THE GULSTONIAN LECTURES ON EPILEPSY.

Delivered before the Royal College of Physicians of London.

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LECTURE III.*

THE last question is, What is the nature of the tissue-change? Can we form any opinion as to the character of the alteration in the grey matter which permits its sudden, explosive discharge? This problem is essentially the same, whatever theory of epilepsy is adopted. The violent muscular contractions suggest a corresponding overaction of the nerve-cells; and it has seemed to some, as Dr. Hughlings Jackson, that such repeated overaction can hardly be other than an indication of increased functional activity—of increased, if abnormal, nutrition. On the other hand, others, and foremost among them is Dr. Radcliffe, have maintained that the general conditions of epileptics and the phenomena of epilepsy suggest impairment of functional power, and that a symptom so abnormal as an epileptic fit can hardly result from normal nutrition, to whatever excess it may be carried.

Modern physiology teaches us, I think, in what direction an answer is to be sought. It is necessary, in the first place, to avoid too direct an inference from the outward manifestation to the internal process. No one probably now imagines that the nerve-force, which sets a muscle into action, forms part of the force liberated by the muscle; and, although the two are no doubt proportioned, the difference between them may be as great as between the energy liberated by an electrical spark and by a charge of gunpowder which it fires. We may easily overrate the amount of nerve-force needed to cause violent muscular action, and may also overrate the relation which the nerve-force bears to the potential energy of the cells concerned.

Every nerve-cell may be described as a storehouse of latent energy, and may be compared by analogy to a charged Leyden jar, or, what is more convenient for our present purpose, to a bent spring. The energy of the spring is due to the force which has bent it, and depends on the resistance which keeps it bent. The recognition of the resistance to action in nerve-cells, and of the part which it plays in normal processes, is a most important step in modern physiological thought. Of the nature of the resistance, we can form perhaps no idea, but its existence in nerve-cells may be taken as hardly needing proof; for we are unable to conceive of their action without it, and the idea has underlain such expressions as "nerve-tension", and the like, which have been long employed. The fact is probably only the highest instance of a series of relations which runs through the whole of organic nature. The formation of every organic compound, for instance, consists in, so to speak, the bending back of the spring of chemical affinity in a process by which force is held latent by some unknown resistance.

The application of this mode of thought to the morbid action of the nervous system promises to yield clearer views of many problems of disease, and for this step we are chiefly indebted to some important and suggestive papers by Dr. Ringer. The state of a nerve-cell, whether at rest or in action, may be regarded as depending on the proportion which its tendency to liberate energy bears to the resistance within it. The action of a nerve-cell may depend on the production of force within it being increased, in excess of the resistance, or on the resistance being unduly lessened. The cessation of its action may depend on the tendency to liberate energy being diminished, or on the resistance being increased. It is highly probable that all the phenomena of "inhibition" depend on increase of resistance by the action of one cell upon another. It is, indeed, conceivable that the interaction of the nerve-cells of the brain may be such that their mutual influence is a balanced resistance, and the amount of force in the brain nascent, but restrained, may be infinitely greater than any manifestations of energy which we perceive could possibly suggest. We may, perhaps, ultimately learn to regard the effect of stimuli in developing nerve-action, not after the analogy of the mechanism of a piano, each blow upon a key directly exciting the sound, but rather after that of an organ, in which the move-

ment of a key merely lessens the resistance to the action of a pent-up force.

Do not, I would ask, the phenomena of epilepsy best accord with the theory that the unstable equilibrium of grey matter depends on instability of resistance, rather than on any primary change in the energy-producing action of the cells? When loss of blood develops general convulsions, by its action on an otherwise healthy nerve-centre, can we conceive, I would ask, as Dr. Radcliffe has asked before, that it acts by increasing the production of nerve-force? It appears far more intelligible to regard its effects as a sudden diminution of resistance, by which the pent-up nerve-force is suddenly released, the nascent energy developed. The arrest by the ligature of fits which begin deliberately seems to point in the same direction. A strong peripheral impression causes such a change in the grey matter to which the discharge is advancing, that the discharge is arrested there, and passes no further. We can hardly conceive the effect of the impression except as causing a local increase of resistance, so that the liberation of energy ceases at the spot, and can no longer occur. Once arrested in this way, it is arrested for a considerable time. If the discharge depended on a primary increase in the energy-producing action of the cells, we should expect it to recur when the ligature was removed. It does not. I have a patient who has for a long time arrested every fit, commencing in the hand, by a ligature above the elbow. At first, if he did not apply the ligature, the attack went on into a general convulsion. He now finds that the sensation, beginning as before, stops, of its own accord, at the spot at which the ligature used to be applied, and never goes on to a fit. According to the view I have suggested, the repeated increase of resistance has led to a corresponding permanent change, resting on an alteration of nutrition, so that the unstable grey matter is limited by a zone of greater stability, where the discharge is arrested.*

Analogy suggests that the internal resistance to action is a higher function of the nerve-cells than the mere transformation of energy. The control is a higher function than the production of the force. Hence the fact that there is an apparent overaction becomes consistent with the theory that there is imperfect nutrition.

The discussion of the real nature of these influences—the liberation of energy, and the resistance to action—how far they are ultimately to be referred to vital, chemical, or electrical changes, is beyond the scope of these lectures. The recognition of one point only appears to me essential—that nutritive changes may underlie them, and that from them nutritive changes may result.

Two other points in pathology remain for mention. I have suggested that failure in resistance, permitting the liberation of energy, constitutes the essential element in discharge, and that such discharge causes the symptoms of the epileptic paroxysm. Physiological considerations make it probable that the resistance in cells may be raised and lowered by the action of cells at a distance. There are a few phenomena of minor attacks of epilepsy which seem best explicable by assuming that the resistance in a centre is suddenly raised (perhaps by the action of other cells) instead of being lowered. A patient, for instance, whose severe fits affect one side only, may have slight attacks which consist only of sudden transient weakness in the side. We have seen that, while visual sensations, due hypothetically to discharges in the visual centres, are frequent warnings, sudden inability to see may also be part of an aura, and such inability to see must be regarded as a sudden inhibition of the visual centre, that is, a sudden increase of resistance within it. A slight increase of resistance affords the best explanation of the apparent diminution in the size of objects. The increase of resistance is equivalent to a diminution in the sensitiveness of the centre, and the effect upon it of the stimulation of a given area of the retina is less than normal, and corresponds to the effect of a similar stimulation of a smaller retinal area. The object appears smaller and farther off, and as the resistance in the centre increases, the object may appear to recede. It is the opposite process to that which occurs in lessened resistance in the centre. This commonly results in such liberation of energy as affects consciousness. There is a visual impression, and perhaps a sense of a ball of fire, and as the discharge spreads, this seems to become larger and larger. The effect is the same as if an increasingly large area of the retina were stimulated. There is one centre which is usually inhibited, rather than discharged, in epilepsy—the centre for the motor processes of speech.

The phenomena of hystero-epilepsy seem to point to an instability of grey matter of different seat and perhaps different kind. The excessive violence of the muscular spasm in that disease is comparable only to the convulsions of epilepsy and tetanus. The phenomena appear due to the more deliberate and sustained overaction of centres which are

* Continued from page 467 of last number.

* That variations in tissue-resistance may influence the spread of a discharge which has already commenced, has been recognised by Dr. Hughlings Jackson (BRIT. MED. JOURNAL, January 11th, 1879).

concerned in specialised, co-ordinated movements, in emotions, and also in intellectual processes. The fact that some patients in these attacks fall suddenly, and remain still for a few moments before the co-ordinated movements commence, has been regarded by Dr. Hughlings Jackson (adopting a theory of the late Dr. Dickson) as evidence that the "co-ordinated" phenomena are always due to the release of lower centres from the control of the higher by the temporary discharge of the latter. He has urged that an actual discharge of grey matter can only give rise to inco-ordinate "brutal" effects. That co-ordinated convulsive action may succeed attacks of true epilepsy is beyond all question; but it is also, I think, beyond question that the co-ordinated phenomena are not always preceded by epileptic symptoms. In some cases which I have watched, the attacks commenced with the co-ordinated movements which characterised them throughout. The tonic rigidity, which initiates the attacks in other cases, cannot be regarded as a true epileptic symptom; it is rather part of the hystero-epileptic attack, and may recur during its course. Thus I think that there is in many cases no evidence of true epileptic symptoms, that the co-ordinated convulsion, which constitutes the chief symptom of the hystero-epileptic attack, may exist alone.

Even for the cases in which this symptom succeeds a true epileptic seizure, it may be doubted whether the hypothesis of "loss of control" affords an entirely adequate explanation. We can discern no difference between epileptiform attacks which are, and those which are not, followed by such symptoms. This suggests that this occurrence depends upon some other condition, a condition pertaining to the nervous organisation of the individual and not to the form of attack. This is supported by the fact that cases in which epileptic attacks are succeeded by "co-ordinated convulsions" present (as we saw in the first lecture) distinct characteristics in age, sex, causation, conditions under which the fit occurs. In these respects, they resemble the patients in whom such attacks of hysteroid character occur without any preceding epileptoid symptoms. These facts make it probable that there is in these epileptics, in addition to the instability of nerve-tissue which causes epilepsy, also the instability, whatever it may be, which gives rise to hysteroid attacks. "Loss of control" may be the immediate mechanism by which this instability is thrown into operation, but will not alone account for its existence, and is no necessary condition for its manifestation. Nevertheless, in some patients the hysteroid tendency may be so slight that it never manifests itself spontaneously—never except during the post-epileptic state. Hence I think that the old conclusion is the true one; that all patients who present these co-ordinated convulsions after epileptic attacks are really the subjects of both epilepsy and hysteria. The existence of the epileptic symptoms may be obvious, or may be most difficult to detect; initial simple tonic spasm cannot be regarded as evidence of their existence, and it may be practically impossible to say in many cases, without the rare opportunity of witnessing the onset of the attacks, whether there is or is not an initial truly epileptiform seizure. This is the explanation of a point in the method of these lectures which may have occasioned surprise; while endeavouring to distinguish the two diseases, I have not ventured to separate them. Such a separation I believe to be practically impossible; to attempt it would entail an error, the amount of which would be unknown, and certainly large. I have therefore considered each fact with regard to the entire series; endeavouring to ascertain, however, the extent to which it is true of the cases with and those without hystero-epileptic or co-ordinated convulsion. The former term, "hystero-epileptic", has been employed in its popular meaning, without reference to theory, as a designation for the severer forms of hysteroid attack, in which their violence, severity, and recurrence bring them practically nearer to the disease which we call epilepsy than to that which we designate hysteria. I cannot but think that it would be a gain if we were to discard the dubious word hysteria, even in its modification, as a designation for such attacks, and call them simply "co-ordinated convulsions".

Such forms of attack, in which the convulsive phenomena are violent in degree and disorderly in sequence, are in the lowest grade of co-ordinated phenomena; while the highest are those forms of involuntary action which we call automatic. In these, complex actions are performed, sometimes with considerable skill, of which the patient is subsequently entirely unconscious. Dr. Hughlings Jackson has long maintained that these are really post-epileptic phenomena. That they are commonly such, all must, I think, agree. The older view was that these were true epileptic events, the result of such changes in the brain as might in another degree or part constitute an ordinary epileptic attack. While recognising that they are commonly post-epileptic, I doubt whether the older view must be entirely discarded. Many of the phenomena mentioned in the first lecture may make us hesitate to assent to the conclusion that the discharge of epilepsy must always cause inco-ordinate symptoms. What, for instance, is the vision of an old woman

making ugly faces, which one patient had sometimes as the aura of a fit, sometimes as an attack of *petit mal*, but an automatic action of a co-ordinated discharge in the centre for visual ideas? The remarkable aura which I detailed at length suggests the same explanation, and so do many others that were alluded to. I have seen a patient suddenly, while being watched, without the slightest pallor, hesitation, or any symptom to indicate a discharge in the highest centre, proceed to empty his pockets, take off his coat, or to perform some action which in him characterised, and I think constituted, his attacks of minor epilepsy. With regard to the attacks of post-epileptic automatism, the same difficulties face us as in the case of the co-ordinated convulsion. We have the facts that in one individual attacks may always be followed by automatic action, and in another similar attacks may never be so followed. This suggests that there is, underlying the phenomenon, a condition of the centres on which it depends. Were it not for this condition, the epileptic attack would not lead to it; and this condition may, in rare cases, cause its primary manifestation.

[To be concluded.]

THE CROONIAN LECTURES ON SOME POINTS IN THE PATHOLOGY AND TREATMENT OF TYPHOID FEVER.

Delivered before the Royal College of Physicians of London.

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LECTURE II.

WHEN we proceed to consider the clinical and pathological phenomena of typhoid fever, and to compare them with those of allied diseases, it is evident that we may divide them into two classes—1. Those which are peculiar to this form of fever; 2. Those which are common to it with many other febrile diseases;—the first being the characteristics of the species, the second those of the genus. To the former class belong the etiology and mode of dissemination, the period of incubation, the intestinal lesions, the roseola, and the type and duration of the fever; to the latter, the high temperature, the febrile consumption and wasting, the albuminous infiltration and softening of many organs and tissues, and various disturbances of the principal functions of the body.

I propose first of all to consider the pathology of some of these secondary phenomena, which are indeed of primary importance in the treatment; for our control over the former is so restricted that, when once the system has become infected with the typhoid poison, it is out of our power to prevent it from producing the typical lesions. If we examine carefully the organs of a fatal case of typhoid fever, or of any of the infective fevers, or even of other febrile diseases which are not due to an infective process—though here the changes are less constant and less marked—we shall find some or all of certain morbid conditions.

The first is a granular or albuminous infiltration of the cells of most of the glandular organs, especially the liver, the kidneys, the salivary glands, the pancreas, the gastric glands; and also a similar change in the striped muscular fibres, both of the heart and of the voluntary muscles. The change consists in the appearance in the protoplasm of the cells and fibres of extremely minute granules, soluble in acetic acid, giving a clouded appearance which has been compared to that of ground glass; this obscures the nuclei, and causes, when advanced, some enlargement and irregularity of form. In extreme cases, the cells thus affected disintegrate into a detritus, this being usually, though perhaps not necessarily, associated with fatty degeneration. Unless the change is very extensive, the affected organs do not show any marked alteration to the naked eye; they may be somewhat enlarged, of diminished consistency, and, unless congested, paler than usual, with their normal shiny appearance somewhat dulled. Virchow, as is well known, regarded this granular infiltration as an inflammatory parenchymatous exudation; and in many cases these changes are associated with processes which are unquestionably inflammatory, but they may occur independently, and must probably be looked upon as essentially of a degenerative nature; often, indeed, they are but the first stage towards fatty degeneration. A change, which is probably of the same nature, occurs