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ON THE FUNCTIONAL ATTRIBUTES OF THE CEREBRAL CORTEX

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§ 1—INTRODUCTION.

THE following pages¹ embody an attempt in some degree to estimate the functional significance of the experimental data acquired during the last two decades on the cerebral cortex.

The immediate occasion of the attempt was a study of the "sense of effort" which has occupied my thoughts at intervals during the last few years, and has inevitably connected itself with reflections upon the psychophysiological

¹ This paper is in the same grooves of thought as a paper published in *BRAIN* last year on the Sense of Effort.

It contains in fact most of the considerations that form an area of indistinct consciousness surrounding the area of more distinct consciousness relating to the Sense of Muscular Effort.

The two affirmative propositions—"This is a fact," "I think this is a fact"—bear widely different significations in common language, but are psychologically of identical meaning; they give expression to an identical psychological conclusion, and in scientific analysis as distinguished from special pleading the first assertion, however clearly and dogmatically it be propounded, can never carry more weight than the second; conviction in the mind of an onlooker or listener should depend, not upon the sense of certainty felt to be possessed by the utterer of a conclusion, but upon the verifiable premises of that conclusion exposed to critical examination.

I have attempted to make some distinction between the more psychological and the more physiological aspects of the question; a satisfactory distinction has been impossible, physiological descriptions necessarily

interpretations that have been given of cortical activity, more especially as presented in the writings of Hitzig, Ferrier, Munk and Bastian. It has no less inevitably connected itself with purely psychological considerations, as presented more especially in the writings of Bain, Wundt and James.

These are indeed by no means a complete list of the considerations and data underlying the particular topic of study designated by the title "sense of effort"; the significance of the phenomena of attention, and more especially their relation to "motor" and "sensory" events continually presented itself; the phenomenal content, common to the psychological terms—"will," "memory," "effort," "attention," had to be reckoned with; the everlasting and never solved problems coming to our minds under the various verbal labels "free will," "determinism," "predestination," "fatalism," "moral responsibility," "automatism," &c., occasionally obtruded themselves.

I allude to these ultra-scientific matters, only to say that, in my opinion, the superficial logical connection between the study of neural phenomena, and an "*explanation*" of the final causes of neural processes, by animistic references to "force," "matter," "will," "faculties," "mind," in short by any kind of disguised anthropomorphic materialism, is in relation to positive knowledge neither scientific nor ultra-scientific but anti-scientific. Beyond every "*explanation*" we are faced by the insoluble mystery, confessed but not

include psychological inference, psychological descriptions are in part based upon physiological phenomena, seen, reported or imagined. The attempt has, however, led me to separately present in § 4 a least positive group of considerations which, although I have not been able to omit them altogether, can be omitted in the reading without prejudice to the main argument. In common with many thinkers, I regard logical forms of verbalisation as an outward and visible sign of the inward processes of ratiocination which are functional attributes of the cerebral cortex; this belief has to a considerable extent governed my mode of thought, and I could not suppress § 4 without committing a reticence of exposition. I am as sensible as the most exacting critic can be of the unsubstantial and uncertain character of any inferences from external formulations of thought, to the hypothetical neural processes of which sensation, perception and ratiocination are the subjective concomitants.

explained in the words:—"It is the spirit that quickeneth," recognised but not comprehended in Aristotle's conception of the immaterial Psyche—nutritive, sentient and intelligent.

Therefore, in spite of its superficial logical consequence, I shall premise, as a fundamental postulate, that voluntary movement, objectively studied, is reflex movement. It is no addition to description, and, although at times convenient or even necessary, in many instances it serves to arrest description, to refer voluntary movement to Will. What is Will? What is Force? What is Matter? We do not know. Our business as students of nature is with the relations between phenomena—subjective phenomena in our own consciousness mentally projected outside our own consciousness, and as a general working hypothesis, believed to be the effects of corresponding objective phenomena around us. What is consciousness? We do not know.¹

But assuming the real existence of ourselves and of our surroundings, we may, having recognised that we do not actually *comprehend* the verbo-mental images—Force, Matter, Motion, investigate and describe the phenomenal relations which, for convenience, we attribute to these elementary notions. Description comes to its dead end when we have traced back the phenomenal sequence to that point where we find it necessary to evoke an imaginary substantive force. A neural process for instance ceases to be described when it is traced back to the force of "will," or to the property of "excitability." The fault of which we are most often guilty, even when we have fully recognised the distinction between comprehensible relation and incomprehensible essence, is to *prematurely* invoke a final cause. The fault of which we are sometimes guilty is, after having invoked a final cause, *a quo* or *ad quem*, whether prematurely or not, to proceed through the dead end as if the phenomenal sequence were unbroken. And still grosser faults are possible—faults of observation, faults of interpretation, faults of language, and faults of candour.

¹ What consciousness is we do not know. All that we do know is consciousness. Obviously the paradox depends upon thimble-rigging with the word "know."

The particular theme now submitted to analysis is the functional significance of the cerebral cortex; the question to be answered may be put thus: Have we reached any true dead end to knowledge in the conclusion that the cerebral cortex contains "sensory centres" and "motor centres," and if so, what signification do we attach to these final terms?

It will, I think, be of service towards the answer to these questions to begin by quoting some of the most pregnant passages from Hitzig and Munk. Owing to the wealth of the German language as regards psychological terminology, in comparison with English or French, the German interpretation has been by far the most searching, and by reason of this very wealth as well as by reason of the subtle harmonics and connotations in the individual words, that interpretation has remained practically ignored by us. Even Ferrier has not in his "Functions of the Brain," given at all an adequate account of Hitzig's view, still less of Munk's; Bastian ignores them both; and James, while giving abundant evidence of our great indebtedness to Munk, strangely enough goes the length of saying "that Munk's absolute tone about his observations and his theoretic arrogance have led to his ruin as an authority."

The most weighty critic of Munk is Goltz, less on account of his polemical skill than because of the weight of his experiments. We cannot but hesitate from an unreserved admission of Munk's data in so far as they have been derived from dogs, when Goltz brings before us dogs, first with one hemisphere removed, then with both hemispheres removed, apathetic and stupid it is true, yet able to feel, to move, to snarl and bark, to stand on their hind legs, to see and even to feed, and liable to have epileptic fits.¹

¹ GOLTZ. Siebente Abhandlung über die Verrichtungen des Grosshirns. *Pflüger's Archiv.*, li. 1892, p. 570.

Wundt has recently published a critical review of Munk's doctrine of cortical projection in the 6th volume of his "Philosophische Studien," 1890. His criticism is particularly directed to Munk's retino-cortical scheme as being inclusive of two non-congruent conceptions of centralisation, relating (1) to distinctness of vision (sensation) (2) to complication of vision (perception). He characterises the view that high-level vision, hearing &c., are effected at distinct foci of visual, auditory, &c., areas, as a revival of the phrenological faculty theory, and as leading into a psychologically untenable position.

But the question of interpretation may to a large extent be kept distinct from that of localisation; certain principles if true, are true whether cortical functions be localised or concentrated or diffused.

I have already on a previous occasion¹ pointed out the identity of idea between Munk's *Seelen-blindheit* and *and-taubheit*, *Rinden-blindheit* and *and-taubheit*, and Hughlings Jackson's well-known hierarchic scheme. And in translating Munk's language I shall take the liberty of rendering the German prefixes *Seelen*—and *Rinden*—by the English prefixes *higher-level* and *lower-level*, a rendering which appears to me to be less question-begging than a literal translation of these two very heterologous words, and justified by what Munk himself says of the sense in which he uses the terms.²

Hitzig³ says:

"A portion of the convexity of the dog's cerebrum is motor (using this expression in Schiff's sense), another portion is not motor."

"The motor portion, speaking generally, is situated anteriorly, the non-motor portion posteriorly."

"By electrical excitation of the motor portion one obtains combined muscular contractions of the opposite half of the body."

"With very weak currents the muscular contractions can be localised in definite and narrowly restricted groups of muscles. With stronger currents excitation of the same or of closely adjacent spots, causes other muscles, and even muscles of the same side of the body to participate in the contraction. The possibility of isolated excitation of a definite muscular group,

¹ "An Introduction to Human Physiology," 1891, p. 533.

² "Ich habe diese Bezeichnung im Jahre 1877 nach vieler Ueberlegung gewählt und ihr vor 'Vorstellungsblindheit' oder 'Erinnerungsblindheit,' die am nächsten lagen, aus guten Gründen den Vorzug geben zu müssen geglaubt. Indem ich wiederholt definierte: *Seelenblindheit*=Fehlen der Gesichtsvorstellungen, der Erinnerungsbilder der Gesichtswahrnehmungen, dürfte ich die Benützung von 'Seele' für gerade so unbedenklich halten, wie wann ich α Blindheit oder β Blindheit gesagt hätte. Dass trotzdem Missverständnisse von seiten flüchtiger Leser nicht ausgeblieben sind, kann mich um so weniger veranlassen die Bezeichnung aufzugeben, als ich eine bessere noch bis heute nicht gefunden habe." *Munk*, *Gesammelte Mittheilungen*, 2nd edition, note to p. 41.

³ Hitzig. *Untersuchungen über das Gehirn*. Berlin, 1874, p. 11.

Fritsch and Hitzig. *du Bois-Reymond's Archiv.*, 1870, p. 310.

using very weak currents, is, however, limited to very small spots, which for the sake of brevity, we may designate as centres."

"It is an assured fact that a considerable portion of the nerve-mass forming the cerebral hemisphere (say nearly one-half) is in immediate relation with muscular movement, and that another portion, at least directly, has no evident connection with it."¹

"If we raise the question whether our excitation effects were caused by a direct action upon particular centres in which the voluntary motor impulse arises, or whether they are attributable to an excitation of subjacent fibres, or whether any third possibility exists, our answer must be far more reserved."

"Even if we assume as proved, that the movements in question are liberated by the ganglionic substance—and it is not proved—this would not be enough to prove that with these movements which are liberated by the internal event, that precise portion of the cortex furnishes the substratum of the first outward step in the series beginning with the origination of a sense-impression, and finding a temporary end in the expression of will evidenced as muscular movement."

"It is, on the other hand, far from inconceivable—and the notion is favoured by our knowledge of the anastomotic structure of these parts—that that portion of the brain which includes the birthplace of a will to move, is of another, or perhaps of a more complex nature, and that the parts which we have called centres only constitute agencies, exchanges, in which an arrangement of muscular movements occurs, similar to that effected through the grey matter of the spinal cord and basal ganglia, but more purposeful."

And later (page 30) describing the state of the two dogs upon which circumscribed portions of the brain (the "centres" for the anterior extremity) had been extirpated.

"Both animals, after extirpation of a portion of the (by us) so-called centre for the anterior extremity, had only partially lost the power of moving that limb, and had apparently suffered no loss of sensibility. But they evidently had only an imperfect knowledge of the state of this limb; they had lost the power of forming to themselves complete representations of its states; they were thus the subjects of a symptom, which appears

¹ Untersuchungen, p. 25-26.

in a very similar fashion in certain manifestations of tabes, only in this case, there was certainly no lesion of a sensory tract. One might express himself thus to characterise this condition more closely. Some kind of motor communication still persisted from mind to muscle, whereas, somewhere in the connection from muscle to mind there was a break. Possibly this break affected the terminus of the hypothetical path of the muscular sense, in any case it coincided with the position of the injured centre."

"Speculations concerning cerebral and mental activities have deservedly fallen into such discredit, that one may wisely observe an excess of caution in dealing with such matters."¹

"... We have, hitherto, avoided committing ourselves to a definite opinion whether we excited cells or fibres, termini or stations in the psychomotor circuit, and we shall still refrain from doing so. On the other hand, we have proved that the stimulus did act through known reflex channels. But if anyone should conclude, on account of these differences [polar differences, length of contraction] that the parts originally excited were not central expansions of the motor nerves, he clearly would have no grounds for the conclusion. For there is nothing to prove that central fibres (or cells) in connection with peripheral fibres (or cells) must react precisely like these peripheral elements."

"On the contrary, it is probable from all that we know that they react differently. And finally, the effects we have described have no analogy whatever among peripheral phenomena. One must, indeed, rest content with the assumption that the peculiarities depend upon the peculiar properties of the central organ."

"... And yet I regret to have used the expression 'muscular sense,' for it has caused all kinds of misunderstanding from the very first. Nothnagel has, meanwhile, characterised the symptoms in question as disturbances of 'muscular sense,' far more definitely than I have. This determines me to specify my own conception more precisely, although I am most unwilling to depart from the reserve that I have imposed upon myself. For dealing with these factors as they become better and better known to us, an unknown magnitude and one which can hardly be ignored forces itself upon us—the Will. We stand towards this factor, as we do towards the elementary forces; we may know the modes of its manifestation, but not its essence, nor its actual nature."

¹ Untersuchungen, p. 52-53.

"We characterised the state of the animals we had operated upon as follows: They evidently had only an imperfect knowledge of the state of the affected limb, they had lost the power of forming complete representations of it; and we were justified in this conclusion by the analysis of the perturbations of movements that existed after destruction of that spot of the cortex, the excitation of which had caused movements of the now abnormally acting muscles. The question now before us may be thus simply formulated."

"Is the excluded centre the organ which alone governs the movements in question, or is there in addition to the general organ of will—assuming such to exist—another central motor organ in the same circuit of discharge?"

"To be as brief as possible we may refer to common experience of moderate movements by normal muscle. It is known that the image of movement caused in our own consciousness by muscular action is exceedingly clear. An expert painter would for instance be able to faithfully reproduce a posture assumed by himself with closed eyes. Nevertheless consciousness knows nothing of the moving factors; even the representations obtained by direct observation or otherwise, are so intimately connected with another department, that they contribute but little to the perception of motor forces regularly operating at the periphery. These are indeed scarcely known to us as regards each particular form of movement, they are only discoverable by inference; and digital palpation, *e.g.*, is far more helpful to this end than the repetition of the movement."

"Still it is clear that very exact representations of the state of muscle must be produced—as we learn from these very images of movement—and it is equally clear that these images are attributable chiefly to the perception of the muscular state, and only in minor degree to joints, skin, &c.: this we learn from the well known illusions of movements occurring in the paralysis of ocular muscles."

"If nevertheless our representations of the muscular state of our own body do not overstep the threshold of distinct consciousness, and thus enable us to see into the true nature of the process, we must attribute this failure to a very general law. We are able to distinguish the state of particular organs only in so far as is necessary and sufficient for their use in the uninterrupted maintenance of their function."

"But within such limits, the apprehension of these mainly unconscious representations of each particular phase of move-

ment, constitutes one of the necessary conditions of a normal progress of its succeeding phase; subsequently, (considering apparent muscular repose as a phase of movement) one must recognise that muscular states in general are among the various causes that guide the organism in its voluntary movements, and that regulate these movements. Let us assume an entire absence of all other sensory stimuli and perceptions, so that we have to do with a simple motor machine of such a kind set in motion by the muscular impulse, we may then very well imagine it as sufficient for the execution of purposive movements."

"We have recognised in the above described portions of cortex an organ the function of which coincides with that aspect of the psychical phenomenon that we have been considering, and I do not see the necessity for admitting that Will as such involves a specific and different motor organ. When in consequence of the joint action of several sensory impressions, either present or past, the motive of a movement arises, such motive never takes form thus: "Innervate muscle a b c, so that arm n, shall move through angle x," but thus: "take," "write," "speak," &c. The organs we are acquainted with, appear to me sufficient to enable us to form a conception of the normal progress of the movement when once it has been thus initiated. Points of detail are it is true still sufficiently obscure." (*Untersuchungen*, p. 59-61).

Munk¹ in his third communication to the Berlin Physiological Society says:—

"I have now to direct your attention to a third department of the cerebral cortex of the dog—the portion CDE, Fig. 1. You recognise at once that it is the portion of the cortex upon which so much consideration has been bestowed during the last eight years, since the first investigations of Fritsch and Hitzig. If, nevertheless, a clear understanding has not been arrived at as regards the functions of this cortical department, this must be attributed to the fact that we do not yet possess a guiding point of view for a correct appreciation of the experimental data. The further I advanced in the knowledge of the auditory, and more particularly of the visual region, the more orderly became data that appeared at first irreconcilable; and if last year I was obliged to content myself with presenting to you our cortical department as the motor area in contrast with the posterior sensory area, now I believe myself able to invite you to a deeper comprehension of this cortical region as well."

¹ MUNK. *Du Bois-Reymond's Archiv*, 1878, p. 162.

"In a word our cortical region CDE is the sensory area (Fühlsphäre) of the dog. Just as the cortex of the occipital lobes stands in relation to vision, and that of the temporal lobes to hearing, so the cortex of the parietal area stands in relation to common sensation (Gefühlsinn); in this as in the other cases we have the locus where perception is consummated and where representations, the memory images of perceptions, have their seat. Let it be clearly understood, however; that it is not sensation of the skin only that is here in question, but sensation in its broadest sense, the sensation of the whole body." (p. 171.)

" . . . After extirpation on a dog of a portion of cortex in the area CDE of one hemisphere, the sole and regular consequence (when inflammatory reaction has passed off) is disturbance of motility and sensibility on the opposite half of the body, in one part or in another according to the situation of the extirpated spot. With the lapse of time these alterations gradually vanish, sometimes less, sometimes more completely, so that finally in the most favourable cases our dog recovers the demeanour of a normal dog."

" . . . Still these disturbances are very imperfectly characterised by the terms, disturbance of motility, disturbance of sensibility." (p. 173).

Then follows a careful description of the state of matters in a representative dog having suffered extirpation of D on left side, three to eight days after operation.

"The cortex of the parietal lobes of the dog is the sensory area of the opposite half of the body, and is composed of a number of regions each of which is related to a particular part of this half. In the percipient central elements of each region terminate contiguous fibres that subserve cutaneous, muscular and neural sensations (Innervationsgefühle) of each corresponding part of the body. Within each such region the sense-representations (Gefühlsvorstellungen) of each such part of the body have their seat, so that the former constitute the special sensory areas of the latter, *e.g.*, of the anterior or of the posterior extremities. Within the province of each such sensory area of particular parts of the body, small extirpations entail partial loss of sensory representation of the part, larger extirpations its complete loss—high-level paralysis (loss of high-level motility and of high-level sensibility) of the part; but in any remaining parts of a sensory area there can be a restoration of sense-representation.

By still more extensive extirpation, low-level sensations may be impaired, and only a portion of high-level sensation may be recoverable. The impairment and the failure of restitution are the greater, the smaller the remaining sensory area. Its complete destruction on one side has as its necessary consequence permanent loss of all sensation—low-level as well as high-level—total paralysis (total loss of movement and of sensation) of the related part of the body." (p. 176.)

" . . . Perceptions and representations in the domain of common sensation (Gefühlsinn) have been so variously characterised and defined by various authorities, that in the absence of a particularly solid groundwork, our study of the sensory area would have been completely fruitless. I have, therefore, prefaced our consideration of the sensory area by a survey of sensory perception, by going over the organs of the body, the nerves of which subserve perception, and from which perceptions evolve representations. Still, in the consideration of neural sensations, I have restricted myself to defining them as perceptions derived from motor impulses accompanying active movements; it thus remains to be added what are the organs, of which the modifications can reach consciousness as neural sensations. These organs, as I have stated above, are the subcortical ganglia or centres in brain and cord, that invoke movements. As in the infancy of the animal, the representations of movement are developed from its first purely reflex movements; as in the adult animal, the representations of movements of a part of the body, can still arise in its sensory area, even if (e.g., in locomotion), this sensory area is not actually participating in the accomplishment of such movements; as finally, the representations of movement lost in consequence of cortical extirpation in a given part of the sensory area, may be re-formed out of the reflex and automatic movements of the affected part, it cannot but be that fibres ascend to the cerebral cortex from subcortical motor centres or ganglia, as well as from the skin and from the muscles—fibres that subserve cortical perception of these subcortical centres."

"Our neural sensations are indeed, for the present, to be distinguished from what has hitherto very generally received the name of neural sensation (Innervationsgefühl)—from the "perception of the intensity of voluntary effort in connection with voluntary movement. "Will," "voluntary movement," with seat and origin in the cerebral cortex, are indeed very convenient, and may, therefore, also be valid expressions, but they are destitute

of a phenomenal physiological substratum. All we know of the cerebral cortex is that it is the place of perceptions and the seat of representations. Beyond this, it is merely admissible to assume as has been assumed by Meynert on somewhat different data, and by Wernicke, that representations of movement are the causes of so-called voluntary movements; that with the production of such a representation to a given degree (and indeed with a production *via* association, not *via* its ordinary constituent sensations) *eo ipso* the corresponding movement is elicited; and that the greater its formative representation the greater the resultant movement. The "perception of the intensity of voluntary effort, in connection with voluntary movement," might indeed be the attribute of a representation of movement; a true perception might still obtain only indirectly, quite independently of the "Will," and the percept would then be nothing but the neural sensation in the sense just set forth." (p. 177-8.)

§2. SENSORY AND MOTOR CENTRES.

"Sensory" and "Motor" are used almost antithetically. "Sensory" connotes *physically* centripetal changes ending *subjectively* in feeling. "Motor" connotes *physically* centrifugal changes beginning, according to some thinkers spontaneously in a motor centre, according to others consequently upon previous centripetal changes.

I need hardly say that I adopt the second view, every centre must be sensori-motor, *terminus ad quem* as well as *terminus a quo*; and in the two different components of the double term I do not recognise any phenomenal division of the central process into sensory and motor. Strictly I picture one undivided change taking place in any centre, resulting from impulses to it and resulting in impulses from it; therefore as far as possible I avoid the term motor, believing that it is apt to imply the spontaneous origination¹ of force, but if speaking loosely in accordance with custom, I occasionally use this term and refer *e.g.*, to a motor area, I feel it impossible not to add a guarding phrase to exclude that crude and (to me) inconceivable implication. Between centripetal and centrifugal impulses, I see a single physical process, one

¹ This is explicitly declared in Bain's theory of Spontaneity.

and indivisible; to call it sensory or to call it motor, or even to call it sensori-motor, are to my thinking, imperfectly and improperly to designate it by more or less subjective terms—with more or less obstructive connotations. All that I can recognise in the notion of a centre is an organ of elaboration receiving and giving out impulses. By the term motor I denote that it emits but fails to denote that it receives; by the term sensory I denote that it receives (and imply that *It* feels) and fail to denote that it emits impulses; by the term senso-motor I denote reception, “feeling,” and emission. All these meanings when closely analysed are illegitimate, and convey too little or too much. Experimentally I may not predicate “feeling” of any centre, but only of the hypothetical *ego*, I may only infer from visible movement that other animals “feel” and that sensations similar to my own are associated with the activity of certain centres.

This inference is unavoidably included in the word “sensory” every time we use it, and it is only in the loose language of the laboratory that we have any justification for speaking of a “sensory centre.” We are always in presence of a process manifesting itself by movement or by want of movement, and although we may for practical purposes say in the laboratory that such a centre is sensory because after its removal signs of sensation have vanished, or motor because after its removal paresis has resulted, we have no strict logical right to say that a centre is sensory, or that it is motor, nor even that it is sensory and motor. We are in presence of a central process associated with centripetal and centrifugal processes, and we have no right to say that the centre is motor, or sensory, or both, otherwise than inferentially, and guarded by a clause of interpretation to indicate that we are conscious of the inference included in the term. The inferences contained in these qualifying adjectives are at once recognised by placing oneself in the critic’s attitude. How do you know that the central process is motor and not sensory? Answer—By the movements which are its consequence, and by the absence of movements which I have learned to be expressive of sensation. How do you know

that the central process is sensory and not motor? By the movements which are its consequence or by the absence of movements which are the consequence of its annihilation, and here we clearly see that the process if sensory must also be motor, directly or indirectly, if it is to manifest objective signs. How do you know that it is sensori-motor? Motor by its movements, sensory of necessity. But in this answer we finally see that if motor may be predicated of a process seeing that it is the antecedent of motion, the word does not predicate enough, while to add "sensory" predicates too much.

So that in sum we may say—the central process is motor, *i.e.*, it produces movement; it must have centripetal antecedents, *i.e.*, motion is not spontaneously generated; it may be sensificatory, *i.e.*, a conclusion inferred from the data *sensatio mea et motus tuus*. "Sense sure you have, else could you not have motion," and we may find it convenient to use the term motivity or senso-motivity to designate this property of certain nerve-centres.

Is it not plain that inference from experiments as to motor and sensory centres and processes must have much deceptive verbal connotation to very little phenomenal denotation? What are the phenomena denoted by the "sensory centre" of a dog or the "motor centre" of a monkey, by a sensation, by an "afferent centre"? And how slight have often been the phenomenal data from which it has been said that this animal has lost such and such a sensation, that such and such a function is *localised* in this or that part of the brain. I understand the phenomenal meaning of an afferent tract or of an efferent tract, but not that of a motor centre or a sensory centre. I picture a wave of change passing through a cell, but do not know at what transverse section of the wave to label it motor or sensory. The property of cortical grey matter is sensomotivity; the most typical so-called motor cortex is senso-motor, the most typical so-called sensory cortex is senso-motor.

This has been no merely verbal disquisition, but on the contrary is fundamental to the subject. We must recognise that one indivisible *thing* is cut into two ideas by these *words*

"sensory" and "motor." A central process is not sensory or motor but senso-motor (in a guarded sense), and a centre is an "organ of return of action."

Let us apply this notion to the central process which we consider as associated with muscular movement, and see by its aid what we are to understand by such expressions as "motor idea," "idea of movement," "kinæsthetic idea."

By those who make a distinction and contrast between a motor and a sensory process, the motor idea is a mental picture of *movements about to be made*, the kinæsthetic idea is a mental picture of the results of *movements just made*. The motor idea is considered as an antecedent to motion, the sensory idea as its consequent. It does not matter whether we further admit that we may have the idea of movement without actual movement, or as the antecedent of a future but deferred movement, or as the consequent of a movement made long ago, we must in all cases recognise that it is illegitimate to say that the idea of movement is exclusively a motor idea, an image of an imminent motor discharge, or that it is exclusively a sensory, *i.e.*, kinæsthetic idea, an image of the results of previous similar movements. It is both. Each movement is an element in a series of movements, its idea or image is at once the consequence of past movements and the antecedent of the movement in consummation, which movement in consummation as fast as it progresses is attended with renewed impressions from the periphery pouring into the sum of past sensation at the centre. In a movement made at this moment, the image of the past similar movements is at the same time the image of the actual movement, which by its results repeats a similar image guiding the movement itself in its progress, and educating the centre towards future similar movements. It is not possible to distinguish a "kinæsthetic" image of past movements from a "motor" image of impending movements. The two words denote one thing.¹

¹ Thought of movement is memory of movement, or more generally the *thought* is the *remembered*. An impulse, an intention, a resolution, a prayer, is a more and more concentrated act of attention, of memory, of thought. An identical neural process is the essential phenomenon wrapped up and presented in these very different words, will, attention, memory, belief, thought:

Nevertheless we can and do recognise in the use of the word *sensu-motor*, which connotes the notion that the centripetal generates the centrifugal phenomenon, the principle that phenomena have generators and consequents antecedents; we are reminded by the word that a centre is an organ of return of action and that the type of all motor-action is a reflex act. This principle is recognised by all leading workers and thinkers—by Hitzig in his conception that the motor area of the cortex is a “muscular sense” area; by Munk in his conception that it is the motor “Fühlsphäre;” by Bastian in his conception that it is the centre of “kinæsthetic” impressions. These three conceptions are but one and the same conception, which I most explicitly and unreservedly accept as a fundamental article of thought.

But beyond this fundamental and simple article that all action is literally “ex-cited,” that any centre is an organ of return of action, or more generally still, that as far as our experience goes all phenomena are generated, not self-created, the ways part, and in the further description of the neural government of the body, which various observers and thinkers feel themselves entitled to give, variety and contradiction arise. Yet neither a purely motor view on the one hand, nor a purely sensory view on the other, can constitute for us a complete and satisfactory theory; each view is true in so far as it contains half the truth, but neither is true in so far as each contains only half the truth; the whole truth is contained in the sum of both views. The Rolandic area is *sensu-motor*. And in the mental picture that we form of the neural government of the body, it is not sufficient to admit by itself, either the undoubtedly true principle that all motion immediately or remotely is an outcome of centripetal constitution, or the equally undoubted truth that the cortex is “motor” in so far as its excitation immediately or ultimately produces motion—but we must also admit (as I have urged elsewhere) that “in the co-ordinate function of any synergic group of muscles at least three conditions must be fulfilled: (1) impulses must be emitted from the centre in appropriate manner and degree; (2) the muscles must be ‘in touch’ with the spinal

axis, duly responsive to the reflex ebb and flow arising from altering peripheral states; (3) the muscles must be in touch with each other, directly reacting against that passive extension which antagonist muscles undergo from each other."¹

It will however be convenient to postpone further consideration of this view to a later stage.

§ 3.—CORTICAL CENTRES.

A little reflection and a very slight acquaintance with "la belle doctrine" of cortical localisation, as that doctrine has been stated and supported and controverted by various authorities and writers, will very adequately correct any disposition we may feel to regard the examination of the terms used as a barren dialectical exercise. The differences of view have involved matters of fact far less than inferences from fact as embodied in words; identical words having been used of very unequal phenomenal value, and similar phenomena having been dressed up in words of apparently widely differing and even contradictory meaning. The effects of excitation and of lesion in the Rolandic area evidently have not been and cannot have been of such different phenomenal value under the hands of Fritsch and Hitzig, Ferrier, Munk and Goltz, as seems to be indicated by the terms "muscular sense centre," "Fühlsphäre," "motor centre," "sensory centre."² It can only have been by a defect in the valuation of words that we have had presented to us denial of localisation, in presence of descriptions of "motor" and "sensory" centres as definite as descriptions of carrots and of turnips.

The form which the verbal controversy has taken in this country is represented with greatest clearness and

¹ WALLER. "Physiology," p. 466, also pp. 341-2.

² I need hardly point out again that my paper does not directly involve any question of actual localisation. Its main arguments are equally applicable, whether "sensomotility" be the property of the entire cortex or of only its Rolandic area, whether we admit motor and sensory centres as taught by Hitzig and by Ferrier, or the cortical map of Munk, or indifference of function in the unrestricted sense of Flourens, or in the restricted sense of Goltz.

intensity by the Ferrier v. Bastian issue concerning the functional significance of the Rolandic area.

In the early days of cortical experimentation Ferrier designated the results of cortical stimuli as "motor." He says:¹

"By the term motor centre, as applied to a particular part of the cortex of the brain, I have endeavoured to signify the fact that this part is in direct communication with the motor tracts and their ganglia, and that its function is to excite co-ordinated muscular action of a definite kind of the nature which we call voluntary."

Here and elsewhere Ferrier defines the sense in which he uses the term "motor," and having done this is legitimately entitled to use the term to designate as "motor," those parts of the nervous system which he finds to be concerned in the production of movements.

But Bastian, confining his attention to Ferrier's experiments, altogether objects to the word "motor," emphatically denies that the Rolandic area is motor, and designates it as a "Kinæsthetic" area. It is not difficult to see how this apparently fundamental difference has arisen. Ferrier had in view movement and its central origin, and used the term motor as if implying the spontaneous origination of motor impulses in supreme psychical centres; he nowhere defends himself from the truism that phenomena cannot, as far as we know, be spontaneously generated, and that what we call a "motor" impulse is necessarily not an uncaused motor impulse, but a resultant of antecedent phenomena; indeed he appeared to adhere to a view of central initiation by the introduction of the somewhat unfortunate term "psychomotor." He may or he may not, consciously or unconsciously, have been influenced by the well-known Aberdeen doctrine of motor spontaneity as taught by Bain. In so far as Bastian entered a *caveat* against this neat "motor" doctrine at a time when it was dominant in this country, he rendered good service to our true knowledge of the subject. And that service would have been still more complete if it could have been made plain to us that kinæ-

¹ West Riding Lunatic Asylum Reports, 1874, p. 47.

thesis implied neither more nor less than is expressed in the analyses of Meynert, Wernicke, Schiff, Fritsch, Hitzig, and Munk. Then the turn might have been understood and welcomed as the expression of a familiar German postulate, viz., one indivisible process—kinæsthesia, ideally composed of æsthesia leading to kinæsis. If that is the sense in which Bastian has used the term, I unreservedly accept it as denoting a fundamental postulate.

The phenomena antecedent to "motor impulses," (leaving on one side effects of nutrition or injury or experimental stimuli), are what are commonly designated as sensory, or more properly centripetal phenomena. Whereas Ferrier with his attention focussed upon the effect, calls the centre to which he traces it "motor," Bastian goes back a step further and refers the muscular movement to its centripetal antecedents which he designates "kinæsthetic." These antecedents are for him so striking that he denies their central resultant to be motor; yet he recognises that centrifugal processes occur along efferent tracts (which he designates "internuntial") connected with the Rolandic area, and it is plain that in denying that that area is "motor" in Ferrier's sense, all that he really denies is that it is *spontaneously motor*. He contradicts "la doctrine brutale" of motor centres.

If anyone can doubt that this is the correct interpretation of Dr. Bastian's position concerning the Rolandic area, his mind will be set at rest by referring to the diagram, in which Dr. Bastian has represented the physiological anatomy of the "kinæsthetic" process. (BRAIN, 1892, p. 31. Fig. 1.)

He has there made the matter perfectly clear, and in so doing has shewn how unnecessary it has been to burden and obscure his presentation of so simple a matter by such terms as "kinæsthesia" and "internuntial." The facts presented are admitted by everybody, the words in which they have been clothed are mystificatory. And the principal part in this disguise is evidently that played by the word "internuntial" as applied to the pyramidal tract. Dr. Bastian's contention is that the pyramidal tract is not efferent but internuntial, that therefore the cortical area from which

it proceeds is not motor. In Dr. Bastian's diagram a Rolandic centre is pictured in connection with an afferent tract and an efferent (internuntial) tract, a spinal centre on the afferent tract is designated afferent, that on the efferent tract is designated efferent; but the Rolandic centre (by calling the pyramidal tract internuntial and not efferent) is made to be on an afferent tract only. Therefore the Rolandic centre is not "motor," the only "motor" centres are bulbo-spinal.¹

That Dr. Bastian's views are principally if not exclusively determined in opposition to the views of Dr. Ferrier, is equally clearly indicated in this his most recent publication.

"The so-called motor centres of the cortex were not, of course, originally supposed to be termini for afferent impres-

¹ It is surely inconvenient to speak of afferent and efferent centres, afferent and efferent impulses, and more verbally correct to speak of afferent and efferent tracts, centripetal and centrifugal processes. As regards centres the difficulty of qualification is greater. We may indeed speak of afferent and efferent centres when they are known to be on afferent and efferent tracts; but as regards cortical centres this cannot be done. "Motor," "Sensory," and "Sensori-motor" are the words which I find it necessary to use in elementary teaching, but they are not unobjectionable. The central terminus is *ad quem* and *a quo*; but whether the arrival and departure platforms are conjoined in one "Sensori-motor" centre or in "sensory" and "motor" centres commissurally joined, is a different question. The present tendency is to accept the "sensori-motor" character of the cortex in the sense indicated by Munk's account of his own experiments.

The truism that by sensory centre or motor centre we cannot mean a centre which feels or which causes motion, but at most a place where sensation occurs or a motor impulse is elaborated, is worth making particularly clear if only as a matter of definition.

I have two laboratories, in one of which I learn, while in the other I teach. The first is my sensory laboratory, the second is my motor laboratory, and the Edgware Road is the main internuntial tract between them. But the laboratories are themselves neither sensory nor motor, they are places in which sensation and motion are produced. And in this restricted sense they are not purely sensory, nor purely motor; the first is motor through the second, the second is sensory from the first.

One of my friends has a single large laboratory in which he learns and teaches. It is his senso-motor laboratory, and as a matter of convenience it is divided into rooms in which most sensation occurs, and rooms in which most motion occurs. But here again the rooms are not purely sensory nor purely motor.

Now *a priori* it can be that in that psycho-physical laboratory called the cortex cerebri there are sensory departments and motor departments. It cannot be that the cortical departments are purely sensory or purely motor, but to what degree, if any, sensation and motion and particular forms of either are concentrated or localised in particular departments can only be ascertained by the *a posteriori* method, by actual experiment.

But experiments are conflicting, Goltz's dogs absolutely contradict the dogs of Munk, and complete (*i.e.* logically insulated) data on the human brain are few.

"sions; when first discovered, they were said and they are still "maintained by Ferrier to be true "motor centres." (BRAIN. "1892, p. 30).

As a matter of fact, the excitable area of the dog's brain when first discovered by Fritsch and Hitzig, was by them inferred to be a "muscular sense area." Ferrier, while maintaining that the Rolandic area is "motor" inasmuch as it gives rise to motion, most assuredly would not assert them to be "true motor centres" in the absolute and exclusive sense implied by Bastian.

Goltz in his first¹ (and subsequent) papers on the brain, lays far more stress upon loss of sensation than upon loss of motion in consequence of cortical ablation.

Schiff² interpreted Hitzig's extirpation and excitation effects to signify that the cortex is organ of return, and lays stress on the loss of sensation due to extirpation.

The necessary functional connection between "motor" cortex and centripetal impressions (using "motor" in its carefully guarded sense to qualify a point of cortical departure and not a point of origination), is experimentally demonstrated in the observations of Bubnoff and Heidenhain.³ Cortical excitations (dog) producing contractions after an interval of .03" to .04", did so during excitation of the central end of the divided sciatic⁴ after an interval of .6" to .12". Here was a retardation of "motor" elaboration, due to centripetal impressions; under different conditions a precisely opposite effect of the same cause was observed, viz., a shortening of the reaction-time, i.e., a sign of increased "motor" or centrifugal tension.

And the whole spirit and tendency of Munk's investigations have been in the broader sense which is thus entirely ignored by Dr. Bastian. From first to last (1877 to 1889) Munk, in his interpretation of his own results, holds prominently before us the "arrival aspect" rather than

¹ Goltz. *Pflüger's Archiv*. 1876, xii., p. 9.

² Schiff. *Rivista Sperimentale*, 1876.

³ *Pflüger's Archiv*. xvi., p. 164, 1881.

⁴ It is, perhaps, not superfluous to say that this is not quoted in support of any centripetal influence along an efferent tract.—*Vide infra*, p. 382 *et seq.*, and Bastian (BRAIN, 1892, p. 26, footnote.)

the "departure aspect" of cortical processes, so prominently indeed that he has given to the Rolandic department of the cortex the name of Fühlsphäre.

Professor James interpreting the physiological phenomena from a psychological and critical standpoint, comes to a very similar conclusion; he very happily compares the motor area to the "mouth of the funnel," through which pour outgoing impulses, caused by incoming impulses. His analysis also leads him to recognise the illusory character of a sharp distinction between motor and sensory centres.

"... so that really it (*i.e.*, a 'motor' centre) is only the "mouth of the funnel, as it were, through which the stream "of innervation, starting from elsewhere, pours; consciousness "accompanying the stream, and being mainly of things seen "if the stream is strongest occipitally, of things heard if it is "strongest temporally, of things felt, &c., if the stream occupies "most intensely the motor zone."—(*Psychology*, vol. i., p. 65.)

I very cordially agree with this kind of verbal representation, distinctly adding, however, that I do not agree with the arbitrary and dogmatic exclusion of the motor zone from the "sphere of consciousness," elsewhere enunciated by Prof. James, and approvingly quoted by Bastian, Ferrier, Gotch, and Horsley.

"If the motor cells are distinct structures, they are as "insentient as the motor nerve-trunks are after the posterior "roots are cut. If they are not distinct structures, but are "only the last sensory cells, those at 'the mouth of the funnel,' "then their consciousness is that of kinæsthetic ideas and sensations merely, *and this consciousness accompanies the rise of* "activity in them rather than its discharge."—(*Psych.* ii., p. 517.)

It is to the last sentence mainly, indicated by italics (not in original¹) that I take exception. *Non sequitur*. The assertion is no more than an assertion; it is no necessary associate of James' second alternative, and, in my view, he has no right to thus distinguish between the "rise" and the "discharge," nor to attribute consciousness exclusively to either.

¹ In all other parts of this paper italics in quoted passages are italics in the originals.

"All paths are paths of discharge, and the discharge always takes place in the direction of least resistance, whether the cell which discharges be 'motor' or 'sensory.' The connate paths of least resistance are the paths of instinctive reaction; and I submit as my first hypothesis that *these paths all run one way, that is, from 'sensory' cells into 'motor' cells, and from motor cells into muscles, without ever taking the reverse direction.*

"A motor cell, for example, never awakens a sensory cell directly, but only through the incoming current caused by the bodily movements to which its discharge gives rise. And a sensory cell *always* discharges or normally tends to discharge towards the motor region. Let this direction be the 'forward' direction. I call the law an hypothesis, but really it is an indubitable truth. No impression or idea of eye, ear, or skin comes to us without occasioning a movement, even though the movement be no more than the accommodation of the sense-organ; and all our trains of sensation and sensational imagery have their terms alternated and interpenetrated with motor processes, of most of which we are practically unconscious. Another way of stating the rule is to say that, primarily or connately, all currents through the brain run towards the Rolandic region, and that there they run out, and never return upon themselves. From this point of view, the distinction of sensory and motor cells has no fundamental significance. All cells are motor; we simply call those of the Rolandic region, those nearest the mouth of the funnel, the motor cells *par excellence*."—(*James' Psychology*, vol. ii., p. 581).

I am again in agreement with this mode of view, and most unreservedly so with the concluding paragraph. But I would appeal to Prof. James himself, to bear witness to the inconvenience of thought and expression into which he must have repeatedly found himself forced by the use of these two words, "motor" and "sensory," with their implications of distinction between motor centres and sensory centres. I have elsewhere given expression to the same idea in the following terms:

"Any motor or discharging centre must also be a 'sensory' or receiving centre; it must be excited as well as excite. Any 'sensory' centre must also be motor, directly or indirectly; else we could have no objective tokens of sensation; every centre, whether called motor or sensory, is *terminus ad quem* as well as *terminus a quo*."

And with regard to the law of forward direction I fully agree with James that it is not an hypothesis but an indubitable truth, at any rate as regards efferent channels. That the vortical movement, of which organisms are the scene, and their nervous systems the highest organic expression, *should* exist in one direction is *a priori* probable. That the vortical movement *does* normally exist in one direction in the nervous system, is known to everyone who knows that stimulation of the central end of an afferent nerve, or of the peripheral end of an efferent nerve produces movement, while stimulation of the peripheral end of an afferent nerve, or of the central end of an efferent nerve produces no movement. Yet that it *may* possibly be produced in an opposite direction, at least in centripetal channels, is indicated by more or less convincing considerations, viz., by Du Bois-Reymond's negative variation in the posterior roots, and by Gotch and Horsley's observations of spinal discharge, *viâ* the posterior roots. Wundt indeed accepts the possibility to such an extent that he speaks of "sensory-centrifugal" channels and accepts without comment the view that a hallucinatory disturbance of the brain may propagate itself down the optic nerve to the retina.¹ But to weigh this doubtful point is foreign to my present purpose.

That an effective impulse cannot occur *up* an efferent channel, is proved by the absence of reflex action when an efferent nerve is excited.²

§ 4—ELEMENTARY CONSIDERATIONS.

I had fallen into the habit of imagining neural processes on a three-level scheme, before even knowing the name of Hughlings Jackson, and had adopted as typical diagrams of reference the two schemata (Figs, 1 and 2) which I now reproduce from notes put on paper in 1876, adding to them the expressions then used, and the Jacksonian expressions that I have since learned. But I have never regarded such a three-level scheme as anything more than a convenient

¹ *Physiol. Psychol.*, vol. ii., p. 432 (3rd. ed.) Engelmann's retino-motor fibres are not here in question.

² See also page 386.

scaffolding upon which to hang certain functional conceptions.

I shall give diagrams in the endeavour to employ every possible means of exhibiting the tenour and tendency of my mode of view, and in the belief that a presentation in black and white of the schemata and of the symbols which have served as a standard of meditation, will help to expose the drift of reasoning—necessarily in large measure psychological, and therefore liable to extravagance of abstraction.

A principle that has greatly contributed to my thought-guidance is the notion that every neural process, even the lowest, is on the type of the psychological process of inference. I have gradually associated this notion with doctrines proceeding from empirical psychologists, more especially from Wundt, Bain and Fechner;¹ viz., from Fechner the relation between stimulus and sensation, from Bain the law of relativity, from Wundt the doctrine of neural inference. Without undertaking an adequate (and therefore lengthy) exposition of the relation knitting together these several topics, I must now content myself with a bare indication of the kind of relation which appears to me to exist, enough to justify the schema of inference which has served as one of my guiding notions. I will do so as briefly as possible in the following paragraphs:—

It appears to me to be very essential to recognise the applicability of the law of relativity in the most elementary sensation as well as in the higher provinces of judgment. In Fechner's theory a sensation is a comparison between two stimuli; the proper symbol indicative of its objective composition, is therefore a ratio $\frac{a}{b}$, and not any absolute quantity a or b .

I have gradually come to adopt the logical expressions: *major premise*, *minor premise* and *conclusion*, in analysing the factors of the simplest sensation, as well as of the most com-

¹ I name only those sources from which I have consciously derived instruction. I notice that James states that Wundt and Helmholtz have more recently "recanted" (i.e., retreated from the neural inference position). As far as my reading of Wundt goes, this seems to me a very gratuitous and misleading supposition on the part of Professor James. ("Psychology," ii. note to p. 111.)

plicated mental judgment. This is not intended to imply that the syllogistic terms of logicians correspond to any physiological factors in a neural process, but is rather the consequence of early associations, and of subsequent experience of the great convenience of this mode of formulation as regards the detection and localisation of fallacy in particular cases. And I think that the use of these terms is perfectly compatible with an arrangement of factors in the form of an equation, which although in substance identical with the three syllogistic propositions, appears to me more closely physiological than these as regards the representation of the neural process of inference.

But at this juncture it is of particular importance to exercise all possible care to avoid confusion between sensation—the subjective phenomenon, and sensificatory change of state, its imaginary objective substratum. And it is to guard against this fallacy that in symbolising the phenomena I find it useful to reserve Greek letters for the subjective and Roman letters for the objective state.

The simplest present sensation σ is not the concomitant of any isolate present state of organ S , but the resultant of a comparison based upon state now and state before now, *i.e.*, of a ratio $\frac{s}{S}$.

The organic state now is the sum of many previous alterations of state; many sensations σ the concomitants of many previous ratios $\frac{s}{S}$, may be conceived as summed up in the state of sensibility Σ .

This state of sensibility Σ has as its organic basis a material state, which (having regard to the total Σ by many elementary σ , each concomitant with a ratio $\frac{s}{S}$) is properly represented by the ideal symbol $\frac{s}{S}$.

Thus Σ the specific sensibility, the subjective resultant of past experience, is the concomitant of $\frac{s}{S}$, the imaginary organic sum of the antecedent series of organic ratios $\frac{s}{S}$.

To this resultant state (subjectively Σ objectively $\frac{s}{S}$) I apply the term "personal ratio," meaning to connote in this expression that the personal state now is composed of compared objective phenomena and not of *absolute* objective

phenomena. Delbœuf has long since expressed this idea with the utmost clearness.

This personal ratio I take to be the subjective attribute of the organic *major premise*, each new sensificatory change of state I regard as a *minor premise*, and I consider that a *conclusion* is formed by the compounding of these two premises; this compounding or neural inference may, I think, properly be represented symbolically in the form of a multiplication, in which the product represents the conclusion, and at the same time the new major premise in relation to subsequent minor premises.

$$\frac{s}{s'} \times \frac{s}{s'} = \frac{s \ s}{s' \ s'} \text{ or subjectively } \sigma.$$

But here it is necessary to justify the transition from the use of the word "ratio" to that of the word "premise," and the use of a fractional symbol for both words.

The terms of any premise or proposition are the subject and the predicate (joined by the copula); in a major premise the subject enumerates and the predicate denominates. The major premise "all men are mortal," may be written $\frac{\text{all men}}{\text{all mortals}}$ in which we enumerate "all men" as forming part of "all beings denominated mortals."

The minor premise "*Socrates is a man*" may be written similarly $\frac{\text{Socrates}}{\text{men}}$ by which we enumerate Socrates as belonging to the denomination men just enumerated in the subject of the major premise. And the syllogism may be written :—

$$\frac{\text{men}}{\text{mortals}} \times \frac{\text{Socrates}}{\text{men}} = \frac{\text{Socrates}}{\text{mortals}}$$

major. minor. conclusion.

This is not an essay on logic, else we might proceed to urge that in a syllogism admitted to be valid by unanimous verdict, the major premise may be a truism, and the minor premise a clearly demonstrated fact; but I am at present only concerned to justify the indiscriminate use and identical symbolisation of a ratio and of a premise. The short account just given may suffice to indicate what I conceive to be the essential identity pervading the whole range of neural inference phenomena from the simplest sensation to the highest judgment.

But this description has only represented the process starting from given major and minor premises; it does not represent the mode of formation of a major premise, nor exhibit what in the process of sensation or judgment must be conceived as the order of procedure. A very few words will however suffice.

The starting point of the growth of sensibility is the ultimate fact "sensation," or the minor premise; sensibility or the personal ratio is the resultant of many past sensations or minor premises, *i.e.*, centripetal induction or synthetic generalisation of particular experiences. The starting stimulus in each act of sensation or of judgment is a sensificatory minor premise, and the consummation of a sensation or of a judgment is a compounding of premises in consciousness, attended with what can only be indicated as an intuition of parity or disparity between the psychological factors on the two sides of an equation, *i.e.*, major, minor on one side, conclusion upon the other.

I do not wish to press this symbolic mode of representation too far, yet I feel entitled to urge the picture as a useful one in bringing home to us the essential similarity of neural process concomitant with the whole range of subjective phenomena from the simplest sensation to the most complex judgment. It is in my opinion most congruent with the derivation of voluntary from reflex action, and with a physiological conception of the mechanism of ratiocination. It would be absurd to say that this or any other mode of symbolisation "explains" judgment, attention, sensation, but it is not too much to say that it clearly exhibits the possible factors of neural inference; each act of observation, each determination of conduct, is the resultant of two factors (1) the major premise or central state, more or less ad-tended to: (2) a particular minor premise or group of sensificatory stimuli, also more or less ad-tended to (within the remaining sensificatory field, less or more ad-tended to.) And in any theory of ratiocination, we must recognise that the primitive mental process is one of centripetal induction, its immediate consequent one of centrifugal deduction, and all higher mental processes on this reflex type compounded

of centripetal induction of minor premises with centrifugal deduction from a major premise.¹

Considering the centripetal aspect of neural processes we may with advantage appropriate to the three-level scheme the three terms *impression*, *sensation*, *perception*—using “impression” as the lowest-level term, “sensation” as the middle-level term, “perception” as the highest-level term; and taking impression to denote an effect that does not reach consciousness, sensation (= *empfindung*) to denote a felt impression, perception (= *wahrnehmung*) to denote a sensation in its felt circumstances, i.e., in relation with the group of associated sensations forming the mental picture of the moment (= *vorstellung*). The relative significance of these more or less abstract and figurative terms may be assisted by comparison with a more concrete case. Taking cerebral cortex as the analogue of retina: the mental picture or representation or cerebral field of attention is analogous with the retinal field of vision; the perception is the analogue of the distinct object in the centre of the field, sensations the analogues of surrounding objects within the field viewed indistinctly; impressions the analogues of surrounding objects in the field that are not seen, although they can be seen. This indeed is rather more than an analogy, there is actually an association between physiological field of vision and psychological field of attention; retinal field is really retino-cerebral field; the great psychological importance of visual investigation is attributable to this association as well as to the fact that the visual field is the more complex and more susceptible of experimental analysis than any other of our sense fields.

¹ Readers of Spencer will probably recognise the close similarity of this mode of representation with that contained in the “Principles of Psychology,” vol. ii. I have not consciously borrowed them from that source, yet had I not there found the equation between two ratios urged as a more natural mode of representing ratiocination than the syllogistic form of logicians, I do not think I should have ventured to promulgate such a symbolism, although strongly persuaded of its greater fidelity to physiological phenomena. “Every ratiocinative act is the indirect establishment of a definite relation between two things, by the process of establishing a definite relation between two definite relations.” Spencer’s “Psychology,” vol. ii., p. 16.

I have long been in the habit of making use of the following table and schemata, and have found them of service in guiding my thoughts. I have refrained from publishing them, believing the considerations embodied in them to be admitted as truisms, and feeling no call to add another verbal skeleton to the literature of the subject. But having used them and finding that they have led me to certain peculiarities in the mode of view, I find it necessary to expose them to criticism, as forming an introductory basis of what I believe to be a (in some degree) rational reading of cerebral psycho-physiology.

GENERAL ATTRIBUTES OF THE CEREBRO-SPINAL CENTRE.¹

I.—REFLECTION.—Every central element is an organ of return of action. It is excitable-exciting.

II.—RESISTANCE.—Each central element has a specific capacity for centrifugal tension resultant from centripetal impressions.

III.—RADIATION.—Centripetal impulses to a central element radiate to connected elements.

Inferior impressions influence superior centres ; superior expressions influence inferior centres, in accordance with degree of resistance and strength of impulse.

IV.—RETENTION.—After each period of activity and restoration there remains an altered constitution of central elements, in which resistance to homogeneous activity is diminished, to heterogeneous activity increased.

These four R's embrace all the phenomena that came under my consideration, but I subsequently supplemented them by the following article, which is in reality an amplified statement of Article IV. The fundamental phenomena of exhaustion and replenishment (although constantly effective conditions of nervous processes) did not appear to me to belong to the present order of considerations, and are therefore not formally acknowledged here.

V.—SUMMATION (AND INHIBITION).—Two or more impulses summing in centrifugal tension or expression

¹ Each of the propositions enunciated on this page is considered to be preceded by the words "It is conceivable that . . ."

may be individually "friendly" or "unfriendly" to one another.

Homogeneous impulses or groups of impulses received simultaneously or successively by the same central elements produce a sum of effect greater than the effect of a single impulse or group of impulses—*i.e.*, are "friendly."

Heterogeneous impulses to the same central elements produce a sum of effect smaller than, or different from, the effect of a single impulse—*i.e.*, are "unfriendly."

Any two or more impulses or groups of impulses, whether homogeneous or heterogeneous, successive or simultaneous, upon different central elements, may be friendly or unfriendly; they are more commonly unfriendly.

The terms "impression" and "expression" are used above in a very general sense to denote centripetal impulses from the periphery towards a centre, and centrifugal impulses from a centre towards the periphery.

The impression to a superior centre is constituted by the impressioned resistance of an inferior centre.

Given three centres ranking I., II., III., and indicating impression, expression, and alterations of resistance or tension by appropriate letters and numerals. An impression I_3 enters III., produces an alteration R_3 and liberates E_3 . Altered R_3 constitutes impression I_2 , I_2 enters II., produces an alteration R_2 and liberates E_2 . Altered R_2 constitutes impression I_1 . I_1 enters I. alters R_1 and liberates E_1 . A second identical impression I_3 will meet with less resistance in III., it will produce a smaller change R_3 and conceivably a larger liberation E_3 ; a smaller change R_3 will constitute a smaller impression I_2 , therefore a smaller change R_2 , therefore a smaller impression I_1 , &c.

As the consequence of repeated identical impressions we thus have:—diminished resistance, the diminution being greatest in III., less in II., least in I.; diminished impression, the diminution of I_2 being greater than that of I_1 ; increased expression, the increase being greatest of E_3 , less of E_2 , least of E_1 , or otherwise: the minimum effective impression diminishes with repetition; the diminution is greatest in the case of I_3 , less in that of I_2 , least in that of I_1 , and we have as the

final result of repeated activity, greater diminution of R_3 than of R_1 , and a scale of mos. diminished minimum I_3 to least diminished minimum I_1 (with constant expression) of most increased E_3 to least increased E_1 (with constant I_3); which I take as signifying how, *With repeated exercise the reflex function specific to each centre, ranges in degree from most perfected in III. to least perfected in I.*

FIG. 1.

It involves these three assumptions:—resistance diminishes with repetition; impression ascending from lower to higher centre varies in the same sense as the resistance overcome in the lower centre; expression descending from higher to lower centre, or from lowest centre to periphery, varies in an opposite sense to the resistance overcome in the expressing centre.

The terminology used may prove a stumbling block to the word-picture I am attempting to compose. Impression and Expression are used in a peculiar and most general sense to denote impulse *to* and impulse *from* a centre. The diagram will, doubtless, make that sense plain. It contains this ambiguity. In the case of centre III. for instance, we have, properly speaking, two lines of expression *from* that centre,

one E_3 towards the periphery, the other I_2 towards the centre above. And to the same centre III. we have *two* lines of impression, one I_3 from the periphery, the other E_2 from the centre above. Thus I_2 is expression with reference to III., impression with reference to II.; E_2 is expression with reference to II., impression with reference to III. A similar ambiguity attaches to I_1 and E_1 . But it was necessary to make choice of symbols, and this has been done in accordance with direction to or from the highest centre.

I have employed the physical figurative term *resistance* after some hesitation, but no distinctly preferable term offered itself. Perhaps the words tension or elasticity come closer to the idea intended to be conveyed. We may imagine a centre as an elastic reservoir of tension, in which an ingoing impulse may be absorbed with increase of tension which may or may not express itself in an outgoing impulse. But to this physical term we must attach chemical connotations, considering that the impulse elaborates greater chemical instability or actual discharge. Thus "resistance" is equivalent to chemical "stability," diminution of resistance is diminution of chemical stability. But what now do we mean when we say that an impulse in passing through a cell "meets with less resistance," or produces a smaller alteration of tension, and how can we picture this in chemical terms? Such modes of expression obviously give a most imperfect picture of what actually takes place. An impulse does not properly speaking *pass through* a cell, it evokes elaboration within the cell, and in this view resistance is chemical inertia; but diminished resistance is not diminished chemical inertia, it is diminished chemical change or diminished chemical friction. I have said enough to indicate the imperfection of idea which is apt to be connoted by either a physical or a chemical figure of speech. Yet I have perforce made use of such a figure, viz., "resistance," choosing it as the most familiar, but guarding myself from the appearance of pushing the physical analogy suggested in the word.

The effects of an impulse upon a cell (indicated by the word alteration of resistance, and in the diagram by the letters R_1 R_2 R_3) I picture to myself somewhat as follows—

taking for illustration the centre III. The impulse I_1 influences cellular energy; it increases¹ potential energy, it may render potential energy actual; it tends to overcome or actually overcomes inertia, and in the latter case it effects friction. The actual effects of this cellular elaboration are the impulses E_3 and I_2 . In saying that a second impression I_3 meets with less resistance, I imagine that the cell matter is more unstable; this would seem to imply that the impulse would more easily disturb the whole mass of the cell, producing a more extensive change and thereby greater E_3 and I_2 . But I do not admit this. I imagine the greater instability of the cell to be *molecular*, so that the disturbance by a second impulse can propagate itself with less diffusion, overcoming less chemical inertia, and producing less friction, than the disturbance produced by a first impulse. The analogy with electrical resistance may make this point clear, but it is only an analogy. An electrical current entering by one point of a wet sponge of lower resistance would propagate itself with less diffusion, overcoming less chemical inertia, and producing less friction than in the case of a wet sponge of higher resistance.

Thus it is that I am led to imagine that the disturbance of a nerve-cell by a second impulse I_3 gives rise to a *greater* impulse E_3 and to a *smaller* impulse I_2 , than did a first impulse I_1 .

First impressions are the most intense in consciousness. Skilled labour is carried on with weak guiding impressions and weak consciousness.

It is more difficult to frame a diagrammatic representation of mechanism to fit our idea of synthesis, association, summation, inhibition. That which I am about to give is planned on a three-graded dichotomous type, and connotes what by many physiologists will be considered a false notion, viz., the representation of *different* peripheral parts in the *same* highest centre cell. Again, it assumes what anatomically we have no right to assume, viz., three levels of representative cells; all that we positively know as regards an anatomical

¹ For the sake of simplicity I leave out of view the anti-motor or inhibitory impulse.

justification of the schema, is that the total number of fibres between cortical and spinal cells is smaller than the total number of fibres between spinal cells and periphery.¹ Nevertheless, I have adhered to a three-level scheme as being most in accord with current psychological doctrine. And the representation of different parts in the same highest cell is in my view a material form of the psychological doctrine of the "unity of consciousness."

FIG. 2.

We cannot indeed make any sort of analysis of the centripetal process from impression to perception without imagining some sort of grading of nerve-cells; and the real anatomy of a hierarchic scheme is from this point of view quite subordinate to the ideal conception. The three terms adverted to above, viz., impression, sensation, perception, are as convenient and distinctive as any, and there is indeed no absolute necessity for adopting the further term apperception, which to some minds seems to possess very

¹Sherrington's recent observation of "geminal" fibres in the spinal cord is perhaps another anatomical sign of an increasing number of fibres from above downwards above the spinal relay.

misleading connotations. The phenomena of hypnosis—more especially as presented to us in Bernheim's analysis—imperatively require us to adopt some such schematisation; *e.g.*, a hysteric hemianæsthetic, who asserts truthfully that she (or perhaps he) cannot feel, see, smell, taste or hear any impressions made on half her (or his) body, can be proved to have sensations of touch, sight, &c. He believes he cannot feel, and, therefore, does not feel, yet, all fraud excluded, it may be *proved* by appropriate devices that he does feel these sensations, but thinks that he does not do so, and, therefore, does not perceive them. Words are inadequate shortly to describe such a condition, yet it is an indubitable reality, and intelligible enough when regarded in terms of imaginary imprints on two different cerebral levels I and II. In the hysterical hemianæsthetic the imprint on II is effected, but the consequent imprint on I is travestied or untranslated owing to some functional disorder or incapacity of I, yet the physiological sequence from III to II and from II to I proceeds, and can be proved to proceed, if by any suitable device the false belief be circumvented—*e.g.*, such a person "blind" of one eye will see a real object doubled by a prism held in front of the sound eye; which proves that his "blind" eye is not "blind," that he has sensation by that eye when he does not think he is using it, but no sensation when he does think he is using it. Such a person may be and often is an impostor, but even a genuine sufferer would be set down as a malingerer in nine cases out of ten. Yet such a case might be perfectly genuine, the patient being at the same time "truly blind" of one eye, yet "truly not blind" of that eye, according to the sense in which we read these words "truly" and "blind." And the same may hold for any other sense; a genuine hysteric might be unable to hear or smell or move, because of her own conviction of impotence, yet able to hear or smell or move as soon as that conviction had been overcome. All these facts are best realised by a reference to two levels of cortical action II *sensory*, and I *perceptive*.

Although I shall refrain from entering upon a full and detailed consideration of the localisation doctrine, I can hardly

escape making some allusion to the obvious bearing of this upon the physiological doctrine of cortical representation, at the two opposite poles of which we have the "positive" contention of Munk, who experimentally verifies the projection doctrine of Meynert, and the "negative" contention of Goltz, who experimentally verifies the indifference doctrine of Flourens.¹ The antithesis thus shortly stated is at any rate as regards Goltz somewhat too absolute. Goltz indeed does not uphold absolute indifference, but rather an indeterminate and rudimentary predominance on a large scale and in the rough, of different function in different regions. His observations on dogs are irresistibly convincing; the observations of Munk upon dogs in proof of the projection of retinal upon cortical areas, require stronger confirmation than they have yet received before they can be admitted as real; Schäfer operating on monkeys confirms the principle in a very guarded manner, and describes the projection very differently from the view of it given by Munk on the same animal. In the presence of such mutually destructive data, we are at liberty—pending further developments—to attach weight to purely psychological considerations. In accordance with the doctrine of the unity of consciousness, we are called upon to believe that our perception of things seen, things heard, things felt, &c., may take place in connection with the modes of motion of a single organ.² Pictorial and verbal expression of this theory or belief is given by Wundt in his doctrine of a frontal organ of apperception; the views of Hughlings-Jackson that the frontal cortex is possibly the highest level centre, and of

¹ Many other names might of course be quoted; Ferrier, Schäfer, Horsley and Beavor, Charcot, Déjerine, and most clinical observers are "localisers" more or less strict. Brown-Séquard, Schiff, Dupuy, are "indifferentists" with more or less reserve. But there are many shades and degrees of opinion between the two extremes. Schäfer, for instance, is in some respects (G. Fornicatus) a localiser, but in others (hearing) an indifferentist; Exner is a "diffuse localiser;" even Goltz may be characterised a "diffuse localiser" inasmuch as he makes reservations to the complete indifferencism of Flourens; and the position held by Brown-Séquard and by Dupuy is very similar.

² "We see that a perception can have in a nerve-centre no definite localisation, but only a diffuse localisation. No one excited fibre or cell produces consciousness of an external object; the consciousness of such external object implies excitement of a plexus of fibres and cells" (Spencer's "Principles of Psychology," second edition, 1870, p. 562).

Ferrier,¹ that it is connected with the intellectual faculties, are in perfect agreement with this well-known Wundtian schema. Considering the state of probability constituted by physiological experiments and by psychological considerations, I find sufficient justification for adopting a neural schema symbolising a superposition of heterogeneous impressions and a common origin of heterogeneous expressions $\alpha \beta \gamma \delta$, in and from a single highest level cell I.² Considering, however, the probabilities as regards the cortical mantle formed of many such connected cells I., and the subordinate masses of cells II. and III. I adopt a view intermediate between the positive pole "localisation" and the negative

¹ Accurately speaking, this view is unreservedly adopted by Ferrier in the first edition only of his "Functions of the Brain," although it still remains in his second edition. But quotations will best serve to show the subordinate importance and indeterminate character of the anatomical localisation as compared with the ideal conception. Ferrier's present view is substantially identical with that of Munk, although Ferrier very justly in another place (p. 429) criticises and rejects Munk's nominal antithesis, "psychical" and "cortical." But, as has been remarked, Munk uses the terms in a naked denotative sense, meaning no more than might have been indicated by the terms α and β .

The centres of inhibition being thus the essential factor of attention, constitute the organic basis of all the higher intellectual faculties. And in proportion to their development we should expect a corresponding intellectual power.

"In proportion to the development of the faculty of attention are the intellectual and reflective powers manifested. This is in accordance with the anatomical development of the frontal lobes of the brain, and we have various experimental and pathological data for localising in these the centres of inhibition the physiological substrata of this psychological faculty" (Ferrier, first edition, p. 287.)

"The hypothesis of Hughlings-Jackson and others, that 'in addition to the sensory and motor substrata which have been demonstrated and defined by physiological and clinical research, there are other and higher motor as well as sensory centres, in which all the motor and sensory functions are again represented, and form the substrata of the higher mental operations . . . receives no confirmation from the facts of experiment, nor does it appear to me at all necessary to explain the facts of normal or abnormal mentation. We have in the sensory and motor centres of the cortex the substrata of the respective forms of sensory perception and ideation, and of the individual acts of volition, simple and compound, as well as of the feelings associated with their activity. It seems more reasonable to suppose that there may be higher and lower degrees of complexity or evolution in the same centres than to assume the separate existence of more highly evolved centres, for which no evidence is obtained by the results of experimental research'" (Ferrier, second edition, p. 460).

Wundt rejects the view that our supreme perceptions of different sensations are separately consummated in separate centres, and characterises it as a revival of the phrenological faculty scheme. (*Phil. St.*, vi., p. 1.)

² It is, perhaps, hardly necessary to say that the expression "a single cell I." made use of on account of the diagram, must be taken to mean "a single mass of cells I."

pole "indifference." I do not think that I can express that position either more accurately or more shortly than I have already done elsewhere.

"The 'nerve-centre' of mammalia and of man is a collection of nerve-centres occupying the cerebro-spinal axis, with more or less diverse special offices under their control—communicating each with the other upon occasions, yet separately active upon other occasions—having functions which are localised at certain parts, yet not strictly confined to these parts—playing upon and influencing each other in all directions, yet in some directions rather than in others, and maintaining some kind of precedence and rank, so that while all may influence all, yet some are usually guided and controlled by others—variously organised through past excitations, yet still variously organisable by excitations to come.

"The state of doctrine [i.e., of localisation], may be made plain by an analogy. The various activities making up the business of the brain do not all take place all over its surface, as in a country without towns or villages, where all kinds of industry go on in every hut or tent; nor are the different activities absolutely restricted to certain spots, as if in walled towns. The brain cortex is not comparable with either of these extreme cases; its territory must be recognised as possessing towns with special industries, but towns with straggling and overlapping suburbs, and industries which are indeed predominant, each in a given centre, but not exclusive of all other industries in that centre, nor excluded from other centres in which other industries predominate."

¹ My reading of Mosso's psychophysical observations (*Du Bois-Reymond's Archiv.*, 1890, p. 129), to the effect that prolonged intellectual effort subtracts from voluntary muscular workability, is in accordance with this intermediate view. If there are concentrations of different functions within an imperfectly differentiated organ, we should expect high-level exhaustion of any particular kind of activity to be greatest as regards that kind, but to exist in a minor degree as regards all other kinds of high-level activity. This, of course, applies only to the *central* component of fatigue, and not to the peripheral component attributable to the circulation of disintegration products. We may picture the functional depression as being of the entire cortex, but deepest at particular situations of concentrated activity. The converse change, viz., diminished capacity for intellectual effort after prolonged muscular exertion, is familiar to most brain-workers. On the other hand, most people have experienced that although one kind of mental activity fatigues the brain with respect to a different kind, yet such collateral fatigue is apt to be less marked than the principal fatigue, and so much so that a change of activity may feel restful.

And now, comparing the degree to which the functional and structural organisation has reached at different levels, I think we are justified in stating the following antithesis :

At the lowest level the representation is most definite and fixed, the canalisation of conductors is most closed, the differentiation of the central organ is most complete. At the highest level the representation is least definite and most fluctuating, the canalisation of conductors is most open, the differentiation of the central organ is least complete.

Contrasting the two extremes of cerebro-spinal action, we have in the purest reflex action an immediate, "fatal," unchosen response to peripheral stimulation ; the nervous path is deeply canalised, alternative reactions are few, the particular reaction to a particular stimulus is easily predicted. In the purest voluntary action we have a delayed, "free," chosen response to a given sensation. The nervous path is not preformed ; alternative reactions are numerous ; the particular reaction to a particular sensation cannot be predicted.

It is another and a further and an essential question whether the greater diversity of function and division of labour is of such degree in the human brain, that particular parts of the cortex have become the special organs of particular parts and functions of the body. And towards the answer to this question of degree, the deduction from experiments upon animals is of little value ; brain-surgery and clinico-pathological investigations of pure and uncomplicated cases of Jacksonian epilepsy and cortical monoplegia are the necessary and alone trustworthy premises of inference.

The localised effects of localised excitation of the cerebral cortex of dogs and of monkeys do indeed give fairly concordant results, which amply suffice to establish the principle that excitation of different parts of cortex and of subjacent corona gives different effects. But the converse test by localised extirpation has altogether failed to come up to the expectations that were first formed ; in the hands of Goltz extirpations have been made of larger and larger areas of the dog's cortex, of an entire hemisphere, and finally of both hemispheres, without producing permanent paralysis, while

in Munk's hands local extirpation of different areas on the same kind of animal (not to speak of monkeys) gave different local and restricted loss of sensation and of motion.

As is well recognised, the contradiction does not merely illustrate that different observers looking for different effects *see* different effects, but is in part owing to whether immediate and temporary defects or permanent deficit (*Ausfallserscheinungen*) were described as paralytic effects. Although *a priori*, a temporary paralytic effect is a possible result of a cortical ablation, we have no right to assume *a posteriori* that the defect is due to lost central matter; it may equally well be an inhibitory effect due to irritated central matter, as has been urged by Brown-Séquard and by Goltz.

And it is one of the fundamental principles of the Nancy school that the symptoms of an organic lesion include not only such as may be termed direct and organic, the immediate result of destroyed tissue, but also a varying and variable swarm of satellite symptoms, the secondary and functional effects of the primary. The organic blot is, so to speak, surrounded by a functional halo; and it is one of our greatest sources of doubt and difficulty that these fringe effects may, to use the language of Brown-Séquard, be effects of dynamogeny or of inhibition, and that we may even with extreme care be totally unable to distinguish and assign to their separate cause (1) the organic paralytic effects, and (2) the functional paralytic effects, the *ausfallserscheinung* from the *hemmungserscheinung*, total loss from functional suppression. It would therefore be most unsafe to reject Goltz's contention that a true localisation of function requires as its proof a permanent and not merely a temporary defect. And yet a doubt remains, even the lapse of time yields no sharp line of distinction; when shall we say "this is finally lost, this is at present suppressed," after Bernheim has taught us to recognise in human pathology that widespread and *chronic* functional modifications surround the organic lesion and its direct physical effects. Every physiologist recognises the physiological importance of the distinction urged by Goltz; some have also recog-

nised the meaning of Brown-Séquard's language, but for the present it must, I fear, be admitted that very few of us sufficiently recognise how that fundamental conception underlies the curative treatment of the Nancy school. The medical (and human) aspect in this department is the department of the pure physician, the physiological (and human) aspects concern the pure physiologist.

The principle of functional diffusion from an organic focus, foreshadowed in the clinical study of Jacksonian epilepsy, clearly evidenced in the comparatively simple but scanty data of the laboratory, comes to its full maturity in the hardly less simple but far more numerous and more minutely readable data insulated for us in the Nancy doctrine of so-called hypnotism. As "true localisers" we should expect to find a limb completely paralysed after its cortical centre had been destroyed, *e.g.*, in a dog. But this is not the case; the limb can be moved, and at first sight its movements seem perfect; it is certainly not completely paralysed. On closer examination some defect of movement may indeed be detected, some clumsiness of the limb indicating that its movements are inaccurately co-ordinated, and these imperfect movements will probably be found to be such as are normally most highly specialised and *not* such as are most profoundly automatic. But even this amount of defect is not apt to be permanent—the apparent awkwardness soon wears off, movements seem to be relearned. The clinical history of a dog or of a monkey having suffered a removal of some portion of the Rolandic area altogether negatives a strict localisation of function, and at most suggests its local concentration. We may imagine a company of cells more particularly concerned in particular manœuvres of particular parts, and we see these manœuvres rendered clumsy by disturbance of the company: we are obliged to imagine that even if these cells are wholly destroyed, the lost control may become more and more perfectly assumed by other cells educated from the periphery, as the clumsy movements are re practised, corrected, and learned. We thus picture the cortical organ in a semi-fluid state of differentiation, still variable by new instruction, rather than

as a petrified and invariable collection of specialist organs tied down to particular functions and exclusively performing these functions.

Such diversity of duty gradually superimposed upon uniformity of capacity, seems to me a sufficient physiological ground upon which to reconcile the assured results of cortical excitation, the hardly less assured absence of permanent paralysis after cortical extirpation in dogs, and the psychological doctrine of the unity of consciousness in man. The view is (I think) substantially that offered by Wundt in his doctrine of apperception, and whether we go on to hold with Wundt that the seat of supreme or maximum consciousness consists in a frontal organ of apperception, or with Munk that it is the central area of each particular *Fühlspähre*, it seems to me that we must admit the cortex to be *one* organ, *qua* the ideas and concepts composed by impressions confluent through different afferent channels. Wundt's view, to my mind, gives clear diagrammatic expression to this necessity, Munk's view leads us into the admission of departmental consciousness; the first postulates one supreme consciousness, the second postulates many supreme kinds of consciousness. Although we may (and must) admit a physiological division of function and of organ up to a certain point, I think that in the present state of knowledge we need not (and therefore must not) admit any psychological subdivision of the supreme organ of consciousness.¹ To take a very simple case, we must admit that the notion of a bell is given to us in *one* auditory-tactile-visual organ, even though there be a discrete centralisation of auditory, tactile, and visual impressions. Nor does it seem to me to be of essential importance whether we admit that supreme "composite" consciousness is the attribute of a part of the cortex, or of the whole cortex acting as one psychological organ of confluence although composed of differing physiological departments. What does seem to me to be of essential importance is that a physiological division of function, ascer-

¹ The phenomena of "*audition colorée*" (viz., the association of certain colours with certain sounds, the latter "*suggesting*" the former) naturally come to mind in this connection.

tained by experiment, should not lead us to admit a plurality of supreme consciousness.¹

Guided by these considerations we may, I think, safely advance a step further in the *a priori* projection of thought:

The higher the brain the greater the mass of unformed formable material. But the further the differentiation shall proceed of formable material, the further it must already have proceeded of formed material. The higher brain must include subordinate highly specialised and economically-working mechanism-guiding parts. Otherwise it would not be free to be developed by new impressions and new combinations of impressions. And yet the further the brain has developed within the span of life of its possessor, the greater the differentiation of function and of organ out of the mass of formable material. We should expect greater concentration and fixity of function in the older brain, we find evidence of that greater concentration (with correspondingly dwarfed signs of dissimilar capacities) in the conduct of every matured and specialised man; experimenting on the cortex cerebri we should *a priori* anticipate clearer and sharper results from an old dog or an old monkey than from a young dog or a young monkey.² Thus it may be truly said, with reference to the functional attributes of the cerebral cortex that the higher the brain the less the fixity, the older the brain the greater the fixity.³

¹ Wundt has expressed this with great clearness in his recent criticism of Munk ("Philosophische Studien," vol. vi., 1890).

² Küssmaul makes the following apposite remark:—"One thing may be safely stated, however, and that is that the age of the patient is an important factor for the compensation of the injury. Children have been known to learn to speak after astonishingly extensive destruction of the left region of speech, and even of the entire cortex of the left hemisphere, while in old persons remarkably small foci of disease in the cerebral substance sometimes cause permanent aphasia." (Ziemssen's "Cyclopaedia," vol. xiv., p. 803.)

³ The following quotation from Darwin ("Descent of Man," i. p. 88).—"Little is known about the functions of the brain, but we can perceive that as the intellectual powers become highly developed, the various parts of the brain must be connected by the most intricate channels of inter-communication; and as a consequence each separate part would perhaps tend to become less well fitted to answer in a definite and uniform, that is, instinctive manner, to particular sensations or associations"—given by Dupuy as an opinion in support of the doctrine—"greater freedom of communication and of function in the higher brain," appears to me misleading in so far as the order of development is represented as being from the more special towards the more general, from heterogeneous towards homogeneous. The main drift of development exhibits itself as being towards specialisation, from the undifferen-

The schema given above (fig. 2) has, for several years past, furnished me with a concrete mechanical image to which I could refer the psychological phenomena of association.

If impressions of the two kinds α and β occurred simultaneously, and have been compounded in II., the repetition of α will be specifically liable to revive that compound effect which subjectively is the memory of α and β .

Similarly in the case of any other two or more kinds α , β , γ , δ , if their simultaneous or successive association in the past has been sufficiently frequent to leave a fused effect, the repetition of a single member α or β or γ or δ may revive that effect, *i.e.*, subjectively the memory of previously associated impressions.¹

The same schema (fig. 2) has also served to materialise my conception of the influence of attention upon particular incoming or outgoing impulses. I imagine an effort of focalised attention to consist in a state of increased tension along any single tract, α or β or γ or δ , together with a state of diminished tension along the remaining tracts $\beta\gamma\delta$ or $\alpha\gamma\delta$ or $\alpha\beta\delta$ or $\alpha\beta\gamma$. In which conception I tacitly assume the senso-motor character of the series of

tiated towards the differentiated; still it must be admitted as conceivable that secondary connections between more or less differentiated centres may become established, making *against* rather than *for* their functional severance; but the main and primary movement is disconnection, reconnection is secondary and subsidiary.

Herbert Spencer in the 1870 edition of his "Principles of Psychology" (and also in the 1855 edition), gives the clearest possible *a priori* account of the localisation question. Alluding to the phrenological scheme of localisation, he says: "Whoever calmly considers the question cannot long resist the conviction that different parts of the cerebrum must, *in some way or other*, subserve different kinds of mental action. Localisation of function is the law of all organisation whatever; and it would be marvellous were there here an exception."

"Either there is some arrangement, some organisation in the cerebrum, or there is none. If there is no organisation the cerebrum is a chaotic mass of fibres, incapable of performing any orderly action. If there is some organisation it must consist in that same "physiological division of labour" in which all organisation consists; and there is no division of labour, physiological or other, but what involves the concentration of special kinds of activity in special places."

"Among fundamental objections to this view, *i.e.*, the phrenologists," the first to be set down is that they are unwarranted in assuming *precise* demarcations of the faculties." ("Principles of Psychology," 2nd ed., vol. i., p. 573-4.)

¹ The Greek letters are not here used to denote subjective as contrasted with objective states.

cells I., II., III., and their susceptibility of undergoing augmentation or diminution of tension by impulses, *i.e.*, of undergoing alterations of excitability.¹ It is with this schema in mind that I am led to say, in somewhat figurative language, that "sensificatory stimuli affect us best when we meet them half-way by some action of our own, by an attitude of attention or by actual movements." In using such language I imagine a particular state of motion in I. causing in II. or in III. a state of increased excitability with regard to impressions homogeneous with previous impressions, of which the particular state of motion in I. is the material legacy.

§5—THE SENSE OF EFFORT.

I have on a previous occasion insisted upon the close connection between the family of sensations, individually denoted as sensations of movement, sensations of muscular contraction, muscular sense, sense of effort, and their after-effect, the sensation of fatigue. From the physiological point of departure the phenomena of effort open to objective study are neuro-muscular phenomena; but from the psychological standpoint the phenomena of effort extend over the far wider area of psychical experience, and are mainly, if not exclusively, approached by subjective study. I have said mainly, because, although in many cases the psychological presentation has been exclusively introspective and interpretative, yet in others it has included certain objective results of deliberate experiment—to give an instance—the psychical experience denominated "effort of attention" has been introspectively analysed, but what may be termed the phenomenal fringe of the subject, has been experimentally studied by psychophysical methods.

It was objected to my presentation of the matter that the general term effort was misused. Although I had not

¹ Everyone will naturally call to mind in this connection thought-reading tricks, and actual alterations of nutrition, brought about by the excessively focalised attention, which is the essential feature of the hypnotic state; the observations of Tarchanoff (*Pflüger's Archiv*, vol. xlv., p. 46) of local cutaneous electro-motive changes caused by attending to particular parts of the periphery, are also a physiological case in point.

specifically in view these forms of effort, I was alive to this use of the term; but I did not think it suitable to attempt to include them in the argument and I expressly limited that argument to the case of muscular effort. But so far from admitting that these more psychological forms of effort are to be dissociated from the most physiological form of effort, viz., muscular effort, I am disposed to insist upon the essential similarity of mechanism which runs through the whole range of effort experiences—sense of muscular effort, efforts of attention, of memory, of imagination, of will, of restraint. I can, however, do little more than offer an argument and state an opinion, appealing by the way to some few “experiments” (if experiments they may be called) which seem to indicate that an “effort of attention” is entitled to be spoken of as “nascent motor impulse.”

We may attend to something we are *feeling*, or to something we are *doing*, *i.e.*, we may apparently have a *sensory* or a *motor* or a mixed form of attention, this, however, is no valid distinction, for in attending to what we are *doing* it is evident that we are attending to the thing being done and felt as well as to the thing about to be done. So far then the phenomenon of attention is consequent upon sensificatory antecedents.

But it is not possible to have any great concentration of attention without some alteration of motivity—movement or arrest of movement—greater muscular tension or smaller muscular tension—and the so-called thought-readers illustrate this fact clearly enough. In attending to what we are feeling we always do something, *i.e.*, use certain muscles. The phenomenon of attention is an antecedent of motor consequences. A desire (*i.e.*, an imagined effect, *i.e.*, a remembered similar effect) is the antecedent and the consequent of movement. The psychological—I had almost said dialectical—dilemma, as to whether attention belongs entirely to motor neural processes or whether it is merely a form of sensation, does not seem to me a very serious one in terms of psychical phenomena.

Attention is “increased sensomotility,” at the same time

consequence of sensificatory antecedents, and antecedent of motor consequences. I use this word to denote that *one* property of a higher nerve-centre which has been improperly chopped into two properties, sensibility and motility of sensory and of motor centres. It is the homologue of the most general term excitability and of the most special term suggestibility. "Excitability," a name for the property of certain centres, motion with sensation. "Suggestibility," a name on the same pattern, from suggestion to conduct.

Let us review some cases to test the applicability of our definition—attention increased sensomotility. A chef d'orchestre picking out a weak instrument, a schoolmaster keeping an eye on a particular boy, a meditating monk or a howling dervish, a student in presence of a subject to be mastered, in short anybody doing anything, has his attention focalised upon his task in hand; we say that he concentrates his attention by voluntary effort; but if we imply that he originates the effort of will, we go beyond our tether.¹

It is most in accord with the principle "voluntary action

¹ I do not clearly understand Bastian's distinction ("On the neural processes underlying attention and volition") between the neural processes of attention and of volition, nor his conception of voluntary attention as "a compound or fusion of the two states volition and attention." The two names seem to me to denote *one* state or process or "alteration of sensomotility;" we have always diffuse sensomotility (states of attention and of inattention) and the plus state of sensomotility or "ad-tentive" state may explode in one volitional act or confine the explosion of other volitional acts; attention is nascent volition, volition is expressed attention; or otherwise, thought is nascent action. I cannot call to mind any example of purely sensory attention without motor concomitants; the indivisible double aspect of the process imposes itself upon me with such force that I am obliged to think of it under the compound word alteration of sensomotility. Not implying, however, that the process is to be regarded as compounded of sensory and motor factors, but to indicate that it exhibits subjective with objective symptoms—alterations of feeling with alterations of attitude—although itself a single and indivisible phenomenon. To the objection that we may have altered attention without altered movement, I would reply that such an apparent instance of pure sensory attention is a fallacy, and that an unseen motor consequence can always be made manifest as a visible motor consequence by intensification of attention without alteration of kind of attention.

To another possible objection that attention may have for its consequence not motion but arrest of motion, it is enough to reply that arrest of motion is as much a motor phenomenon as motion itself. If I attend to an object at the periphery of the field of vision, the cerebral process includes the production of an intensified fixation of the point of regard. If by an effort of imagination I reverse the position or relief of a transparent vessel, the cerebral process includes the production of an alteration in the diameter of

a disguised reflex" to refer attention, the psychological phenomenon, to increased sensomotivity, the physiological phenomenon. We can often enough in given cases discern the antecedent stimuli that may have elicited states of attention, but when we attempt to get behind the phenomena at the true cause of the state, we recognise that we are in presence of an ultimate property of cerebral living matter. We do not know.

We have, however, in the conditions and consequences of variations of attention, a field of exploration extensive enough to satisfy the most industrious aspirations to positive knowledge. Hypnotic phenomena, education, mental health and mental diet, in a word the influence of man upon man, are in the main determined through varying attention, varying sensomotility, varying influences by varying influences. And as a right theory is often the motive influence of right practice, so it becomes of great importance to acquire a right view of the physiological determinism of the phenomena of attention.

Much of our most valuable knowledge is analogical, one of the most valuable analogies with reference to attention, is afforded by retinal phenomena; the analogy is more than an analogy, for the retinal phenomena that we have to utilise are in reality retino-cerebral, so that a study of retinal

the pupil. Every antecedent has its consequences, every consequence has its antecedents, any phenomenon is a transverse section in the stream of change. Every centripetal impression has its motor consequences, every movement has its centripetal antecedents, any central neural phenomenon is a transverse section in the stream of neural change. We may call the stream past and future, centripetal and centrifugal, but the phenomenon itself is both or neither. I conceive the physiological phenomena underlying "emotion" as being similar in composition to those underlying attention, i.e., as sensomotile, and not purely sensory nor purely motor, nor a resultant of separable sensory and motor factors. We know subjectively that an emotion entails feeling as a resultant of centripetal impulses, objectively that it entails movement or arrest of movement, the resultant of centrifugal impulses. Emotions are feelings expressed by motions, and the motor element is even more obviously manifested in emotion than it is in attention. But of the process itself we cannot say in either case that it is either *only* sensory or *only* motor; objectively imagined an emotion is the result of an alteration of nerve-substance, which alteration is the consequence of centripetal impulses and the antecedent of centrifugal impulses. And it is no drawback to the second half of this view, that the expression of an emotion can be restrained, for in restrained emotions there is the centrifugal tension or tendency, and it may be, actual movements suppressive of a natural muscular expression.

focalisation and of retinal contrast is in large degree a study of cerebral focalisation and cerebral contrast, which are among the most important phenomena of attention. I am aiming at one particular group of considerations that can, I think, with advantage, be presented from a very general point of view in this connection, viz., contrast phenomena.

Stated in general terms:—the value of any given constant is estimated to be + or – according as its surroundings are – or + ; magnitudes are over or underestimated amid smaller or larger surrounding magnitudes, colours are intensified or weakened according as they are observed on similar or upon complementary¹ backgrounds; and psychologically any sensation is intensified or weakened according as it occurs upon a similar or a dissimilar field of attention. This law of contrast, simultaneous as well as successive, applies to the two portions of the field of attention which we have referred to as “distinct” and “indistinct”; and here again we may, with profit, represent by a ratio $\frac{\alpha}{\beta}$ the sensation resultant from its two component sensations α and β , α being the distinct area and β the indistinct area of attention. This resultant sensation will obviously vary with greater or smaller concentration of attention, according as the distinct component α forms a larger or smaller proportion of the total α and β . And now the law of contrast is evidently applicable; just as two antagonistic colour sensations mutually intensify each other, so do any two antagonistic masses of sensation; in this case the antagonists are the more intense distinct and the less intense indistinct portions of the total sensory field; the first diminishes the second, the second diminishes the first; in the hypnotic state the first annuls the second, and attention is at its maximum of concentration. Fixed attention is in fact the essential element of hypnosis of all degrees and however produced. In the normal state a person may be more awake to a particular kind of stimuli, less awake to all others; he is “pre-occupied,” and his pre-occupation may be trifling or moderate or considerable. In the hypnotic state it is excessive, the hypnotised person is over-sensitive to a

¹ Or antagonistic (Hering).

certain focalised portion of his environment, under-sensitive, or it may be impervious towards the whole unfocalized field.

In my previous paper (*Sense of Effort*, BRAIN, 1891), I offered as additional to the exclusively cerebral or spinal reflex view of movement, the following groups of considerations.

The initiation of action—Hitzig's image of movement, Munk's Gefühlsvorstellung or Bewegungsvorstellung, Ferrier's motor idea or impulse, Bastian's revival of kinæsthesis—is demonstrably attended with a consumption of force-producing material.¹ With this change manifested as diminished workability (motor fatigue) we have a sensation popularly termed fatigue (sensory fatigue), and I have urged above that the association of the two aspects is such as to forbid us to dissociate them; "motor" fatigue and "sensory" fatigue are one and the same thing, to which for want of a better word we may apply the term senso-motor fatigue. Moreover I have argued in my previous paper that the incidence of "fatigue" (which is the *result* of movement) may be taken as a guide to the incidence of "effort" (which is the *concomitant* of movement), and have concluded that "effort" and "fatigue" depend upon *changes at the centre and at the periphery*.¹

¹ This argument was the main motive of my paper. I find that James, who ostensibly pronounces himself in favour of the entirely peripheral origin of effort-sensation, commits himself to the position I take, viz., the distinction between peripheral and central factors.

"Effort of attention is thus the essential phenomenon of will. This volitional effort, pure and simple, must be carefully distinguished from the muscular effort with which it is usually confounded. The latter consists of all those peripheral feelings to which a muscular exertion may give rise ("Psychology," vol. ii. p. 562.)

Dr. Delabarre ("Mind," July, 1892, p. 383) objects that the fault of my reasoning lies in its major premise, which contains three unwarranted assumptions: (1) That the objective signs of exhaustion are always indicative of a previous expenditure of energy in the same parts. (2) That a subjective sense of fatigue is indicative of a corresponding previous effort and sense of effort in the same parts. (3) That objective signs of exhaustion are indicative of a subjective sense of fatigue, and objective signs of effort of a subjective sense of effort, localised in the same parts.

I really cannot discern these three assumptions in my major premise, which was clearly stated and printed as follows: *The material changes subsequent to action, are co-extensive in the motor organ with the material*

I admitted the peripheral sensificatory factor as matter of common knowledge, but offered evidence against the view that it is the sole factor of the sensations of motion to the exclusion of the concomitants of central emission. I also admitted as matter of common knowledge

changes accompanying action. The three assumptions are introduced by Dr. Delabarre rather than implied by me, and his objections to them are not objections to the premise itself.

This is a sufficient answer to the criticism, but I wish to clear the issue rather than to retrench myself behind a logical defence, and will therefore reply as follows to Dr. Delabarre's three assumptions:

(1) That Mosso's experiments (alluded to on p. 368 of this paper) do not appear to me to be at variance with the general truism that effects and after effects must have the same loci of incidence. The truism applies to the area of diffusion as well as to the focus of maximum at which the expenditure of force-producing material and the production of fatigue-products is presumed to occur, and experiments do not allow us to hold that the fatigue-products thrown into circulation can ever produce an effect at all approaching in intensity the local effect at the place of origin of such fatigue-products. (2) Delabarre's objection that the second assumption "is negated by the fact that the feeling of fatigue located in the eyelid is not at all commensurate with the amount of work this organ has accomplished, and is sometimes excessively strong in the morning, after complete repose of the muscles involved," introduces terms of comparison from which I carefully refrained, indicating as my reason that the introspective comparison of sensations is particularly liable to fallacy in this connection. I had and have nothing to say about any correspondence of magnitude between fatigue and effort, being well aware of the fact that the apparent magnitude of feeling varies with associated and contrasting circumstances. To trace any correspondence between two subjective magnitudes it is necessary to secure identity of circumstances and conditions. (3) The third assumption that objective signs of exhaustion may be utilised as indicators to a subjective sense of fatigue, although not contained in my major premise, is however made in the inference from premises to conclusion. So far from ignoring it and letting it smuggle itself unperceived into the argument, I repeatedly pointed out this assumption as the weak link in my chain of proof (*Sense of Effort*, p. 191, 192, 433). It was, so far as I could see, the one logical breach that might be employed by an adverse critic to break the proof, and I was careful myself to indicate that breach; I claimed no more than the *highly probable* parallelism between

$$\frac{\text{sense of effort}}{\text{kinetic effect}} \quad \text{and} \quad \frac{\text{sense of fatigue}}{\text{loss of kinetic power}}$$

and therefore the highly probable character of the conclusion at the end of the inference. But I urge in defence of this breach three considerations: (1) That the weak link in my chain is no sign of the strength of any rival chain. (2) That "to take the demonstrable signs of material changes of objective physiological fatigue as indicators to the undemonstrable material changes of subjective psychological fatigue, keeps us closer to phenomena than the hitherto universally adopted procedure of drawing conclusions from the suggestions afforded by simple introspection." (3) That, although probability is not proof, yet proof is no more than overwhelming probability, so that in the opinion of some the high probability of my conclusion may be held as amounting to proof.

Dr. Delabarre himself grants as much, and rather more, in some introductory sentences that may be compressed as follows: "Granting that objective

that the various components of the sensificatory factor arising from skin, joints, tendons, and possibly muscle itself, must be perceived centrally through afferent channels. I see no reason for adopting the theory of Lewes, that our knowledge of motor change can be derived from centripetal impulses along the efferent tract.

signs of exhaustion are indicative of a subjective sense of fatigue, and granting that the exercise of central energy is indicative of a sense of effort localised where the effort takes place, the central sense of motor innervation is proven." And although it may be psychologically allowable to say that "we know without elaborate experiment that the central organ exerts energy," I, for my part, prefer to this *a priori* and indefinite premise, observed physiological data, i.e., objective and measurable signs of central expenditure.

Professor G. E. Müller (*Zeitsch. f. Psychologie, u.s.w.*, 1892, p. 122), also adversely criticises my paper on the "Sense of Effort." The following sentence sums up his criticism:—"Verfasser schliesst auf die Entstehungsweise der normalen Empfindungen des Muskelsinnes ohne Berechtigung aus der Entstehungsweise der Ermüdungsempfindungen. Auf letztere schliesst er ohne Berechtigung aus der Entstehungsweise der objectiven Ermüdungserscheinungen, denen er ohne stichhaltige Beweisgründe einen vorwiegend zentralen Ursprung zuschreibt." That is to say, he rejects my data and my reasoning. As regards data, time and space do not allow me at present to review them in detail, but I shall take an early opportunity of doing so with reference to Prof. Müller's searching and therefore valuable criticism. I am well aware of their imperfection, and hope that other observers may assist in their experimental purification and prosecution. As regards reasoning I wish Prof. Müller had not gone outside the inference as formulated by me on p. 190. I urged a conditional or probable conclusion, and he represents me as claiming to have produced an unconditional proof. I had guarded myself from this claim as clearly as I was able, e.g., on pp. 190, 191, 192, 483. I admit that the sequence considered by Prof. Müller to be "ohne Berechtigung," does fairly and correctly represent the view I held and hold, but I cannot accept the omission of the qualifications that I recognized to be necessary. The main object of my paper was to urge the examination of this complicated matter with the neuro-physical change (objective) rather than the psychical perception (subjective), as the point of departure. Whereas psychologically we have to deal with four (or more) distinguishable kinds of phenomena produced in voluntary movement, physiologically we have to deal with one kind of phenomenon, i.e., the material (motificatory and sensificatory) change, and upon a balance of evidence and of probability to form an opinion as to the organic distribution of that change. Starting from the concept of a single neuro-physical change with kinesis as its objective expression, aesthesis as its subjective concomitant, I examine the kinetic power at the centre and at the periphery as the accessible objective indicator to the neuro-physical change producing kinesis and aesthesis. I do not see reason to admit that kinesis and aesthesis are produced by separate neuro-physical changes, still less to admit that qualitatively distinct aestheses in the same neural segment by the same cause (e.g., sense of movement and sense of fatigue) are produced by separate neuro-physical changes.

The hypothesis serving as my guide may be put thus:—

- I. Neuro-physical change occurs with voluntary motion, (1) in the cortex, (2) in the spinal cord, (3) in the end-plates, (4) in the muscle, (5) in the tendons, articulations and skin. 1 and 2 are central, 3, 4 and 5 are peripheral. [For reasons given in my previous paper (p. 181) I exclude nerve-fibres from consideration. Nor is it necessary in the present connection to consider a sixth component, viz., spinal and cortical

I do not share this view because I see no evidence to support it. Indeed, the supposition has appeared to me to be so purely gratuitous that I did not even mention it in my previous paper.

changes consequent upon 3, 4 and 5; these secondary central changes are admitted by every one, and to include them in the present field of thought might possibly distract attention from the point at issue, viz., the existence of primary central changes; whether such "primary" changes are truly primary, i.e., uncaused, or resultant from antecedent centripetal impulses is another question that has already been considered in this paper.]

- II. The sensation of motion arises from neuro-physical change in one, some, or all of these situations.
- III. Neuro-physical change in consequence of voluntary motion, having as its objective sign physiological or "objective" fatigue, occurs in 1, 2, 3, 4 and 5.
- IV. The sensation of fatigue arises from neuro-physical change in one, some, or all of these situations.

I find evidence (criticised and rejected by Müller) that objective fatigue is central and peripheral, I infer that the neuro-physical change, with motion and after motion, is central and peripheral. If the sensation of fatigue is the subjective concomitant of that neuro-physical change, then subjective fatigue has central and peripheral sensificatory factors. If the sensation of movement is the subjective concomitant of the neuro-physical change, then the sensation with movement has central and peripheral sensificatory factors.

As far as I can see, the acceptance or rejection of my reasoning from the phenomena of objective fatigue to the distribution of its neuro-physical basis, and from the latter to the distribution of the neuro-physical basis of objective movement, and of the sensations of fatigue and of movement, must in last resort depend upon a preference between the following conceivable alternatives. The objective movement and the subjective sensation of that movement depend upon the disintegration of *one* neuro-physical substance S. M. or of two distinct neuro-physical substances S and M. The two alternative con-

FIG. 3.

ceptions may be symbolised by the (a) and (c) in fig. 3. An intermediate alternative in which all neuro-physical matter is regarded as sensificatory, but only some such matter as also motor (some being only sensificatory) may be symbolised by (b) in fig. 8. I regard concept (a) as the most probable, concept (c) as the least probable. Concept (b) which represents that material other than inorganic may contribute to aësthesia, is in this connection nearer to alternative (a) than to alternative (c), if we recognise that every sensificatory change influences centrifugal tension. Subjective fatigue and the sensation of movement depend upon the disintegration of *one* neuro-physical substance or of *two* distinct neuro-physical substances. I regard the former as the more probable alternative. "*Neither more, nor more onerous, causes are to be assumed than are necessary to account for the phenomena.*"—(Hamilton.)

Dr. Bastian invokes Gotch and Horsley against it in the following terms:

"The lack of sensibility accompanying the action of motor centres has now been experimentally demonstrated by Gotch and Horsley."

I do not know whether Dr. Bastian has fully appreciated the gaps and pitfalls between Gotch and Horsley's data, and the conclusion which he draws from them. But in so far as he is a disbeliever in a retropulsive wave along an efferent tract he is, in my opinion, justified. The Bell-Majendie experiments on nerve-roots prove this point, by demonstrating that it is not possible (in the absence of recurrent sensory fibres) to obtain a reflex effect by stimulation of the central end of an anterior root, or by stimulation of a mixed nerve after section of the posterior roots.

Gotch and Horsley,¹ although they have not gone so far as to claim to have "experimentally demonstrated the lack of sensibility accompanying the action of motor centres," have indeed drawn a conclusion which does not seem to me to depend upon the data from which they draw them, but upon other and far more assured and familiar data. They consider that:

"As the electrical method affords the first opportunity of an experimental contribution to this subject, it is interesting to find how strongly its application bears out Bastian's position. In this chapter it appears probable that the kinetogenetic portion of the centre is the afferent [*i.e.*, that in which the potential energy is converted into kinetic] side of it, and the more especially when it is seen how readily the centre discharges into the afferent nerve channels (*i.e.*, actually 'backwards' as compared to the course of ordinary afferent impression)."

"The method has further enabled us to ascertain what connections and facilities for conduction the efferent, or so-called motor side of a nerve centre possesses, and instead of finding, as might have been expected from the ordinarily expressed beliefs on this subject, that we have to deal with a source of energy that was readily aroused, and freely connected with its neighbours, we found, to our surprise, that it afforded nothing of

¹ Croonian Lecture. *Phil. Trans. R.S.*, 1891. Summary of Cap. xi., p. 509).

the sort, and that its power of conducting impulses centripetally was practically *nil*.

"Curiously enough, this last point was foreseen also by James, who in his celebrated work on the 'Feeling of Effort,' 1880, while endorsing the views of Bastian, says that 'the electrodes of the physiologist,' if applied to the central end of the anterior root, would not arouse any 'sentient,' i.e., afferent impulse in the cord. We are happy to find that our experimental results, unusual though they were, are, nevertheless, in close agreement with the deductions of the logical method of these distinguished writers."

I do not dispute either the electrical data, or the conclusion that impulses do not reach the brain from muscle by running up efferent tracts, but I do not think that the latter is borne upon the former; to my mind the facts indicate an altogether different kind of conclusion, and the conclusion is established from far more direct and simple data. As regards the second point, it is enough to remark that the absence of movement on stimulation of the central end of an anterior root—forming part of the ordinary class demonstration of Müller's experiment on the nerve roots of the frog, and of Majendie's classical demonstration of the same fact on mammalia—proves it conclusively. As regards the first point, while it is possible that the facts may be congruent with this established truth, it is equally possible that they may illustrate quite a different matter; it is well known that the negative variation in nerve is elicited in far greater amount by electrical stimuli of nerve than by either functional or mechanical stimulation;¹ doubt has been felt and expressed whether the "negative variation" and the nerve-impulse are co-extensive phenomena;² the large variations up and down the uninterrupted fibres of the posterior roots and columns, as compared with the small variations down the anterior roots and the absent variations up the cord

¹ DU BOIS-REYMOND. "Thierische Elektrizität," vol. ii., chap. 6, p. 507. Electrical excitation gave a deflection of 40°, whereas non-electrical excitation gave only 1° to 8°, and the powerful functional tetanisation produced by strychnia gave 1° to 8° or even 4° deflection.

² KUHN. "Zeitschr. f. Biol.," xxii., p. 839, 844 *et seq.*, in admitting "double conduction" in nerve, bases his conclusion upon the results of his gracilis experiment rather than upon the evidence by negative variation.

above the anterior cornual cells, suggest that they are dealing with an electrical phenomenon depending (at least in part) upon the physical continuity or discontinuity of fibres, rather than with any pure and simple electrical *measure* of the magnitude of functional disturbance in the nerve. That this last assumption (*i.e.*, measurement of functional impulse by its electrical indication, or properly speaking by galvanometric deviations) is improbable, is further indicated by some of the authors' own results. They obtained large effects down the afferent tract with centrifugal discharge, and diminished effects down the efferent root; in most cases this centrifugal discharge was by electrical excitation, but in a few cases it was functional discharge; these few cases are the real key to the validity or the reverse of Gotch and Horsley's contention that nerve-impulses pass freely down (as well as up) the afferent tract and root, diminished down the efferent root from the cord, and not at all up the efferent root to the cord. But even if the contention be admitted as valid, it is difficult to see what additional cogency as regards the kinæsthesia doctrine would be added to the far more cogent facts (1) as to the afferent root, the known failure of response by excitation of its central end, and (2) as to the posterior root, the known passage of centripetal impulses along it. The non-existence of an effective retropulsive wave *up* the efferent tract is proved by (1). The existence of a retropulsive wave *down* the afferent tract (as urged by Gotch and Horsley), interesting as it may be, would add no strength to our knowledge of the centripetal transmission of peripheral sensificatory stimuli.¹

It is difficult to understand how any logical thinker can be led to translate this maze of inference into "an experimental demonstration of the lack of sensibility accompany-

¹ If more proof were needed of the non-existence of a retropulsive wave from muscle up motor nerve we might adduce the observations of Kühne, who altogether failed to find any evidence of its existence in experiments made *ad hoc* upon the frog's gracilis ("Zeitschr. f. Biologie," xxii. p. 833).

Du Bois-Reymond, discussing the evidence of double conduction afforded by the negative variation, remarks that the absence of reflex response to stimulation of the central end of a motor conductor is intelligible on the assumption that the central stations of motor nerves are inexcitable by the retropulsive wave ("Thierische Elektrizität, ii., p. 574).

ing the action of motor centres." It will be very much to be regretted if this verbally clear but phenomenally obscure dictum should be uncritically quoted as a "datum experimentiæ;" I have therefore done my best to disentangle the argument and to make plain its possibilities and probabilities of fallacy.

This has been a side issue; so far from having led into it, I deliberately ignored it in my first paper after the most careful consideration; but as it has been raised in authoritative quarters (in a presidential address to the Neurological Society, and in a Croonian Lecture to the Royal Society), it had to be entered upon.¹

I have also advanced what I consider to be a very important extension of view with regard to the neuromuscular government of the body, viz., that in addition to

¹Bastian, until quite recently (*BRAIN*, 1887, p. 44) quoted Wundt and Bain as the exponents of the cerebral emission theory. I pointed out (*BRAIN*, 1891, p. 436) the injustice of attributing to Wundt an opinion expressly repudiated by him, and signified adhesion to Wundt's real opinion, as contrasted with Bain's. And Bastian now (*BRAIN*, 1892, p. 2) makes a similar distinction between Bain and Wundt. This is the passage upon which I had relied as regards Bain's position:—

"As the nerves supplied to the muscles are principally motor nerves, by which the muscular movements are stimulated from the brain and nerve-centres, our safest assumption is that the sensibility accompanying muscular movement coincides with the outgoing stream of nervous energy, and does not, as in the case of pure sensation, result from an influence passing inwards, by ingoing or sensitive nerves. It is known that filaments of this class are distributed to the muscular tissue, along with the motor filaments; and it is reasonable to suppose that by means of them the *organic* states of muscle affect the mind. It does not follow that the characteristic feeling of exerted force should arise by an inward transmission through the sensitive filaments; on the contrary, we are bound to presume that this is the concomitant of the outgoing current, by which the muscles are stimulated to act. No other hypothesis is adequate to represent the total opposition of nature between states of energy put forth, and states of passive stimulation."—Bain, "Senses and Intellect," 2nd ed., 1864, p. 92.

But I now learn from Prof. Bain that he also admits the participation of a peripheral factor.

So that, after all, it is permissible to bracket Bain and Wundt, not as maintaining the exclusively central source of the sense of effort, but as holding it to be of mixed origin; the two names need not be separated. I have hopes that it may eventually be allowable to add Bastian's name to these; for the present, however, this addition is not permissible, he still holds to the exclusively peripheral origin of the sensifactory changes with *muscular* effort, and distinguishes this form of effort as differing in kind from effort of attention, volition, &c., of which he presumably admits a *central* material substratum. Of course, by saying that Bastian holds to the exclusively peripheral origin, &c., I do not mean to imply that he ignores the truism that sensation is *consummated* centrally. But for the present he holds that the sensation of movement (kinæsthesia) is *initiated* solely at the periphery, whereas Wundt (and Bain) hold that it is initiated at the centre, and at the periphery.

(1) central initiation,¹ (2) reflex response, we must admit (3) direct response, in the perfect quantitative co-ordination of muscular movements.

This third item was very cursorily alluded to in my previous paper in a note to p. 244, and opportunity may therefore be taken to more adequately develop it.

The view that reflex spinal action plays a very large part in the government of muscular action is very generally accepted, and the extent to which spinal control is held to be effective has of late years been greatly augmented. Among published papers I may quote Exner's² and Chauveau's.³ I may add that in 1880, when the question occupied my thoughts very largely, I was strongly inclined to the view that reflex muscular action responsive to the extension of muscle or tendon or ligament must be a chief factor in the co-ordination of muscular movements. I still admit this view, but not to the extent that I might have done, nor as unreservedly as is done by Exner, Chauveau and many others, owing (1) to the fact which I demonstrated in 1880,⁴ and (2) to the important principle long ago discovered by Heidenhain,⁵ but even now scarcely appreciated at its full value by neurologists or by physiologists.

(1) The positive item which withheld me from supposing that muscular co-ordination is altogether by reflex action from the cerebro-spinal axis, was the phenomenon of "tendon-reflex," which is demonstrably not a reflex from the cord, but a direct phenomenon of which a continuous reflex tonus is a necessary condition. The phenomenon being recognised as a direct response to an extensile excitation, we cannot fail to recognise the direct excitability of muscle itself by sudden tension, as well as its indirect excitability

¹ This word might be taken in an active or in a passive sense, to imply a spontaneous or a provoked initiation of nerve-impulses. It is here taken in the second sense in accordance with the physiological postulate: voluntary action = a species of reflex action. But see also the concluding paragraph of this paper.

² EXNER. Ueber Sensomobilität. *Pflüger's Archiv.*, xlviii., 1891, p. 592.

³ CHAUVEAU. On the Sensorimotor Nerve-circuit of Muscles. *BRAIN*, 1891, p. 145.

⁴ "On Muscular Spasms known as 'Tendon-Reflex.'" *BRAIN*, 1880, p. 179.

⁵ HEIDENHAIN. Mechanische Leistung, Wärmeentwicklung und Stoffumsatz bei der Muskelthätigkeit. *Leipzig*, 1864.

viâ a central controlling organ, as being contributory to a balance of power between muscles, variously stretched by each other's contraction, and as supplying a most rapid corrective of any sudden fault of balance.

And this direct responsibility being recognised, we cannot fail to see that all that is said in exposition of the reflex adjustments of position by reflex contraction responsive to tension, applies with additional force and even clearer "zweckmässigkeit" to direct adjustments of position by direct contraction responsive to tension.

The principle may be summed up in the short dictum—*ut tensio sic contractio*. As has been said the visible contraction, in response to a direct extensile stimulus, requires, as a necessary condition, that subsensible degree of reflex action, which, for want of a better name, is called reflex tonus, and if the reflex arc is cut at any point the direct contraction fails. But the dictum "*ut tensio sic contractio*" may be discovered to apply also in some degree to muscle isolated from the nervous system.

(2) The principle of muscular action contained in Heidenhain's often quoted but little known monograph, published nearly thirty years ago, may be summed up in this double proposition:—*ut tensio, sic labor, sic calor*.

The proof that muscular work increases with muscular tension is contained—I had almost written concealed—in every text-book of physiology. Up to a certain limit an excised frog's muscle does more work in contraction the greater the extending load, and within a somewhat narrower limit the actual contraction or shortening increases with the load. Thus we have illustrated the dictum "*ut tensio sic contractio*" in the accurate sense of the word *contractio*, *i.e.*, amount of shortening, as well as the dictum "*ut tensio sic labor*." It is important to recognise now that in our first use of the dictum "*ut tensio sic contractio*" we have not used the word in its accurate sense but have included in it connotations which properly belong to the word "*labor*." When we summed up the principle of direct response to extension in the words "*ut tensio sic contractio*," this last word implied amount of work as well as degree of shortening in

the muscular contraction, and the proposition "*ut tensio sic labor*," which as we have seen applies to excised muscle, might therefore have been substituted for it.

Heidenhain's observations show further that the increased work with increased load does not occur at the expense of the other form of actual energy manifested by contracting muscle, viz., heat, but that with increased load there is increased heat as well as increased work. *Ut tensio sic calor* as well as *ut tensio sic labor*.

These relations teach us that, other conditions being constant, but with variable load or tension or resistance to contraction, the amount of energy manifested by isolated muscle is variable. The strength of contraction (*labor*) varies with the mechanical task to be accomplished; more weight or more tension or more resistance to contraction elicits a larger production of muscular energy in the forms of work and of heat, the isolated muscle evolves energy in proportion to demand by tension.

This is the second feature to which I have referred to above as the "direct responsibility" of muscle, a property which in my opinion plays an important part in the accomplishment of exact movements appropriate to varying states of tension.¹

§6—APPERCEPTION.

We have already had occasion to refer to Wundt's theory of Apperception (p. 372). It is indeed impossible to make any progress in the analysis of cortical function from either a physiological or a psychological point of departure without

¹ The principle appears to me to be of such importance that we ought to be particularly careful not to admit it uncritically, but on the contrary to expressly recognise the limited extension of the heat-data that form its chief basis. Heidenhain's conclusion is exclusively based upon experiments with excised frog's muscle; attempts to verify the principle with mammalian muscle made in the Leipsig laboratory by Lukjanow (*du Bois-Reymond's Archiv.*, 1886, suppl. p. 117) yielded in this respect a perfectly negative result, the principal *ut tensio sic calor* could not be experimentally verified. On the other hand it is one of the principal conclusions of Chauveau's researches on human muscle (*Le travail musculaire*, Paris, 1891), but in this case a critic acquainted with the subject might easily find technical loopholes for objection. Another point of deficiency that can only be filled in by thermo-electric experiments of extreme delicacy is the failure of proof of any difference in amount of heat evolved by the extension of living *versus* dead muscle.

involving oneself in a consideration of that theory. The nature of the problems of which it is offered as an explanation, is such that the explanation itself requires much further explanation, and that it has been read or imagined as conveying a meaning diametrically opposed to the whole spirit and tendency of Wundt's teaching. No one has abjured faculty fetishism in psychology more plainly and explicitly than Wundt, yet we find him repeatedly pointed at as a faculty-monger and triumphantly refuted.

Of recent years this misrepresentation has been particularly noticeable in England, and judging from a review of Münsterberg's *Beiträge* by Dr. Titchener, the fashion seems to have been set in Germany.¹ Yet, making the most liberal allowances for the intrinsic difficulty of the subject, and for the natural preference by a hurried reader for second-hand allusions written in English to the original expositions written in German, the travesty remains an amazing one to anyone acquainted with Wundt's writings. To go back no further than the beginning of this year, we may refer to the last presidential address to the Neurological Society as an instance in point. Wundt's "so-called faculty of apperception" (p. 20) is there alluded to and refuted, but to the reader acquainted with Wundt's writings there is hardly any trace of Wundt in the so-called Wundtian position there presented and demolished. Indeed it is only too clear that this authoritative contradiction, which may not unreasonably be expected to influence opinion in this country, is based upon the case as presented by Wundt's adverse commentators rather than upon that contained in Wundt's own declarations, and the Wundtian position carried by Dr. Bastian seems to me to represent not what Wundt thinks, but (on p. 20) what Croom Robertson thinks Münsterberg thinks Wundt thinks, and (on p. 15) what Bain thinks Wundt thinks. The bald faculty puppet presented to us

¹ "Dr. Münsterberg's polemical aim is the demolition of a *soi-disant* 'theory' or 'metaphysic' of Apperception. Like pulpit atheism, this theory is a thing of straw, which the author sets up and knocks down with a wearisome monotony. The serious part of the matter is that he has fathered the 'metaphysic' upon Wundt." (*Mind*, 1891, p. 530.)

by Bain¹ and by Bastian as Wundt's child, was long ago torn to pieces by Wundt himself² (*Phys. Psych.* 1886, p. 354). This much is simple matter of historical fact to be verified or refuted by reference to Wundt's classical work.

Wundt's position appears to me to be in the main similar to that of Munk³ (α and β cortex function) (with the difference already alluded to in the footnote on p. 333) and of Jackson (high-level and middle-level cortex function) and to that which is presented in § 4 of this paper. Indeed my own views have been in large part formed by reflecting upon a hint originally derived from Wundt, *i.e.*, the notion of neural synthesis. But of this I have already spoken. I hasten to add that in giving this reading of Wundt's attitude *re* apperception, I do not pretend to make Wundt responsible. The misconceptions liable to arise in any transfer of abstract thought from mind to mind, have multiplied around these words—will, attention, thought, apperception—and the association of contradictory connotations round the same words in different minds, have almost destroyed their value as middle terms between mind and mind.⁴ Yet I feel obliged to offer my interpretation for whatever it may be worth; it

¹ Wundt. (*Phys. Psych.*, 3rd ed. 1887. Vol. ii., p. 389, note 7). "Den seltsamen Bericht, den Alex. Bain seinen englischen Lesern von dem sonstigen Inhalt meiner psychologischen und metaphysischen Ansichten gibt, kann ich hier mit Stillschweigen übergehen. Er beweist, dass dieser ausgezeichnete Gelehrte seine Kenntniss meiner Psychologie nicht dieser selbst, sondern dem Berichter irgend eines Lesers verdankt, der die Lectüre meines Buches mit seinen Reminiscenzen an irgend einen Cambridger Theologen des 17 Jahrhunderts verwechselt zu haben scheint."

² Wie hat sich doch die neuere Psychologie mit der Ueberwindung auch der letzten Spuren der obsoleten Vermögenstheorie gebrüstet! (Wundt, *Phil. Stud.*, vi., p. 17). The term "faculty," in consequence of its adoption by the phrenological inventors, has acquired connotations that have altogether unfitted it for employment except as a polemical missile. Otherwise it often presents itself as a most suitable term to denote particular psychological properties; "memory" as a psychological faculty of a special cerebral organ is a physiological non-sense; sight, smell, as physiological faculties or susceptibilities to particular forms of excitation (*i.e.*, in a passive sense with the connotations of the French word "faculté," as implying a potentiality, *e.g.*, *faculté d'agir*) would be in many cases a suitable and most convenient term, if only the word had not been debauched. But as it is, the word is only fit to be used as an abusive label for psychological vagaries. Perhaps it may become disinfected by time.

³ As regards historical order the two names should, of course, be transposed.

⁴ A. Marty remarks incidentally that "es bedarf natürlich einer ganzen Reihe von Verwechslungen und Aequivocationen um von hier aus [*i.e.*, Apperception = das Bemerken] die Apperception für identisch mit dem Willen zu erklären. (*Zsch. für Psychol. und Physiol. der Sinnesorgane*, 1892, p. 140).

is at any rate the resultant of a strenuous "effort of apperception" excited by Wundt's own writings.

The points of *opinion* aroused in me by the careful perusal of Wundt's exposition (that in the 3rd edition of his *Phys. Psychol.*, 1887, as well as that in the 2nd edition (French trans., 1886), are :

1st. That he altogether rejects the faculty theory of cerebral activity.

2nd. That "apperception" is no more than his label to denote high-level neural synthesis.

3rd. That Wundt's doctrine of apperception is in no opposition to the associationist doctrine of the English school (the two Mills, Bain, Spencer), but fully inclusive of that doctrine.

4th. That Wundt's frequent indentification of "Apperception" and "Will" illustrates the impossibility of dividing the supreme central process into sensory and motor. "Apperception" (the term with centripetal connotation) and "Will" (the term with centrifugal connotation), are thus distinct terms denoting one indivisible senso-motor process of "Thought."

But this is only my opinion, and I am open to correction in this somewhat obscure and dubious matter. All that I venture to assert positively is that Wundt has been misrepresented to English learning by the paraphrases of Bain and of Bastian, precisely as Kant was misrepresented to a former generation by the careless eloquence of Coleridge. The Kantian faculty puppet was fathered by Coleridge, not by Kant; the Wundtian faculty puppet now slipping into English literature, is equally illegitimate.

It is not possible to describe the highly abstract bearings of the Wundtian doctrine of "apperception" by any short paraphrase, or by quotations extracted from their context. It is not possible in this case to get anywhere near the author's meaning without careful study of his own writings. With the express reservation that short quotation is in this case insufficient, I will select some passages in which Wundt's meaning seems most to come to a point, although point apart from shaft is not a very convincing instrument.

The last two references taken from a more recent paper, are given to indicate what indeed may be found indicated at greater length in the *Physiologische Psychologie*, viz., the great extension given by Wundt to the term "Will," and his view that reflex action springs therefrom, and not *vice versa*—a view shared by many leading psychologists, among others by Delbœuf, and—taking the spontaneity doctrine in its broad sense—by Bain.

"Den Eintritt einer Vorstellung in das innere Blickfeld wollen wir die Perception, ihren Eintritt in den Blickpunkt die Apperception nennen."—Vol. II. p. 236.

"Weiterhin muss aber sogar die Apperception als der primitive Willensact angesehen werden, der bei den äusseren Willenshandlungen stets vorausgesetzt wird. Bedingung für die Ausführung einer Willensbewegung ist die Apperception der Vorstellung dieser Bewegung."—Vol. II. p. 243-4.

"Ein wesentlicher Theil der Schwierigkeiten, welche zu jener Annahme einer Entwicklung des Willens aus den Vorstellungen geführt haben, verschwindet sofort, wenn man die Apperception als die primitive Willensthätigkeit anerkennt. Von einer Zeit der Willenslatenz, in der sich erst die Vorstellungen, welche eine Beherrschung der äussern Bewegung möglich machten, im Bewusstsein ansammeln müssten, kann dann an und für sich nicht mehr die Rede sein. Die innere Willensthätigkeit ist von Anfang an mit dem Bewusstsein gegeben, da es ein Bewusstsein ohne Apperception nicht gibt, und die äussere Handlung erscheint als eine Bethätigung des Willens, welche von der inneren Handlung der Apperception nur in ihren Folgen, nicht aber in ihrer unmittelbaren psychologischen Beschaffenheit verschieden ist. Als Phänomen des Bewusstseins betrachtet besteht nämlich die äussere Willenshandlung in der Apperception einer Bewegungsvorstellung. Die wirklich erfolgende Bewegung und die daraus entspringende weitere Wirkung auf Bewusstsein und Apperception ist erst ein secundärer Erfolg, welcher nicht mehr ausschliesslich von unserm Willen abhängt: die Apperception der Bewegungsvorstellung oder der Willensentschluss kann erfolgen, ohne dass die Bewegung eintritt, sobald der Zusammenhang der physischen Werkzeuge, die bei der Bewegung zusammenwirken, in irgend einer Weise gestört ist."—Vol. II. p. 470.

"Da aber weiterhin die Contraction des Protoplasma auf den späteren Entwicklungsstufen deutlich den Character eines psychophysischen Vorganges an sich trägt, d. h. einer physiologischen

Leistung, welche zugleich von psychischen Vorgängen begleitet ist, so verlangt der Grundsatz der Continuität aller Entwicklung, dass dieser Character auch schon jenem primitiven Spaltungs—und Contractions vorgang nicht fehle: er wird in diesem Sinne als ein einfacher, von Empfindung und Gefühl, eingeleiteter und begleiteter Willensact zu deuten sein" (*Phil. Stud.*, 1889, p. 339).

"Jede Uebung besteht in der Mechanisirung ursprünglich mit Bewusstsein geübter Willenshandlungen" (*Phil. Stud.*, 1889, p. 378).

And the senso-motor view seems to me very explicitly set forth in the page on which these sentences occur:—

"In jedem Gefühl ist eine Willensrichtung, in jedem Wollen eine Gefühlswirkung enthalten . . . das Gefühl ist das erste Stadium eines jeden Willensactes. . . ." (*Phil. Stud.*, 1890, p. 376).

I cannot pretend to have fully mastered Wundt's meaning; his treatment of apperception as the determinant of Will, and in some places his apparent use of the two terms as inclusive of each other, and above all his presentation of will as the fundamental property of living matter, are depths of doctrine to which I cannot claim to have penetrated. My reading of Wundt's adoption of "Will" as the fundamental protoplasmic neurility, is that it gives expression to the fact that the functions of living things in their lowest, as in their highest manifestations, are not *finally* explained by reference to passive properties, that "excitability" and "association"—the last words of physiology and of psychology—are not the last words of philosophy. He appears to prefer to denote his foundation concept by the active term WILL rather than by the passive term EXCITABILITY. To me the choice seems fundamentally indifferent, and that the same dead end to positive knowledge has been reached whichever term we adopt. The truth that "it is the spirit that quickeneth" seems to me as clearly apparent beyond "*excitability*" as it is in "*will*," and it is merely a matter of verbal convenience to us as students of phenomenal determinism, whether we adopt "Excitability" as our foundation concept, and say that voluntary movement, objectively studied, is reflex movement; or whether we take "Will" as our foundation concept, and say that amoeboid

movement, subjectively idealised, is voluntary movement. The inspiration of all *living* matter is equally manifest to us, whether we look through man to protoplasm and see in amoeboid movement the first signs of "will," or through protoplasm to man and see in conduct the highest expressions of "excitability."