

CEREBRAL LOCALISATION.

A REVIEW AND FORECAST.

BEING THE

MARSHALL HALL PRIZE ORATION.

BY

DAVID FERRIER, M.D., LL.D., F.R.S.

(Received October 23rd—Read October 23rd, 1883.)

MR. PRESIDENT AND FELLOWS OF THE ROYAL MEDICAL AND CHIRURGICAL SOCIETY,—I have in the first place to express to you my grateful appreciation of the great honour you have done me in awarding me the Marshall Hall prize of this year, and of the further high privilege you have accorded me, in asking me to address you on this occasion.

To be the recipient of a prize associated with the name of Marshall Hall, and for work done in that department of medical science which he himself so adorned and enriched, is a distinction of which I am indeed proud. I know of no name in the ranks of scientific medical inquiry in this or any other country which stands higher than that of Marshall Hall, and no work which has done more to advance the physiology and pathology of the nervous system, and not this only, but medicine and surgery in general, than his investigations into the nature and conditions of reflex action. This he was undoubtedly the first to formulate and expound, and clear from the vagueness

and confusion which prevailed before him. The importance of his researches can scarcely be over-estimated. If it were possible to eliminate from modern medicine all that relates to reflex action, its bearings and applications, we should practically de-rationalise the better half of pathology and therapeutics. Few scientific inquirers have been more keen to perceive the practical bearing of their own researches than Marshall Hall himself ; and most of the principles which guide us in the recognition and treatment of diseases and symptoms dependent on reflex irritation, were first clearly laid down by him.

Not long prior to the time when Marshall Hall was pursuing his investigations on the spinal cord and medulla oblongata, Flourens was engaged on his ever memorable researches on the physiology of the cerebral hemispheres. And Flourens also, to whom he dedicated several of his memoirs, was almost the first to perceive and recognise the value of the work done by Marshall Hall which many of his countrymen endeavoured vainly to detract from and depreciate.

If, at the distance of fifty odd years, we compare the relative stability of the work done by these great men, that of Marshall Hall on the spinal cord, and that of Flourens on the cerebral hemispheres, we find that the doctrines enunciated by Marshall Hall, modified perhaps as to detail, and further extended by the numerous researches of recent years, are in all essential points those which still prevail, and show no signs of failing ; while those of Flourens which have also exercised an enormous influence on clinical medicine and pathology,—What shall we say of them ?

In the brief sketch which I purpose giving you of the present position and probable future of this question, I fear that however much I may try to avoid it, I shall appear more in the character of the advocate than the impartial judge. But I am fully conscious that I am before a highly critical audience who will not allow any bias of mine to warp their own just judgment.

The views of Flourens are familiar to you all. They seemed to the scientific world at once in accordance with the facts of experiments on animals—at least of the lower orders—and in harmony with the prevalent metaphysical conceptions as to the unity and indivisibility of mind. Like the mind itself, said Flourens, the organ of mind was also one and indivisible; there being no differentiation of function, but each and every part possessed of the potentialities, and capable of exercising every function, pertaining to the whole. These doctrines speedily met with general acceptance among physiologists, but there were a few keen clinical observers, Bouillaud, Andral, and others, who saw in the phenomena of cerebral disease facts which appeared wholly inexplicable except on the hypothesis of a differentiation of function in the cerebral hemispheres; such for instance as the occurrence of limited paralyses in connection with limited lesions. But unable, on the other hand, to account for the facts of experiments on animals on the principles of localisation which the clinical data seemed to demand, they wisely suspended their judgment, neither denying the facts of experiment nor doubting their clinical observations, believing that the apparent discrepancies between human pathology and experimental physiology would one day be cleared up and dispelled.

This was eminently the attitude of my distinguished predecessor in the honorable position I now hold. Hughlings Jackson, neither contesting nor trammelled by the doctrines of the “school-physiology,” saw in the occurrence of limited and unilateral muscular spasms in connection with certain cortical lesions, phenomena which to him signified irritation or discharge of grey matter directly related to muscular movements; and with a keen and prophetic insight, which far outran the slow march of verified fact, he evolved many brilliant and fruitful conceptions as to the anatomical substrata of the organ of mind.

There were, however, many other clinical facts, particularly those relating to loss of speech in connection with

lesions of the left hemisphere, which formed the subject of so much lively debate and discussion twenty years ago, which remained equally obscure and mysterious, whether on the hypothesis of localisation or the reverse. It is not going too far to affirm that up to the time when the researches of Fritsch and Hitzig inaugurated a new era in cerebral physiology, general doubt, difficulty and confusion prevailed. It would even now be premature to claim that all this has been dispelled, but we may say truly that the questions in dispute have assumed definite and manageable shape, and that every day the dark atmosphere is being cleared.

I will not weary you with an account of the controversies which arose as to the true significance and interpretation of the results which followed the application of electrical irritation to different regions of the cerebral hemispheres.

On this, as well as regards the consequences of destructive lesions of the cortex the most divergent views continued to be entertained and expressed.

The fundamental question "Localisation or no localisation?" was brought to a crisis at the meeting of the International Medical Congress here in 1881.

You are all aware, many of you doubtless heard and saw, that Prof. Goltz, the chief of the few remaining champions of the Flourentian system, brought with him from Strasburg a dog in which he had, long previously, at intervals destroyed a large extent of the cortex of both hemispheres, and which he exhibited before the physiological world as a practical refutation of the theory of cerebral localisation. He enunciated the following theses.

1. The cerebral cortex is the seat of the higher intellectual functions. Removal of large portions of both hemispheres degrades the intelligence.

2. It is impossible by any localised cerebral lesion to cause paralysis of any muscle. The animal operated on, retains volitional control over all its muscles.

3. It is also impossible by any localised cortical lesion to cause permanent loss of any sense. The animal retains

all its sensory faculties. After removal, however, of large portions of the cortex defective perception is induced.

4. Animals in which the parietal regions have been destroyed are permanently awkward in their movements, and defective as regards tactile sensibility. Animals in which the occipital lobes have been destroyed are, as a rule, more demented than those in which only the parietal regions have been destroyed.¹

He pointed to his dog as an illustration of the truth of these propositions ; awkward in all its movements, especially those of the hind legs, but paralysed as to none ; defective in all its faculties of sense, and sensory perception, but neither blind, nor deaf, nor otherwise totally deficient.

Some exceptions were taken by my colleague, Prof. Yeo, and others as to the exact accuracy of Prof. Goltz's description of the extent to which the respective motor and sensory faculties were impaired, but I pass over these as not essential to the matter in hand.

When the brain of the dog was examined by a committee appointed by the section, it was found that the lesions, though extensive in both hemispheres, were less than Prof. Goltz had imagined, and did not implicate the *whole* either of the so-called motor regions, or of the regions of special sense as defined by his opponents.

Over against Prof. Goltz's dog were set two monkeys, in the one of which the greater portion of the region in the left hemisphere called motor had been destroyed many months previously. The animal had remained hemiplegic on the right side from the date of the operation, and at the date of exhibition presented the typical features (*viz.* the semiflexed arm, and rigidly clenched fingers) of hemiplegia with late rigidity as seen in man. The hemiplegia was seen and admitted by all, and the permanency of the result was sufficiently plain to all conversant with the incurable nature of hemiplegia with late rigidity in human patients. It was afterwards proved, by a committee of investigation, that the lesion in this case affected only the

¹ 'Trans. Internat. Med. Congress,' 1881, vol. i, p. 228.

cortical matter and subjacent medullary fibres in the region where it was stated to be ; and the fact of descending degeneration of the pyramidal tracts of the right side of the spinal cord was also established at a later date.

With the exception of the motor paralysis of the right side, the animal was in all other respects perfectly well ; full of intelligence, and acute in all its sensory faculties and powers of perception.

The other animal in which the superior temporo-sphenoidal convolution had some time previously been destroyed on both sides, presented a marked contrast to the first. There was no question as to this animal's motor powers, for it bounded about and climbed hand over hand with the greatest agility and vivacity ; nor was there any question as to its sensory faculties except one, viz. hearing, and in this respect it was admitted to be totally deficient ; for as was remarked by one present, it was the only animal in the room that was not startled by the explosion which I made.

In this condition of total deafness, with retention of all its other faculties both sensory and motor, it had been since the operation and continued so till its death a long period subsequently.

The brain of this animal was exhibited at a meeting of the Physiological Society, and will be described in another place. Suffice it to say that the lesions were confined to the cortical and subjacent medullary fibres, in the regions I have indicated.

It is at once obvious that the second and third of the propositions enunciated by Prof. Goltz, and it was so admitted by himself, are demonstrably erroneous as applied to monkeys, and that in their case the localisation of function was incontestably proved.

To some, however, it seemed as though these demonstrations instead of definitely settling the question of cerebral localisation, had made confusion more confounded. A word or two, therefore, on the lessons taught on this occasion, and on the conclusions to which they logically lead.

Localisation of function being admittedly established in the case of one vertebrate animal; then, given facts of experiment on another animal of the same type seemingly at variance with this law, there is no alternative between either denying the universality of the law, or holding that the facts though seemingly at variance are in reality in perfect harmony with it.

I cannot conceive it possible that in the present state of biological science any one will be found to maintain that animals constructed on the same anatomical type can differ so fundamentally in their physiological constitution as would be implied in the first alternative. We may assume, therefore, that the whole question turns on how, on the principle of localisation, we are to account for such striking differences as are observable between the effects of destruction of the cerebral hemispheres in the different orders of animals.

It would be difficult on cursory examination to say whether a frog or pigeon had had its cerebral hemispheres removed or not. For they each maintain their normal attitude, and the one will hop and the other walk or fly and otherwise respond to sensory stimuli in a manner little, if at all, differing from the normal. The universal powerlessness, however, of even a dog, and much more so of the higher vertebrates after a similar operation would be very apparent.

These facts, admitted by all, clearly demonstrate the existence of great differences in degree among the different orders of animals, in respect to the part which the cerebral hemispheres play in the ordinary modes of activity, and response to various forms of external stimulation.

If it is difficult in some animals to detect the results of removal of the *whole* of the cerebral hemispheres, how much more difficult must it necessarily be to determine the result of removal of only a portion. No one would think of denying a definite function to the spleen, though it is confessedly difficult to determine the results of its extirpation.

The neglect of the most obvious lessons of comparative

physiology has been the cause of much useless and barren controversy :—facts being urged in opposition to other facts between which no antagonism really exists. If to these considerations, founded on comparative physiological research, we add that unless the *whole* of a given cortical centre be destroyed, the portion remaining may suffice for the continuation, in some degree at least, of the function allotted to it, we may arrive at a completely satisfactory explanation of the condition of Prof. Goltz's dog in thorough harmony with the fundamental principle of cerebral localisation.

I have purposely excluded the evidence of clinical medicine in reference to this question ; for the exact solution of problems in physiology by the fortuitous experiments of disease in man is always difficult, and nowhere more so than in the domain of cerebral function. The phenomena of cerebral disease are so complex, the factors at work are so indeterminate, that they seldom admit of exact analysis and computation. But though of themselves clinical observations may be unable to carry rigid conclusions as to cause and effect, all physiological generalisations otherwise founded must square with the facts of clinical observation before they can be admitted as an integral part of the institutes of medicine. And on many points, particularly where subjectivity is concerned, observations on man constitute the only reliable means of interpretation of vital manifestations.

Apart from some differences in matters of detail which do not affect the main principle, localisation of cerebral function is accepted by every recent writer on cerebral disease, as the only possible explanation of the phenomena. Mere numbers and authority, however, will not stand for evidence, and I do not quote them as such. But, as it would be impossible for me to set forth the evidence here, I can only refer inquirers to what I have elsewhere written on the subject, and to the facts contained in the more recent publications of Charcot et Pitres, de Boyer, Grasset, Nothnagel, Exner, and Ross, as well as to the

cases, such as those recently recorded by Sharkey, which are constantly appearing in every medical journal.

Assuming therefore that the principle of cerebral localisation, has become established in physiology and clinical medicine, the ground is cleared for the consideration of further questions of great scientific and practical importance, on which opinion is not unanimous, and on which therefore further research is desirable. The first is, is the localisation of centres a matter of indifference or accident; or is it dependant on structural peculiarities and connections which render each centre as distinct from others, as, *e.g.*, one limb from another, or the organ of vision from the organ of hearing? Or, to put it in the form of a simile, is the brain crust divided into fields each of which though yielding one kind of produce *usually*, may if need be, yield any other; or into fields which yield only one kind of produce, and *can* yield no other; or, according to a still third supposition, is it divided into fields, each of which yields *most* of one kind of produce, but also more or less of every sort.

Each of these three suppositions has its advocates and defenders. If the localisation is merely accidental or indifferent, perhaps the function of a part lost, may be taken on by some or other of those remaining.

If it is structural, this would be as impossible as that the organ of hearing should perform the functions of the organ of vision, or that the nervous apparatus which moves the leg should also move the tongue, or act as the centre of taste.

The question is one which ought to admit of definitive settlement by properly directed research.

The indifferent theory has been adopted by many, under the name of "functional compensation," as affording the most satisfactory explanation of the apparent recovery after destruction of the cortical centres of motion in certain of the lower animals.

If it should appear, however, that in monkeys and still higher animals, it is possible by total extirpation of a

given centre, to cause complete and permanent annihilation of a given faculty of sense or motion, all others remaining intact, it is obvious that the hypothesis of functional compensation is uncalled for.

Those who consider this established, explain the apparent recovery in the lower animals, on the principles already laid down respecting the differences in degree in which the motor faculties, and powers of response to sensory stimuli, are affected by destruction of the cerebral hemispheres in different orders of animals. The so-called functional compensation of the hemispheres is in reality no compensation at all, but only a manifestation of the activity of lower centres.

In addition to the facts of experimental physiology demonstrating the possibility of entirely and permanently annihilating a given function or faculty, others are adduced in favour of a rigid structural localisation of centres.

Though the grey matter of the cortex appears similar throughout, and composed of elements common to every region more or less, yet recent histological researches, particularly those of Bevan Lewis, have succeeded in determining the existence of numerous peculiarities in structural arrangement and collocation in different regions of the cerebral cortex.

The significance of these structural peculiarities is by no means apparent, but that they exist and have a meaning, which may one day become clear, there is no reason to doubt.

Next, and perhaps of more intelligible signification than these histological characters, are the facts, now indubitably established, both by physiological experiment and human pathology, that when certain regions of the cortex, viz. those termed motor, are destroyed, *and no others*, secondary degeneration ensues in the pyramidal tracts, down the whole length of the spinal cord.

The inference drawn from these data is, that just as the motor nerves are connected with the anterior horns of the spinal cord, so the pyramidal tracts of the spinal cord are

connected with certain regions of the cortex cerebri, showing therefore a fundamental difference as regards anatomical connections between one region of the cortex and another.

It is argued also that if the pyramidal tracts had other connections, destruction of other regions than the motor would induce secondary degeneration also,—which is not the case ; and that these connections if they existed should prevent the atrophy and degeneration which occur on destruction of the motor area.

As regards regions related to the organs of sense, it is claimed as an established fact, that the destruction of a certain cortical region on both sides, causes complete and permanent loss of vision, followed in due course by atrophy of the optic discs.

This is relied on as proving that this region is anatomically the cerebral expansion of the optic tracts ; and that the complete abolition of vision, and subsequent atrophy of the optic nerves, show that this region is alone the centre of vision ; and that if the optic nerve has connections with any other region or regions, these are unable to form the substrata of the visual sense, or to prevent wasting of the optic nerve.

No similar observations have as yet been made on the trophic relations, if any, between particular cortical areas and other organs of special sense.

Some cases have however been published in favour of the occurrence of atrophy of certain cortical regions, as the result of long standing deprivation or congenital absence of some limb or organ of sense. Many of them have, however, an extremely slender foundation, and much is required in the way of future research before conclusions can be safely founded on them.

But if succeeding research should establish these facts to be free from doubt and uncertainty, and if all the statements should be substantiated in reference to the permanence of the effects of cortical lesions, and the consecutive degeneration of certain tracts and organs, the question of in-

different or rigid structural localisation will have been definitively settled.

After the fundamental principle of localisation itself however, all other points involved in this, whatever may be their scientific interest, sink in importance, as regards practical aims, in comparison with those relating to the exact delimitation of the respective cortical centres. Here we meet with considerable diversity of opinion among those who have experimentally and clinically investigated the subject. This has been made use of by some, and very illogically, as an argument against localisation altogether. It might be alleged with just as much reason that any differences of opinion among the Fellows of this Society on questions brought before them, are a sign of instability on the part of the Society itself.

I should require a treatise, rather than the limits of a brief sketch, to place before you the various facts and arguments on each head, and I should have to import many things of my own as yet unpublished, for which this is neither the time nor place.

I content myself, therefore, with merely indicating the points in which there is more or less harmony, or the reverse.

The convolutions bounding the fissure of Rolando are, without exception, regarded as having a special relation to the motor powers. Whether motor disorders are invariably caused by lesions in this region is a point of capital importance. If clinical research should establish the existence of an unexceptionable case of total destruction of this so-called motor region without motor paralysis, such a case will do more towards demolishing the whole theory of the existence of motor centres in the human brain than the many hundreds of cases adduced in support of it. But naturally, considering the harmony which otherwise exists between the positive clinical instances and the established data of experimental physiology, we have a right to demand something more than mere vague assertion or crude investigation before a negative instance can be admitted as

proved. None such has as yet been put on record. All the doubtful cases, extremely few in number, bear only on the precise limits of the respective centres, and not on the general question of the existence or not of a special motor area.

The centres of hearing have been shown before the physiological world to be in the superior-temporo-sphenoidal convolutions, and numerous clinical observations have been put on record favoring the existence of a causal relationship between lesions of this region in the left hemisphere, and the occurrence of the defect termed "word deafness,"—a defect in the re-presentative functions of the auditory centres. Nothing, however, comparable to the absolute deafness, presentative and re-presentative, seen in monkeys from bilateral destruction of these regions, has as yet been observed in man.

The centres of vision are still the subject of some differences of opinion; but these have reference not so much to the general position as to the exact delimitation of the anterior boundary, and to the relations of each centre to the central and peripheral portions of the retina respectively. Clinical observations are being published in considerable number demonstrating the occurrence of isolated affection of vision in connection with cortical and subcortical lesions in the occipito-angular region; and the differences between the published views on the exact relations of the centres and organs of vision, are in process of being settled.

The centres of smell and taste are less definitely determined, owing to unavoidable difficulties in experimentation; but the greatest differences of all obtain in reference to the localisation of the centres of common and muscular sensibility. A considerable number of physiologists place them in the so-called motor region; while I have all along held, and hold, both on experimental and clinical grounds, that the centres of common, including muscular, sensibility, are anatomically entirely distinct from those of motion, and situated in a special cortical region.

It is of the utmost importance that this question should be settled, for it involves vital issues in the regional diagnosis and differentiation of cortical from other cerebral lesions. In this respect clinical observation—careful and accurate work—has a transcendent value; and there is every reason to believe that this, in combination with the more precise experimental method, will at no distant date succeed in solving outstanding difficulties, securing general harmony of opinion, and placing cerebral localisation and the regional diagnosis of cerebral disease on a firm and unassailable foundation. In no department of medical science is more activity being manifested; and there is no field more inviting or more likely amply to repay cultivation.

The following words of Marshall Hall are perhaps more pertinent now, than they were to his own time: “In every point of view there is much to be done. Every encouragement should be given to the diligent and devoted investigator; every obstacle, every kind of injustice, every source of disgust and of indignation, should, for the sake of science, for the honour of our institutions, be removed. The physician, who devotes himself to investigation, especially, makes a thousand sacrifices; his path requires cheering, and should not, as it need not, be unjustly obstructed or beset with thorns.”¹

Though the value of scientific investigation is by no means to be measured by its practical utility, yet the value of a scientific fact or principle is enhanced in the eyes of all mankind when it is useful as well as true.

Tested by the standard of practical utility, what, it is asked, has cerebral localisation done, or is it likely to do, towards a more successful treatment of cerebral disease than we yet can boast of?

Up to the present I think that, with a few, though significant exceptions, the benefits of the scientific doctrine of cerebral localisation have been absorbed, like so much

¹ ‘New Memoir on the Nervous System:—On the True Spinal Marrow,’ p. 94.

latent heat, by medical science itself, as distinct from medical or surgical practice.

It has been a lamp to lighten the path of the clinician through darkness almost chaotic; it has sharpened clinical vision, so that now many things are clearly seen which were formerly supposed not to exist; it has given a precision to clinical and pathological descriptions which will be searched for in vain in the older records of cerebral disease; it has cleared our conceptions as to the significance of numerous symptoms, and rationalised many purely empirical generalisations, and is every day bringing us nearer to that which Virchow has termed the goal of modern medicine; viz. the localisation of disease.

But when this has been reached as regards cerebral disease, when we are able to determine the exact nature and position of the *materies morbi*; is it at all likely that we shall stop here? This would be very improbable, even though we should as yet have none, or only the most vague notions in what direction practice might be influenced.

But there are already signs that we are within measurable distance of the successful treatment by surgery of some of the most distressing and otherwise hopeless forms of intracranial disease, which will vie with the splendid achievements of abdominal surgery. What can be more terrible than the agonising headache, the torturing sickness, the racking convulsions, the loss of sight, the progressive paralysis and mental infirmity, and miserable death from a cerebral tumour, which we daily see and foresee, and are powerless to avert by any means known to our art? It is natural that physicians should hesitate to advise surgical operations on the brain, and that surgeons should stay their hands until the principles of diagnosis of reachable disease should have become established with as near an approach to certainty as is possible, where all is hidden from the eye and hand.

But granting this, is there any reason why a surgeon should shrink from opening the cranial cavity, who fear-

lessly exposes the abdominal viscera? The peritoneum was until a very recent date held sacred and inviolable. The dura mater and brain are much in the same position now.

But after what I have seen of the unfailing safety, the freedom from all untoward results as regards health and life, with which the most formidable, and repeated operations can be performed on the brain and its coverings, under stringent antiseptic precautions,—and these on animals of the most delicate, almost human, organisation,—I cannot but believe that similar results are capable of being achieved on man himself.

Apart from secondary inflammation and its consequences, which can be absolutely prevented, there is no risk to life from even extensive destruction of the cerebral hemispheres. It is true that in attempts to remove tumours, or locally treat other forms of disease, we may injure or destroy healthy portions of brain tissue. And what will follow?

Not necessarily mental disorders or appreciable mental defect. With the exception of certain functions arrogated by the left hemisphere, we have for mental purposes practically two brains; and diseases are not always on the left side. The records, and our daily experience of disease and injury of the brain, show that considerable portions of brain substance may be destroyed without great danger to mental stability. But paralysis may ensue, more or less extensive, or defects in special sense, according to the position and amount of the lesion. Such risks undoubtedly exist, as well as others incident to the operation itself, as also to all surgical operations.

But the choice is not between this and any other mode of treatment, but between running these risks and certain death.

And I have no doubt that there are many who, if they had the choice, would go through life, if not enjoying it, halt or hemipic, rather than perish miserably after, it may be, years of incessant suffering.

Ne sutor ultra crepidam! I think I hear remarked.

I applaud the sentiment, but as in this matter I have had the experience of the surgeon, as well as of the physiologist and physician, I have thought that I might venture on these suggestions without appearing to go very far beyond my "last."