Dupuy (E.)

# RESEARCHES

INTO THE

# PHYSIOLOGY OF THE BRAIN

BY

EUGÈNE DUPUY, M. D.,

CORRESPONDING MEMBER OF THE SOCIÉTÉ DE BIOLOGIE OF PARIS; MEMBER OF THE AMERICAN NEUROLOGICAL ASSOCIATION; 1st VICE PRESIDENT OF THE N. Y. NEUROLOGICAL SOCIETY, ETC.

[*REPRINTED, WITH CORRECTIONS, FROM THE LONDON MEDICAL TIMES AND GAZETTE, Nos.* 1410, 1411, 1413, 1422 *and* 1427.—1877.]



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A CRITICAL REVIEW OF THE

PREVAILING THEORIES CONCERNING

THE PHYSIOLOGY AND THE PATHOLOGY OF THE BRAIN:

LOCALISATION OF FUNCTIONS, AND MODE OF PRODUCTION

OF SYMPTOMS.

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# RESEARCHES INTO THE PHYSIOLOGY OF THE BRAIN.

Our knowledge of the functions of the brain, although extensive, is so inaccurate, the attempts made by different authors to elucidate some of the most important problems of cerebral physiology are so one-sided, and the writers for the most part appear to have been so much influenced one by another, on account of the numerous difficulties lying in the way of controlling by experiment the experimental data, added to the natural disinclination of most men to undertake the tedious labour of critical investigations, that both physicians and physiologists have come to accept most eagerly any work written by any author who ignores all doubt as to the absolute truth of his theories, as an inexhaustible mine of arguments and facts for the support of their own speculations, and in this manner they elevate such an author to the position of a universally accepted authority.

Such has already been the fortune of Dr. Ferrier, and it is for that reason that I undertake to write the present review of the whole question of localisation of functions in the brain.

We have in Dr. Ferrier's book(a) all the experimental data discovered both by Fritzsch and Hitzig,(b) and later by Dr. Ferrier. These data have led those physiologists to bring forward this doctrine, which is, unequally expressed in their respective books: that the cortex cerebri is a region which can be excited into activity by electrical stimuli; that each and every convolution of the brain contains one and more *centres*, the anterior and antero-parietal convolutions contain centres which govern motion, and the posterior and postero-parietal convolutions contain centres for common and special sensations. The existence of all these centres can be made apparent by irritating the areas of the cortex with electricity. Dr. Ferrier has even gone further than Hitzig, for besides the sensory centres he describes also centres of inhibition, of visceral sensation, of the senses of thirst, hun-

(a) David Ferrier, "Functions of the Brain." 1876.

(b) Hitzig, "Untersuchuugen ü. das Gehirn," 1874.

ger, etc. All those experimental facts, however, were known to the profession before their appearance in book form. Those of Dr. Ferrier have attracted so much attention from the very beginning, that we have since read every day of pathological cases and other facts which are said to find their true interpretation in the theories of that physiologist, at the same time that they are constantly being published with the view of strengthening the deductions arrived at by him.

Dr. Ferrier has made the doctrine of localisation of functions in the brain almost his own. He has so elaborately varied the former experiments of Fritzsch and Hitzig—the experimental starting-point of localisation of functions in the brain,—and so laboriously contrived to support by his discoveries the very original deductions of Dr. Hughlings-Jackson,—all with such partial success,—that I very sincerely believe his book to be a new departure in experimental cerebral physiology. It is therefore not without hesitation that I presume to offer some points of criticism on the deductions contained in Dr. Ferrier's work. I think that the facts which I am enabled to bring forward belong to the *other* side,—Dr. Ferrier having, notwithstanding his high merits, fallen into the common error of being *one*-sided. I desire only to present some facts which cannot be interpreted by the theories of Drs. Ferrier and Hitzig—some deductions which, if they are logically deducted from true premises, as I believe, cannot be explained by the theories of Hughlings-Jackson.

I shall take successively, and in the same order, as far as I can, as they are put forth by him, the points on which I differ from Dr. Ferrier. I have in the beginning to take issue with him on his exposition of the anatomy of the medulla oblongata. This point is not without great importance. The decussation of the conductors of motor impulses, in the anterior pyramids, goes strongly to support the principal deductions of Dr. Ferrier. But it so happens that in man, as in the other animals, the conductors of the order of the will to the muscles do not decussate entirely in the anterior pyramids. Few of these conductors decussate there, and superficially too. Before the demonstration of that fact by Sappey and Duval,(c) the experiments of Schiff, Vulpian, and Philippeaux,(d) and a few cases of lesion of one pyramid without loss of motion in man, constituted strong evidence of the fallacy of the accepted teaching. The cases, therefore, which I shall adduce further—of lesions of different parts of one hemisphere causing paralysis of motion on the corresponding side of the body instead of the opposite—will not be disposed of (to suit his theory) by Dr. Ferrier, on the ground that they are exceptional, "just as there are exceptions to the rule that the heart is situated to the left and the liver to the right." And, even were it so, I have collected some cases which I shall adduce further, and which militate against such an assumption.

The next subject refers to reflex actions. I take notice of the fact, accepted by Dr. Ferrier, that the reflex action which would otherwise result from a stimulus is altogether restrained or inhibited if a sensory nerve in some other part of the body is simultaneously irritated. Also that he attempts to weaken Pfluger's doctrine of psychical or intelligent action on the part of the spinal cord. His arguments are those of Goltz and others. Pflüger's doctrine is the same which was most forcibly advocated, chiefly by Legallois, Dugès, of Montpellier, and latterly illustrated by some clever experiments of Vulpian. The weight of arguments and facts given by some authors goes to show that in man, if not as clearly substantiated as in other

(c) Sappey and Duval, C. R. Société de Biologie, 1876 passim.

(d) Vulpian and Philippeaux, C. R. Société de Biologie, 186.

animals, the doctrine is virtually true. The deductions arrived at by Durand de Gros,(e) and latterly by Claude Bernard,(f) are proof of what I advance. The counter-experiments of Goltz are susceptible of another explanation that given them than by him. He did not eliminate this great source of error, which is disposed of by careful differentiation of phenomena due to irritation from those due to loss of function. The inhibitory action of the spinal cord upon itself is not considered; no more is the different influence of each and every part of the cerebrum upon the medulla oblongata, and also the result of ablation of that last organ, considered as a respiratory and circulatory centre, which has strong bearing on the point at issue. For the manifestations of their functions, parts are entirely dependent upon the properties of their tissues, and the properties of the tissues are a mere result of their nutrition. It is out of place to argue at any length on that subject just now; but not so, however, to remember the acceptance by Dr. Ferrier of the theory of "physiological association in the spinal cord between the centres of movements of the same kind in both limbs, due either to their commissural connexions or the organisation of past experience." There are certain experiments of Weir Mitchell,(g) which he quotes for another purpose, but which I think can receive explanation by that theory. Weir Mitchell has seen that on suddenly faradising the brachial plexus in a case of amputation of the shoulder-joint, in which all consciousness of the limb had long since vanished, the patient said at once, "My hand is there again; it is all bent up; it hurts me." Ferrier says that the explanation of these curious phenomena is correctly given by Weir Mitchell, who writes that the excitation of the sensory nerves calls up in idea the correlated movement—*i.e.*, the movement which in the actuality of past experience had coincided with the sensation now revived by the faradic stimulus. I believe that this important fact, together with the theory offered in explanation, comes within the theory of Legallois, of Dugès, of Pflüger, and others. I may be mistaken. I only put in apposition Legallois, Dugès and Pflüger's experiments with that of Weir Mitchell. It is a landmark of which the utility will be seen when I come to examine Dr. Ferrier's views on the nature of conscious sensation. It seems to me even now, however, that if he considers that experiment of Weir Mitchell together with the explanation offered, he accepts implicitly the doctrine of psychical or intelligent action on the spinal cord. In science we have to take the consequences of our logical deductions; and it is not contrary to sound logic to say that the phenomena of memory and "*partant*" of volition can be explained by the theory of the "*organisation of past experience.*" When an impression is transmitted towards the centre it reaches a group of nerve-cells. These cells only become affected by the impression through a nutritive process. Now, if an impression is frequently sent through one channel, to one centre, it happens that that centre becomes more and more apt to be affected by that impression. We know, on the other hand, that nerve-cells are not permanent structures any more than other structures,—they are potentially permanent. The process of nutrition, *i.e.*, assimilation and disassimilation, make all structures imperceptibly but constantly undergo a process ot destruction and reconstruction, so that after the lapse of some period we happen to have a cell materially different from the first one (the materials of which it is made, however, being composed of the same elements), yet endowed with the same function, but better adapted. It is unnecessary for me to dilate on that subjet. Let me give an illustration. The respiratory function is carried on through a reflex process. Our volun-

(e) Durand (de Gros), "Electro-Dynamisme vital," "Les Origines animales de l'Homme," "Du Polyzoïsme," etc.

(f) Claude Bernard, "Discours de Réception à l'Académie Française."

(g) Weir Mitchell, "Injuries of Nerves," etc., p. 350.

tary control over the respiratory movements is of a limited extent. If the inspiration is delayed beyond a certain period, the *besoin de respirer* becomes so urgent that voluntary control is no longer capable of restraining the reflex or automatic activity of the respiratory centres. Dr. Ferrier very nicely puts it—"Respiratory movements may continue after all afferent nerves connected with the centre have been divided. In this case there is a true automatic activity conditioned by the state of the blood itself. The diminution of oxygen and accumulation of oxidation products in the blood act as a stimulus to the inspiratory centre, and this again reflexly excites the expiratory movements. When the blood is artificially hyperoxygenated, the movements of respiration come to a complete standstill—a condition termed apnœa. Non-aeration of the blood, resulting from obstruction of the function of respiration, powerfully excites the movements both of inspiration and expiration; and ultimately, if the obstruction is not overcome, causes general convulsions of the whole body, as in asphyxia." Brown-Séquard has the merit of having been the first to establish that theory on experimental data more than twenty years ago. It is clear that the *besoin de respirer*, which is supposed to overcome the force used by the will in order to stop inspiration, is not a force. The will is not overcome by another force. The will, considered physiologically, must have its seat in anatomical structures; and anatomical structures cannot fulfil their functions when their nutrition becomes impaired. Therefore, the force of the will disappears as soon as the conditions of existence of the will are altered, and the automatic activity which governs the respiratory function comes into play. That *automatic activity is organised past experience.* But I do not intend to enlarge on that subject. I am only dealing with it in the physiological point of view; not the metaphysical.

After considering the nature of reflex action, I come to speak of equilibrium. As I differ from Dr. Ferrier on the preceding subject, I am driven to differ from him again when he states that, "without the labyrinthine impressions, optic and tactile impressions are of themselves unable to excite the harmonious activity of the sense of equilibrium." The sense of touch and the sense of sight are made subservient to a sense of equilibrium. Goltz has the credit for the discovery of that new sense. But, is there a *sense* of equilibrium? and is there a centre of equilibrium in the encephalon? It would have been good first to establish in the most irrefutable manner the existence of that sense; but this I consider that the experiments of Goltz and his followers fail to do. They can be interpreted in another and more rational manner: they must, in fact, be interpreted in that other manner. To say nothing of the experiments of Brown-Séquard(h) and the deductions which he has drawn from them, I will only say that Cyon(i) has *proved* beyond dispute that the labyrinth is not a centre of equilibrium. He has at the same time, therefore, given the true explanation of Goltz's experiments. Cyon shows that those disorders of the motor apparatus induced by operations on the semicircular canals do not occur in a uniform manner in different species of animals. In frogs these disorders are almost exclusively limited to the muscles of the trunk; in pigeons, the muscles of the head are those principally involved; in rabbits, those of the eyeballs. Each semicircular canal influences in a special manner the movements of the eyeballs. The excitation of one canal always produces ocular movements in the two eyes; but in the eyeball on the opposite side to the canal operated upon the movements take place in a contrary way to those on the same side. The pupil contracts on the side of the operation,

(h) Brown-Séquard, "Experimental Researches applied to Physiology and Pathology" 1853, page 189; and elsewhere. (i) Cyon, *Gaz. Méd. de Paris*, 1876, page 201.

and remains dilated on the opposite side. The oscillatory movements which follow tetanic contractions of the muscles of the eyeballs after irritation of the canals disappear when the acoustic nerve on the sound side is divided. When a rabbit, having had section of both acoustic nerves performed, is put on a rotating plane describing rotatory movements, the same phenomena are observed which have been described by Purkinje, and which lately have ve been made the object of interesting researches by Mach. This proves that those phenomena do not arise from the displacement of the endo-lymph of the semicircular canals, as that physicist laboured to show. The phenomena of Purkinje are due to cerebral disorders produced by grave alteration of the circulation which the animals undergo, in the experiment referred to, chiefly in intracranial vessels farthest from the axis of rotation.(k)

I have to examine now the functions of every cerebral organ taken individually. The optic lobes, or corpora quadrigemina, come first. It is unnecessary to draw conclusions based upon facts of comparative anatomy. Dr. Ferrier says "that with the view of breaking up these ganglia in the monkey he passed a wire cautery in a horizontal direction through the anterior occipital fissure, so as to traverse the nates or anterior tubercles of the corpora quadrigemina. The result of the procedure was that the nates were ploughed up and disorganised by the cautery." Dr. Ferrier thinks that the concomitant lesions of the hemisphere had nothing to do with the causation of the follwing phenomena. The animal was rendered *completely blind*.(l) The tests applied, however, leave room for some doubt in my mind as to the completeness of the loss of sight; for Dr. Ferrier adds, "The pupils were *dilated* and *inactive*: the left somewhat more than the right." I am of opinion that the dilatation of the pupils beyond the normal limit of dilatation is sufficient to prevent the animal from distinguishing objects. I am careful not to say that it *destroys sight*. The sense of sight may be, or may not be, lost in the experiment reported above. All I know is this: that I have seen a person unable to write, to read, to distinguish anything upon a table covered with books and instruments beyond a mass, all because he had taken by mistake a toxic dose of belladonna, and suffered consequently with dilatation of the pupil. Oculists have frequent occasion of treating persons who cannot see, not through loss of the sense of sight, but through paralysis of the ciliary muscle. I have seen also cats, on which I had performed the operation of tying the ends of very fine and long copper wire conductors of a faradic machine to both first cervical sympathetic ganglia, become unable to distingnish objects, their pupils being excessively dilated by the operation. These cats could not find their way in the room; they struck at every piece of furniture. I do not desire to be understood to say that the optic ganglia and their ganglionic appendages have no relation with the sense of sight; I wish only to show that, at the very least, the inability to see for the animals operated upon by Dr. Ferrier can be explained by another cause than the one he urges; that there was no incapacity through destruction of a *centre*, but through perversion of the external organs of sight. The reasons for this view of mine are obvious. It should have been proved first that the sense of sight is localised in the parts destroyed by the wire cautery, because, as matters stand now, there is begging the question. Nor are other experiments of Dr. Ferrier's more conclusive. When he irritated with electricity the exposed optic thalami and corpora quadrigemina, he observed,

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(k) The hypotheses of Goltz, Mach, and Crum-Brown would have much more value than they have if they did not consist, as they do, in begging the question; for it is assumed that the liquid of Cotugno does not entirely fill the ampullæ. That *point is not granted*. Therefore, it is not possible to consider their hypotheses any longer; because, if the liquid fills the ampullæ entirely, there can be no notion of direction conveyed.

(l) The italics throughout are mine, except when otherwise indicated.

besides the dilatation of the pupils, movements also. If this experiment proves anything, it proves that the parts acted upon are centres for general movements of the body, as well as for other functions. We know that the corpora quadrigemina when irritated have a powerfull effect on the viscera. Pathological cases up to this time throw a very dim light on the obscurity which still exists concerning the special function or functions of the thalami optici. We know nothing more certain on that subject now than when Vulpian wrote ten years ago that we knew nothing certain concerning them.

I suppose that the functions of the cerebellum are investigated next because of the relation which exists in the mind of Dr. Ferrier between the sensory centres, the optic bodies, and the corpora quadrigemina, and the cerebellum as co-ordinating centre. I shall say little concerning the cerebellum. Experiments very carefully conducted have shown that it can be removed entirely, and yet the animals *spontaneously execute all the movements of the eyeballs*, which, Dr. Ferrier thinks, are incited from that organ. Ollivier and Leven (m) have show that pricks in any part of the cerebellum will give rise to a variety of motor symptoms in the eyeballs—a variety of such a kind, that it is impossible to associate any special order of motor phenomena with a particular lesion. Diseases have given us cases of all kinds, but none are more obscure than those of the cerebellum. I have also seen that irritation of the dura mater in the neighbourhood of the pons, in an animal deprived of the greater part of the cerebrum and cerebellum, and which has not lost too much blood, will give rise to motor phenomena of the eyeballs identical with those observed by Dr. Ferrier after irritating the cerebellum. Luys(n) has reported two cases, with autopsy, of deafness of long standing. He saw at the autopsy that the acoustic nerves, in each of the two cases, were destroyed on both sides. He followed the degeneration through the posterior portion of the optic thalami up to the fourth ventricle, and also the cuneiform convolusions of the *occipital lobule*. But he writes, " Sections of the cerebellum and of the protuberance, examined in the same manner [microscopically, for he speaks of the neuroglia being diseased?], showed no particularity to mention." He believes, therefore, that the centres of these nerves are in the occipital lobules. His testimony, on account of the eminence which he has gained as an advocate of localisation, is of value, but it seems to me to be right against Dr. Ferrier's theories, as will be made still more apparent further on. What I have said above, and the experiments of Cyon, impel me to reject altogether Dr. Ferrier's theory of the functions of the cerebellum. There is another hypothesis in science concerning the cerebellum. Rolando wrote in the beginning of this century that he considered the cerebellum to be an organ of "renforcement"; that it played the part of an "electro-motor." Luys has propounded the same view; so has Weir Mitchell. I entertained the same notion some years ago; I thought, as my predecessors did, that I had even demonstrated the truth of that theory. I am compelled to change my opinion now. Animals deprived of their cerebellum are not weaker than they ought to be after such a severe traumatism; and if they appear weaker just after the cerebellum is extirpated than after the cerebrum is removed, it is simply because in the first case the blood-supply of the medulla oblongata is more diminished and altered than in the second instance, although the vessels destroyed are smaller. Besides, the cerebellar peduncles have very powerful inhibitory effects on the spinal system. Far from me the idea of being hypercritical, I believe that nothing helps more to lead to discovery than the judicious use of hypotheses. But we must consider an hypothesis to be nothing more.

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(m) Ollivier and Leven (reprint 1884, Paris), " Recherches Expérimentales sur le Cervelet."

(n) Luys, " Annales des Maladies de l'Oreille et du Larynx," tome i, pages 318-19.

In that disposition, when we come to discover that what we thought true is not true, we have made great progress already. This is the kind of progress which has been made up to this time; it will no doubt be conducive to progress of a more positive nature. For these considerations I think Dr. Ferrier is again begging the question when he writes "that though the faculty of co-ordinated progression may be retained notwithstanding the destruction of the cerebellum, yet the loss of equilibrium practically renders it impossible; but if the faculty of locomotor co-ordination is destroyed by lesion of the optic lobes and pons, equilibration must necessarily be rendered impossible. The centre may remain intact, but its afferent and efferent factors are either wholly or partially interrupted or annihilated." I say that there is begging the question, because Dr. Ferrier has first laboured to show that the senses of sight and of tact are less essential than the *sense* of equilibrium, of which the organ is in the labyrinth; and this being taken for granted, he shows now that they all have one common superior centre—the cerebellum. By that theory we can understand how lesions of either of the senses of tact and of sight, or of the one which has its seat in the labyrinth, can bring on incöordination; for the connexions between them and the centre (cerebellum) are broken (in cases of destruction of the corpora quadrigemina or optic lobes, for instance), which is pratically equivalent to destruction of the centre, as he very clearly puts it in the quotation given above. But before testing the value of that ingenious and captivating theory— so simple, too,—may I not ask what are the proofs given that the cerebellum is the centre of co-ordination; and what are the proofs that the connexions of the senses of sight and of tact, and of the sense (so called) of equilibrium, which has its seat in the labyrinth, with the cerebellum—if they happen to have any—by means of direct fibres, are for the purpose of co-ordination? It will be seen that as soon as the first postulate is not taken for granted on sheer assertion, the whole fabric falls to the ground. I will not refer to the phrenological hypothesis concerning the cerebellum, because it is absurd. My object is only to show that Dr. Ferrier's theory must be considered to be only identical in value with those which were propounded by his predecessors.

By far the most important of the subjects treated by Dr. Ferrier are those which comprise the functions of the cerebrum. I read here a well-deserved homage paid by Dr. Ferrier to Fritzsch and Hitzig for having been the first to demonstrate experimentally the fact of "definite localisation"; and at the same time the statement that vital differences exist between him and Hitzig "in regard to the extent of the localisation, and with respect to the true character and significance of the phenomena." These discrepancies do not arise from the methods of exploring the brain used by the authors named. Hitzig has used the faradic current quite as extensively as Dr. Ferrier, besides the galvanic current. There is a difference between the effects of the two currents; but pratically, and in the point under consideration, there is not: both have the same results. Hitzig and Ferrier maintain that they have, each by his special method of exploring, proved that the *cortex cerebri is excitable*, and that the contractions in various groups of muscles which they have observed on irritating the *cortex cerebri* are the results of *excitation of the grey matter* of the hemispheres.

I, for one, take exception here. I do not believe that the movements which are observed to follow electrical irritation of the cortex cerebri are due to discharge of the cells then irritated. When Dr. Ferrier published his first researches, three years and a half ago, I wrote a little pamphlet in which I examined the validity of the theories advanced by Hitzig and by himself, and also by Dr. Hughlings-Jackson. I tried at the time to control by experiment the experimental data of Hitzig and Ferrier. I urged several objections against those new theories. Of all the objections, pathological and experimental, which I brought forward, Dr. Ferrier has only recalled one; and that one, although not without importance, is not the most important. By placing the sciatic nerve of the frog, prepared after the manner of Matteucci, on the posterior part of the hemispheres, I found that active contractions of the gastrocnemius muscle of the frog resulted when I irritated the anterior convolutions of the brain, thus showing that the current had passed along the whole extent of the hemisphere. That fact is undoubted. It has been ascertained by several other experimenters by different methods. I said that the fact permitted us to think that the electricity was conducted to the basal ganglia, and there excited *these ganglia and the nerves which spring from them.* (a) Ferrier does not accept this view. He writes—"The effects of irritation of the basal ganglia are capable of exact estimation. Irritation of the corpus striatum is followed by general contraction of the muscles of the opposite side of the body; and it is impossible, by applying the electrodes directly to the surface of this ganglion, to produce localised contraction in any one muscle or group of muscles."

In the winter of 1874, I showed to several distinguished physiologists in London that the impossibility to which Dr. Ferrier refers is no impossibility. Professor J. Burdon-Sanderson, in a paper published in the *Proceedings of the Royal Society*, (b) says. "Dr. Dupuy found that after ablation of those parts of the hemispheres which contain the supposed centres, movements, similar to those described by Dr. Ferrier, can still be produced by electrical excitation of the cut surface." Dr. Burdon-Sanderson, in order to settle the difference between Dr. Ferrier and myself as to the identity of the phenomena observed by us both individually, repeated the original experiments of Dr. Ferrier, and my own also. He investigated the most characteristic of the combined movements so accurately described by Dr. Ferrier, as produced by excitation of particular spots on the anterior part of either hemisphere, by comparing them with those produced by excitation of deeper parts, as I had done. The results of his experiments are exactly what I had myself seen. He exposed the anterior portion of the hemispheres, which comprises the several spots by the excitation of which the following characteristic movements could be produced: — 1. Retraction of the left forepaw, with flexion of the carpus, accompanied by similar movements of the hind leg. 2. Closure of the left eye, and elevation of the upper lip. 3. Retraction of the left ear. 4. Rotation of the head to the left side. The active spots for these several movements are as follows:—For (1), a point immediately behind the outer end of the crucial sulcus; for (2), the surface about the outer end of a sulcus which lies immediately behind (1); for (3), the surface behind the sulcus last mentioned; for (4), a spot a centimetre further back on the same convolution. These active spots are well defined; their limits and relations are in exact accordance with the statements of Dr. Ferrier. Now (as I had done), Dr. Sanderson severed from the deeper parts (by a nearly horizontal incision, made with a thin-bladed knife, and the instrument withdrawn without dislocation of the severed parts), the area of the surface of the right hemisphere which comprises the active spots above mentioned; and the excitation of the active spots thereupon repeated, the result is the same as when the surface of the uninjured organ is acted upon. Moreover, if a similar incision is made in a parallel plane, but at a lower level, this is not the case; (c) but on removing the flap, and applying the electrodes to the cut surface, it is found that there are on it active spots which,

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(a) Dupuy, "Examen de quelques Points de la Physiologie, etc.," Delahaye, page 23, con. 3. 1873.

(b) *Proceedings of the Royal Society of London.* June, 1874. No. IV.

(c) Putnam, of Boston, has found the same thing. He has concluded that the movements produced in the first case really came from the cortex, as he thought that his negative results proved. What follows disposes of his theory.

as regards the effect of excitation, have the same topographical relation to each other as the former. If, further, a surface of brain is cut away, so as to expose the outer and upper part of the corpus striatum, and the electrodes are applied to this surface, the movements (1), (2), (3) are produced in the same way as before, but more distinctly; the active spots are quite as strictly localised, and their relations to each other are the same as at the natural surface.

Professor Burdon-Sanderson, from these facts, believes that it is not in the convolutions, but (according to the old doctrine) in the floor and outer walls of each lateral ventricle, that the centres for such movements are to be found.

For aught I know, the old doctrine is as good as the new one. Both are substantiated by the same series of experimental data. The experiments related above, therefore, do not seem to me to warrant this conclusion drawn by Dr. Ferrier, that "his positive results (like which Dr. Burdon-Sanderson's and mine are as positive, being identical), determined by exact experiment, effectually dispose of vague statements respecting the supposed influence of currents conducted to the basal ganglia."

The counter-experimental results just adduced above, therefore, still leaves open the question of knowing (1) whether the cortex cerebri *is* or is *not* excitable by electricity; (a) and (2) whether certain convolutions contain organs which are essential to the production of the muscular movements of the character described by Hitzig and Ferrier.

Let us see how Dr. Ferrier has disposed of the arguments which are so contrary to the doctrine of localisation. He says that as the cortical centres act downward on the muscles, necessarily through the basal ganglia and motor tracts, the application of electrodes to the medullary fibres (which connect each centre with the corpus striatum) is essentially equivalent to the stimulus caused by the functional activity of the centre itself. There is here a *pétition de principe*. It is known that fibres will convey impressions when irritated; but that fact does not disclose the functions of the parts with which the fibres are connected. For instance, tickling the sole of the foot induces laughter; this does not imply (at least, I never heard) that the centre for the innervation of the muscles which are concerned in the act of laughing is in the sole of the foot. If I argued like Dr. Ferrier, however, I think I ought to believe that the sole of the foot contains centres which innervate the muscles concerned in the act of laughing! His postulate and mine are identical.

But he writes very excellently, "The mere fact of motion following stimulation of a given area does not necessarily signify a motor origin. The movements may be the result of conscious modification, incapable of being expressed in physiological terms, or they may be reflex, or they may be truly motor in the sense of being caused by excitation of a region in direct connexion with the motor parts of the erus cerebri. The method of stimulation by itself is incompetent to decide these questions, and requires to be supplemented by localised destruction of those areas, stimulation of which is followed by definite motor manifestations."

There are experimental facts of that order which have been adduced by Hitzig. Gudden, Carville and Duret, together with similar ones found by himself, which Dr. Ferrier brings forward to prove his postulate.

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(a) I do not speak of other modes of irritating the cortex; for mechanical, chemical, and physical (except electricity) have never given results. There is one exception, however. Nothnagel has defined a spot in the cortex of the brain of rabbits, which, on being pricked with a needle, drives the animals into a variety of movements. He calls that *centre*—"*nodus cursorius*." Unless the rabbits used by that experimenter differ from those usually found in the markets of France, England, and America, I cannot account for the results he has obtained. It is more than likely that he has pricked the underlying ganglia.

Localised destructions of those areas, in the hands of all those who have performed that experiment, have resulted in slight paralysis (*paresis*), which some have called "paralysis of muscular sense." (Carville and Duret, Gudden, Brown-Séquard; Schiff called it "paralysis of tactile sensation," Hitzig, "paralysis of muscle-consciousness"). I have found in 1873 (a) that destruction (scrubbing) of the whole surface of one cerebral hemisphere does not prevent the animal from using his legs. The paresis which results from removal of the centres is not *permanent*. After a few days the animal uses his leg as well as before. He can, in some instances, use it in a normal manner a few hours after he has recovered from the effects of the ether administered, and before inflammation has set in; as in the case of the dog of which I have just spoken. It is good to take notice, also, that although *all the centres* of one hemisphere are destroyed, the anterior leg only shows signs of paresis, and only in the carpal articulation. Goltz (b) has since shown that by washing away the cortex cerebri of a dog, that kind of paresis ensued (as described under different names as above), which disappeared to such a degree that the dog—which had been taught to give the paw on command before the operation, and who could not give the right paw now (the cortex of the right hemisphere being washed away)—after the lapse of some eight days began to give that right paw when asked, and in about four weeks he could do it as well and as readily as before the operation.

This would be the place to examine the question of supplementation of one centre by another—but I shall do it further on.

I have come to the point where I must show that the last hypothesis of Drs. Ferrier and Hitzig, supported by pathological facts of Professor Charcot and his pupils, is only acceptable as any other former hypothesis; and even with less credit than the one which Flourens has introduced in science. I refer to the theory which maintains that the cortical motor centres are centres for movements *willed*, *executed towards an end*, *non-automatic*. Dr. Ferrier writes: "It may be confidently asserted, and perhaps it may one day be resolved by experiment, that any special tricks or movements which a dog may have learnt could be effectually paralysed by removal of the cortical centres." That is equivalent to saying that all actions which are not automatic are impossible after removal of cortical centres. But even that hypothesis has no more value than the others; it is destroyed by facts. The experiment of Goltz previously cited is against it. I have between 1873 and 1876 had quite a number of dogs and cats under experiment, and I have *invariably* found these animals capable of using their would-be palsied limbs, not only for purposive voluntary movements, but for tricks. I remember a dog which would give the paw when asked, and stand on his hind legs and make salaams. I believe, therefore, that the counter-experiments which I have brought forward do not tend to show that the cortex cerebri is excitable by electricity, and also that certain convolutions in the neighborhood of the Sylvian fissure and the fissure of Rolands contain psycho-motor centres composed of cells of grey matter.

Anatomy, normal and morbid, has furnished a good array of facts, which are brought forth to support the doctrine of localization. That kind of evidence has been adduced chiefly by Professor Charcot and his pupils. I refer also to the discovery of giant-cells in the paracentral lobule by Betz and Mierzejewski. There is, however, no proof that those cells are motor. They are said to be motor; they are pyramidal, resemble those of the anterior horn of the spinal marrow, and they are found in a locality which is said to be motor — the paracentral lobule. But is that not again a *pétition de*

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(a) "Examen, etc.," *loc. cit.*, page 24.

(b) Goltz, *Pflüger's Archiv*, B. xiii., I. p. 43.

*principe?* For this is the argument: the paracentral lobule is a motor centre because it contains pyramidal cells which are motor. The pyramidal cells which are in the paracentral lobule are motor because that lobule is motor! That is the logic which has led to the adoption of the doctrine of localisation of functions on anatomical basis—from Luys and Meynert to Professor Charcot. But Professor Charcot has also pathological cases of the same nature as those of Hughlings-Jackson and Hitzig. I have not been able to collect all the cases reported, but I have carefully analysed those which are collected from different authors and gathered in the pamphlet of my friend Dr. Lépine. (a) I have found that there were *several lesions* in all the brains examined—lesions situated in *different parts* of the cerebrum. The convolutions, the crown of Reil, the several ganglia of the corpora striata and thalami optici, the pons, etc., were found diseased in both hemispheres sometimes. Yet those cases are given as proofs of the accuracy of localisation! The only reasoning which have led to the rejection of all other lesions but those found in the convolutions of the paracentral lobule, in order to explain the production of the paralyses observed during the life of the patients, is the same through which the conclusion was gained that the said lobule is motor and that its cells are motor, and which I have just reported above; in fact, another *petitio principii*. Every one of the *other* lesions found in the brains examined, of those observations gathered in Dr. Lépine's pamphlet, have been known to produce paralysis—at all events, to have been found associated with paralysis. Why not ascribe to them also the phenomena observed during the life of the patients? There is no room for the statement that the great seeming relation of cause to effect which exist in the cases of lesions of the convolutions warrants the inferences drawn; the frequency of apparent relation of cause to effect is just as great (I was going to say greater) between the other lesions and the symptoms produced. The other cases, reported by other physicians as "illustrating the new discoveries of motor centres," etc., I found equally liable to the same criticism. Dr. Lépine has somewhere in his pamphlet said what is in the mind of all advocates of localisation, about aphasia — *i. e.*, that instead of looking out for cases of aphasia where there was no destruction of the insula or destruction of other parts, we ought to collect all the cases where there is disease of the convolutions of the insula, as the number would soon prove the localisation of speech beyond dispute. I believe now that that wish is accomplished. Cases are only reported partially, and only those which substantiate localisation; or when cases contain elements of both kinds (for and against), only those lesions which are supposed to bear out localisation are laid stress upon.

Another argument, however, has been adduced from his pathological cases by Professor Charcot. (b) It is, that disease of the paracentral lobule is accompanied by descending atrophy, which destroys those bundles of fibres supposed to be the conductors of the orders of the will to the muscles; and as, according to Professor Charcot, such descending atrophy is only met with in that region, therefore the conclusion seems to force itself that the cells of the *cortex cerebri* are the centres of innervation of those degenerated bundles of fibres.

That theory cannot be entertained more than the others, notwithstanding the would-be elaborate logical inferences. It is, in essence, another example of begging the question. I explain. There are two systems of tissues to be considered here — the nervous and the connective (whatever may be the nature of this last, pratically it must be considered as different from the first). The nervous tissues receive their blood-supply from vessels which

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(a) Lépine, "Des Localisations dans les Maladies Cérébrales, Paris, 1875.

(b) Charcot, Soc. de Biologie, in *Gaz. Méd. de Paris* 1876.

penetrate into the cortex and somewhat deeper into the white matter from above, and from the arteries external of the ganglionic masses later in their progress; the connective tissues are fed by blood vessels and capillaries, which are made up of anastamoses coming from the opto-striated system of arteries. It is now easy to understand how one kind of tissue can be *primarily* effected. To say nothing of former pathologists, let me remember that in a very elaborate paper Miezerjewski (a) has again shown that in general paralysis, for instance (which gives rise to that kind of paresis which most resembles the one produced by destruction of so-called psycho-motor centres), the nerve-tissues become diseased only secondarily. Their supporting elements becoming diseased, they become altered and impaired. There is no temerity at all in asserting that there is no relation of cause to effect, concerning the place where the disease of the supporting connective tissues is found, and nutritive disorder which has brought it on. Disease of the connective tissues follows no laws as to *direction* of its progress. The cases of Professor Charcot, therefore, I believe give no additional proofs that the atrophy of the fibres arose from disease of the convolutions with which they are connected. We are left in the dark as to the functions of the bundles of fibres destroyed, and unless we assume that the convolutions with which they were connected were *motor*, we cannot say what were their functions.

But it may be said that if those fibres were not motor, some other function which they did subserve would have been found impaired—which was not the case, as only motion was lost. Granted; but what physician does not know that parts can be destroyed, yet no outward symptoms be produced? It is true that lesion of the *internal capsule* is always accompanied by loss of sensation. But the *internal capsule* is not a centre; it is not only a tract of nerve fibres; it contains, moreover, bloodvessels, which traverse it, and go to some other region. When those fibres are destroyed, the bloodvessels are also destroyed. How can we know whether the symptoms which we observe in that case do not arise from the consequent impaired nutrition of the parts supplied by those vessels, and on the reflex or inhibitory actions which might then result, or from inhibitory action set up by the diseased fibres? This point ought to be cleared.

Dr. Ferrier has, I am afraid, missed a fine opportunity of exercising his critical judgment through one-sidedness, when he has adduced as an argument, "*sans réplique*," the famous and much-to-be-regretted experiment of Dr. Roberts Bartholow, of Cincinnati. I am at a loss to discover how such an inaccurate observation (not to say more) can have captivated him to the point that he has laid stress upon it. He evidently believed that it constituted a good proof that irritation of the *cortex cerebri* (b) in man, in regions corresponding anatomically to the motor centres in the brain of monkeys, also gives rise to movements on the opposite side of the body.

Let us see. The experiment of Dr. Roberts Bartholow is the following, in his own words (as little abridged as possible, because I doubt that all those who quote it have read it); it is entitled "Epithelioma of the Scalp of Thirteen Months' Duration; Exposure of the Dura Mater; Experiments on the Function of the *Posterior Lobes*" (*sic*) (c):—"The part of the brain uncovered was about two inches in diameter in the postero-parietal region. The edge of the ulcer is thickened and hard; the excavation secretes a great quantity of pus. As portions of the brain-substance have been lost by in-

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(a) Miezerjewski, "Des Lésions de la Paralysie Générale," in *Archiv de Phys.* Reprint. G. Masson, 1874.

(b) Ferrier, *loc. cit.*

(c) Roberts Bartholow, *American Jour. of the Med. Sciences*, 1874, page 305: *ibid.*, page 309; *ibid.*, page 310. No italics in text.

jury or by the surgeon's knife, and as the brain has been deeply penetrated by incisions made for the escape of pus, it was supposed that fine needles could be introduced without material injury to the cerebral matter. The needles being insulated to near their points, it was believed that diffusion of the current could be restricted. Observation 3. *Passed an insulated needle into the left Posterior lobe, so that the non-insulated portion rested entirely in the substance of the brain.* The other insulated needle was *placed in contact with the dura mater*, within one-fourth of an inch of the first. When the circuit was closed, muscular contractions of the right upper and lower extremities ensued, as in the preceding observations; faint but visible contractions of the left orbicularis palpebrarum and dilatation of the pupils also ensued. Mary (the patient) complained of a very strong and unpleasant feeling of tingling in both right extremities, especially in the right arm, which she seized with the opposite hand and rubbed vigorously. The needle was withdrawn from the left lobe, and *passed in the same way into the substance* of the right. When the current passed, precisely the same phenomena ensued in the left extremities, and in the right orbicularis palpebrarum and pupil. When the needle *entered the brain substance*, she complained of acute pain in the neck. In order to develope more decided reactions, the strength of the current was increased by *drawing out the wooden cylinder one inch.* When communication was made with the needles, her countenance exhibited great distress, and she began to cry. Very soon the left hand was extended as if in the act of taking hold of some object in front of her; the arm presently was agitated with clonic spasms; her eyes became fixed, with pupils widely dilated; lips were blue, and she frothed at the mouth; her breathing became stertorous, she lost consciousness, and was violently convulsed on the left side. After another experiment the patient remained in bed, and was stupid and incoherent; in the evening she had a convulsive seizure. She afterwards had profound unconsciousness, was paralysed on the right side of motion and sensation, had convergent strabismus. Autopsy: Before making an inspection of the needle wounds, the brain was placed for twenty-four hours in a solution of chromic acid. When sufficiently hardened, careful horizontal sections were made of the upper part of the hemisphere, to ascertain what injury, if any, had been done to the cerebral matter. *The track made by the needles could be distinctly traced on both sides.* On the left side the needle had entered the upper parietal lobule of Ecker, the gyrus centralis posterior of Henle, the postero-parietal lobule of Turner, one inch from the longitudinal fissure, *and had penetrated a depth of one inch.* The track of the needle was marked by some diffluent cerebral matter, two lines in diameter. On the right side the needle had entered the same convolution, but more posteriorly and one inch and a half from the great longitudinal fissure. *The needle on the right side had also penetrated to a greater depth, one and a half inch*, and its track through the lobe was marked as on the other side by a line of diffluent matter."

I have used Dr. Bartholow's *own phraseology all through* in order to be more accurate. I only beg the reader to notice that the needles being insulated up to their extremities and introduced to a depth of one inch and a half *into the substance of the brain*, the aim of the experiment has beeu lost sight of, *as no cortical matter coutd possibly have been irritated* in this experiment. I refrain from any kind of comment whatever. The case *in all its aspects* speaks too eloquently for me to say anything. This is the *experimentum in corpore vili* of which Dr. Ferrier has written (a)—" In addition to the pathological evidence of the existence of differentiated motor centres in the human brain supplied by the observations of Hughlings-Jack-

(a) Ferrier, *loc. cit.*, page 296.

son and others, we have the experimental confirmation of the same in the investigations of Dr. Bartholow"!

I have another side of the question of localisation yet to examine. I have to speak of the centres of sensation, common and special. The localisation of the different senses is not supported by such an array of pathological facts as those regarding the motor centres. The sense of sight Dr. Ferrier localises in the angular gyrus. The experiments performed by him are of two sorts: he has irritated with electricity and destroyed with a cautery the area of the angular gyrus—the seat of the sense of sight. When he has used electricity, he has seen general movements of the head to the opposite side, and very often contraction of the pupils. These phenomena, he thinks, are purely reflex, consequent on the excitation of the subjective visual sensation. On destruction of the angular gyrus in both hemispheres, however, the loss of vision is complete and permanent (his animals did not survive long). I repeated the last-named experiment some time during the summer of 1875. I was successful enough to keep one dog alive several weeks, in the laboratory of the Muséum in Paris. Although the animal did not distinguish objects at first, he could see perfectly well at the end of that time. He *had his pupils widely dilated;* they returned gradually, with alternations of contraction and dilatation, to the normal state. At first no light could make them contract. In human pathology there is not a case on record, *even unequally viewed*, of destruction of that angular gyrus followed by loss of sight. Dr. Hughlings-Jackson, (a) who the last ten years has been on the look out for such cases, says that he has never met with one. Diseases, such as tumors, produce blindness in a very *indirect* way, by leading to acute changes in the optic nerves, on which blindness may follow. There is not, he says, so far as he can judge, a particle of evidence from clinical medicine to prove that destruction of any part of the cerebral hemispheres produces defect of sight. Yet Dr. Ferrier gives it as a proven fact that there is a visual centre in the angular gyrus, and goes on to discuss the theory of decussation, complete or incomplete, of the optic fibres. He adopts Professor Charcot's view of double decussation. How that hypothesis accounts for the various kinds of loss of sight, partial or total, I am unable to see. But one thing is certain ; according to that theory, the angular gyrus of one hemisphere only animates the eye on the opposite side. I have taken out the left eyeball in a dog, and cauterised the left angular gyrus-centre. The animal *did see!* Goltz has performed a similar experiment, with identical result, I believe. Dr. Ferrier does not admit the so-called law of substitution, but he thinks that in his experiments there has been "a compensation from the centre in the other hemispheres." I only discover in that whole mode of reasoning the fatal recurrence of *petitio principii.* The experiments which I have made, and that performed by Goltz, dispose of even the theory of "compensatory action." It is needless to observe that there is no reason why the movements in the eyeballs, frequently associated with movements of the head to the opposite side, and which follow electrical irritation of the angular gyrus, as Dr. Ferrier has himself seen, should be reckoned by him to be only reflex — only due to awakening of subjective visual sensation. Unless the fact be granted beforehand that that gyrus contains a visual centre, the whole theory, as I have said, is a *petitio principii.*

The sense of hearing is found to be localised in a portion of the superior tempero-sphenoidal convolution; this conclusion has been reached through the two methods of investigation usually employed by Dr. Ferrier.

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(a) J. Hughlings-Jackson, "Clinical and Physiological Researches on the Nervous System," page xli., *note.*

Electrical irritation of this convolution in the monkey, he has found to result in certain definite phenomena—sudden retraction or pricking up of the opposite ear, wide opening of the eyes, dilatation of the pupils, and turning the head and eyes to the opposite side. All these phenomena, like *similar ones* noticed in the experiments on the sense of sight, are said to be of the same nature—reflex. Destruction with a cautery of both those gyri renders the monkey deaf altogether. I have seen all the phenomena described by Dr. Ferrier, except the *complete deafness*, after destruction of both gyri. I have experimented on dogs and rabbits. I have seen the dogs recover the faculty of hearing after some weeks. The only cases of destruction through disease of the acoustic nerves, involving their whole course up to the fourth ventricle, in man, that I know of, have been put on record by Dr. Luys. I have reported them earlier in this paper. Luys has seen that the lesion consisted in the destruction of the grey matter of the occipital lobule; he has given a plate which leaves no doubt as to this localisation. (I do not wish to be understood to accept Luy's localisation.) I believe that it is not unjustifiable to ascribe loss of hearing, in the experiments reported above, to another cause than destruction of the centre of that sense. When I come to state what I think is the most acceptable provisional theory, I shall try to explain what that cause is. As I have no desire to swell this review into a volume, I shall enter into no further details concerning the other senses—those of tact, of taste, and of smell. Their existence being localised in different regions of the cortex by the same order of facts as are used by Dr. Ferrier with regard to the other senses, already spoken of, the same kind of arguments can be raised against the validity of the proofs adduced.

I will, however, note one fact to show how deductions are drawn, as to function of parts, on false premises. I choose the sense of smell, because it is a typical instance of what I advance. Dr. Ferrier, after relating the experiment of Magendie which showed disappearance of the faculty of smelling after section of the trigeminus, remarks that the experiment did not prove that the fifth nerve was the nerve of smell properply so called, but that the integrity of the fifth nerve was necessary to the due functional activity of the olfactory nerves. But he also says that " when the tactile sensibility was entirely gone in the nostril, the *vapor of acetic acid* (!) caused copious lachrymation—a fact which shows that some afferent fibres still remained functionally active in the nostril, "evidently the olfactory nerves," which, however, owing to the loss of common sensation, were not of themselves sufficient to convey the impressions of odors." If, notwithstanding destruction of the sensitive nerve, *acetic acid* induced lachrymation, it must be through a reflex of the olfactory nerve, as Dr. Ferrier shows; and if the olfactory *can* give such a reflex action under such circumstances, it is evident that it is impressed in the same manner as a sensitive nerve. Now, what signifies his postulate that the integrity of the *sensitive* nerve is necessary for the conveyance of the impression of odors?

Absence of the fifth nerve does not destroy the faculty of tasting nor the faculty of seeing. It does so in some cases where there are reflex actions at play—inhibitory actions. On the other hand, there are cases on record of destruction of the first pair of nerves, and yet preservation of the sense of smell. The celebrated case reported to Claude Bernard is in the memory of all. He mentions the case of a woman, who during life disliked the *smell of tobacco* (it requires a special sense to smell tobacco, whereas *acetic acid* acts on the fifth nerve as well!), who knew that a *sink* near her bedroom was dirty by its smell, etc., in whom the remarkable fact was discovered of the congenital absence of the olfactory nerves! How to explain this case? Is it explained in accordance with what Mr. Lewes maintains, that *neural processes* are uniform in character, the diversity of their results (sensation, motion, or secretion) depending on anatomical connexions? Is it explained by the theory of *organised past experience* of the fifth nerve from childhood upwards?

Another very original part of Dr. Ferrier's work is the one in which he treats of the occipital lobes. Dr. Ferrier believes that the viscera have fibres which transmit their special sensations to the sensorium, (a) and that the centres of these fibres are in the occipital lobe. The sense of *hunger* has its seat there. So we have a *sense of hunger*. The reasons given by Dr. Ferrier for this new discovery are, that his animals could drink, but *refused to eat*, when the occipital lobes had been destroyed. At first sight I suspect that the animals in this experiment do not eat for the same reason that they do not when they have had both angular gyri destroyed—because the masseter muscles have been cut through or separated from their occipital-parietal insertions; which is a sufficient cause to explain the difference as to taking food between this experiment of removal of the occipital lobule, and destruction of the anterior lobes when the animals do eat (the masseters are not cut away so extensively from their insertions then). In fact, the animals cannot masticate. Dr. Ferrier says himself that one animal which was kept alive on the *fifth* day after the operation of removal of the occipital lobule, and which had up to that day refused food, "took a cold potato offered, smelted it several times, and at last, as if struck by a new idea, began to eat with great relish. From this time it began to take food, and recovered." As it is impossible to suppose that within five days after their destruction the anatomical tissues in which the sense of hunger is localised could have been regenerated, I cannot understand how the reappearance of that sense can be accounted for. Let us not forget that on anatomo-pathological evidence Luys has localised in these same occipital lobules the sense of hearing, (b) and that Dr. Joffroy, (c) one of the ablest pupils of Professor Charcot, and once a *collaborateur* of Duchenne, has localised trophic centres; lesions of that lobule, he thinks, bring on sloughing of the sacrum, etc. It is needless for me to add that there are at least twenty-five cases *above criticism* of lesion of those lobes, without any phenomena whatever but hemiplegia.

I think that with regard to the motor centres of the cortex cerebri of the monkey and of man Dr. Ferrier is at variance with what observation teaches. I have had no opportunity to experiment on monkeys, but Dr. Ferrier agrees that in the other animals the so-called lost psycho-motor functions of that region can be recuperated, while in man loss of the cortical centres leaves lasting paralysis. I do not know of one single case of destruction of the so-called motor centres in the brain of man having caused lasting paralysis. My experience goes the other way. The cases which I shall report, and which belong to those series published by Professor Charcot and others, will bear out my statements.

I have now reached that interesting chapter of Dr. Ferrier's treatise where he considers the hemispheres psychologically. I have little to say in respect to that matter on account of my little familiarity with it. It appears to me that the whole of it, in its masterly exposition, is one continuous begging of the question. All his psychological deductions, I own, are based upon psychological facts; but those facts I have proved, I trust, to have been considered only in a one-sided way—viewed unequally, as the phrase goes.

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(a) Compare Hughlings-Jackson, *loc. cit.*, page xv., *note, et seq.* He says that the arteries, the heart, and the different viscera, etc., are represented in the highest nervous process.

(b) Luys, *loc. cit.* See page 8.

(c) Joffroy, C. R., Société de Biologie, in *Gazette Médicale de Paris*, 1876 *passim.*

I know that I am liable, on *primâ facie* evidence, to be charged with levity for this statement, considering that there is a seeming concordance between the theories of the advocates of the localisation doctrine and the deductions of Herbert Spencer, of Professor Bain, and of others. But let it be remembered that this concordance only proves that we have several valuable *similar or identical* deductions, reached through different processes of reasoning; it does not prove that any one of the theories advanced is better grounded. The two eminent philosophers whose authority is adduced by Dr. Ferrier seem to me to have availed themselves, in lieu of premises from which to evolve their powerful reasoning, of results of the physiological and anatomical researches of Professor Laycock, Dr. Carpenter, Dr. Hughlings-Jackson, Professor Broca, Professor Helmholtz, Professor Brown-Séquard, Professor Ferrier himself, Professor Hitzig, etc.,—which results, notwithstanding their extrinsic value, are still *sub judice*. The foregoing considerations warrant me, I presume, in pointing out the strangeness of adducing authority of such an origin by way of philosophical confirmation of their theories by the advocates of the localisation doctrine.

Here I relinquish this theme, for fear of being lost if I once launch into a subject too unknown to me.

I have next to expose what views are held concerning speech. The question of aphasia is one which, for the last thirteen years, has agitated pathologists, physiologists, and psychologists equally. Dr. Ferrier has handled this subject in the same manner that he handled the others.

## Part II.

The oldest precise localisation known was established fifteen years ago by Professor Broca. His theory teaches that the faculty of speech is localised in the third frontal convolution of the left hemisphere. There exist a great number of pathological cases which show a very frequent coincidence of disease of that convolution with loss of speech. But ought we to adopt the theory of Broca, so ably maintained by Hughlings-Jackson, and which is even said by Dr. Ferrier to derive support from his experiments on the brains of monkeys?

First, there are cases of aphasia which have occurred with destruction, not of the left, but of the right frontal convolution. It has been answered against this fact that the patient must have been a left-handed person; hence the rule holds true. But, secondly, there are cases of aphasia with lesions seated in other parts of the left hemisphere than the left frontal convolution, or even of the island of Reil, which, according to the greater number of authorities, also is concerned in the faculty of speech; and, thirdly, there are cases in which the island of Reil and Broca's convolution have been found diseased, and no aphasia observed There are numerous cases enough of this last description, and of the preceding one, to enable me to state at least that if Broca's convolution is the seat of the faculty of speech, it is not the only one; but of course this is only a *pis aller*, because, if we look more into the subject, it will very soon appear that Broca's convolution is not, more than any other convolution, the organ of speech.

The proofs which I can adduce to support this statement are numerous, and, I believe, have some value. It is necessary for me to state at once here that I cannot consider the six or seven different kinds of aphasia usually treated of by authors; physiologically, they are only several degrees of one kind of aphasia.

The cases which I will report now have been well observed, and published by physicians of merit who have recorded them for the purpose of elucidating the subject of aphasia. I shall, therefore, not mention the old cases contrary to the would-be speech-centre, found in the book of Dr. Bateman, and in the works and papers of other authors.

The first and most striking case of destruction of the so-called speech-centre without consequent aphasia is the celebrated American crowbar case. I believe I was the first to report that extraordinary case in France. Dr. Ferrier, in commenting on it, lays it down that only the anterior portion of the frontal lobe was destroyed by accident. It is well known that the subject of that observation, while occupied in blasting a rock, was wounded by the unexpected explosion of the blast at which he was engaged. The tamping-iron which he was using was several feet long and *an inch and a quarter* in diameter; it passed, according to measurement which I have made, through the brain on the left side, in such a manner as not only to destroy the left Sylvian artery, which sends a special artery to Broca's convolution, but it actually destroyed the greater part of the island of Reil. A great quantity of brain-matter was discharged for several days after the accident, in consequence of sloughing. I had an opportunity of seeing that cranium in Boston last winter, and also the iron bar. The man (Gage) was never aphasic, nor paralysed. There is another case, recorded by Theodore Simon, (a) which proves that a notable loss of substance of the brain-matter in the orbital region can exist for years without giving rise to appreciable symptoms, and that the destruction of the *insula* on the left side is not necessarily followed by loss of speech.

There is a case recorded by my excellent friend Dr. Troisier, in which aphasia has existed with no other lesion but one found in the postero-parietal region. The following case is also very interesting, as it shows partial aphasia with very limited paralysis in a patient, observed by one who took the notes for the purpose of elucidating the subject of localisation of functions in the brain. The patient was observed in the wards of Dr. Luys. There was right paralysis of the tongue and of the face; sensation intact. (b) " The left arm and hand are paralysed to a slight degree as to motion and sensation. The right eye and ear are impaired; there is no apparent lesion of the transparent media of the eye. The vocabulary of the patient is exceedingly limited, and consists of only a few words, which are used for all purposes. There is impossibility of reading; A is taken for B, etc. Cannot write her name. Cannot write, although the pen is well held; writes C*o*roline for C*a*roline; her name being Madame *Coha*don, she writes Madame *A*don, and then Madame *Coa*don, etc." The autopsy discovered in the left hemisphere a yellow (ochre-colored) softened patch having destroyed the first and second temporal convolutions; "above and forward the lesion is limited exactly by the sulcus which separates the insula from the sphenoidal lobule; behind, it bends round the posterior extremity of the fissure of Sylvius, leaves untouched the whole third convolution, but ascends on the *pli courbé* (gyrus angularis), of which the greater part is so destroyed, and is found a little further in the sulcus which separates the superior parietal lobule from the inferior one. On the occipital lobe it penetrates a little in the parieto-occipital sulcus, destroys a part of the internal and of the external portion of the cuneiform lobule." In the hemisphere itself there was detected a *lacuna* in the white substance, with very neat lining, corresponding in site to the junction of the first temporal convolution with the lobule of the gyrus angularis; also, a very small *foyer* (spot of softening) with yellow borders in the posterior portion of the thalamus opticus. In the right hemisphere there was

(a) Théodore Simon. *Deutsche Klinik*, 1873, Nos. 17 and 18.
(b) Sabourin, Société Anatomique, rapporté dans *Le Progrès Médical*, 1877, p. 70.

nothing in the convolutions; but in the white substance there were two pisiform lacunæ close together, above the internal capsule, one centimetre and a half from the base of the corresponding convolutions. In the lenticular gangl on of the corpus striatum there was also a yellow matter resulting from old disease, extending from behind forward, exactly on the limit of that nucleus and the external capsule. The softening extended up to the most distant portion of that nucleus lenticularis. There was also in the centre of the thalamus opticus another old lacuna, as large as a hempseed. In the pons, on the right side, above the large inferior fasciculus, there was a small darkish patch five millimetres in size. All the other organs were healthy.

This observation is remarkable in more than one aspect. But I desire only to observe here that there was aphasia without destruction of the third frontal convolution or of the insula.

Dr. Brown-Séquard, in his Lectures in course of publication in the *Lancet*, will doubtless bring out such a large number of similar facts, that I have no occasion for reporting more here. I beg to observe that I have given cases well observed and well recorded by physicians of ability, which prove the postulate written above—that aphasia may exist without or with lesions of Broca's convolution, and also with or without lesions localised in other parts of the brain.

The experimental proofs of localisation of the speech-centre in the left third frontal convolution in its posterior part where it overlaps the insula, Dr. Ferrier pretends to have discovered; for he says that this region in the brain of man "corresponds with the situation of the motor centres of articulation in the monkey." Now, in the monkey, on the irritating only of one side, he has had movement in both sides of the tongue and in the orbicularis of the lips; but on what grounds a similarity is established between the two phenomena, contraction of the muscles of the tongue and aphasia — the first in the monkey, and the second in man, — I fail to see. It must not be forgotten that man may suffer from loss of speech through paralysis of the tongue, the aphasia then being apparent only; and that if the experiment on the monkey proves anything, it proves only that last point, judging bp analogy. Now, if aphasia is not due to loss of function of a speech-centre situated in Broca's convolution, how are we to explain it? I believe that it is due to a reflex process—a process of inhibition. That there is a reflex inhibitory element in it ean be seen by reading carefully even the old authors. Trousseau has recorded several instances to the point. Indeed, his lecture on that subject of aphasia contains nothing but cases which, when properly analysed, go a good way to overthrow Broca's theory. One patient who could say nothing but "*Oui*," one day, having let fall his handkerchief, a lady who was near him picked it up; upon this he said "*Merci*" (*i. e.*, Thank you) in a high and intelligible tone. He was asked to repeat the word; it was several times uttered before him, but in vain—he never afterwards could say it. (a) Again, another patient, who for three months after the onset of the disease could say only a few words with no meaning, and the same in all circumstances, yet one day, two weeks after the stroke of disease, said to his wife, "My dear." Again it was in vain that he was asked to repeat it. (b) In that same celebrated lecture Trousseau has recorded Professor Charcot's case of aphasia as complete as it could be, existing with the integrity of Broca's convolution. Professor Broca had himself examined the brain of that patient microscopically. So that when Professor Charcot said last year, in the Société de Biologie, that he rejects all his former cases up to that time as being incompletely observed,

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(a) Trousseau, "Cliniques de l'Hôtel-Dieu," t. ii., p. 586, deuxième édition.
(b) Trousseau, *loc. cit.*, p. 592.

we beg leave to retain this one, because a double examination was made by himself and by Broca, and microscopically. It is needless for me to state that this case was a reflex inhibited instance like the others ; but the following is more striking still. It is reported by Dr. William Wadham. (a) A boy, who was the subject of hemiplegia of the left side, and who was ambidextrous, was subsequently affected with aphasia, which continued complete for three months. The only lesion found was a nearly complete destruction of the island of Reil on the right side. The author, in arguing on this case, very properly shows that this aphasic does not come into the category of left-handed persons because he was *ambidextrous;* and also of his *subsequent* recovery of speech, that it was not a paralysis of the tongue, notwithstanding appearances detected at the time, because his mother had to teach him the words after a long process, consisting in making him repeat after her each word. Moreover, if it was paralysis of the tongue, and not aphasia, having recovered from that paralysis sufficiently to articulate, however imperfectly, one word or phrase, he would quite as readily have given expression to his thoughts, whereas "Yes" and "No," "Good morning," and "Dr. Wadham," were the only words he had succeeded in learning. He was, however, able to write and spell correctly. He was only deprived of the power of converting his thoughts into words.

Dr. Wadham has very judiciously observed also that this case cannot be considered as a "left-handed case" in which the seat of speech is transferred to the right hemisphere, because the recovery of speech took place without any repair of the cerebral lesion, as shown at the post-mortem examination. He believes that the boy, although strongest in his left hand, being ambidextrous, the other (left) speech-centre came somewhat to fulfil the function which was only partially developed in it. Here I no longer agree with Dr. Wadham. First, it is to be observed that Broca's convolution, or its analogue on the right side, is said to have been healthy; and, secondly, there must have been reflex or inhibition processes in the case, because the parts destroyed cannot account for the presence of hemiplegia.

Another very interesting case is recorded by Dr. Schaltz. (b) A patient had at one time, after a traumatism in the parietal region, vertigo, pains, etc.; fifteen days later, paralysis of the arm and leg on the left side, loss of speech. Eleven days later, right hemiplegia with exaggerated reflex sensibility; after lasting one day, that hemiplegia disappeared. On the seventeenth day the aphasia, which was absolute, began to pass away; after three weeks there was nothing left of it. Three weeks after his entry into the wards, an operation was performed in order to remove the pus, of which the presence was shown by manifest signs and the presence of a tumor as large as a hazel-nut. On that (left) side of the head, nine centimetres below the saggital suture, and on the tract of the coronal suture, some pus was discharged, together with white matter. He, two months and a half afterwards, was well to all intents, except that there was a slight prolapsus of the left superior eyelid. He began to recover even a quarter of an hour after the operation.

Certainly in this case, again, there was another influence at work than destruction of the island of Reil, or of Broca's convolution, or of pressure by pus. Firstly, if those causes were at work, it would show that the aphasia depended upon the lesion of the speech-centre; but how explain the *left* hemiplegia then? And if this *left* hemiplegia is due to a reflex action, why not the aphasia also, since the speech-centre was not touched, or, if it was

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(a) William Wadham, M. D., *St. George's Hospital Reports*, vol. iv., 1869, p. 245 *et seq.*
(b) Schalz in Hayem's *Révue des Sciences Médicales*, t. i., p. 661.

touched, how could the patient recover speech so speedily after the operation?

Another case by Dr. Proust. (a) A young man, in a fight with some soldiers, received a bayonet-wound in the left parietal region on October 8, 1876. He never had any symptom till after several days, and then even only headache. On the 19th and 20th of the same month Dr. Proust saw him in his ward, having then aphasia and paresis of right superior limb, and facial hemiplegia. The three symptoms increased slowly. (Dr. Proust watched the patient with great care; he is well known for having contributed to that special subject of aphasia a very valuable memoir.) Trephine was applied *loco dolente*. The fractured bone was taken away, and, at the same time, all the symptoms diminished. The dura mater was not even perforated. The operation was hardly terminated and the wound was not yet dressed, when the patient was already cured. On comparing by measuration the region of the seat of the injury in his patient with data obtained upon crania of other deceased patients, Dr. Proust was able with almost mathematical precision to localise the injury received by his patient only in the left ascending parietal convolution in its middle part. The Doctor adds, besides, that probably it engaged the frontal convolutions by nutritive troubles of neighborhood; hence the ready explanation of the aphasia, the right paresis, and the paralysis of right face. But of course this view cannot be entertained. How can such be the case, since the patient recovered so rapidly? The nutritive changes cannot have disappeared in such a short space of time.

All those cases, I believe, go far to show that loss of speech does not depend upon destruction of a would-be speech-centre, but to an inhibitory reflex action. Language—speech—is essentially a reflex process altogether. It is verily an "organisation of past experience." Speech in man is not different from speech in birds; the only difference said to be detected between acquired speech in a bird and in a boy, is that the boy has the advantage of being able to associate a certain idea with a certain speech, whereas the bird is said to be deprived of that faculty—by some altogether, by others to some degree. I have had opportunity to watch speaking birds several times, and I repeat it, I can detect no difference. We must remember that in a child speech is evoked by external circumstances; so with the bird. It is only when a child has grown that speech appears to be spontaneous; but even then, after all, it is brought about by a reflex process or a process from past organised experience acting upon articulatory centres. The bird when well educated does perfectly associate the words "Good morning, sir!" with the presence of a stranger or of somebody coming into a room, and a child does nothing else.

That a reflex action is at the bottom of all that process, can be found out by a series of considerations which many can confirm for themselves. My friend Dr. Onimus has studied cases of aphasia in that direction, and he has recorded some very interesting instances. (b) A concierge (janitor), who was suffering from a stroke of paralysis and aphasia, was recovering to some extent, and words were being taught to him. One day Dr. Onimus pointed out to him a statuette made of plaster-of-Paris which was on one of his tables, and asked him to call it by name. He could not do it. The Doctor then discovered that if some other subject was pointed out to him, and only the first syllable said, he would repeat it readily, and, as if moved by a spring, the whole word would come out. He therefore told him again to name the statuette, and said, by way of prompting, *sta*, the first syllable of

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(a) Proust, *Bulletin Général de Thérapeutique*, t. xci., 11 liv., December 15, 1876.
(b) Onimus, *Journal de la Physiologie*, "De l'Anatomie de Robin," 1873, No. 6.

"statue," but the patient could not finish the word. His wife, who was present, said, "Why can't you say an *es*tatue?" He at once said an *es*tatue. Now, the uneducated people in Paris do not say a *sta*tue, but an *es*tatue. That aphasic patient, therefore, with many other examples known to all, shows very conclusively that speech in man, as well as in birds, is acquired by the same process. I beg leave to submit that perhaps when birds have been educated to talk for several generations consecutively, uninterruptedly, the subject of aphasia will receive a great deal of light.

What I have said of aphasia applies, I believe, as well to other paralyses from brain diseases. I am of the opinion that the *so-called* hemianæsthesia following destruction of the internal capsule is a reflex inhibitory phenomenon. I have said so already. I will now prove it. Professor Charcot has insisted very much on that one localisation. Veyssière, and Carville, and Duret, and Ferrier, have all based upon experimentation the same theory, that the internal capsule is the corner of nerve-matter which, when it is the seat of disease, causes loss of sensation for half of the body. Besides the arguments which I have already given to show how such a loss of sensatian could happen, I will say now that Professor Charcot has himself within a few months shown that that anæsthesia is not an absolute one. While experimenting on the so-called metallo-therapic method, he has found that sensation, general and special, can be recalled in paralysed parts by the application of a gold coin or ring to the limb deprived of sensation. (a)

Now, it is clear that if in those cases of undoubted destruction of the so-called internal capsule-centre, of old standing — more than ten years — sensation can have been awakened even for a few hours, the non-permanent manifestation of that sensation is not dependent upon destruction of a centre, because it would be impossible to have the results now obtained.

Again, in the cases published by Dr. Lépine, and of which I have already spoken, and in the case of Dr. Wadham reported above, and the case of Dr. Sabourin, certainly the phenomena observed during life did not correspond with the lesions detected. Let anyone think over the last case in particular, and he will see how impossible it is to make it agree with the theory of localisation.

Moreover, it is denied by nobody that some cases of undoubted destruction of brain substance can exist, and yet the function, temporarily absent, appear again. Dr. Hughlings-Jackson has written on that subject — "The slightness and transientness of a paralytic symptom depend on the slight extent of lesions of nervous organs, not on slight *degree* of change. .. (The patient) recovers, because he has lost *a small quantity* of that tract (motor.) For it is manifest to those who make post-mortem examinations that recovery from paralysis occurs when a part of the motor tract is permanently wanting." (b) It is evident from that quotation that the paralysis must have some reflex element in it. For, during its existence, as well as after its disappearance, the same lesion existed in the motor tract; and if it is not so, how to account for the paralysis or for its disappearance? I know how unbecoming of me it is to criticise the opinion of such a distinguished physician and such an acute clinician as Dr. Jackson, but I am impelled by facts to urge that the size or extent of the lesion has no influence on the transientness or slightness of a paralytic attack. There are numerous instances of what I advance here. Let us only remember the magnitude of the brain lesion in the case of Dr. Sabourin. Some other element is concerned in that production of disorder and its duration. It is proved that the nature of the lesion, tumor, softening, hæmorrhage, traumatism, etc., have no influence.

(a) See, for details, C. R. de la Soc. de Biologie de Paris for 1877.
(b) J. Hughlings-Jackson, "Empir. and Scien. Inv. of Epil.," *Med. Press and Circ., passim.*

considered specifically. It remains only to admit that the lesions do create an influence by the mere fact of their presence, which influence is the agent at work: that is to say, that, irrespective of size, the lesion acts just as an irritant applied to the skin does — to awake a reflex process. It is by the foregoing theory also that those cases can receive i nterpretation in which we see that a lesion localised in one hemisphere will cause simultaneously, or one after the other, paralyses on either side of the body.(a)

I am aware that it has been stated with regard to those cases which appear to the advocates of localisation contrary to their theory, that the observations were incompletely taken, or that an unseen lesion may have existed, or that there was an anomaly, just as when the heart is situated to the right, etc. Those objections are more apparent than real. To give only two instances, the American crowbar case and the case of Dr. Sabourin militate against such a view. And how can we attribute to an unseen lesion the paralysis observed, without at the same time admitting that the large lesion seen in a motor (so-called) centre, but on the corresponding side, has caused no symptom, and in this way destroy our very argument? And, moreover, a case of anomaly cannot be considered, because we know of cases in which one lesion in one hemisphere has caused paralysis on the corresponding side, which was cured to some extent, and later another lesion occurring in the other healthy hemisphere has again brought on paralysis, not only on the formerly paralysed side, but also on the one which was not originally so affected. (b) The loss of sight, amaurosis, is amenable to the same reasoning. Let me only say here that two of the very ablest pupils of Professor Charcot have just published researches which demonstrate very plainly two very diherent things. I explain: Dr. Ferrier has localised the sense of sight in the angular gyrus in man. Dr. Féré has observed a patient who for three years has had pain, etc., consequent upon a fall, which was followed by a wound in the right parietal region; the only symptoms now remaining are contractions, which specially show themselves when the patient does not pay attention, in the orbicularis, palpebrarum, the muscles of the eye, and the zygomatic muscles. Dr. Féré made an attentive study of that patient, and by means of careful measuration and comparison, after Broca's method and his own method, with a number of crania, arrived at the conclusion that the spot of the bone depressed by the fall coincided with the posterior portion of the *pli courbe*, or at the very least with a region very near that spot; which is concordant with Dr. Ferrier's views. (c) But Dr. Pitres, another pupil of Professor Charcot, shows (d) ten cerebra marked by cortical lesions, from which (the symptoms during life being kept in mind) he concludes that the destruction of the *pli courbe* (gyrus angularis) is accompanied by *no symptom in the muscles of the eye;* (e) that destruction of the three superior quarters of it gives rise to paralysis of the face, but not of the limbs.

It is impossible for me to examine in detail all the various subjects comprised in the study of cerebral physiology, and chiefly those which Dr. Ferrier has examined in his work, without writing a volume. I have no inclination and no aptitude for that work, but I must, however, say something on the chapter of Dr. Ferrier's volume, "The Hemispheres considered Psychologically." First, any candid reader will readily perceive that those metaphysical deductions do not spring from the author's experiments, nor even from the deductions or theories which might legitimately be derived from them even if they were accepted to prove what their author claims. Secondly, it appears to me that he has given one illustration—a capital one—which

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(a) Brown-Séquard's Lectures, *Lancet*, 1876, pp. 211 *et seq.*, 245. 279 *et seq.*
(b) Brown-Séquard, *loc cit.* (c) Féré, *Gaz. Hebd. Méd. et de Chir.*, No. 9, 3 Mars, 1876.
(d) Pitres, *ibid.*, p. 812, 1876. (e) No italics in text.

destroys from the very foundation the theory of localisation of functions : I allude to the case of Laura Bridgman, quoted several times by Dr. Ferrier himself.

That person, deaf, dumb, and blind, communicates with the outer world by means of digital language. It has been observed that, just as some persons when deeply thinking allow their lips unconsciously to move as if they were speaking, and indeed that some others when thinking whisper audibly, and in some instances speak aloud, so Laura Bridgman likewise is seen to have unconscious movements of the fingers, just as if she were communicating with somebody, when she is in reality only thinking. The same thing happens when she is dreaming.

Now, it is clear to everyone that by an educational process of long duration the would-be motor centres in the convolutions (to please localisers) must have aquired also the faculties inherent to the speech-centre which in her case were deficient; or if it be said that the speech-centre was intact, that there was no aphasia, but trouble of the external organs of speech, then it must be conceded again that a channel has been established between the speech-centre and the fiinger-centres for the conveyance of the translation of thought into digital language. It can even be argued that in her, if a larynx could be constructed at the end of her fingers, she would actually speak phonetic language just as an educated parrot. The conclusion to draw from this is that there is no part of the cortex specially or specifically endowed with a certain function, but that any such part can become endowed. I know the experiments of Carville and Duret, which, according to them, enable them to establish the theory of "*suppléance*"—*i.e.*, vicarious, supplementary functions established in the neighbourhood. But it is known also to all those who have made experiments on the brain such as Carville and Duret have published,(a) that in a dog they have "taken away the *centre* for the legs in the *right* hemisphere; the voluntary motor paralysis showed itself as usual. After six or eight days the animal had entirely recovered power in its *left* leg. They (we) then took away the centre for the legs in the left hemisphere. Evidently, if the '*suppléance*' (supplementary function) of the two hemispheres existed, paralysis of the *left* leg ought to have appeared. It did not. The dog was paralysed on the right side just as if he had had no lesion of the left hemisphere before." It is evident in this case that the *left* leg must have derived its nerve-impulse from some centre not situated in the *left* hemisphere, nor, certainly, from a regeneration of its own *right* centre. Therefore some portion of the cortex in the neighbourhood must have acquired *that* function which it is supposed it had not before, according to the theory of those who teach that the cortex contains specified centres.

I believe that I have said enough on this subject to show that there are other facts in science besides those taken notice of by Dr. Ferrier in his treatise, and that even the facts analysed by him can be shown to prove the contrary of the theory which he has established. I will observe here, again, that unless one starts with the idea that there are motor centres in the cortex cerebri, it is impossible to succeed in establishing their existence in that tissue, which, after all, may be as well endowed with a sensory function as with any other.(b)

I shall now proceed to state very briefly what I consider to be the explanation of the experimental results of Hitzig, Ferrier, and others.

First. I must state that it has not yet been proved by any means that the cortex cerebri in any portion of its extent can be irritated by any means, mechanical, physical, or chemical, and that even if we were to suppose that

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(a) Carville and Duret, *Archives de Physiologie*, 1875, page 446.
(b) Eugène Dupuy, "Examen," etc.: Thèse Inaugurale. 1873, page 24.

the electricity used by experimenters excites the fibres which are in communication with the cortical cells, and which unite them with lower centres, we could gain no knowledge as to the nature of the function of those cortical cells, because a nerve-fibre will conduct in either sense, afferently or efferently.(a)

Second. And experiment which shows that when both centres for the two anterior legs are destroyed, the animal very soon uses its limbs as well as before the experiment, suggests the idea that the transient paresis which resulted immediately after the experiment, instead of being a withdrawal of influence of a centre, is, on the contrary, an irritative influence exerted by the lesion. The subsequent disappearance of all paresis, and the cicatrisation of the brain-tissue, show that fact plainly.

Third. When, an animal being deeply narcotised, an irritation of the exposed sciatic nerve gives rise to most violent contractions in the muscles of the leg, whilst all reflex actions have disappeared, if the cortex cerebri then does not respond to electricity it is wrong to say that it is because it is first to lose its excitability, as Drs. Ferrier, Carville and Duret pretend, after Flourens. There are many experiments which go against this. Here is one very striking, which I have had occasion to make several times, and which Onimus has specially noted in one of his papers. If from a large healthy frog the right hemisphere is taken away, the animal presents this well-known appearance, that its left side takes the position usual to a brainless frog; the tonicity of all its muscles is increased on that one side, the anterior and the posterior legs are held closer and firmer against the body, which is itself inclined on that side. If now chloroform is administered, all changes. The left side becomes weaker than the right side, which its tonic contraction made it overpower; that effect becomes more and more apparent as the narcotic effect advances, until both sides of the body are equally influenced by the anæsthetic; the frog can then no longer rest on its anterior limbs, and its posterior ones are in a state of resolution. At the same time that the effects of the narcosis pass away, the leg of the sound side in correspondence with the existing hemisphere assumes by degrees its normal position, and comes near the body; whilst those of the right side, which are deprived of their cortical centres, are yet in extension and resolution. A little later, that one also resumes its former position as before the narcosis, and at one time the position of all four limbs is identical; but, when the narcotic effect has entirely passed away, the limbs which are in relation with the side of the hemisphere operated upon are again in a state of stronger *tonus*, and the body of the animal is again more inclined towards the side opposite to the destroyed hemisphere. That experiment also shows that the cephalic centres are the first to recover their excitability. In the cat and the rat the effect *is exactly identical, mutatis mutandis;* so that, in the experiment for the elucidation of the problem of localisation of functions, if there is any one part of the cerebrum which does not react when electricity is used (the animal being absolutely narcotised), it must be the lower eentres, *i. e.*, the corpora striata and thalami optici. But this last fact is not proved; on the contrary, if in an animal so deeply chloroformed the electrodes are applied directly on those ganglia, we do have strong contractions. If the cortex cerebri has any influence on motion, it must be in the sense of a sensitive centre, and not as a motor, because from the foregoing we see a radical difference between it and the other ganglia.

Fourth The objection that the movements which arise from electrical stimulation of the cortex (which the foregoing experiments and arguments would enable us to consider sensitive, and which are thus of a reflex nature)

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(a) See specially the most ingenious experiments of Professor Paul Bert, in *C. R. Soc. de Biol.*, 1877, in *Gazette de Méd. de Paris*, and also in *Le Progrès Médical.*

cannot be reflex, (a) because a reflex "movement is rarely limited to the same group of muscles," is worthless, as everybody knows that no movements are more limited than reflex movements. Indeed, let those who pretend that all actions performed by a brainless frog are purely reflex and have nothing intelligent or psychic, study the beautiful harmony of movements of a frog so prepared. After the ablation of the medulla oblongata, reflex movements are still very localised.

Fifth. The argument derived from this fact, that it can be foretold that on irritating such a point of one convolution in a brain, or in the brains of all animals of one species, or in any convoluted brain, care being taken to ascertain homologous points in all, such a movement will arise in one limb,(b) does not prove the irritated cortical area to be motor at all. It must be kept in mind that no other agent but electricity will produce the phenomena under consideration, and that electricity, as I have experimentally proved, travels aud diffuses. Now, when making use of a physical agent, every time that we shall have identical circumstances we shall also have identical results; and allowing that the electricity diffuses to the base of the brain, (c) to different ganglia, etc., it is natural that in all convoluted brains we should have the same results, and be able to foretell them; but the mere fact that when the sulci in different brains (species) assume a different direction, the point to be irritated to produce one given movement also varies—showing that always a constant relation of the area of the cortex of the brain with the base of the brain or the ganglia must be an element for obtaining the sought-for result—destroys the only objection, as shown by the different situations of some centres in the brain of the cat, the ape, the dog, etc.

Sixth. The fact that irritation of the same area (d) of dura mater will give rise to the localised movement in certain group of muscles identical with the following irritation of the cortex,(e) and that even mechanical irritation will give the same results, is contrary to the theory of Hitzig, Ferrier, Carville, and Duret.

Seventh. The fact that irritation of the cortex after the manner of the forenamed experimenters has given rise, when the leg centres only were acted upon, to peristaltic movements of the intestines, to hyper-secretion of saliva, to increase in the blood-pressure, to contraction of the spleen,(f) etc. tends to show that if the opinion of Hitzig and Ferrier and others is to be maintained, that the cortex is a motor centre, we must *ipso facto* admit that those *same* centres of motion are centres also for the different functions just enumerated

Moreover, within the last year Dr. Brown-Séquard has shown that burning of the cortex in a large area will bring on paresis—in some instances on the same side, in others on the opposite,—and always vaso-motor paralysis.(g) Bochefontaine has also given some reasons for believing that the elements acted upon are not those which make up the cortex; and Eulenburg and Landois(h) have shown that in irritation of the cortex the so-called motor areas act very much like vaso-motor centres.

I had already in 1875 advanced the view (which appeared to me to be well substantiated by experiments) that the phenomena which we saw in those experiments were reflex, being the results of vaso-motor influences.

The facts and arguments detailed above had satisfied me that the cortex cerebri was no part in the process which produced the phenomena of con-

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(a) Carville and Duret, *loc. cit.*, p. 437.
(b) Ferrier, *loc. cit.* (c) Dupuy, *loc. cit.* (d) Dupuy, *loc. cit.*
(e) Bochefontaine, *C. R. Soc. de Biol.*, 1875, December.
(f) Bochefontaine, and Bochefontaine and Lépine, *C. R. Soc. de Biol.*, ann. 1875-76, *Gazette Méd. de Paris*, 1875, pages 575, 643; and 1876, last quarter.
(g) Brown-Séquard, *Archives de Physiologie*, 1875, No. 6.
(h) Eulenburg and Landois, *V. Archiv*, Bd. lxvi., Heft 4, s. 489.

traction of groups of muscles after irritation of certain areas with electricity; and I discovered that those points of the cortex which are called "ideo-motor" are those to which a comparatively larger artery from the pia mater penetrates, not into the cortex, but deeper, into the white strands beneath; also that the only spot in the homologue of the angular gyrus of man in the dog which does similarly give rise to movement and to hypersecretion of saliva, is also the one at which there is such an arterial arrangement. I have given details on that subject elsewhere. (a) Those vessels are provided with nerve-fibres, which go along with them, or singly, into the brain-tissue. The arteries also send smaller twigs, which ramify like roots of trees into the cortex alone, but everywhere in it; whilst those with which I am concerned have no office with the cortex.

I had found by my former experiments (b) that substances which diminish or increase reflex action by acting through the vascular system, also increase or diminish the intensity of the phenomena observed in the experiments under consideration; and taking notice that the cerebral pia mater contains a very great number of ganglion cells and fibres which are those of which I have just spoken above, and which are in relation with other cells found in the tissues under the convolutions, I came to the conclusion that the movements observed after irritation of the cortex cerebri are of the same nature as those which are seen when the four main arteries of the brain are simultaneously tied. That, in fact, we had in one limb what Kussmaul and Tenner have observed in the whole animal in their experiment.

I have noted that the time, from the instant of applying the electrodes to the cortex to the actual production of the movement, is sensibly the same as that which is required to stop the bleeding of a small artery in the spinal cord and laid bare, when a branch of a communicating nerve is irritated with electricity, and that this is fifteen or seventeen times longer than the period required for the performance of a voluntary action, and nearly once longer than for the performance of an ordinary reflex action, but which, according to Professor Schiff, (c) is between eleven and thirteen times longer than for a voluntary one.

Against this view some objections have been raised. Dr. Brown-Séquard (d) has stated that the elements arising from the pia mater have certainly nothing to do with the process, because he has seen the same vaso-motor effects follow the burning, by the actual cautery, of the cortex after peeling off the pia mater as before. To this argument I have found this answer: that the mere peeling off of that membrane induces vaso-motor phenomena, so that the subsequent application of the cautery to the denuded cortex adds nothing to the results.

Those strands of fibres with which the bloodvessels have some office are in connexion with the spinal cord, the medulla, the pons, and the basal ganglia (apparently), if am to judge by the direction of their course.

One word more before ending, about consciousness. I am not prepared to deal with that huge subject metaphysically. I will only say a few words about it because I have undertaken to review the subject of the functions of the brain in the same manner as Dr. Ferrier has done it. If I do not accept his views, I wish it to be understood that the hypothesis which I will give is a mere hypothesis, and nothing more. I must say in the beginning, however, that I agree with Dr. Ferrier and with many others that "we may succeed in determining the exact nature of the molecular changes which occur in the brain-cells when a sensation is experienced; but this will not bring us

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(a) Dupuy, *C. R. Soc. de Biologie*, in *Gaz. Med. de Paris*, 1875, pages 376 and 600.
(b) Dupuy, Thèse Inaugurale, "De l'Examen," etc., 1873, page 23.
(c) Schiff, "Lezion di Fissologia," etc., second edition; Firenze, 1873. Appendix.
(d) Brown-Séquard, *loc cit*, *passim*.

one whit nearer the explanation of the ultimate nature of that which constitutes the sensation." Montaigne has written long ago—"Cette apparence de vérisimilitude, qui les fait prendre plutôt à gauche qu'à droite, augmentez-la; cette once de vérisimilitude qui incline la balance, multipliez-la de cent, de mille onces ; il en adviendra enfin que la balance prendra parti tout à fait, et arrêtera un choix et une vérité entière. Mais comment se laissent-ils plier à la vraisemblance, s'ils ne connaissent le vrai? Comment connaissent-ils la semblance de ce de quoi ils ne connaissent pas l'essence?" (a)

This being the case, I beg leave to state that in the beginning of this review I have said that instead of stating that "the brain is the organ of the mind, and that mental operations are possible only in and through the brain, is now so thoroughly well established and recognised, that we may without further question start from this as an ultimate fact;"(b) I think that there is good reason, on the contrary, for believing that the lower centres so called, the spinal centres, also are the organs of mind to some degree. Vulpian, commenting on reflex actions in a frog deprived of its brain, keeping in his mind the beautiful adaptations of purposiveness of its movements, says — "What would an animal still possessing its cerebrum do more?" Yet he has concluded that there was no will in the actions of that frog! How do we know? What is will? Professor Dugès, of Montpellier, as I have already said, found, after experimenting on the *Mantis religiosa*, that when the head and prothorax have been taken away, the posterior portion, retaining its four limbs, resisted all efforts made to overthrow it, and manifested anger by trepidations of wings. On the anterior portion of the animal he removed the head, so that the animal consisted of nothing more but the prothorax. That portion of the body lived for more than one hour, agitated its limbs, and sometimes caught the fingers of the experimenter, and inserted its fins in the flesh.

I have seen a salamader deprived of what represents its cerebrum, and kept in a cool and dark place during summer, and placed in a dish covered with a plate, for several days change its place without being excited to do so by any circumstances that I could appreciate.

Those facts, and the reasonings which can be deduced from them, authorise me, I believe, to state that Dr. Ferrier is not right when he says "that it must follow from (?his) experimental data that mental operations in the last analysis must be merely the subjective side of sensory and motor substrata."(c)

We must not accept either this other proposition of his, that the physiological activity of the brain is not, however, altogether co-extensive with its psychological fnnctions.(d) It is through the obligation he has been all through his work to beg the question that he has written the postulate which is followed by this, that the "brain as an organ of motion and sensation, or presentative consciousness, is a single organ composed of two halves; the brain as an organ of ideation, or representative consciousness, is a dual organ each hemisphere complete in itself." He has been bound to make that distinction, although there is no ground for it, because, as he says himself, "when one hemisphere is removed or destroyed by disease, motion and sensation are abolished unilaterally, but mental operation, are still capable of being carried on in their completeness through the agency of one hemi-

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(a) That appearance of verisimilitude which impels them to incline to the right rather than to the left—augment it; that ounce of verisimilitude which bends the scale—multiply it by a hundred, by a thousand more ounces: it will come to pass at last that the scale will incline altogether, and will thus fix upon a choice, and a whole truth. But how do they allow themselves to believe in a similitude of a truth if they know not the truth itself? How can they know the like of that of which they know not the essence?—Michel Montagne, "Essais," liv. ii, chap. xii. (b) Ferrier, *loc. cit.*, page 255.

(c) Ferrier, *loc. cit.*, page 256; also Hughlings-Jackson quoted by Ferrier.

(d) Ferrier, *loc. cit.*, page 257.

sphere." If the view is taken that diseases of the brain do not arise from the fact that a centre has been destroyed, but that the diseased portion of nerve-matter does start the symptom, in the same manner that washing the pleural sac in the operation of empyema, in some instances, brings on an attack of paralysis of the arm on the corresponding side, which passes away to recur again after every washing—as shown by Leudet and others—of course this view of Ferrier's is still more unacceptable. I will state first that it so happens than when gross lesions of the brain do give rise to psychic alterations, it is in the lower centres that the lesion exists, and not only in the cortex. Trousseau repeatedly asked himself why the patient suffering from hæmorrhage on the brain always cried, whilst when only aphasia, only lesion of the convolution existed, no crying was observed. Lesions of the upper part of the pons, chiefly of the anterior right part, are very prolific of emotional symptoms. The emotional state is a psychic one; and Dr. Brown-Séquard and Professor Charcot have noted that fact so often noticed by Trousseau. Dr. E. C. Seguin has two years ago written a good paper on that subject. Dr. Fleury (a) has shown that there was a difference as to frequency of emotional symptoms, according as the right and left hemisphere is diseased. It is necessary that Dr. Ferrier should prove that consciousness and motion are two such different things — that is to say, that one can exist without the other—as if the mere fact of motion does not prove consciousness of the motion process, which is equivalent to saying that a thing can be without being, before we can adopt the distinction he has drawn.

I believe that sensation being a factor of consciousness, it is not unreasonable to suppose that we can conceive consciousness to be that which is at work, or rather which is developed from anatomical elements, when the brainless frog executes all the movements so well observed by Pflüger; the *Mantis religiosa*, without head and without posterior segment, reduced to its prothorax alone, and the salamander without cerebral ganglion, execute purposive actions.

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Since those pages were written, February, 1877, some very interesting papers have been published, amongst which a memoir by Dr. Couty, in the "Archives de Physiologie" for the beginning of the year 1877. As those researches appear to me to carry additional evidence of the vraisemblance of the theories at which I have arrived, I take pleasure in pointing out Dr. Couty's memoir. Dr. Russell Reynolds has tried to explain that the facts concerning metallotherapy, as published from the records of the Salpetrière, relative to the disappearance of symptoms, in lesions of the internal capsule, by an effect of the imagination, I think it proper to call attention to the remarkable report of Dr. Dumont Pallier, published in the C. R. of the "Société de Biologie" for 1877, which shows very conclusively that in Professor Charcot's cases there was secondary sclerosis of bundles of fibres, the original disease having lasted more than ten years. So that again I believe those facts do support my theories, as it is impossible not to admit that in those cases reflex actions were at work.

The case of Dr. Bartholow quoted in my paper, in the original publication of the *Times and Gazette* contained a material error which I have corrected—as it stands it constitutes an excellent proof of what I have asserted: that it proves not what it is said to prove.

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(a) De Fleury, "Dynamisme comparé des Hémisphores Cérébraux chez l'Homme." Paris, 1873.