

# B R A I N .

JULY, 1888.

## Original Articles.

### ON THE FUNCTIONS OF THE TEMPORAL AND OCCIPITAL LOBES: A REPLY TO DR. FERRIER.

(With two Plates.)

BY E. A. SCHÄFER, F.R.S.

I SHOULD have been content to pass Dr. Ferrier's polemical article in the last number of 'BRAIN' in silence,<sup>1</sup> were it not that the author directly contravenes the accuracy of the descriptions which I have given of the post-mortem condition of certain of the brains which had been operated on by Dr. Sanger Brown and myself, and indirectly accuses me of falsifying the representations which I have given in my paper:—"But Professor Schäfer's figures (Figs. 5 and 6) of the brains of these monkeys must be regarded more as a sort of pictorial representation of his own idea, or wish, as to the extent of the lesions, rather than of the reality. Having myself examined both preparations, I most emphatically deny the accuracy of Professor Schäfer's diagrams. In neither the one nor the other were the superior temporal gyri completely destroyed: considerable portions remaining absolutely intact in both hemispheres, and notably so in the brain which Fig. 6 professes to represent."

Now it happens fortunately that the brains in question are still existent. It also happens that, in spite of the fact that they have been sliced in order that the depth and extent of the lesions might be more accurately gauged, they are capable

<sup>1</sup> "Schäfer on the Temporal and Occipital Lobes," by David Ferrier, M.D., F.R.S.: 'BRAIN,' April 1888.

of being pieced together and photographed so as to show accurately what is really the extent of the lesion in each case. And from the photographs, which I here reproduce (Figs. 1 and 2), it is clear that Dr. Ferrier (whether owing to the fact, that the examination which he made was hasty and superficial, or whether he was unconsciously influenced by preconceived opinions) has committed a serious inaccuracy, in stating that "considerable portions of the superior temporal gyri remain absolutely intact in both hemispheres, and notably so in the brain which Fig. 6 professes to represent."

Before I allude to the photographs at greater length, I may be permitted to spare the reader the trouble of referring back to the original paper, by reproducing the paragraph in which I have described the characters of the lesions:—

"In six monkeys we have more or less completely destroyed the superior temporal gyrus on both sides. I say more or less completely, because *in one or two a small shred of grey matter belonging to this convolution was found post mortem*, but practically the lesion was complete in all six, some of the grey matter within the fissures bounding the gyrus being all that could be taken to represent the convolution, and even this *being deprived of its medullary centre*. . . . But in order to make assurance doubly sure, we in one monkey . . . separated up the fissures bounding the gyrus and scooped it out entirely from the very bottom of the fissures, so that not a trace of the convolution in question should remain (Fig. 5)."

Now, except for this brain, it will be seen that I have not stated that every trace of the superior temporal gyrus has been removed in the six instances quoted, nor is it so represented in my diagrams. On referring, for instance, to Fig. 6, it will be seen that a very narrow strip of the gyrus lying next the Sylvian fissure is represented as having been left. But even this might in a diagram (which is all that the illustrations profess to be) with complete justice have been represented as being removed, for *as the sections show* it is only a shred of grey matter in the depth of the fissure that is left, the rest of the grey matter and the white matter belonging to it having been completely removed along with the remainder of the gyrus and even the remainder of the lobe-

How complete the removal has been is so evident from the accompanying photographs (Figs. 1 *a* and 1 *b* from the two sides, and Figs. 1 *c*, 1 *d*, 1 *e*, 1 *f*, and 1 *g* in section, the sections passing vertically through the lesion, at various points from behind forwards) as to require no words of mine to accentuate.

On the other hand, I have distinctly stated that in the brain represented in Fig. 5 precautions were taken to remove every trace of the superior temporal convolution on both sides.

Dr. Ferrier, on the other hand, boldly asserts that "considerable portions remain absolutely intact in both hemispheres." What do the photographs show?

An observer unskilled in the examination of cerebral lesions of this kind would, I think, come to the same conclusion as Dr. Ferrier has done, from the superficial examination to which alone he appears to have subjected this brain. There is certainly, especially well marked upon the right side (see the photograph, Fig. 2 *a*), a convolution or what appears to be such, exactly in the situation of the superior temporal, although somewhat less prominent than that well-marked gyrus. But if we observe the sections (Figs. 2 *c*, *d*, *e*) carefully, the convolution in question turns out to be the island of Reil! The superior temporal gyrus has in truth disappeared, and, as is frequently the case with these artificial cerebral lesions, the underlying parts have tended to be protruded towards the surface. Only at the extreme tip of the lobe is there a shred of grey matter left belonging to this gyrus, and that shred is devoid of medullary centre (Fig. 2 *e*, on the right side of the section).<sup>1</sup>

<sup>1</sup> The diagrams which I gave in my paper, although only outlines, accurately represent the superficial extent of the lesion in each case. The paper itself being merely a résumé of a longer and more detailed account of the experiments, the joint production of Dr. Sanger Brown and myself, which was read before the Royal Society on Dec. 15, 1887, and is shortly to appear in the 'Philosophical Transactions,' I did not judge it to be necessary to illustrate the abstract by more elaborate drawings. All the figures which Dr. Brown and I have published of the brains which we have operated on (and the same remark applies to those operated on by Mr. Horsley and myself) have been executed with the greatest care and fidelity to nature by my assistant Mr. E. P. France, who has in every case first traced out upon a glass plate placed over the brain all the outlines of the convolutions and the exact extent of the lesion, and has then transferred the tracing to paper and elaborated the drawing. By this

Having been forced, sorely against my inclination, to take up my pen in vindication of the accuracy of the facts which had been impugned by Dr. Ferrier, I may perhaps be permitted to indulge in a short notice of one or two of the other points which my critic has raised. And first let me notice that he evades the main argument regarding the superior temporal convolution, and endeavours to conceal the evasion, by raising a side issue as to whether there is any shred of the grey matter of that gyrus remaining in any of the cases recorded by Dr. Sanger Brown and myself. My argument was this:— In a *single instance* recorded by Dr. Ferrier (in conjunction with Professor Yeo) what purported to be complete destruction of the superior temporal gyrus on both sides<sup>1</sup> was followed by complete loss of hearing, which lasted for thirteen months (“*manifestly totally deaf*” are the words used<sup>2</sup>). In *six cases* recorded by us, in every one of which the removal was as complete as—probably more complete than—in the monkey described by Ferrier and Yeo, hearing was not only not permanently abolished, it was not permanently affected so far as it was possible to determine in monkeys. In one at least of these six cases the convolution in question was not merely cauterised, but was radically removed on both sides with exactly the same result. What, then, is the natural inference

---

means a representation is obtained as faithful as any photograph, and far clearer; because it commonly happens, when the superficial grey matter only has been removed, that the subjacent cerebral substance becomes pushed up towards the surface, and the original depression is thus obliterated. Thus there is a want of the necessary contrast of light and shadow requisite for a clear photograph, and it is on this account that so little can be made out regarding the extent of lesion in many of the photographs which illustrate the paper by Drs. Ferrier and Yeo, in the ‘Philosophical Transactions’ for 1884. In the two cases represented in this paper, however, I have easily succeeded in obtaining clear photographs, because the lesion has been so profound as to leave a considerable gap, in spite of a certain amount of pushing up of the subjacent cerebral substance which has occurred.

<sup>1</sup> I here reproduce (Figs. 3 a and 3 b) Dr. Ferrier’s photographic representations of the brain of the monkey which he described as “totally deaf,” in order that they may be compared with ours, and because they plainly show that the destruction of the superior temporal gyrus was the reverse of complete. This is still more obvious in the sections (‘Phil. Trans.’ 1884, Plate 22, Figs. 25 to 38) which show that upon the right side nearly half the convolution remains untouched.

<sup>2</sup> Ferrier and Yeo, p. 506. The italics are mine.

to be drawn from a comparison of these results, but that the auditory faculty is not *localised* in the superior temporal gyrus? <sup>1</sup>

Now this position is not touched by any of the arguments which Dr. Ferrier has brought forward. His statement, that in our experiments considerable portions of this convolution have been left, the photographs clearly show to be erroneous, at least for the two cases which have been directly called in question, and it would be not less erroneous for the other four cases which we brought before the Royal Society. And even if it be admitted, that shreds of grey matter have been left here and there, they do not in any case amount to as much as remained in the case of the monkey which Dr. Ferrier supposed to be deaf: this we may gather both from the authors' own admission as well as from the photographs they give both of the whole brain (which I have reproduced) and of sections through the lesion. Dr. Ferrier's other arguments consist, in the first place, of an appeal *ad misericordiam*, to the effect that if we do not localise the auditory sense in the temporal lobe (and preferably in its superior gyrus) there is nowhere else to put it, all the remaining parts of the brain being already tenanted; and, in the second place, of the citation of two clinical and pathological records, which are introduced, with a blast of trumpet, as "cases of transcendent importance."

One of these, reported by Wernicke and Friedländer (in 'Fortschritte der Medicin,' 1883, p. 177), was of a syphilitic

<sup>1</sup> When we find a statement made to the effect, that a function is *localised* in a particular part, the statement, if it means anything at all, surely implies that it is connected with that part to the exclusion of others. This is the sense in which the expression has been used in describing the localisation of functions in the brain, and of this no one can be better aware than Dr. Ferrier himself. In this sense Dr. Ferrier has endeavoured to prove that the *cerebral auditory faculties are localised in the superior temporal gyri*. But if it is shown, even in a single instance, that these gyri can be entirely removed without loss of hearing, it is certain, whatever may be the amount of negative evidence brought forward on the other side, that those gyri cannot be the sole seat of auditory perceptions; in other and looser phraseology, cannot be *the* auditory centre. The case is put very clearly and forcibly by the late George Henry Lewes, in a review of the first edition of the "Functions of the Brain" in 'Nature,' 1876: "Whenever a function persists or reappears after the destruction of an organ, this is absolutely conclusive against its being the function of *that* organ."

growth in each hemisphere, in a woman, long the subject of epilepsy, and later on of hemiplegic attacks on both sides. Other symptoms appeared gradually, including deafness, imperfection of speech, and general stupidity, so that the patient was looked upon as of aberrant intellect.

Although he professes to give the particulars of this case at some length, all that Dr. Ferrier says about the condition of the brain is that "an extensive lesion was found in each temporal lobe, invading the superior temporal convolution on both sides (see figures accompanying paper). The rest of the brain exhibited no abnormality." On referring to these figures and to the authors' description, I find the conditions to be very different from that which the above account would lead one to suppose. In the first place, the lesion (a gummatous infiltration) is not symmetrical on the two sides. On the left side it involves much more of the temporal lobe than on the right, but even in the former the anterior third, if not the anterior half, of the lobe is normal. On the right side the tumour, equal to about a shilling in diameter, occupies the junction of the parietal, occipital, and temporal lobes, but involves very little indeed of the latter, and hardly any of the superior convolution. But the authors state that the new formation extended on both sides in the depth, so as to embrace the part of the corona radiata with which the temporal lobe is connected, and they are of opinion that (not the superior convolution only, but) the whole lobe was thereby put out of function by it. (No figures illustrating the depth of the infiltration accompany the paper.)

The authors infer that the temporal lobes are the central seats of the auditory sense. Dr. Ferrier, however, would go further, and say that this case is "entirely in harmony with my own more strict localisation of the auditory centre in the superior convolution." One would have felt grateful had Dr. Ferrier pointed out how this "harmony" is maintained. I at least fail to see the connection! For as we have just seen, *not the superior convolution merely, but the whole temporal lobe* was cut off by the tumour from its deeper connections; and it is certain, from the mental condition of the patient and the pareses which were observed, that parts of the brain other than

those which were the actual seat of or were cut off by the new growth must have been involved.

The other case is one reported by Dr. Shaw, who himself is careful to remark concerning it, that "it is not strong evidence of this localisation of centres of hearing and vision," and is otherwise very guarded in the inferences he draws from it. Even Dr. Ferrier regards it as "not being absolutely conclusive." Under these circumstances I may perhaps be excused treating it as a serious item in the argument.

"Cases of transcendent importance" appear to be rare, and Dr. Ferrier must fall back upon his experiments on monkeys. And upon these he has thrown a flood of light by the publication of the notes which he gives at pp. 13 to 16 of his article. To any impartial mind these show conclusively, that the monkey which was reported in the 'Philosophical Transactions' as having been for thirteen months "manifestly totally deaf," was far removed from that condition. "Animal occasionally turned its head" (towards sounds). "Occasionally it turned its head coincidentally with the noise." "Every now and then doubt seems raised by the animal looking at the same time a noise is made." "More than doubtful." "Signs of reaction to sounds are less evident" (this was a month after the operation; they were therefore evident before). "Occasionally it is judged to hear." "Occasionally, as before, there was an appearance of a start or a look round apparently conditioned by the noise" (this was thirteen months after). It is unfortunate that these remarks were not published at the time the case was brought before the notice of the physiological world. They would have permitted conclusions to be drawn other than those arrived at by the authors, and we should have had one error less to eradicate from our notions regarding the functions of the brain.

Dr. Ferrier (pp. 9 and 10) draws attention to "the difficulties that have to be encountered, and the fallacies that are apt to be committed, in investigating the sense of hearing in the lower animals. Care must be taken to discriminate between the sense of hearing proper, and mere reflex reaction to sudden, sharp, or loud sounds, a reaction not incompatible with real deafness. Mere coincidences in animals keenly on

the alert to everything going on around them, must be scrupulously eliminated. The tests of hearing should be such as to entirely avoid attracting the animal's attention by other channels of perception, such as sight, imitation of other animals, a sense of proximity conditioned by vibration, agitation of the air, and such like."

We can now see by these notes that he has given to us how he has himself carried out his precepts. For we read that this identical animal "pays no attention to sounds—*tapping, scratching, &c.—on side of cage,*<sup>1</sup> which alarm its companion towards either side. This was carried on repeatedly for the space of an hour without eliciting any sign of perception."<sup>2</sup> "July 5th. Made several experiments with electric bell *placed against side of cage.*" "July 7th. I made repeatedly a *scrapping sound on floor* with foot as if going up to cage." "Dec. 12th. Does not pay any attention to *scratching on cage.*" The animal therefore did not respond to *vibrations which must have been transmitted to the parts of the skin in contact with the cage!* Why has not Dr. Ferrier localised cutaneous perception also in this gyrus? We read that on "March 31st. While sitting to-day having its back scratched a pistol cap was exploded close to cage: not the slightest start, though the others and Mr. N., who was scratching the monkey, made a sudden start." Cannot Dr. Ferrier see that he is trying to prove too much? Mr. N. is scratching a monkey and suddenly starts, but even under these trying circumstances this remarkable monkey remains absolutely quiescent! Dr. Ferrier is capable of being amused (p. 17, line 1) at my comments on the above record;<sup>3</sup> but I think he

<sup>1</sup> The italics are mine.

<sup>2</sup> We are not surprised to hear that afterwards, "as soon as I come within sight, it looks interested."

<sup>3</sup> I here desire to enter a protest against remarks being attributed to me which convey an entirely erroneous impression of my argument. Dr. Ferrier must be aware that to publish statements as having been made in any discussion before a scientific society, of which no short-hand report has been taken, and without having previously submitted them to the person to whom they are attributed, is a most unusual proceeding, and one which is easily capable of creating a false impression of the opinions enunciated; and I regret that he has not hesitated, for the sake of endeavouring to secure a momentary argumentative triumph, to break through the salutary rule which prohibits such irregular publication. His adoption of this course necessitates my here recording what it was I really did



need hardly have gone so far to seek amusement: there is plenty to be obtained from the record itself. But Dr. Ferrier tells us that he does not rely upon this one case alone; there are others from his earlier experiments (recorded in the 'Philosophical Transactions' for 1875) which he regards as corroborative of this one, if not of equal importance in the chain of evidence for his localisation of hearing. I can only express surprise that even their author should continue to attach the least importance to the experiments upon the special senses which are recorded in the paper referred to. So far as I know, not a single physiologist who has repeated them has been able to corroborate Dr. Ferrier's results, or to accept the conclusions which he was led to deduce from them. What scientific value can be attached to statements regarding the apparent absence of certain perceptions in animals which have hardly recovered from the stupor of an anæsthetic drug, or in which active inflammatory processes are going on in the cerebral substance? The danger of drawing conclusions from experiments made under these conditions is obvious, and is in no way better exemplified than in the fact, that their author has found it convenient, in certain cases, to rely upon their crudity, and tacitly to abandon positions which, trusting to their results, he had deliberately assumed.<sup>1</sup>

Feeling to all appearance the weakness of his case for the localisation of all auditory perceptions in the superior tem-

---

say in the few remarks which I made at the meeting referred to, in contrast to what has been attributed to me. Dr. Ferrier states that, after hearing the above notes respecting his monkey, I suggested first that they showed that the animal was not really deaf, and then, "almost in the same breath," suggested that the monkey was stone-deaf, not only after but before the operation. In making this statement, Dr. Ferrier has perverted both the order and the sense of my remarks. *Before the notes were read* I said that our six cases proved to demonstration, that if Dr. Ferrier's monkey were really stone-deaf after the operation (which I did not myself believe to be the case) he must by accident have lighted upon a monkey which was stone-deaf before the operation. *After the notes were read* I said that it must be perfectly clear to all who heard them, that the monkey was not really deaf at all. And I think the excerpts which I have above given will render this equally clear to my readers.

<sup>1</sup> *E.g.*, the localisation of appetite for food in the occipital lobes: 'Functions of the Brain,' 1st edition, 1876, p. 193.

poral gyri, Dr. Ferrier proceeds to invoke the evidence of two experimental observers who (as probably they themselves, in common with all the rest of the world, believed) have arrived at results which are diametrically opposed to this conclusion. It is 'certainly astonishing to hear the names of Munk and Luciani called as witnesses in favour of Dr. Ferrier's views on this point. Munk, in dogs, extends the area for perception of auditory impressions certainly over the whole of the temporal lobe—he distinctly states that he was not able to obtain similar evidence in monkeys; and Luciani did not succeed in obtaining permanent deafness with complete destruction of the cortex of the temporal lobe, even including the *cornu ammonis*. When, therefore, I stated that the supposed *localisation* (using the word in the sense above defined, which appears to me to be the only legitimate use of it) of the auditory perceptive faculties in the temporal lobe *in monkeys* (and *à fortiori* in the superior gyrus) has no experimental evidence in its favour, I merely recorded a demonstrable fact, and one which is still more patent, since the opportune publication of the notes on the monkey which was described as "manifestly totally deaf," but which nevertheless "evidently reacted to sounds."

Before leaving this subject of the superior temporal gyrus, it is necessary that I should refer—as briefly as may be—to Dr. Ferrier's statement, that I have charged him with misrepresenting me.

The statement is based upon the following note in the joint paper by Mr. Horsley and myself,<sup>1</sup> which is referred to by Dr. Ferrier, and which I think it will be well to give in full: "Dr. Ferrier is mistaken in the statement ('Functions of the Brain,' 2nd edition, pp. 310, 311), that we have been able to corroborate his observations upon the localisation of this (auditory) function in the superior temporo-sphenoidal gyrus in monkeys. We have so far neither obtained any distinct corroboration nor refutation of them, but regard the question as still open."<sup>2</sup> And in the text (same page) we say, speaking

<sup>1</sup> 'Phil. Trans.,' 1888, B. p. 20.

<sup>2</sup> I had myself previously made a similar statement in a review of the 2nd edition of the 'Functions of the Brain' in 'Nature,' 1887, p. 465, in these

of the temporal lobe, "We have not as yet systematically pursued the question of the cortical localisation of auditory sensations, although it is right to state, that such incidental observations as we were able to make upon this function have almost uniformly proved negative. We have not as the result of any bilateral operation been able to convince ourselves that deafness has been established in a single case." And in a note to this we remark, "It is right to add that we were not specially intending to investigate this point (the localisation of auditory perceptions), and that the lesions of this lobe which we record were performed with another object, viz. to arrive at the hippocampal region. But, as we have been careful to note all the symptoms that we could observe, they are not without their value on this point."

There is here no charge of misrepresentation; we merely remark that Dr. Ferrier has mistaken our, mostly verbal, communications, which were made to him during the progress of our experiments, and which were purely private and friendly and not intended for publication. Dr. Ferrier does not hesitate to seize upon a casual expression in a hurried note of invitation, in order to vindicate his position. In such a note, which was written at an early stage in the course of our joint work to ask him to come and inspect with us certain monkeys in which we had, as we thought, removed the whole temporal lobe of one side, including the hippocampus (which was the part we were at the time really trying to get at the function of), I mention as a reason for the invitation that we have been unable to detect any paresis of sensation (which, according to Dr. Ferrier's previous experiments, should have supervened), and I add in parenthesis "except auditory." Now this addition was made because it so happened that we were not certain with regard to

---

words: "Dr. Ferrier is mistaken in supposing 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."

one of the monkeys which we wanted Dr. Ferrier to test for hemianæsthesia, whether it was slightly deaf on the side opposite to the lesion.<sup>1</sup> The words "except auditory" were added to include this case and for the sake of being exact, certainly not for the sake of informing Dr. Ferrier that in all the monkeys from which we had removed the temporal lobe we had obtained deafness. If we informed Dr. Ferrier that this animal appeared to us slightly deaf when the ear of the same side was stopped, we must also have informed him that all the others, with equally extensive temporal lesions, showed no signs of deafness. But the question of audition was not raised at that time. We had not intended to deal with the localisation of auditory perceptions, and believed with most other people in this country in Dr. Ferrier's "auditory centre," as evinced by his "totally deaf" monkey. If in all the other animals with temporal lesions we failed to get any sign of deficiency of auditory perceptions, we were quite inclined to ascribe it to an incomplete removal and at any rate we soon came to the conclusion, that there was nothing to be made out from monkeys in this direction by unilateral lesion, and left the question open for future investigation.

But I will assume for a moment, for the sake of the argument, that these two words "except auditory" (the occurrence of which I have above explained) might have been taken by Dr. Ferrier to mean, that we had found in all the several monkeys referred to in my letter (in which be it remembered the exact condition of the brain was not at the time known<sup>2</sup>) a deficiency of auditory perceptions.

In my letter I specify *the whole of the temporo-sphenoidal lobe*, including the hippocampus, as having been removed in

<sup>1</sup> It was the only one in all our series of experiments that Mr. Horsley and myself had any doubt about at all. Our notes with respect to this monkey run thus ('Phil. Trans.,' vol. 179, 1888, B. p. 37): "30. *Lesion*.—Ablation of right temporo-sphenoidal lobe and hippocampus major. *Result*.—As regards sensibility, we could detect no difference on the two sides. As regards hearing, when the ear on the same side as the lesion is stopped, the animal seems unable to hear slight sounds." We have figured the lesion (Plate 5, Fig. 30), which is very considerable, extending even beyond the limits of the temporal lobe.

<sup>2</sup> Cf. Ferrier. *op. cit.* ('BRAIN,' April 1888, p. 9), who now strongly deprecates the "premature publication of conclusions while the evidence was incomplete, seeing that the animals were still alive and the actual lesions undetermined."

the monkeys Dr. Ferrier is invited to come and see; how would this justify him in the statement: "My results have been *entirely confirmed* by Horsley and Schäfer, who have informed me that their experiments have shown, that it is *only when the superior temporo-sphenoidal convolution is destroyed that hearing is impaired or abolished*, and that no such effect is produced by entire removal of the rest of the temporo-sphenoidal lobe"?<sup>1</sup> It requires very clever casuistry to twist any sort of correspondence out of the two statements. Dr. Ferrier has not hesitated to charge me with want of ingenuousness,<sup>2</sup> but it seems to me that the accusation comes with ill effect from that quarter.

Our critic next comes to the consideration of the short paragraph in which I have dealt with the evidence afforded by our experiments on the functions of taste and smell, which were assumed by Dr. Ferrier, on what even he regards as insufficient evidence, to be *localised* in the antero-inferior extremity of the temporal lobe. After apologising for the weakness of the case, he concludes that, "inasmuch as Professor Schäfer, while unable to confirm, is also *unable to adduce any facts opposed to the positive results* obtained by (Munk, Luciani, and)<sup>3</sup> myself, it does not seem necessary to discuss the question at greater length." While agreeing with the last remark, so far as regards the mode of discussion of these questions which Dr. Ferrier has introduced, I may be pardoned for taking exception to the first, which I have italicised. The monkey whose brain is shown photographed in Figs. 1 *a* and 1 *b*, and in section in Figs. 1 *c* to 1 *g*, showed, after the first condition of idiocy<sup>4</sup> induced by the operation had passed off, complete possession of the faculties of both taste and smell. He was exhibited by us at a large meeting of the Neurological Society at which Dr. Ferrier was himself present; and the disgust which he manifested on

<sup>1</sup> 'Functions of the Brain,' 2nd edition, pp. 310-311. The italics are mine. The statement in the latter part of the paragraph I freely admit.

<sup>2</sup> *Op. cit.*, p. 12.

<sup>3</sup> I have ventured to put Munk and Luciani in parenthesis, because so far as I have been able to gather from their writings, their observations (made moreover chiefly, if not entirely, upon dogs) do not at all correspond with the results obtained by Dr. Ferrier.

<sup>4</sup> Cf. my paper in 'BRAIN,' January 1888, p. 375.

biting a raisin in which quinine had been artfully concealed, and the care with which he afterwards smelt at every other portion of food which was offered to him, were sufficiently convincing to the members present that he both had and knew how to make use of these senses.

A single well-attested "negative instance" like this outweighs any number of positive instances. If an animal in which both temporal lobes have been destroyed so completely as is shown in these photographs still manifests to all the world that it continues to hear, to smell, and to taste, it is very improbable that any one of these three faculties is *localised* exclusively in this lobe.<sup>1</sup>

Lastly, with regard to the visual area of the cortex. Dr. Ferrier will have it that "the chief point at issue between him and myself is the anterior boundary of the visual zone. While he (Schäfer) considers it as being sharply bounded by the parieto-occipital fissure, I (Ferrier) maintain that it embraces also the angular gyrus."

This I cannot allow to be the chief point at issue, nor even that it is a point at issue at all. The chief points at issue are, (1) the connection of the angular gyrus with central vision of the opposite eye; (2) the relative importance of the occipital lobe. According to Dr. Ferrier's experiments, decortication of the angular gyrus of one side produces blindness of the opposite eye (amblyopia); of both sides produces blindness of both eyes. According to our experiments, decortication of

<sup>1</sup> Not that these senses have nothing whatever to do with the lobe. This I have never asserted. I have, on the contrary, confined myself to the demonstrable statement, that the strict localisation of these faculties in the temporal lobe in monkeys has no experimental evidence in its favour. Whether with the lesions we have established there has been any permanent impairment of these senses could not by us, in our monkeys, be determined. All that we have ventured to state is that we have not been able to get evidence of such impairment. As for experiments upon dogs, it may be remarked that their results are not readily transferable to monkeys and man, owing to the extremely different configuration of the hemisphere, and that those who have experimented upon the sensory functions in these animals are by no means in agreement regarding the exact localisations of those functions. Indeed, Luciani—who, curious to relate, is again adduced by Dr. Ferrier as a witness for his case—extends the spheres for these special sense perceptions over areas of the cerebral cortex, so large and overlapping one another to so great an extent that they cannot in strictness be spoken of as "localisations" at all. (See 'BRAIN,' Vol. VII, figure opposite p. 160.)

the angular gyrus of one or both sides is not necessarily followed by any visual defects perceptible to our means of investigation in animals; but complete eradication of the gyrus produces hemiopia (not amblyopia), which is temporary only. According to Dr. Ferrier's experiments, destruction of one or both occipital lobes alone, produces no appreciable effect whatever on vision. According to our experiments—which are merely confirmatory of those of Munk—removal of one occipital lobe only, without the angular gyrus, produces permanent hemiopia; of both occipital lobes, blindness of both eyes, which is also permanent and, so far as we were able to judge, complete. Our experimental results therefore differ *toto cælo*.

In spite, however, of the experiments on total removal of the occipital lobe, and of this alone, which have produced in both Munk's hands and in our own permanent hemiopia or blindness, according as the lesion was on one or both sides, I have been careful to point out the possibility of the angular gyrus forming a part of the cortical visual area (loc. cit. p. 372), although it must be very subordinate to the occipital lobe. In this way the fact may be accounted for, that with eradication of the gyrus visual disturbance of a hemiopic nature is produced. At the same time, as will be seen from the photographic print (Fig. 4), which exhibits a longitudinal vertical section through the hemisphere, such eradication could not fail to affect the fibres of the corona radiata, which are passing to the occipital lobe; and the chief part of the hemiopia, which moreover is not permanent, might be accounted for by the mechanical insult to which these fibres are subjected by so deep a lesion. This is on the whole the more probable explanation, but there may nevertheless be left permanently a blindness of a small part of the visual field which it may be impossible to detect in monkeys. I at present have in my laboratory a monkey from which I have eradicated, as I believe, both *gyri angulares*. The operation was performed six months ago. For the first few days the animal appeared totally blind, but vision gradually returned, and before long was quite acute for distant objects. But for a long time, some three or four weeks, the animal failed to see

objects (raisins, &c.) which were held just below or on one side of its eyes, and even now there appears to be complete absence of vision in the antero-superior and lateral portions of the retinae. It is, however, difficult to prove this, because the creature has acquired the habit of rapidly directing his head and eyes, so as to bring the images of objects on the central parts of the retinae. At an earlier stage it was very striking, for if a raisin were noticed at some little distance off, the animal would evidently see and run right up to it, but then often fail to find it, presumably because its image now fell upon the uppermost (blind) part of the retinae. The early and temporary total blindness in this animal I should be inclined to explain by the mechanical insult of the thin part of the corona radiata which passes under the gyrus angularis to the occipital lobe (see Fig. 4), due to the eradication of the gyrus, and perhaps to pressure from the blood clot which occupies the place of the removed gyrus; the more permanent partial blindness (of the antero-superior and lateral parts of the retinae) to the removal of the angular gyrus, and to some concomitant injury to the (corona radiata of the) lateral part of the occipital lobe, which, as I have elsewhere and by other methods shown,<sup>1</sup> are probably related to those parts of the retinae. Dr. Ferrier, on the other hand, explains such blindness by supposing, that destruction of the gyri angulares produces loss of central vision. In that case it is difficult to account for the excellent central vision which the animal undoubtedly now possesses, unless the doctrine of cerebral localisation for vision is altogether to be surrendered, and we are to assume that other parts of the cerebral cortex are able rapidly to take up and to carry on the functions of the removed parts in addition to those functions which they already possessed.<sup>2</sup>

There remains one more criticism to be answered—that, namely, which alleges that in the monkeys in which we have produced permanent hemiopia or blindness after removal of the occipital lobe, we have extended the lesion to the under

<sup>1</sup> 'BRAIN,' April 1888.

<sup>2</sup> Some thoughtful remarks, bearing relation to this point, will be found in the review by Mr. Lewes which has been already referred to.



surface of the temporal lobe, and have thereby severed "the optic radiations from the corpora geniculata and primary optic ganglia." Optic radiations proceeding whither? Does Dr. Ferrier mean to imply that the optic radiations to the gyrus angularis, "from the corpora geniculata and primary optic ganglia," run along the under surface of the temporal lobe to reach that gyrus? Is he not aware that the lateral ventricle and at least one deep horizontal fissure come between the region he speaks of and these "optic radiations"? And such "optic radiations" as pass into the occipital region, are they not tending towards the very parts which have been removed? A glance at any longitudinal section (such as that which is photographed in Fig. 4) will at once demonstrate the untenableness of the suggestion, that optic radiations proceeding to the angular gyrus can be at all involved in lesions of the occipito-temporal region.

Dr. Ferrier frequently refers to experiments of his own, and also to others by Mr. Horsley and myself, in which we obtained from circumscribed lesions of the occipital and occipito-temporal regions *temporary* hemiopic effects, and explains them all by supposing that our lesions invaded these "optic radiations." He says, "It is evident that the hemiopia observed in these experiments does not indicate the position of the visual centres. For from these facts alone we should have as much reason for placing the visual centre in the temporal as in the occipital lobe." Dr. Ferrier uses this argument as a *reductio ad absurdum*, but it is nevertheless very much to the point. There is, I think, no doubt whatever, both from these experiments in which hemiopia is produced from lesions of the occipito-temporal regions and from the results of excitation of the same region (which produces movements of the eyes upwards and to the opposite side<sup>1</sup>), that the visual area of the cortex *does* extend a certain distance forward beyond what is anatomically regarded as the anterior limit of the occipital lobe, especially on the under-surface. The hemiopia seems generally complete at first, but gradually passes off until the animal's vision appears fully

<sup>1</sup> Cf. the paper in the last number of 'BRAIN' on "Electrical Excitation of the Visual Area in the Monkey."

restored. It is difficult, however, to say if any partial loss of vision remains. I believe the complete hemiopia to be due, in these cases of partial removal within the visual area, to the vascular and mechanical disturbance of the posterior region of the brain which is produced by the operations. This would sufficiently account for its temporary character. That a certain corresponding part of the retinal field remains permanently blind, I have very little doubt, but it is impossible to determine this in monkeys. At least, this seems to me a more reasonable explanation of the hemiopia than the assumption, that all such recorded lesions have extended to the "optic radiations." Whatever may have been the case in Dr. Ferrier's own investigations, this has certainly not happened in the experiments in which I have been engaged in conjunction either with Mr. Horsley or with Dr. Brown, as longitudinal sections through the hemispheres have abundantly shown.

I share to the full the regret which Dr. Ferrier feels for the attitude I have taken up on this question, but not for my own sake nor for that of the truth. It is hardly necessary to say, that the only object I have had in view in making these experiments upon the brain has been to arrive at a more accurate knowledge regarding the functions of its parts. Few men have done so much towards the elucidation of the cerebral functions as my present opponent; and if we are not in entire accord with regard to some points, it is nevertheless to be borne in mind that the number of those upon which we are in complete agreement, and as to which I have had little to do but to confirm Dr. Ferrier's observations, is far greater. With respect to the points at issue I have merely endeavoured to state, as clearly as I could, the experimental facts which I have arrived at (conjointly with my fellow-workers), and at the same time to discuss their bearing upon the views which have been most widely accepted in this country regarding the cerebral functions. That we have finally settled the questions involved, I should be the last to affirm: I am content to have re-opened them. To time and to future observations, experimental and clinical, their decision may safely be left.





FIG. 2 a.



FIG. 2 b.

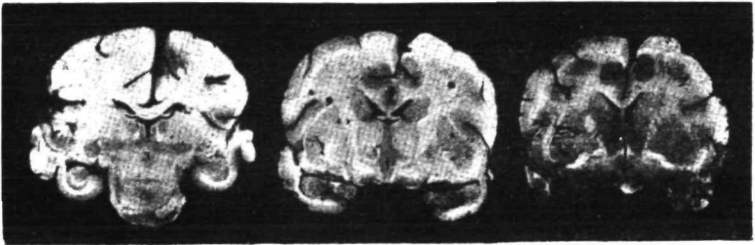


FIG. 2 c.

FIG. 2 d.

FIG. 2 e.

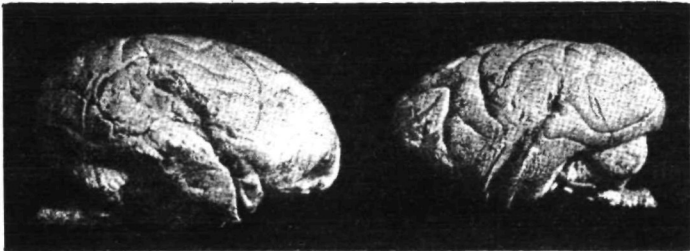


FIG. 3 a (reproduced from Ferrier and Yeo). FIG. 3 b.

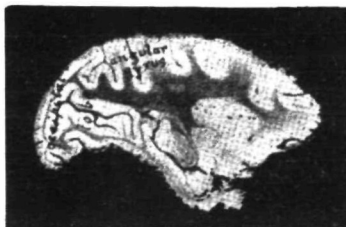


FIG. 4.

SCHÄFER: 'On the Temporal Lobes.'





FIG. 1 a.



FIG. 1 b.

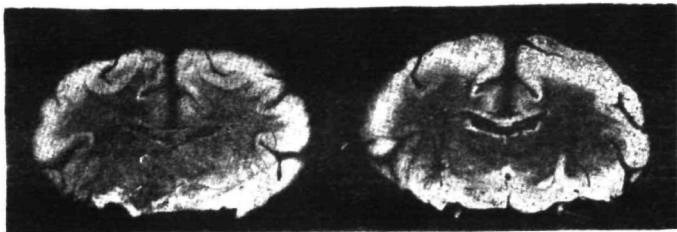


FIG. 1 c.

FIG. 1 d.

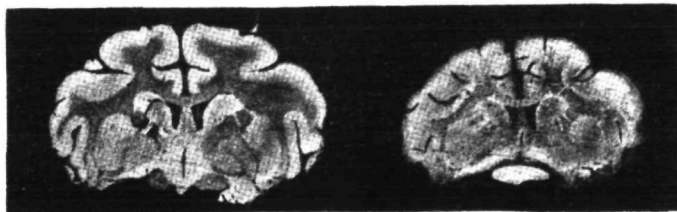


FIG. 1 e.

FIG. 1 f.

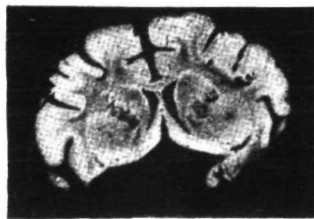


FIG. 1 g.

SCHÄFER: 'On the Temporal Lobes.'

At the request of Dr. Lauder Brunton,<sup>1</sup> I append a table, drawn up by himself, showing the methods he employed and the results he obtained in investigating, at the request of the Neurological Society, the monkeys which were submitted to the Society at its meeting on March 26, 1887. I should mention that Dr. Brunton at his own request was not informed of the nature of the lesion in the monkeys submitted to him, and, except on the last two occasions, other monkeys which had either not been operated upon at all, or which had had lesions established elsewhere than in the temporal lobe, were also handed over to him for investigation. The other members of the Committee who sent in reports (Dr. Bastian and Dr. Waller) content themselves with stating that, after carefully examining the monkeys submitted to them (F. and O.), they were satisfied that they could hear distinctly.

---

<sup>1</sup> Who writes to me, "I see my notes of the reactions given by your monkeys have not appeared in 'BRAIN' yet. I should like them to do so, as I think it is important to have all the data before one in a case like the present, more especially as I see that my name appears in Ferrier's notes regarding another monkey, and people might imagine that I had had more experience in coming to a conclusion regarding the senses of monkeys than I actually have."

---

### DESCRIPTION OF PLATES.

- FIGS. 1 a and 1 b.**—Photographs of brain of monkey F. (No. 6 of the cases recorded by Dr. Sanger Brown and myself in 'Phil. Trans.' 1888), from right and left side respectively.
- FIGS. 1 c to 1 g.**—Vertical sections through the above brain at successive levels from behind forwards. 1 c is taken through the level of the posterior extremity of the superior temporal gyrus; 1 g, through the tip of the temporal lobe. Only shreds, mostly detached, of the grey matter of the superior gyrus remain. The sections were made from collodion, so that these detached shreds have remained *in situ*.
- FIGS. 2 a and 2 b.**—Photographs of brain of monkey O, (No. 12 of the cases recorded by Dr. Brown and myself), from right and left sides respectively. The Sylvian and parallel fissures are blended into a broad gap, within which the Island of Reil comes to the surface—the superior temporal gyrus having been completely removed.
- FIGS. 2 c, 2 d and 2 e.**—Vertical sections through the above brain at successive levels from behind forwards. 2 c is taken through the level of the posterior extremity of the superior temporal gyrus: the injury has here extended to the Island of Reil. 2 d is taken through the level of the middle of the superior temporal, of which no fragment remains. All the other sections through the lesion show the same condition as fig. 2 d except those through the extreme tip of the lobe, where, for a length of about two millimeters, a shred of the grey matter (with its white centre destroyed) remains upon one side. This is shown in Fig. 2 e, on the right side of the section.
- FIGS. 3 a and 3 b.**—Photographs of the brain of the monkey (No. 13) exhibited to the Intern. Med. Congress in 1881 by Professors Ferrier and Yeo (reproduced from their paper in the Phil. Trans. 1884). The superior temporal gyrus is by no means so completely destroyed as in the other two brains.
- FIG. 4.**—Vertical sagittal section through one of the hemispheres of a monkey's brain, showing the relations of the angular gyrus to the corona radiata of the occipital lobe.

OBSERVATIONS ON MONKEYS BY DR. LAUDER BRUNTON.

As it seemed to be important to avoid any preconceived ideas which might render my observations fallacious, I remained in total ignorance of the nature of the lesion which had been inflicted upon the monkey until long after all the observations were finished.

Date.	Indication of Monkey.	Lesion inflicted. (This is subject to criticism.)	Tests supplied to determine the existence or absence of Hearing.	Results obtained.	Opinion formed.
May 9th, 1887.	Tame one (F.) ..	Very extensive double temporal, including whole of both superior gyri.	The door handle was shaken while the animal was looking steadily at me.	It did not look round the first time, but looked round when handle shaken more.	That the animal could hear.
..	Old Jew (H.) ..	Left occipital lobe only.	Door-handle shaken .. ..	The animal paid no attention.	
..	Three others ..	Had not been subjected to any operation.	Door-handle shaken. All sitting at top of cage.	No definite result .. ..	Doubtful whether they could hear or not.
..	Big female Rhoesus (O.).	Both superior temporal gyri removed.	<p>Keys were shaken behind head</p> <p>The same thing repeated ..</p> <p>Snapped finger close behind its ear.</p> <p>Struck two keys together ..</p> <p>Mr. Green squeaked behind and to left of it.</p> <p>On repeating it .. ..</p> <p>Struck bell behind my back ..</p>	<p>looked round to right when I expected it would have looked to left.</p> <p>Ditto.</p> <p>No movement.</p> <p>No reaction.</p> <p>First time it turned to right.</p> <p>No reaction.</p> <p>It looked round to right.</p>	<p>That the animal could hear, but it seems quite deaf on left side.<sup>1</sup></p>

Downloaded from <http://brain.oxfordjournals.org/> by guest on October 2, 2015



			Again.	Ditto, although scratching arm at time.
"	Old Jew (H.) ..	Same as above ..	Striking bell with hammer	No reaction .. .. .
"	Big female Rhoesus sitting with left side to door (O.).	Same as above ..	Door was opened and shut	Looked each time, and to left ..
			This repeated .. .. .	No definite reaction.
			Water splashed on the ground	Looked up to left; couldn't well look to right.
May 10th.	Big female Rhoesus (O.).	Same as above ..	Striking bell behind my back	Looked up to right .. .. .
			Knocking strongly on box	Ditto.
			Whistles sounded .. .. .	Gave no indication of hearing.
Nov. 10th, 1887.	Large female Rhoesus (O.).	Same as above ..	Striking with a whistle or head piece of iron on top or side of cage.	Sometimes, but not always, moved its head as if to seek the sound.
"	When in the cage moved constantly to and fro.	Same as above ..	Fingers snapped; door-handle rattled; knock given on the table; foot scraped along the floor (especially the last).	Sometimes gave no indication of hearing.
				It looked round (when removed from the cage and placed on a table).
				That the animal might be deaf.
				That the animal could hear.
				That the animal could hear.
				That the monkey certainly could hear, while at the same time I do not think it reacts to sounds so readily and completely as a normal animal. <sup>2</sup>

The particulars in the third column and the footnotes have been added by Professor Schäfer.

<sup>1</sup> Apparently because it looked round to the right; afterwards, however, on the same occasion, it looked each time to the left (see below). [E. A. S.]

<sup>2</sup> This conclusion appears to me to lack evidence, considering that the only normal monkeys which were similarly examined by Dr. Bruerton gave no definite indications of hearing (see above). [E. A. S.]