

CEREBRAL LOCALISATION<sup>1</sup>

## II.

WE have considered the main positions first taken up by Dr. Ferrier with regard to functional localisations, and it will be convenient to examine in the same order the criticisms and statements of other observers regarding those positions.

(1) *The Rolandic region.*—The effects of excitation and ablation in this region, so far as relates to the production, or the paralysis, of the movements of voluntary muscles, are almost universally admitted, and to this extent the researches of Dr. Ferrier have received brilliant corroboration. But the inference that this region is therefore of necessity motor has not been so generally acceded to. The attacks to which it has been subjected are based, almost without exception, upon a denial of the statement that lesions of this region do not involve the loss or impairment of sensation in the paralysed parts. It is alleged that, on the contrary, the motor paralysees are invariably accompanied by loss or impairment of sensation, either of the so-called muscular sense (Hitzig, Nothnagel; "sense of movement," Bastian), or of tactile sensibility (Schiff, Tripier), or of sensibility in general, muscular and cutaneous (H. Munk); and it has been supposed that the paralysees of motion which result from these cortical lesions are not true motor paralysees, such as would be caused by destruction of a motor centre, but are rather due to the loss of the sensations which guide the volitional movements, or the ideas of such sensations, of which the part of the cerebral cortex removed is assumed to be the seat.

The question seems, on the face of it, one which is easily determinable. Do animals, and especially monkeys, in which a lesion in the Rolandic region has been established, exhibit loss of tactile (or any other form of) sensibility? Are cases of motor hemiplegia in man which are produced by injury or disease of this region accompanied by loss of cutaneous or muscular sensibility, or are they not? As regards animals, many, indeed most, observers answer this question emphatically in the positive sense. As regards man, the evidence is more conflicting. We have, it is true, the advantage of being able to obtain a direct answer regarding the existence, or absence, of sensibility in any particular case; but on the other hand there is not necessarily the same restriction of the lesion to the cortical gray matter, and the exact localisation is much more difficult of determination. Accordingly we find that cases of motor paralysis from cortical lesions in man have been put in as evidence upon both sides, according as they have been accompanied or not by impairment of sensibility. Dr. Ferrier is, however, very positive upon this point, relying upon the accuracy of his own observations in animals, as well as upon evidence derived from pathological observations in man, and the allegations to the contrary are disposed of by him in the following manner:—

"The conclusion that tactile sensibility is lost or diminished after destruction of the cortical motor area is based on defective methods of investigation and erroneous interpretation of the reactions of the lower animals to sensory stimulation. Though an animal does not react so readily to sensory stimulation of the paralysed side, it does not follow that this is due to diminished or absent perception of the stimulus. An animal may not react, or react less energetically, to a sensory stimulus, not because it does not feel it the less, but because it is unable, or less able, to do so from motor defect. . . . All that the experiments of Schiff and Tripier demonstrate is that motor reactions are less readily evoked on the side opposite the cortical lesion. But the same thing occurs in cases of purely motor hemiplegia in man" (pp. 374-75).

<sup>1</sup> "The Functions of the Brain." By David Ferrier, M.D., LL.D., F.R.S. Second Edition, re-written and enlarged. (London: Smith, Elder, and Co., 1886.) Continued from p. 441.

"Strictly cortical lesions of the motor area do not cause anæsthesia in any form, and it may be laid down as a rule to which there are no exceptions that if anæsthesia is found along with motor paralysis the lesion is not limited to the motor zone" (p. 378).

"The total abolition of the muscular sense (as in locomotor ataxy) does not paralyse the power of effecting movements. Even though the impressions ordinarily generated by muscular contraction are not perceived, yet the person can walk or move his limbs with perfect freedom under the guiding sense of vision. Even with the eyes shut the patient can intend his movements with correctness" (p. 380).

"Loss of the muscular sense never occurs without general anæsthesia of the limb. . . . The statements to the contrary, sometimes met with, rest only on the foundation of a demonstrably false hypothesis as to the nature of the ataxy which it is invoked to explain" (p. 380).

"The idea of a movement may be perfect when the motor centres are entirely destroyed. A dog with his motor centres destroyed has a clear idea of the movement required when asked to give a paw, and exhibits its grief at being unable to do so in an unmistakable manner; and the patient suffering from cortical motor lesion, after making futile efforts to carry out his ideally realised movement, not uncommonly bursts into tears at his failure. There is no defect in the ideation, but only in the realisation, of the movement" (p. 383).

"The cortical centres are motor in precisely the same sense as other motor centres, and are differentiated anatomically from the centres of sensation, general as well as special" (p. 393).

Certainly, if it can be shown that a distinct part of the cortex is concerned with the perception of impressions of general sensibility, this would afford strong *prima facie* evidence against the Rolandic region being endowed with sensory functions. And we shall presently see that such evidence is forthcoming.

(2) The evidence for the second proposition (that the visual centre is situated *exclusively* in the angular gyrus) has not found confirmation, and is virtually surrendered by the author. That the angular gyrus is at all concerned in the visual process is entirely denied by H. Munk, who has shown that complete blindness is produced by removal of the *occipital lobes* alone, without the implication of the angular gyri, and that removal of one occipital lobe produces blindness of the corresponding half of both retinae (hemianopsia). According to Munk, this blindness is permanent; but Luciani and Tamburini, who have obtained the same immediate result, affirm that it may after a time disappear. Dr. Ferrier, however, denies that the mere removal of the occipital lobes is followed by any perceptible deficiency of vision; and in support of this statement, which was already made in the former edition, he quotes the results of his own more recent experiments, which were performed in conjunction with Prof. Yeo, and also certain unpublished results which have been obtained by Mr. Horsley and myself. Dr. Ferrier has, however, been mistaken in supposing that our observations bear out his statement, for we invariably found, when an extensive removal was effected in the occipital region, that hemianopsia resulted therefrom, as described by Munk. But in the few experiments which we performed the blindness was not permanent, only persisting, so far as we could judge, for some days, or, at the utmost, weeks; and in one of these cases, in which we *afterwards* destroyed the angular gyrus, hemianopsia which appeared to be permanent was produced. This is confirmatory of the statements of Drs. Ferrier and Yeo. I am myself, however, not at all sure that the permanence of the result was due to the destruction of the angular gyrus, and may not rather have been produced by the more complete removal of the occipital lobe which that destruction

involved. As for the angular gyrus, the author seems now to admit that the blindness of the opposite eye which he has obtained on destroying that convolution is quite temporary, not, indeed, persisting for more than an hour or two after the operation. Nevertheless, he infers that this loss of vision which he describes is due to the fact that the angular gyrus is concerned with the appreciation of *direct* or *central* visual impressions. I have myself failed to obtain evidence either of permanent or temporary visual disturbances as the result of destruction of the gray matter only of one or both angular gyri; and I confess it is to me somewhat surprising that an experimentalist so experienced, and a reasoner so clear-sighted, as Dr. Ferrier, should have attempted to erect a theory of such importance upon a foundation so insecure!

(3) A similar idea arises in one's mind when one considers the evidence which the author has to bring forward of the localisation of the auditory centre in the superior temporo-sphenoidal convolution. Of course, if this be the case, it must follow that bilateral removal of this gyrus will produce complete and permanent deafness. According to Dr. Ferrier, this is actually what happens; but there is only one case followed by complete recovery from the immediate effects of the operation which he is able to quote in support of that statement. This case is that of a monkey which was exhibited to the International Medical Congress in London in 1881, and the animal certainly appeared to be deaf, for it in no way reacted to a loud noise, such as the report of a pistol fired near its head. But, convincing as this test seemed at the time to most of those present, I may here remark that a test of this character is of little or no value when applied to monkeys. For a perfectly normal monkey, if its attention or curiosity is excited in any way, and especially if it is brought into a strange room and surrounded by strange faces, will often give not the slightest sign of perceiving even a loud sound, such as the report of a pistol, when such sound is suggestive of no ideas. On the other hand, a sound which is habitually associated with an emotional idea, *e.g.* the noise made by the approach of a hostile companion, or a footstep which is associated with the expectancy of food, will generally be instantly reacted to. It is true that Dr. Ferrier, in the case mentioned, has not relied entirely upon the negative result obtained from the pistol-report, but expressly mentions other tests as having been applied by him. One remark which he makes is, however, very significant: "Occasionally a doubt was raised as to whether the absence of reaction to sounds was absolute."

I have always been inclined to think that Dr. Ferrier, in localising the auditory centre exclusively in this convolution, has relied too much upon this single case—especially since his deductions therefrom have not been supported by the results of other experimentalists. Luciani, in particular, insists upon the fact that extensive destruction of the temporo-sphenoidal lobe is necessary in order to produce deafness, and that even then the loss of hearing is not permanent. This statement I can myself fully corroborate. I have recently, in conjunction with Dr. Sanger Brown, entirely destroyed the superior temporo-sphenoidal gyrus on both sides in several monkeys, and in not one of them has there been any appreciable loss or impairment of hearing. On the other hand, when the lesion has involved not only the superior gyrus but also the greater part of the lobe there has in one or two instances seemed to be at first, not an entire loss, but a diminution of the power of appreciating auditory sensations—this condition, however, being recovered from after a few days.

I am aware that in locating the auditory centre in the superior temporo-sphenoidal gyrus Dr. Ferrier does not rely alone upon the result of extirpation, but adduces also the movements of the ear and eyes which follow electrical excitation as evidence that a subjective auditory sensation is thereby evoked. Taken by itself this is no

evidence at all, for similar movements may be obtained from excitation of totally different portions of the cerebrum, to say nothing of the cerebellum and of the lower nerve-centres. It only becomes evidence as corroborating the effect of extirpation. But a single "negative instance" is sufficient to overthrow the hypothesis that the auditory centre is situated in the superior temporo-sphenoidal convolution alone, and would outweigh many "positive instances." We have, however, only the one well-recorded "positive instance" of Dr. Ferrier (and this was not altogether free from doubt) as against several "negative instances" (those of Munk, Luciani, and ourselves; which last have not yet been published, and could not, therefore, be taken into account by Dr. Ferrier). It is probable, therefore, that Dr. Ferrier's inference is too exclusive, and that other parts of the temporo-sphenoidal lobe must be included in the auditory centre.<sup>1</sup>

(4) The view that tactile sensibility is localised in the hippocampal region has naturally been attacked by those who hold that it is to the Rolandic region that the perception of this and other forms of sensibility are to be referred. It would not appear, however, that they have taken the trouble to repeat Prof. Ferrier's experiments upon this region, so that his position can hardly be said to have been seriously assailed. On the other hand, it has received both corroboration and extension from the experiments of Mr. Horsley and myself, the results of which were shown to Dr. Ferrier, and the conclusions arrived at fully concurred in by him (pp. 340-45). These experiments showed in the first place that extensive destruction in the hippocampal region, especially of the posterior part of the hippocampal gyrus, is followed by hemianæsthesia, which is not, however, of a permanent character, but disappears after a few days; and further, that destruction or injury of the gyrus fornicatus (which, as Broca showed, is to be regarded as a direct extension around the corpus callosum of the hippocampal gyrus (see Fig. 2), produces still more marked and far more permanent symptoms of a like kind.

(5) and (6) With regard to the cerebral localisation of the functions of taste and smell, the author in this edition brings forward no new proofs of an experimental nature. But he adduces and quotes evidence from comparative anatomy to show, not only that in animals in which the sense of smell is largely developed (the "osmotics" of Broca) the hippocampal lobule is greatly developed, but also that the development of the anterior commissure, especially of its posterior division, goes hand in hand with that of the hippocampal lobule, and its internal extension, the nucleus amygdalæ, and is therefore to be regarded as a commissure of the olfactory centres. The evidence in the first edition regarding the localisation of taste-perceptions was of the scantiest description, and has been in no way subsequently strengthened, and it is necessary that further experiments should be made upon the subject with the view of testing the opinion which the author has with all caution put forward on the subject.

(7) Upon the special functions of the pre-frontal lobes, or whether any function is in fact specially concentrated in this part of the brain, very little light has been thrown by the researches of the past fifteen years. There is a very prevalent idea that intellectual capacity goes hand in hand with the development of this region, an idea which has existed from the time of the old Greeks, although it was not apparently shared by peoples of yet more ancient civilisation. The idea does not, however, appear to receive any confirmation from the experimental method.

<sup>1</sup> Dr. Ferrier is mistaken in supposing (*vide* p. 310) that the results of the experiments of Mr. Horsley and myself confirm his conclusions regarding the localisation of the auditory centre in the superior temporo-sphenoidal gyrus. The error seems to have arisen from the misunderstanding of a verbal communication. What we did find in one or two cases was that the whole of the temporo-sphenoidal lobe exclusive of the superior gyrus might be removed on both sides without loss of hearing—not the converse, that hearing was abolished on destroying only the superior gyri on both sides. Indeed, we did not in any single instance perform this last experiment.

Animals from whom these lobes have been removed exhibit "a total absence of symptoms" (p. 396). "In my first series of experiments (carried out without antiseptics), I noted, after removal of the prefrontal regions, a decided alteration in the animals' character and behaviour. . . . They had lost, to all appearance, the faculty of attentive and intelligent observation" (p. 401). But that this was due to an extension of the effects of the lesion consequent on the want of antiseptic precautions appears from what immediately follows:—"In some of my latest experiments, in which the lesions were strictly limited (under antiseptic precautions) to the pre-frontal regions, I could not satisfy myself of the existence of any appreciable mental deterioration. . . . A similar total absence of discernible symptoms has been observed also by Horsley and Schäfer" (p. 396).<sup>1</sup>

On the other hand, Dr. Ferrier believes that he has in one or two instances obtained unequivocal evidence that the whole of the pre-frontal lobe is concerned with the movements of the head and eyes, being an extension forwards of the centre for those movements which he had previously described. Nevertheless, he quotes approvingly certain observations of Hitzig and of Goltz upon dogs in which this region had been destroyed upon both sides, and which appeared in consequence to exhibit weakness of memory and lack of attention, without any paralysis of movement or sensation, as tending to confirm, what the comparative study of the relative development of the frontal lobes in different animals and individuals appears to show, "that the frontal lobes, the cortical centres for the head and ocular movements, with their associated sensory centres, form the substrata of those psychical processes which lie at the foundation of the higher intellectual operations" (p. 467). The qualification which I have italicised takes away the whole point of the statement so far as relates to the region under discussion. And a single well-recorded instance in man (such as the celebrated American crowbar case), in which there has been extensive destruction of this region without the occurrence of any appreciable symptoms during life, renders it manifest that there can be no restricted localisation of any special function in this part.

"Munk professes to have found that after destruction of the pre-frontal region in dogs and monkeys, paralysis occurs in the muscles of the trunk on the opposite side. . . . My own experiments, as well as those of Horsley and Schäfer, disprove Munk's assertions in the case of monkeys," and "in regard to dogs they are flatly contradicted by Hitzig, Kriworotow, and Goltz." Moreover, "Horsley and Schäfer have shown that the centres for the trunk-muscles" in the monkey "are in the marginal convolutions" (pp. 400-401). It is not a little curious to observe how in the desire to conform to the prevalent view regarding the frontal region being the special seat of intelligence, both Ferrier and Munk endeavour to prove that the different movements which they respectively associate with this region are particularly related to the development of the intellectual faculties. Munk even goes so far as to assert that the development of the *trunk-muscles* in mammals marches *pari passu* with the evolution of the intellectual capacity. One is surprised that he has not carried the comparison yet further, and drawn attention to the relation between the "wisdom of the serpent" and the complexity of the movements of the reptilian trunk!

The amount of space which it has been necessary to occupy in discussing the question of cerebral localisation may be justified, not only by its general interest and importance, but also by the fact that the author of this work is one of the most prominent exponents of a doc-

<sup>1</sup> Further on (p. 402) the author states that we have noted signs of stupidity in the monkeys in which we had removed the pre-frontal regions. I do not think, however, that such dullness as was exhibited in one or two of these cases was more marked or lasted longer than with equally extensive lesions of other parts of the brain.

trine which, whether wholly or partially right, has revolutionised cerebral physiology and profoundly modified the department of medicine with which this branch of physiology is linked. We can consequently only refer very briefly to some of the principal alterations and additions which we find recorded regarding other subjects.

The structure of the nerve-centres is treated at much greater length than in the former edition, and is copiously illustrated with many original microscopic drawings by Mr. Bevan Lewis and others. The conducting functions of the spinal cord, which were somewhat cursorily dismissed in the former edition, are here considered at length. The view of Brown-Séguard that there is a differentiation within the cord of the paths for different forms of sensibility is subjected to a searching criticism, with the result that the existence of such tracts is entirely rejected by Dr. Ferrier. He, however, nowhere refers to the question of specific paths for temperature-sensations, a question which has become one of much importance in connection with the recent researches of Blix, Goldscheider, and others on the differentiation of specific cutaneous points for these and other forms of cutaneous sensibility. To the question of the existence of a "muscular sense," by which is meant that faculty by which we are aware of the position and movements of our limbs without calling in the aid of our visual perceptions, Dr. Ferrier brings forward a considerable weight of argument to prove that it is not to be regarded as in any way a specific form of sensation, and still less a sense of effort or innervation produced by the appreciation by the sensorium of centrifugal discharges which are emitted from motor centres (Bain, Wundt), but that it is merely the result of impressions of tactile sensibility conveyed by the ordinary sensory or afferent nerves both of the muscles and of the parts acted upon by them, and, as such, can have neither a specific path of conduction nor a central terminus apart from the paths and termini for tactile sensibility.

The functions of the spinal cord as a centre for co-ordinate movements are also treated more fully than before, and it is shown that even in the higher animals each segment of the cord may act as a co-ordinating centre for complex and apparently purposeful movements of the limbs. For it has been demonstrated in monkeys by the author, working conjointly with Prof. Yeo, and in dogs by Bert and by Marcacci, that such movements may be evoked by the excitation of single anterior roots in the cervical and sacral regions. And Dr. Ferrier describes one or two experiments, in which he succeeded in stimulating the anterior cornu of the gray matter alone, and which yielded similar results (*vide* note to p. 77). In relation to the functions of the cord, the tonus of the muscles and the so-called "tendon-reflexes" are carefully considered, and their importance as an expression of the condition of the reflex are pointed out. Many new facts are accumulated regarding the remaining parts of the central nervous system, and their bearing upon the functions of the several organs is gone into in several instances with great care and at considerable length. To most of these it is impossible to refer particularly. It may, however, be noted that the direct excitability of the corpus striatum, at least of its caudate nucleus, which has been denied by Franck and Pitres, is positively re-affirmed, and the motor functions of that organ maintained, by Dr. Ferrier, as the result of new experiments performed by him. But, whether or not it be the case that they are directly excitable, it would appear that the precise functions of the basal ganglia, and the relation which they bear to motion and sensation, are as much a matter of conjecture as ever.

To the chapter which deals with the cerebral hemispheres from the psychological aspect one or two important additions have been made, especially in the part

devoted to the consideration of speech, in which the conditions of "word-deafness" and "word-blindness" are now discussed. The view which has been put forward, amongst others, by Dr. Hughlings Jackson, that there are other and higher centres, over and above those which have been demonstrated by physiological and clinical research, which form the substrata of the higher mental operations, does not receive support from Dr. Ferrier. "It seems more reasonable to believe that there may be higher and lower degrees of complexity in the same centres than to assume the separate existence of more highly evolved centres, for which no evidence is obtained by the results of experimental research" (p. 460).

It might have been expected that the remarkable conditions of the cerebral functions which are met with in both man and animals in an hypnotic state would at least have been alluded to in this edition, but they appear to have entirely failed to attract the author's interest, and the subject is passed over in silence.

In conclusion it may confidently be affirmed that, whatever exception may be taken to this or that statement or opinion, or to the mode in which this or that question is discussed, this new edition of Dr. Ferrier's work, from the care with which it has been revised, the extent of the information which it contains, and the clearness of style and lack of ambiguity which characterise its every page, must prove of the greatest value to the student of neurology, and cannot fail to enhance the high reputation of its author.

E. A. SCHÄFER

#### THE VALUE OF THE NEW APOCHROMATIC LENSES

AT the annual meeting of the Royal Microscopical Society, the Rev. Dr. Dallinger, who was elected President for a fourth year, delivered his annual address, in the course of which he gave a judgment concerning the new object-glasses made with the new German glass, in the following terms:—

In proceeding to fulfil the honourable duty that, by your courtesy, devolves upon me, I purpose in the main to follow the line I have taken in preceding years. I congratulate the Society on its work, and on its steady influence in promoting progressive improvements in the optical and mechanical construction of the microscope, devoid of all prejudice as to how, or from whence, such improvements may come. And whilst, happily, it is not of necessity a President's duty to pass in cursory review the microscopical work of the year, there are times when it may be well for him to review the points of improvement that have been made in the instrument itself.

For the past twenty years I have had an increasing interest in the continuous improvement of the optical appliances of our instrument—an interest which, from the first, applied not only to objectives, but also to eyepieces and condensers, which consecutive calculation, thought, and experience have shown to have a correlated importance.

Eighteen years ago I had, by practice, made myself fairly master of a  $1/25$ -inch objective of that period made by Powell and Lealand. I still possess that lens, and it is as good a lens of its class as they ever constructed. Soon after, I became equally familiar with a  $1/50$ -inch of the same class by the same makers.

By saying that I became master of these lenses, I mean that I discovered exhaustively what they would and what they would not do. By this, I learned definitely what I wanted in lenses, if I could get it; and to get that has been my unceasing endeavour until now. And certainly the quest has not been vain. And my method has been to examine impartially, and possess myself of, English, Continental, or American lenses, whenever they have shown any capacity for doing best what my work proved to me required to be done.

I know that, in estimating the quality of a lens by the class of image it affords of certain test-objects well known to us, a certain amount of empiricism must take place. We do not absolutely know the image it ought to present. But this only applies within very narrow limits. Take the Podura scale: I can give you an image of it with my  $1/25$ -inch and  $1/50$ -inch of twenty years ago. What I, in common with most microscopists, considered then the *best* result, the most sharp, clear, and delicately defined image, with those lenses I can get now; but, with those lenses, nothing better.

But the elements—the essential features that constituted the quality of beauty in that image—are the very elements, the actual features, that every admitted improvement in our object-glasses has brought out more perfectly. So that if I now put, say, the Podura scale under my old dry  $1/25$ -inch objective, and, beside it, another precisely similar scale under a new homogeneous  $1/20$ -inch objective of N.A. (numerical aperture) 1.5, the very qualities of the image which I, and experienced microscopists generally, thought the best twenty years ago are incomparably transcended in beauty and perfectness now.

But that is not, and has not been, my only or my chief test. It has been one more eminently practical, so far as my own work went; at least for some years.

Up to ten years ago, although I had spent weeks in patient effort, no lens that I possessed, or that was within my reach, could be made to reveal the flagella of *Bacterium termo*. The flagella of many minute monads and of such Bacterial forms as *Spirillum volutans*, and even *Bacterium lineola*, I could demonstrate, though some of them with difficulty; but not a trace of that of *B. termo*. But, near that time, Powell and Lealand produced a battery of immersion-lenses on a new formula and of much relative excellence; and with these lenses the flagella of *B. termo* were brought within the range of sight.

Since that time that has been a good lens, to me, in proportion to the greater or less ease and perfection with which it has revealed this delicate fibre. And let me say that such lenses as do this are those that always, without fail, give us the best ideal image of Podura scales and other tests. You will pardon me, I trust, for this amount of personal reference, since it will give a greater relevancy to what will follow.

Improvements of great optical importance have been made during the last few years. The manufacture of homogeneous lenses by Messrs. Powell and Lealand gave us the opportunity, which we could not have with foreign makers, of urging certain modifications. The addition of the correction collar was a minor, but still important, point. But the great point was the increase of the N.A. These makers have shown themselves most anxious, and have spared no efforts, to reach the highest aperture yet attained.

Advancing, say, from N.A. 1.25, they attained to 1.35 in such powers as the  $1/25$ -inch and the  $1/50$ -inch; subsequently to 1.47 in  $1/8$ - and  $1/12$ -inch objectives; and finding these, from my working point of view, of such supreme gain, I urged them still on, and was ultimately rewarded by the possession of a  $1/6$ -inch N.A. 1.5, followed by a  $1/12$ - and a  $1/20$ -inch foci of the same great aperture. From each of these I obtained special advantages over all like powers, but with lower apertures, within my reach.

A question frequently asked may be asked again, In what way do these last increments of aperture aid us? The practical answer is not difficult. Speaking from observation, I may say that all the objectives I have employed for the most critical work fail to produce images by the extreme marginal zone of the aperture. It is the judgment of competent judges that it will be fair to roughly estimate this defective outermost zone at 10 per cent.; so that, from the total measurement of the aperture by Prof.