervix, both on the ridges of its mucous membrane and between their folds, are a very large number of mucous follicles, which secrete a thick, tenacious, and alkaline mucus, which is generally found filling the cervical cavity. Here, then, it will surely be admitted, are structural elements of sufficient complexity to lead us to expect that they are likely to be the seat of pathological alterations of no slight importance. Unfortunately we are met with this difficulty, that we are compelled to base our study of them more on the observation of symptoms and the results of treatment, than on carefully recorded pathological facts derived from accurate *post mortem* examination of the structures themselves in a state of disease.

The diseases we are considering, although they give rise to much suffering and ill-health, are never fatal, so that opportunities for *post mortem* investigation do not arise; and the study of gynaecology is too much in its infancy to have enabled those who pursue it to avail themselves of the rare opportunities that may occur. Still, symptomatology and treatment are not without a real value; and, failing more scientific methods, we are surely justified in learning what we can from them. Modern writers on gynaecology have given very elaborate divisions of the various forms of inflammatory affection of the endometrium, which, I am afraid, the state of our pathological knowledge hardly justifies; such, for example, as cervical as distinguished from corporeal endometritis, chronic hyperplastic endometritis, hæmorrhagic endometritis, fundal endometritis, cystic metritis, and the like. Satisfactory as it would be to be sure that all these conditions actually exist as distinct

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