THE GOULSTONIAN LECTURES

ON

THE LOCALISATION OF CEREBRAL DISEASE.

Delivered at the Royal College of Physicians of London.

By DAVID FERRIER, M.D., F.R.S., F.R.C.P.,

Professor of Forensic Medicine in King's College; Assistant-Physician to King's College Hospital; etc.

LECTURE I.—*March 15th.*

MR. PRESIDENT AND GENTLEMEN,—In these lectures, I propose to discuss a question which is at present attracting considerable attention in the world of physiology and medicine; viz., whether different regions of the cerebral hemispheres have different functions, and whether, therefore, the symptoms of cerebral disease vary with the locality of the lesion.

Assuredly, if the problem before us had been an easy one, it would have been definitively solved long ere now, when we consider the frequency of cerebral diseases and the large amount of attention they have received, not only from physicians and physiologists, but from all who interest themselves in the relations between body and mind. But, notwithstanding all the laborious researches and speculations which have been directed towards the elucidation of this subject, we do not seem even yet to have arrived at any general agreement, except on a very few propositions; some of these even now contested: a position contrasting strongly and unfavourably with the state of our knowledge respecting almost every other organ and function in the body.

It is not very difficult to discover many causes of this obscurity and confusion. Two of these only I will mention as being specially worthy of note.

1. It may be asserted without fear of contradiction that, as regards the nervous system more particularly, morbid anatomy is far from being co-extensive with pathology. We know, and are every day confronted with the fact, that the most widely abnormal deviations from healthy functional activity of the nerve-centres may be manifested, which leave no trace discoverable by ordinary dissection, or even by any of our most advanced methods of investigation. For the sake of mental satisfaction, we are constrained to speculate on the intimate molecular changes in the nerve-tissues which lie at the root of neuralgia, convulsions, and various other forms of functional nervous disorder; but they are at present matters only of speculation, and lie beyond the sphere of verification.

2. The organisation and conditions of activity of the brain are such that we are naturally inclined to believe that interference at any one point must necessarily tend to general functional disturbance. The loosening of a pin in a chronometer, it has been said, will derange the whole timekeeping mechanism; but we should not on that account ascribe timekeeping functions to the one part exclusively. So, in all cases of cerebral disease, there is a continual source of doubt as to whether the effects are the direct consequences of the lesion, or merely the expression of general functional derangement.

And, when we examine the actual facts and records of cerebral disease, we find, in apparently similar conditions, so much diversity, that it seems almost impossible, from a clinical point of view, to separate accidental from essential; to distinguish between direct and indirect consequences; or to determine whether phenomena are related by causation or are mere cases of juxtaposition or coexistence. Nor do the facts of experimental physiology seem so consistent with themselves, or with the undoubted facts of clinical research, as to inspire us with unhesitating confidence as to their accuracy, or as to their applicability to human pathology.

It is not to be wondered at, therefore, that many should still doubt, and reserve their opinion on this question of the localisation of cerebral function and cerebral disease.

Before proceeding to consider the facts bearing on this question, I think it advisable, in view of certain arguments which have been advanced by Dr. Brown-Séquard and others, to state some of the principles which must guide our researches and determine our conclusions. There can be no doubt that the inductive method of agreement, on which we have mainly to rely in eliminating cause and effect in clinical medicine, is one which does not always succeed in distinguishing between causation and co-existence, and is liable to be frustrated by plurality of causes. Though, therefore, we should have much positive

evidence in favour of the localisation of a certain function in a certain region of the cerebral hemispheres, one clear case in which destruction of this region had caused no cessation or disorder of that function would be sufficient to overturn our conclusion.

But, on the other hand, we are not called upon, in the present state of pathology, to show organic alteration in the parts in which we localise certain functions in all cases in which these functions are deranged. When such organic change has been demonstrated in the case of disordered function everywhere else in the body, it may fairly be demanded in the case of the nerve-centres; but we are at present far from having reached that point. We do not always discover organic disease in the heart when the circulation ceases, but we do not on that account doubt that stoppage of the heart was the proximate cause of this effect.

The doctrine of cerebral localisation does not assume, as Brown-Séquard would seem to imply, that the symptoms observed in connection with a cerebral lesion are necessarily the result of derangement of function in the part immediately affected. Every one admits direct and indirect results in cerebral disease. We have no right even to assume any causal relation at all, direct or indirect, between the phenomena, unless the lesion in question is constantly, or more frequently than chance would account for, associated with the same symptoms.

We should think it in the highest degree absurd if anyone were to describe a case of sudden death, in which the only discoverable morbid appearance was a boil on the neck, as a case of sudden death "seemingly caused" by a boil on the neck; and for the obvious reason that such a lesion is not usually followed by a fatal result. So, if we have abundant evidence to show that a certain part of the brain may be diseased without causing any motor paralysis whatever, it would be no less absurd to describe a case of facial paralysis, in which some lesion of this region was the only discoverable anatomical change, as a case of facial paralysis "seemingly caused" by this lesion. Causation must not be invoked where the facts do not warrant anything beyond co-existence or fortuitous collocation. It is, however, undoubtedly true that, if we admit, as we must do, that function may be disordered without discoverable organic change in the organ in which we localise this function, our difficulties as regards diagnosis are greatly multiplied. In medicine, thanks greatly to the aid of experimental physiology, the transition from cause to effect is comparatively easy; but from effect to cause, from symptoms to disease, taxes all our skill, and too often baffles all our efforts.

We may have doubts as to our diagnosis of the nature and locality of a cerebral lesion, though we may have none as to the localisation of function. The two things are quite distinct; and we must not make our imperfections in the one a measure of the other, or imagine that the facts are equally confused with our notions respecting them.

In estimating the value of evidence, we regard as incredible any statement which is opposed to sound inductions. Our inductions having been once established, we attach comparatively little importance to individual instances which harmonise with them; for, not being necessary to establish our generalisation, they derive a dignity mainly from the fact that they are examples of a general law. But a statement which contradicts our uniform experience we are bound to regard with suspicion and refuse to accept, unless it can successfully stand a stringent cross-examination and satisfy all the requirements of scientific evidence. The slightest doubt is absolute failure. If, however, any clear and in every way satisfactory evidence be forthcoming opposed to our generally accepted beliefs or traditions, we are, nevertheless, bound to accept it, and be prepared to convert what we have been accustomed to regard as absolute rules into approximate generalisations, or abandon them altogether if they are clearly shown to be untenable.

Even though the new hypothesis which may be set up in place of our old one may not be more satisfactory, yet the value of the facts will remain, and we must modify our views in accordance with them.

In discussing the localisation of brain-function, it is essential to bear in mind that the functions and diseases of the brain manifest themselves under two aspects—the psychological and the physiological: phenomena which appeal to two distinct methods of investigation—the subjective and the objective. There is no doubt that the state of our knowledge respecting the conditions affecting the one is vastly in advance of that of the other.

That the brain is the organ of the mind, no one doubts; and that, when mental aberrations, of whatever nature, are manifested, the brain is diseased organically or functionally, we take as an axiom. That the brain is also necessary to sensory perception and voluntary motion, is also universally admitted; and that the physiological and psychological are but different aspects of the same anatomical substrata, is the conclusion to which all modern research tends. Nevertheless, it appears, as

far as our present methods of investigation have gone, that diseases of the brain do not affect these functions equally. Diseases which produce very obvious affections of motility and sensibility cause no obvious mental disorder; and diseases capable of producing the most profound mental disturbances do not necessarily affect the powers of motion and sensation. Beyond the great fact that the brain is the organ which is directly or indirectly diseased in insanity, we are yet in the most profound state of ignorance regarding the intimate pathology of this condition. *Post mortem* examination reveals morbid conditions of vascularity, or various forms of degeneration, in the vessels, nerve-cells, neuroglia, etc.; but, with the exception, perhaps, of general paralysis of the insane, we have yet to find out whether there are any morbid appearances specially characteristic of special forms of mental derangement, or whether there is a definite relation between the locality of the lesion and the symptoms observed. We cannot even be sure whether many of the changes discovered are the cause or the result of the disease, or whether the two are the conjoint results of a common cause.

It is no disparagement of the many valuable researches which have been made into the pathology of insanity, to say that we are only beginning to learn its rudiments. Until the various morbid appearances discoverable in the brains of the insane are translatable into their subjective signification, the morbid anatomy and pathology of insanity run in parallel lines, which never meet. A concrete or incorporate mental pathology distinct from mere speculation will only be furnished when we can give the subjective equivalents of morbid appearances, or, conversely, the anatomical substrata of subjective states. It will not, I think, be denied that we are yet a long way from having reached this desirable consummation of our researches.

From the fact that large tracts of the brain-cortex may be disorganised without causing any very evident mental disturbance, and from the further fact that any *one* part of the brain may be so destroyed with a like negative result, the conclusion has been drawn by Flourens and others, that there is no localisation or differentiation of function, but that each part of the encephalon is a micrencephalon, capable of itself of performing all the functions pertaining to the whole. Supposing the conclusions at all justifiable *quâ* mind, it would be altogether unwarrantable to extend this, as has been done frequently, to physiological function.

If we look at this matter a little more closely, we shall find that not merely extensive lesions in one hemisphere may be latent as regards mental symptoms, but even a whole hemisphere may be disorganised with a like negative result. If, however, *both* sides of the brain be disorganised, the annihilation of mind is complete. The logical deduction from these facts therefore is, not that there is no special localisation of function, but that, as far as the fundamental powers of mind are concerned—sensation, emotion, volition, and intellect—one hemisphere is sufficient.

To show that there is no localisation of mental function, it is necessary to demonstrate that the *same* parts may be destroyed in *both* hemispheres without producing mental disturbance. But has this ever been shown? I cannot find the faintest approach to evidence which would justify such a conclusion. That mental symptoms or mental deficiencies have not been recorded in cases of bilateral cerebral lesions, is a negative statement of very little value. Unless a man becomes so demented as to neglect the ordinary wants of nature, or so furious, maniacal, or irrational as to require restraint, there are few engaged in the practice of medicine who think of inquiring narrowly into a patient's mental state; and, even if more attention were directed towards this subject, are we in possession of any means of accurately gauging the mental condition of an individual, so as to be certain that it has altogether escaped damage notwithstanding the presence of a cerebral lesion? I see little to justify and much to contradict such an assumption. A man may not be incapacitated for the ordinary duties of life; but that his mental powers are altogether unscathed even by an unilateral lesion, I venture to question.

And, if it is difficult to test the mental condition in a human being, how much more difficult must it be in the case of the lower animals? And yet, from the way in which some have treated this question, one would be led to believe that nothing was more simple. Flourens' conclusions are, I think, answerable for many erroneous notions which have long dominated cerebral physiology and pathology. One great fallacy has been the assumption that the results of experiments on frogs, pigeons, and other animals low in the scale, are at once capable of application to man without qualification: an assumption which vitiates the conclusions of numerous physiologists of the present day. The very fact that there exist such patent differences between the effects of destruction of the cerebral hemispheres in different orders of animals ought, one would think, to inspire caution in the application to man of results obtained only by experiments on the brains of animals low down in the scale. Physiology should take a more comprehensive

view, and in particular not neglect the facts of clinical medicine and human pathology. To do so in the case of the functions of the brain would, indeed, be the play of *Hamlet* with the Prince of Denmark left out.

These remarks are, I think, specially applicable to a statement made by Schiff at the International Medical Congress at Geneva, to the effect that, as regards cerebral localisation, there was a great difference of opinion between physiologists and physicians; the former being opposed to it; the latter, with an implied sneer at the *practischer Arzt*, being its only supporters. While it is to be hoped that all physicians are physiologists, it is not the less desirable that physiologists should take the facts of clinical medicine and pathology into consideration. Frog and pigeon physiology has too often been the bane of clinical medicine, and tended to bring discredit on a method of investigation which, used properly, we must regard as the sheet-anchor of accurate biological and therapeutical research.

It has been taught since the time of Aretæus, and accepted almost as an axiom by physiologists and physicians that, when paralysis results from lesion of the cerebral hemispheres, it occurs on the side opposite the lesion. This law has recently* been contested by Brown-Séquard, and a few words are necessary on this point.

Brown-Séquard disputes the validity of the law of cross action of the cerebral hemispheres, on the authority of two hundred cases of paralysis occurring with disease on the same side of the brain. Accepting for the time the accuracy of every one of these cases, what conclusion do they justify? If we compare the relative frequency of cross and direct paralysis, it would be a very low estimate to say that, for every case of direct paralysis, we might cite nine hundred and ninety-nine cases of cross paralysis. Are we, then, on the strength of one contradictory instance, to say that the nine hundred and ninety-nine cases carry no weight? This is what Brown-Séquard would seem to imply; but the logical deduction appears to me to be that, if it had been asserted that the paralysis *invariably* occurred on the side opposite the lesion, then the law is shown, by this one exception, not to be an universal law, but a law admitting of exceptions; an approximate generalisation, instead of an absolute rule; but an approximate generalisation, the validity of which, in any particular instance, would have nine hundred and ninety-nine chances to one in its favour. Certainly the conversion of an absolute rule into an approximate generalisation lowers its practical value; for approximate generalisations are applicable only to numbers and not to individual instances. Hence, in a given case, in which it might be of extreme importance to ascertain whether it was in accordance with the rule or an exception, we might be liable to error, if we had no other means of determining this.

But what would be the practical effect of this, say as regards surgical treatment? Supposing it were a question of rephining (a question which may arise more frequently at no distant date), according to the calculus of probabilities, there would be nine hundred and ninety-nine chances to one in favour of the rule in any given case. Even on this very low estimate, would it be a very hazardous thing to operate, granting the advisability of the operation? I question much if there are many surgical operations undertaken for the relief of internal disease which have better chances of being successful. In life and in medical practice, we have to act on probabilities more than on certainties; and if the measure of our belief be our readiness to act, I think few would refuse to perform an operation with nine hundred and ninety-nine chances to one in favour of its being successful, so far at least as exposure of the disease is concerned.

The practical depreciation of the rule would, therefore, be almost infinitesimal; and, if the new (Broca's, Lombard's) observations on cerebral thermometry are correct, this may be even further reduced. But what of the theoretical aspect of the question?

In man, the cross action of the cerebrum, in reference to motion, is true, with the exception of two hundred cases collected from the remotest antiquity down to the present date. Cerebral paralysis is seen every day; and if cases of direct paralysis were occurring at the present time, we should be certain to hear of them. In the lower animals, every physiologist who has seen paralysis produced by cerebral lesion, has seen it on the opposite side, with the single exception, I believe, of Brown-Séquard.

Such being the experience of physicians and physiologists, it is surely more natural to suspect sources of fallacy, or to conclude that there is something very abnormal, than attempt to overthrow a law which has such a preponderance of evidence in its favour. That direct paralysis may occur I am prepared to admit, not only on the evidence of recorded facts, but on anatomical grounds. To overthrow the absoluteness of the rule of the cross action of the cerebral hemispheres, one well

* *Lancet*, January 1876.

authenticated case, in which the paralysis occurred on the same side, is sufficient. But we require clear evidence, not merely of the existence of a lesion in the hemisphere of the same side (for, as we shall see, lesions of a very extensive nature may exist in certain regions without causing paralysis), but of the existence of a lesion in what we recognise as a motor region. A solution of continuity of the fibres of the internal capsule, or a complete separation of the corpus striatum from its hemispherical connections, we should look upon as a necessary cause of paralysis, and we should look for it on the opposite side.

If such a lesion could be shown to have existed with paralysis on the same side, I should regard that as a satisfactory proof of direct paralysis. And such seems to have been established by Morgagni. In a case which he had carefully observed, and examined *post mortem*, he was astonished to find paralysis apparently on the same side as the lesion; but, distrusting his recollection and the accuracy of his records, he asked of his students on which side the paralysis had existed. "All in general and each one in particular answered without hesitation that it was the right side [the side of the disease, which was separation of the corpus striatum from the cortex]; and for this reason", said he, "it is clear to me that sometimes the paralysis occurs on the same side as the lesion."*

I do not here intend to enter on the question whether some apparent cases of direct paralysis may not be capable of explanation in accordance with the usual rule;† but, admitting the possibility of direct paralysis, I would offer a few observations on its mode of causation. In this relation, the recent researches of Pierret and of Flechsig have an important bearing. Flechsig, in his elaborate work, *Die Leitungsbahnen im Gehirn und Rückenmark*, 1876, has investigated the course and relations of the several tracts of the brain and spinal cord, with special reference to their respective periods of development in the human foetus, and to the course and limits of the secondary degeneration which occurs in consequence of cerebral and spinal lesions, according to the researches of Waller and Türck. This is a method which must be regarded as infinitely superior to mere anatomical or histological investigation of the healthy and completely developed cord.

Flechsig shows that the pyramids or pyramidal strands are developed always subsequently to the hemispheres, and from them, and are wanting in anencephalous foetuses. Their connections can be traced above into the cortical regions bounding the fissure of Rolando, and below with the postero-lateral, and partly with the internal aspect of the anterior columns of the spinal cord. These pyramidal strands are subject to very considerable variations, in respect to their decussation at the anterior inferior part of the medulla oblongata, and as to the relative proportion of fibres which proceed down the postero-lateral and antero-internal columns respectively. As a rule, the most of the fibres of the pyramid descend in the postero-lateral column of the end on the opposite side; the rest on the antero-internal of the same side. But occasionally, the rule is reversed; and in one case there was no decussation at all. A similar case has recently been described by Pierret.‡

The strands which are subject to this variation are those which, as we shall see, degenerate in consequence of lesion of the motor centres, and the evidence is of the most satisfactory kind that they are the paths of *voluntary motor impulses*. This being so, we must regard paralysis on the same side as the cerebral lesion as a possible occurrence. How often it has actually happened is another question, which, however, I shall not here attempt to answer.

Various attempts have been made at different times to establish constant relations between lesions of certain cerebral regions and certain symptoms, bodily or mental. Thus Saucerotte, Delaye, Foville, and Pinel-Grandchamp, considered that the grey matter of the hemispheres was specially related to mental functions, and that the medullary fibres and basal ganglia were specially concerned in locomotion. They further adduced cases to prove that disease of the corpus striatum and adjoining medullary fibres and anterior parts of the brain generally, caused paralysis limited to the leg; and that similar lesions of the optic thalamus and posterior parts of the brain, caused paralysis limited to the arm; and that, when the arm and leg were both affected, the lesion existed in the basal ganglia, more in the corpus striatum if the leg were specially affected, and more in the optic thalamus if the paralysis was greater in the arm. Influenced by the imaginary localisation of the sensory tracts in the posterior columns, and by Bell's demonstration of the respective functions of the anterior and posterior roots of the spinal nerves, they regarded the cerebellum, to which they traced the posterior columns, as the seat of sensation. This opinion

was supported by Lapeyronie, Petit-Namur, and others. Bouillaud, from his experiments on animals, and from the facts of clinical research, arrived at the conclusion that lesions of the anterior lobes more particularly caused loss of speech, and in a certain measure gave his adhesion to the doctrines of Saucerotte, etc., respecting the centres of movement of the arm and leg, though he admitted that these were not altogether satisfactory. But he arrived at one other important conclusion which is worthy of special mention. "Even," said he, "though we should admit that certain errors had been made as to the localisation of the seat of the lesions causing paralysis, yet it remains an established fact that there exist in the cerebrum several motor centres. The plurality of motor centres is, in fact, proved by the occurrence of limited paralysis, corresponding to a local alteration in the brain; for it is evident, that if this organ did not contain different centres or conductors of motor impulses, it would be impossible to conceive how a limited lesion could produce a limited paralysis, leaving all other movements intact.

"I am well aware that the preceding propositions appear at variance with the results of experiments on animals. It is certain that after the ablation of the cerebral hemispheres, an animal may walk, run, move its jaws, eyes, eyelids, etc.; and it is not less certain that an alteration of the cerebral hemisphere in man gives rise to a paralysis more or less complete of voluntary motion on the opposite side of the body. Can we refute the one set of facts by the other? No, certainly not. For facts equally positive are not susceptible of refutation. A time will come when new light will dispel the apparent contradiction which exists between them."*

Profound and philosophical remarks which to-day are amply justified!

The investigations of succeeding years provided many cases so absolutely at variance with the localisation of the motor centres of the arm and leg that Andral, who also admitted that there must be distinct motor centres, "since each limb may be separately convulsed or paralysed", earnestly deprecated premature and hasty generalisations as being highly prejudicial to "la belle doctrine" of the localisation of cerebral functions.†

The doctrine of cerebral localisation has in recent years assumed quite a new aspect, and differs so much from older speculations in the kind of evidence on which it rests, as to be essentially a new growth. Hughlings Jackson made the first decided steps in this direction.

Hughlings Jackson has repeatedly directed the attention of the profession to the study of convulsions of cerebral origin, and adduced many cases and arguments to show that they are dependent on irritation or discharging lesions of certain convolutions near, and functionally related to, the corpus striatum. As regards the exact localisation of these motor convolutions, he did not, however, underestimate the difficulties and uncertainties necessarily attaching to the rude experiments of disease. "The damage by disease is often coarse, ill-defined, and widespread."‡ But to Hughlings Jackson belongs the credit of having first indicated the motor functions of certain regions and given a rational explanation of the phenomena of unilateral cerebral convulsions. For though, as Charcot shows, Bravais, in 1827,§ described with great accuracy the phenomena of hemiplegic epilepsy, he did not see their true significance or pathology, in which, after all, the discovery lay, and in the light of which the value of his observations mainly consists.

Similar facts have also been noticed and commented on by Bright and Wilks. Indeed, Bright had formed very clear notions as to the pathology of unilateral convulsions, so far at least as their primary causation was concerned, and their relation to lesions of the opposite cerebral hemisphere. "My reason, then, for supposing that the epileptic attacks in this case depended rather on a local affection than on a more general state of cerebral circulation or excitement, was the degree of consciousness which was observed to be retained during the fits; for although we meet with great variety in this respect, yet in two cases which have occurred to me, the fact of the patient generally remaining conscious has been a remarkable feature, while in each the injury on which the fits depended was of a local rather than a constitutional or a general character."||

Wilks, agreeing with these observations, remarks: "For in these cases, the causes being definite and local, an irritation is set up in the corresponding ganglia beneath, and thus the occurrence of convulsions without loss of consciousness is explained."¶

Hughlings Jackson, however, instead of trying to account for the

* Quoted from Bayle, *Maladies du Cerveau*, page 321.

† This question has been ably discussed by Dr. E. H. Dickinson, "On the Phenomena of so-called Direct Paralysis." *Liverpool and Manchester Medical and Surgical Reports*, 1878.

‡ *Société de Biologie*, January 8th, 1878.

* Bouillaud, *Traité de l'Encéphalite*, p. 279.

† Andral, *Clinique Médicale*, tome v, p. 569.

‡ *Clinical and Physical Researches on the Nervous System*, p. 6.

§ *Recherches sur les Symptômes et le Traitement de l'Épilepsie Hémiplegique*.

|| *Guy's Hospital Reports*, Series i, vol. i, p. 39.

¶ *Guy's Hospital Reports*, 1866, p. 79.

phenomena by transmission of some influence to distant motor regions, regarded certain convolutions as themselves motor, and capable of motor discharge by irritation. But certainly, except in the facts so explained, no other evidence could be adduced in support of the direct excitability of the grey matter of the cortex: for the facts of experimental physiology, taking them at their value, were opposed to the doctrine, inasmuch as it had been apparently conclusively demonstrated that none of the usual stimuli of nerves and nerve-centres, electricity included, were capable of exciting movements when applied directly to the surface of the brain.

This dogma was refuted in 1870 by the important experiments of Fritsch and Hitzig, who showed that, though electricity might be applied to certain regions of the cortex without producing movements, there were others excitation of which invariably produced movements of the opposite side; and that certain movements could uniformly be caused by excitation of certain definite regions. These facts have now been extended and verified by many experimenters on many animals, and even on man himself.

In discussing the significance of these facts, I wish to restrict my observations to those movements which result from excitation of a certain region of the brain—the region which we term motor—in order to avoid discussion at this stage of certain other movements which I regard as the indications of sensation.

It is not unreasonable to suppose that, on applying irritation to that which is the centre of centres—to which, in fact, all the rest of the body must be considered as peripheral—irritation of the grey matter, even though the irritation might not be confined to it, at least entered as one factor into the causation of the resulting phenomena. Every conceivable hypothesis has, however, been invented to degrade the grey matter of the hemispheres, and to exclude it absolutely from all share in the results; and every attempt has been made to discover somewhere else some organ or organs possessed of all those varied and complex forms of activity which we see excited by our stimulation.

One of the latest of the hypotheses which have been invented to account for the phenomena is, that the movements which result from the application of electrical stimulation to the cortex are due to the irritation of delicate vaso-motor nerves, which penetrate the brain-substance and descend with the vessels from the pia mater.

The functions ascribed to the brain-cells are these: "Besides their power of receiving, transforming, and conveying impressions, it is not unphilosophical to imagine that, having been impressed by a certain irritation, in a certain way, for producing a certain effect, their dynamic state, through nutrition, is shaped into a definite channel for the circumstance; hence the ability of a certain group of cells to produce a definite effect, always constant, under definite stimulation of whatever nature!"*

As my main object, however, is to discuss cerebral localisation from a pathological point of view, I will not enter at length into the purely physiological side of the question, which I have elsewhere discussed (*Functions of the Brain*). I would, however, call your attention to some recent researches which seem to me to have effectually disposed of the main objections to the view that the phenomena are the result of excitation of the functional activity of the grey matter of the hemispheres. One objection is founded on the impossibility of localising the action of the electric current to the parts immediately in relation with the electrodes; and it is argued that the effects are in reality due to mere physical conduction to some underlying region or regions, which, however, those who employ this argument either will not or cannot exactly specify.

Similar objections were made to Duchenne's theory of localised muscular electrification, and yet we know as a fact that we can by this method throw individual muscles into contraction with the greatest precision and certainty, notwithstanding the extrapolar conduction which pertains to all animal tissues. And it is the great characteristic of the reactions which ensue on the application of the electrodes to the cortex, that the results are uniform, definite, and predictable, when the electrodes are on one region, while there is a sudden transition to another movement equally definite, equally constant, and equally predictable when the electrodes are shifted to a region in immediate proximity to the former. This is a remarkable fact, no longer disputed, which no mere physical conduction can account for, unless we admit a localisation of numerous distinct physical paths, which is but another aspect of localisation after all.

On the conduction theory, we should naturally expect that the nearer we go to the underlying ganglia and tracts, the more readily the effects should be called forth if it were a question of mere resistance of currents. But we find that electrification of the island of Reil, which is nearest the basal ganglia, is absolutely negative; while electrification of

the more distant postero-parietal lobule by the same stimulus produces an immediate and definite movement. Conduction would seem to be put out of court by such facts. And we find, as Carville and Duret have shown, that the intervention of a fluid cyst between the cortex and the basal ganglia is quite sufficient to interpose a fatal obstacle to the propagation of functional stimulation, though not of electrical currents, just as a ligature round a nerve will stop neurility but not electricity.

But the fact on which most reliance is placed as proof of mere physical conduction is that, after removal of the grey matter of the cortex (stimulation of which is the supposed cause of the movements), the application of the electrodes to the cut medullary fasciculi produces exactly the same movement as before.

What, it is triumphantly asked, could more conclusively dispose of the view that the cortex is concerned in the results, seeing it may be removed without prejudice to them? Apparently, those who argue in this manner forget that there is such a thing as a plurality of causes or conditions. By parity of reasoning, we might disprove in succession the motor functions of the corpus striatum, crus cerebri, spinal cord, motor nerves, inasmuch as we can produce all the effects attributed to their activity by direct stimulation of the muscles themselves. But we do not say, when we faradise the distal end of a divided motor nerve, that the resulting muscular contraction is due to electrical conduction to the muscle, and not to neurility or excitation of the functional activity of the nerve. And it is surely not unreasonable to suppose that, after removal of the cortex, the results following application of the electrodes to the medullary fibres are due to the functional excitation of these fibres; and that our electrical stimulation is merely an artificial substitute for that which normally proceeds from the grey matter of the cortex. It is on this point that we have new experiments which, in my opinion, settle the question definitely.

[To be continued.]

CLINICAL MEMORANDA.

ACCIDENTAL POISONING WITH THE NITRATE OF POTASH.

EXCEPT in the case of articles of diet in common use, such as shell-fish and mushrooms, or of the substitution of some well-known poisonous herb for one ordinarily employed in cooking, cases of domestic poisoning by accident are generally puzzling at first. The following accurately noted case was forwarded to me by the sufferer, a retired surgeon; and, as the accident occurred through an error in cooking committed with the best intention, and therefore not unlikely to occur again, I have obtained his permission to communicate it to the members of the Association.

In order to make some soup more "strengthening" for the especial advantage of the patient, some liquor, in which a pig's face had been cooked on a previous occasion, was boiled down to a jelly, and added to it. This soup was taken at 2 P.M., and it was noticed to be a good deal too salt. No symptom was observed for three hours, when nausea and griping set in; these continued to increase to the eleventh hour, when some whiskey and nutmeg which had been taken was vomited. There was no bile or anything unpleasant in the evacuation. At the twelfth hour, violent watery purging in large quantity set in and continued for an hour, and the pain ceased. No further symptom occurred, but the patient stayed in bed upon fluid diet the next day. Not until the morning of the third day did he feel a good deal of lumbar aching, which he attributed to the confinement. In the course of the morning, the bowels acted naturally, and he went out of doors. At midday, he took two ounces of Hollands in cold water. An hour afterwards, he passed water, which was coffee-coloured from admixture of blood; there was (and had been) no dysuria, or any other fresh symptom. He returned to bed, and the urine having, in the course of a few hours, regained its normal appearance, it was tested, and ascertained to be perfectly free from albumen. He now diagnosed the cause of illness. He stayed in bed upon low diet for three more days; the lumbar aching continued, but the urine remained free from albumen. On the fourth day, he got up, and with one meal took an ounce and a half of Hollands with cold water. This was again followed, within an hour, by hæmaturia, which passed off as before within a few hours, leaving the urine quite free from albumen. From this time, he abstained from spirits entirely, and the lumbar pain gradually disappeared, leaving him in his usual state of health. It is worth noting that the patient observed this lumbar aching to be quite different from the pain caused by lumbago—a disease with which he is also personally acquainted.

J. ASHBURTON THOMPSON, M.D., Islington.

* Dupuy, *Physiology of the Brain*. New York, 1877, p. 13.