PHYSIOLOGY OF THE CEREBRAL MOTOR CORTEX: THE CONTRIBUTION OF CLINICAL STUDY*

BY

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ABRIDGED

[After a tribute to Sir Victor Horsley.]

The methods and the fruits of the experimental study of the motor cortex are known to us all, and the familiar diagrams in which the topographical localization of movements in this cortical region have been embodied by numerous observers look out on us from the pages of innumerable papers and textbooks. On the other hand, this is by no means true of the remarkable contributions of the clinical method to the physiology of the motor cortex. Even contemporary clinicians commonly show an imperfect appreciation of their nature and significance, as may be seen from the unquestioning acceptance they give to the punctate theory of the localization of movements in the cortex, despite the numerous ways in which this theory conflicts with the clinical study of disorders of movement: while for the contemporary physiologist all this clinical work is an imperfectly explored world of phenomena and thought.

The last third of the nineteenth century was a period of great intellectual excitement for those engaged in the study, clinical and experimental, of the nervous system. As we read the original papers of the time upon this subject, naive as is their phraseology in some respects, we can recapture some of that excitement, and can realize how the experimental confirmation of Jackson's hypothesis of a localization of somatic function in the cerebral cortex, by Hitzig in Germany and by Ferrier in this country, stirred the imagination. Thus it was that the philosophic genius of Jackson, and the pioneer work of Ferrier which was stimulated by it, together opened what I have always looked upon as the golden age of British neurology. At this fortunate time Horsley began his work, and came to take his due place in the direct line of those, clinicians and experimentalists, through whom have flowed the two main streams of our knowledge of the cerebral motor cortex.

Experimental Contribution and Hypothesis

What has predominantly occupied the experimentalist during the seventy-five years of investigation of the motor cortex has been the progressively detailed charting and subdivision of the excitable region, and innumerable maps, from the simple ones of the pioneers to the beautifully detailed ones of Leyton and Sherrington, bear witness to the perseverance and accuracy with which these surveys have been carried out.

If the physiologist, absorbed in these studies, has concerned himself at all with the problem of the functional organization of the excitable motor cortex it is because his punctate electrical stimuli soon began to reveal to him a perplexing instability in the reactions of the cortex. The factors underlying this were later to be analysed by Graham Brown and Sherrington, but this analysis cannot be said to have had any deep influence upon the general conclusions formed by the following generation of experimental physiologists as to the plan underlying the representation of movements in the cortex. This plan is generally held to be that the motor cortex is a close-set mosaic of points in each of which is represented, or localized, a physiological unit of movement: "One spot, one movement" for the majority of physiologists, the whole making up a somatotopic projection of moving parts on the surface of the cortex.

There is an engaging simplicity about this conception that is in the true tradition of Newtonian physics, but it has progressively crumbled, almost unnoticed, until upon examination it is found to have no solid foundation. Horsley was one of the first to cast doubts upon its validity when he pointed out that there was no ground for the current assumption that the fragment of movement evoked by a momentary threshold electrical stimulus of a cortical point was all that the point really represented, and that in any case the stimulus was a grossly unphysiological way of exciting a nervous centre.

Graham Brown and Sherrington's analysis of the so-called cortical instability or liability showed that the activity of a cortical point is in fact influenced in a number of definite ways by its past behaviour, and its future behaviour by its present. To the conception of primary facilitation they added that of secondary facilitation with its consequences of the deviation and reversal of the response of a cortical point. These phenomena became the basis of Leyton and Sherrington's exposition of motor cortical function. In them they saw indications of the "rich mutual associations" of cortical points, associations by means of which the units of movement assumed to be represented in individual points were built up into the innumerable combinations and sequences that go to make the normal motions of the intact organism.

[The lecturer summed up the present position by saying that although the cortex is a labile organ, there has been added to it the notion of a perpetual flux of representation, so that the idea of fixed representation is gone, and to call the cortex labile or unstable gives us nothing in its place. Yet the punctate theory still appears to command the formal assent of physiologists, a telling example of the survival of what was once a useful working hypothesis until it had become an obstacle to the penetration of fresh and original thought to the problem of the motor cortex.]

It is a fair criticism of the experimental study of the motor cortex that it has stopped short of giving us a clear insight into the functional organization of that cortex, and that in some of its aspects current physiological opinion shows signs of regressing to a simplicity wholly inadequate to generalize the facts of observation. I venture to submit that this failure is not so much due to a lack of relevant facts as to one of conceptual thinking about them. We may discern at least three factors underlying this. First, successive generations have continued to interpret their findings on the basis of the original punctate localization theory, the permanent validity of which they have assumed. Secondly, they have departed from the conception of a representation of processes—that is, movements—in the cortex in favour of one of a representation of structures—that is, muscles; and thirdly, in coming to their conclusions they have left out of account, by a sort of unawareness, all those facts of observation that did not harmonize with the theory; thus giving an example of the insufficiently recognized truth that the relevance of evidence is always dictated by theory, a quality inherent in the observational order of experience.

[After this brief and somewhat critical review of a vast body of experimental work with its great merits and obvious limitations, the lecturer turned to his main theme, the clinical contribution, which this introduction brought into relief.]

The Clinical Contribution and Hypothesis

That these studies have almost from the first taken a very different path from the one we have just traced we owe to two circumstances: the one inherent in the clinical method, the other fortuitous.

The limitation inherent in the clinical method is that Nature makes the experiment, and not the observer. She makes it how and when she likes, seldom as we should desire to have it, and still more seldom under the eyes of someone interested to assess its significance in terms of physiology. "Experiment," it has been said, "is nothing else than a method of cooking the facts for the sake of exemplifying the law"; but the clinical observer has to take Nature's facts in the raw, and to elicit Nature's laws in these circumstances may be a high adventure of thought.

The second and fortuitous, though most happy, determinant was that the pioneer in the field, Hughlings Jackson, chanced to be at once a scrupulously accurate observer and a man of philosophic genius. He was quick to recognize that once the fact of a topographical representation of movements in a cortical region was established, the further and more precise mapping of that region could be left to that experimental method which Ferrier had begun to show was so eminently suited to the task. He was thus free to turn to what was

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for him the more congenial task of analysing the nature of this representation. To this end he adopted what he called his "double plan": namely, the parallel study of the consequences of destroying and of discharging lesions, that is, of paralyses and of those convulsions beginning unilaterally that we now speak of as Jacksonian fits. In one of his papers Jackson says:

"I considered convulsive seizures and certain cases of paralysis with regard only to the localization of movements in the brain. What interested me most was, not so much the localization of movements in the cerebral hemisphere in the sense that, for example, the movements of the foot are localized here and those of the arm in another place, but the facts of the cases as they bore on a broad principle of localization."

Here, surely, is the authentic method of science, philosophically considered, with its observational and conceptual elements, and here, thus early, the clinical study of the motor cortex set its course for a different goal from that sought by the pioneer experimentalists. For myself, I know of no more fascinating episode in the history of clinical medicine in modern times than Jackson's intensive study of the convulsion beginning unilaterally, as he called it, and the masterly fashion in which he made it yield up so many of the secrets of the cortical organization of movements.

[The lecturer described as the classical example of the "discharging lesion" the Jacksonian fit, and of the "destroying lesion" hemiplegia in all its grades of severity.]

We have learned that, *ex hypothesi*, the motor cortex is a mosaic of discrete points in each of which a muscle or a unit of movement is represented, and, from what we have heard, all these points are to be thought of as equipollent. How remarkable it is, then, that the Jacksonian fit, with this vast and complex representational mosaic at its disposal as it were, should invariably choose to fire off in one or other of three cortical foci: that for thumb and index movements; that for movements at the angle of the mouth; or that for movements of the hallux, and in that order of frequency. An inevitable result of this selective incidence of the discharging cortical focus, for which nothing in the literature of experimental physiological studies has prepared us, is that the Jacksonian fit begins in what has been called a leading part (a part having the greatest number of different movements at the greatest number of different times)—namely, hand, face, or foot. From these observations Jackson concluded that these three cortical foci—"physiological fulminates," as he called them—possessed the lowest threshold of excitability in the motor cortex. Again, in respect of each point of onset of the fit there are certain uniform sequences of spread. Of equal significance as a general characteristic of the fit is that it spreads in a uniform sequence. In respect of the discharging lesion itself, Jackson believed that though this might be restricted, it could yet lead to a widespread convulsion.

Paralyses of Movements

Tracing the general features of hemiplegia, the effect of the destroying lesion, we find that they include the maximal incidence of impairment or loss of movement in the same three leading parts we have already considered—hand, face, and foot, in that order. As hemiplegia increases in severity, or in the comparative study of a series of progressively more severe examples, we see the same compound order of involvement that we found to be characteristic of the Jacksonian fit: as paralysis spreads to include more movements it deepens in the movements first affected.

Now hemiplegia is usually the sequence to a destroying lesion of the motor pathway deep to the cortex, the way out from the cortex, but the study of limited destroying lesions of the cortex itself reveals that the general characters of the paralyses observed are the same in each case. Nevertheless there are certain significant features of destroying cortical lesions to be considered. As Jackson expressed it, the cortex is very tolerant of destroying lesions. If such a lesion be very restricted it may be symptomless, and when productive of paralyses these are apt to be minimal in range and severity and to be capable of a high degree of restoration. Nevertheless, this apparent restoration is not truly complete, for of all the possible movements of a given part some remain absent, but are largely compensated for by other and similar movements of the same part. This so-called "tolerance" of destruction, the high degree of recovery after a destroying lesion, and the nature of the compensation achieved are all incompatible with the theory of an exclusive representation of movements in certain fixed points.

Summing up, then, we have primary involvement of leading parts in paralysis, compound order of paralysis of movements in the same sequence as that of convulsion in the Jacksonian fit: tolerance of the cortex to localized lesions, extensive recovery and partial compensation of lost movements by similar movements of the same part. Thus convulsion and hemiplegia are the positive and negative of one picture, and convulsion "the mobile counterpart of hemiplegia." The destroying lesion reveals yet another feature inherent in the organization of the motor cortex—namely, that it is movements and not muscles as such that are represented. "The cortex knows nothing of muscles, it knows only of movements," as the familiar aphorism of Jackson's has it, and it must at this date strike the clinician as remarkable that any should be found to dispute it.

In the matter of paralysis there is no evidence that a single muscle can be paralysed by a cortical lesion in man. It is doubtful whether in the experimental animal after a cortical ablation any conclusive finding on this matter is possible. On the positive side there is ample clinical evidence that paralyses from cortical lesions and in hemiplegia are paralyses of movements and not paralyses of muscles as such. One of the most familiar examples is provided by the behaviour of wrist extensors in many cases of residual hemiplegia: those in which the power of voluntary clenching of the fist is retained but that of voluntary extension of the wrist is lost. In the former movement the wrist extensors can be seen and felt to contract powerfully, while on the attempt to extend the wrist nothing happens and the muscles remain inactive. As synergists in the first movement they act, as prime movers in the second they are paralysed. Even more striking instances are recorded by Beevor (one of Horsley's collaborators) in the case of movements of the trunk in hemiplegia, showing that muscles paralysed for certain movements retain their full power in others.

[After adducing a philosophical argument against representation of structure—as opposed to process—in the cortex, showing that it can only be the *performance* of the muscle as a moving structure that can be represented:—]

Clinical observation has shown that the motor cortex predetermines the combinations in which muscles shall act simultaneously, and also the sequence in time, the order of action, in which they enter into a voluntary movement. These are those cortical functions of co-ordination in space and co-ordination in time of which Jackson spoke and which Beevor so abundantly exemplified and recorded in his Croonian Lectures (1903).

Jackson's Theory of Representation

At an early stage Jackson found himself compelled to discard the punctate or "cortical muscle" theory of localization of movements as being incompatible with the facts of observation and inadequate to generalize them. What has been spoken of as the tolerance of the cortex to destroying lesions, and the remarkable degree and kind of recovery of movement seen after such lesions, could not occur with a cortex so organized. Similarly, the compound spread of convulsion from discharge of a restricted cortical focus is equally impossible on this basis. Jackson therefore concluded that representation of movements must be multiple in the sense that a given cortical focus must contain the anatomical and physiological substrata of more than a single movement, and that the representations of the movements of a given moving part are not abruptly demarcated and merely contiguous but extensively overlapping.

Implicit in this view, and in strong contrast to that of the experimental physiologists, is the notion that what the motor cortex represents, what is "localized" there, is not a vast collection of elementary units of movement, waiting to be assembled *ad hoc* into those complex combinations and sequences that we know normal voluntary movements to be, but a field in which all possible movements—all the movements the individual has ever learned—have their own separate representations in so far as they differ from other movements. There is, in fact, no conclusive evidence that any hypothetical "units" of movement are represented as such in the cortex.

The hypothesis of multiple and overlapping representations allows of an integration of all the phenomena of facilitation with the known facts, and we no longer have to suppose that what deviation and reversal of response really signify is that a given representation actually changes its location from place to place and moment to moment, and that cortical representation is in a perpetual flux.

[Only on the basis of Jackson's theory of representation was it possible to account for the clinically observed facts. But in the field of experimental physiology the facts pointed to the same conclusions.]

The notion of multiplicity of localizations of movements in a single cortical spot stands out clearly enough from a great number of experimental observations. Thus it has long been known that if the faradic stimulus to a cortical point be prolonged a sequence of primary, secondary, and tertiary movements, as they have been called, can be evoked, and these involve a spreading field of musculature.

[The lecturer quoted from Horsley's Linacre Lecture in which he had drawn attention explicitly to the fact that multiplicity of representations at a single focus had always been an inference that could be drawn from experimental observations; also the recent work of two American physiologists, Murphy and Gellhorn (1945), who, using supra-threshold condenser discharges as their cortical stimuli, had found multiplicity of representation throughout the motor cortex of three animal types—rabbit, cat, and monkey—and also extensive overlap of representation, transcending the boundaries of the three great somatotopic regions, those of the head, upper limb, and lower limb. This multiplicity of representation persists in an isolated cortical focus and is not due to spread of current over adjacent areas of cortex.]

This consummation must be profoundly moving to whomsoever has been bred in the tradition of the great neurologists of the turn of the century. At last a great adventure of thought has come safely to port, and in the words of a phrase from Pope's *Essay on Man*, the motor cortex has been shown to be "A mighty maze! but not without a plan." Just as seventy-five years ago the clinically ascertained fact of a localization of somatic function in the cerebral cortex obtained its brilliant confirmation at the hands of Ferrier, so now the hypothesis as to the nature of that localization, based upon clinical observation and formulated so long ago, finds an experimental verification in the laboratory at the hands of two American physiologists. Now this clinical hypothesis may be regarded as valid on its own merits, and not in need of any further verification.

Conclusion

The material and the conditions out of which the clinical investigator has to forge ordered knowledge are a constant challenge to his capacity for conceptual thinking as well as to his powers of observation. This chapter I have put before you is the story of the response to challenge. This particular challenge, which faces the experimental no less than the clinical investigator, is the challenge to conceptual thinking, a challenge we see so brilliantly met in the work we have been considering, or in that of Sherrington in the realms of experimental physiology. Science, it has been said, is formed by the meeting of the two orders of experience, observational and conceptual, and the former can be interpreted only in terms of the latter.

How appropriate to our thought of Victor Horsley is the conception of challenge and response, for his full and eager life was a series of responses to challenge. His name and fame are part of the imperishable tradition of medicine.

Medical practitioners might like to draw the attention of paraplegic patients to a new quarterly journal entitled *The Cord*. The journal may be ordered from the Editor, Paraplegic Branch of the British Legion, Stoke Mandeville Hospital, Aylesbury, Bucks, a year's subscription costing only 2s. Though the journal is issued by the Paraplegic Branch of the British Legion it will cater also for the interests of those paraplegics who are not members of the Legion. It will provide information about appliances and medical treatment suitable for paraplegics, and how they may best be obtained. General problems of housing and employment will also be discussed. The present number contains an entertaining short story, and announces the offer of a two-guinea prize each quarter for the best short story submitted by a paraplegic reader.

A SUGGESTED HOSPITAL UNIT

BY

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For some time it has been evident to me that the private or pay block in a hospital is obsolete. In such a block the segregation is based on social and not on medical grounds and the patients lose the advantages that the less affluent obtain from specialized nursing and equipment. While by no means an advocate of specialization to the extent that many consider advisable, whereby a "special centre" is necessary before any malady can be treated, yet I think there can be no doubt that a reasonable degree of specialization is an advantage. I am convinced that this applies as much to the nursing as it does to the medical profession, or even more so. I have on many occasions heard surgeons remark that their cases do better in both the public and private wards of special hospitals restricted to the type of work concerned than do similar cases in nursing homes or the private blocks of general hospitals.

Sisters and nurses tend to be interested in special aspects of their work. Thus in a general pay block a fractured femur may be nursed by a sister specially interested in diabetes, and the requisite special equipment is not available, or, if it is, the staff are not familiar with it. It is for these reasons that private beds have been provided in special hospitals. Not only does this apply particularly to fractures and orthopaedic cases, but it applies to genito-urinary, chest, and abdominal surgery, neurosurgery, and gynaecology.

In my opinion, the ideal conditions are those in which a surgeon works as part of a self-contained team of agreeable and co-operative colleagues, medical and nursing, in a self-contained unit having all the facilities required for the efficient execution of his work, both public and private. The unit should therefore have general and private wards; it should, in the case of a surgical unit, have operating theatres, and an *x*-ray room for pyelography, salpingography, encephalography, or the correction of fractures, depending on the type of work engaged in. It is hard to think of any major surgical division that does not, for its finest technique, require the help of *x* rays. There should be facilities for out-patients, both public and private, and a small clinical laboratory. Under such conditions a good technique could be developed: no doubt each "firm" would plan its own special technique, and this might be the subject of friendly rivalry. In this manner the method could be judged by results. In a pay block it is difficult to get a special technique followed in detail, and the procedures are apt to be a blend of the ideas of several surgeons—so often the best features of each method fail to be incorporated in the blend, and one often feels that the *exact* technique of any competent surgeon (if one cannot get one's own) would be better than a hotchpotch of many.

I have been thinking of these things for many years, but I have been stimulated to put my thoughts on paper by the inspiring article by J. C. Spence on the subject of children's hospitals (*Journal*, Jan. 25, p. 125). I submit, as an illustration of what I mean, a plan kindly drawn up for me by Mr. Owen Watson, A.R.I.B.A., and his staff. It does not profess to be perfect in detail, and as I have already put these experts to a great deal of trouble by alterations and revisions I have refrained from further alterations as the plan illustrates my theme. There is, however, one detail that strikes me as needing changing—the plaster room should adjoin and communicate with the *x*-ray room, and it might be an advantage for the laboratory to communicate with the theatre suite. The private wards would be better grouped in one group and should not face the back of another building. Obviously, if such a plan were to be drawn up in connexion with a proposed hospital the nature of the treatment to be carried out there and the available site, as well as the personal views of those who were to work in the unit, would make considerable alteration necessary. I therefore feel that at this stage there is no need to ask for the plan to be redrawn to provide for comparatively trivial revisions.