

**On the 'muscular sense' and on the psychology of thinking / by H. Charlton Bastian.**

**Contributors**

Bastian, H. Charlton.  
Royal College of Surgeons of England

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*With kind regards  
H.C.B.*

ON THE

'MUSCULAR SENSE' (2)

AND ON THE

PHYSIOLOGY OF THINKING.

BY

H. CHARLTON BASTIAN, M.A., M.D., F.R.S.

PROFESSOR OF PATHOLOGICAL ANATOMY IN UNIVERSITY COLLEGE;  
ASSISTANT PHYSICIAN TO UNIVERSITY COLLEGE HOSPITAL,  
AND TO THE NATIONAL HOSPITAL FOR THE PARALYSED AND EPILEPTIC.

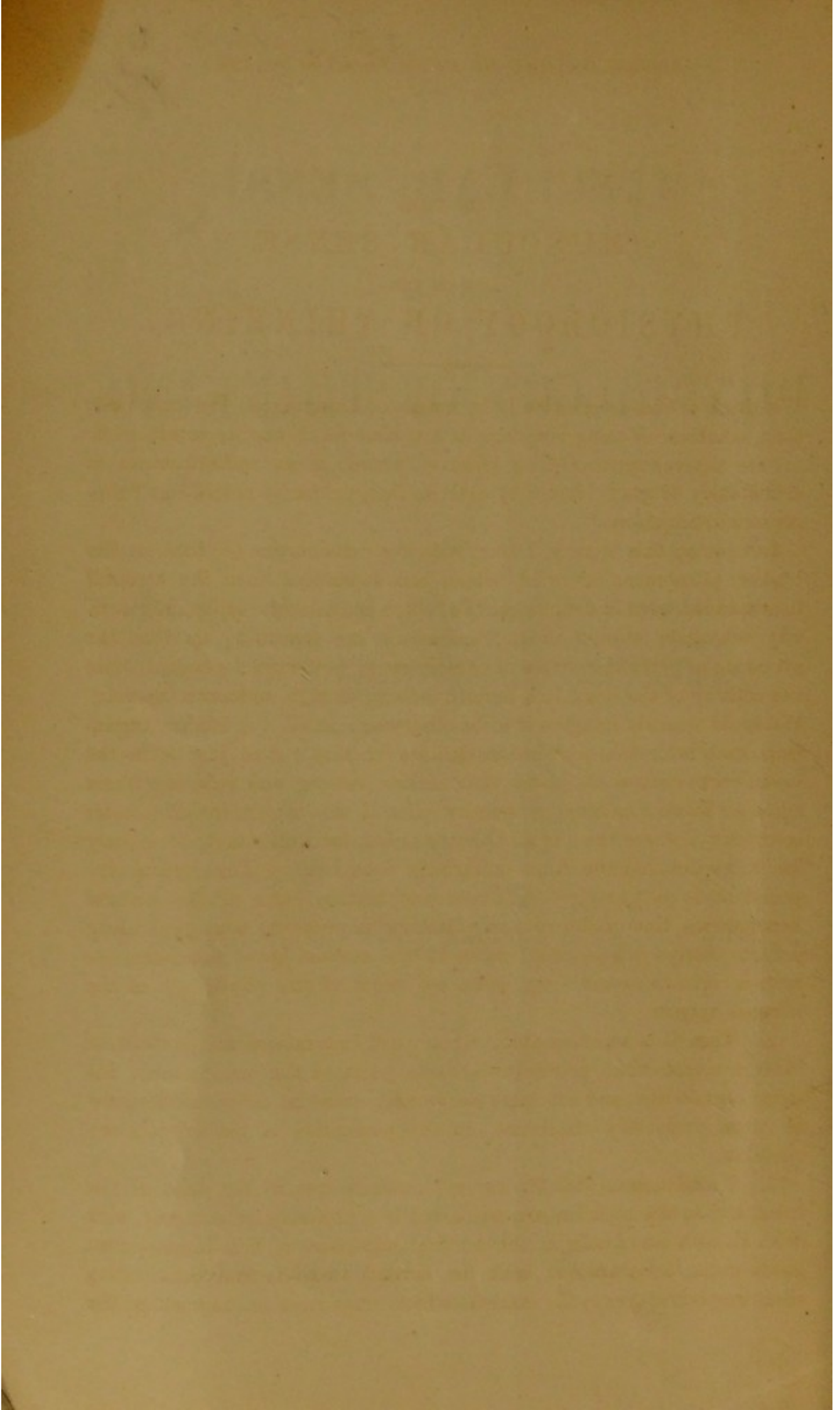


*(Containing a reply to Professor Bain's Note in the Fortnightly Review for April 1869.)*

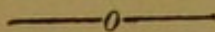
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ON THE  
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WE think or reason principally by means of Language. We may ask, then, whether, in using language as a vehicle for thought, words recur or are revived primarily as ideas of sound, or as remembrances of articulatory efforts? Are they ever, in fact, primarily revived as "suppressed articulations."

In opening this inquiry, I start with the presumption (1) that, in the higher phenomena of mind, which are dependent upon the cerebral hemispheres, certain definite parts of these are always called into activity whenever similar mental operations are repeated; so that the evolution of particular states of consciousness is always dependent upon the activity of the brain in a certain definite though unknown manner. We could scarcely imagine it to be otherwise: in all the higher organisms, each individual part has its definite function. And just as in the lower nerve-centres we know that certain issuing and entering fibres transmit definite motory or sensory stimuli, so, in centres of greater complexity, where there is all the more need for uniformity, it is only fair to expect that the same uniformity does exist. That optical impressions do pass along definite routes to certain parts of the cerebral hemispheres, that auditory and olfactory impressions also pass along definite though independent routes to the cortical grey substance, we have a right to expect, from what we know of the physiology of the nervous system.

2. Then it is assumed that, when past impressions are revived as ideas or recollections, precisely the same parts of the hemispheres, the same nerve-fibres, and the same nerve-cells, must be called into activity as were previously concerned in the perception of the original impression.

3. I also assume that the several sense-centres, at the base of the brain and in the medulla, are connected in a perfectly definite way, each with its own set of cells in the cortical substance of the hemispheres; these cells, in connexion with the several sense-centres, constituting their respective *perceptive centres*, which may exist in regions of the

hemispheres either distinct from one another, or which may be variously interblended. In the perceptive centres, the primary impressions made upon the organs of sense are converted into 'perceptions' proper; that is to say, they receive their intellectual elaboration, and this elaboration implies an intimate cell and fibre communication between each perceptive centre and every other perceptive centre; since one of the principal features of a perceptive act is, that it tends to associate as it were into one state of consciousness much of the knowledge which has been derived at different times and in different ways concerning any particular object of perception. An impression of an object, therefore, made upon any single sense-centre, on reaching the cerebral hemispheres, though it strikes first upon the perceptive centre corresponding, immediately radiates to other perceptive centres, there to strike upon functionally related cells; all this taking place with such rapidity, that the several excitations are practically simultaneous, so that the combined effects are fused into one single perceptive act. Thus, I see an orange at a distance: this, as an object of visual sense, is simply a rounded yellow area; but past experience has led me to know what are the tactual and muscular sensations usually associated with the sight-impressions—how it is really a spherical body, with a somewhat rough surface. Then I have learned also that these impressions are usually associated with a certain odour, with a certain taste, a degree of succulence, and certain internal optical characters, including a divisibility into segments, and the possible presence of seeds within. A combination of any of these, or of a host of other revivable impressions, may go to constitute my perception of an orange, and may flash into consciousness, more or less simultaneously, on the presentation of the object to the visual sense.

4. Then it must be understood also that, in any process of thought, when the cerebral hemispheres are once in action, very many of the phenomena giving rise to the successive stages of the process of thought do go on more or less automatically, by virtue of preexisting associations of contiguity, resemblance, etc., which have probably served to establish definite nerve-connections or channels of communication between the several parts concerned. But, independently of this automatic sequence taking place by virtue of an unconscious but organised memory, we can exercise also, at will, a still more mysterious function—that of voluntary *recollection*. By the exercise of this function, we have the power, at any moment, of recalling to consciousness, by a voluntary effort, any past impressions which have been received in our cerebral hemispheres. This recall may be abrupt, and without previous thought upon the subjects which we recollect. Here, also, it seems that

however brought about, and however inexplicable the process, one of the after-results must be the calling into activity of the particular cells and fibres of that perceptive centre which had most to do with the original reception of the impression; this revived activity then almost simultaneously arousing the parts in other perceptive centres which have to be excited in order that a definite *idea* may be recalled of the original perception. We can exercise a greater power of recall in certain perceptive centres than we can in others; and the corresponding impressions of sense are then said to have a higher degree of intellectual persistence. Preeminent in this respect is our power over the visual perceptive centre. The intellectual persistence of auditory and tactile impressions is also extremely great, though that of olfactory and gustatory impressions is notably less; whilst the various organic sensations can be recalled only to a very limited extent.

But still another sense remains—the so-called ‘muscular sense’—from which we have been said to derive our ‘feelings of movement.’ This, however, is an endowment in many respects so peculiar, that the subject requires a thorough consideration before we can decide how far the impressions of the ‘muscular sense’ are also capable of ideal recall.

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#### ‘MUSCULAR SENSE.’

Much speculation and inquiry has of late years been devoted to the elucidation of this most interesting and important endowment. After a consideration of some of these, Professor Bain\* has written thus: “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 any influence passing inwards by incarrying or sensitive nerves. It is known that sensitive filaments 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 the 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.” Some such conclusion seems, at first sight, to be justified by the evidence; but, as this may not be familiar to most readers, and also

\* *Senses and Intellect*, 3rd ed., p. 76.

because it is desirable that we should arrive at the most exact opinions possible on the subject, I will briefly allude to the principal points of evidence.

J. W. Arnold\* found, after section of the posterior roots of the nerves going to the hind extremities of the frog, that the animal could make use of these hind extremities almost as well as if nothing had been done to the posterior roots; from which he drew the conclusion that the nerve-fibres conveying to the sensorium the impressions which give rise to our knowledge concerning the state of our muscles must be contained in the anterior roots. Brown-Séguard† says: "This experiment is certainly of some value, and we must acknowledge that it is difficult to explain it otherwise than Arnold has done. Moreover, we have found that, after section of *all* the posterior roots of the spinal nerves in frogs, the voluntary movements seem to be very nearly as perfect as if no operation had been performed; and that, if the skin of the head is pinched on one side, the posterior limb on the same side tries to repel the cause of the pain, as well as if no injury had been made. I have also ascertained that, in frogs rendered blind, these experiments give the same result." The same physiologist has shown conclusively, however, that the ordinary or organic sensations derivable from muscles are dependent upon the integrity of the posterior or sensory roots of the nerves. When these are cut, and a galvanic current is applied to the muscles thus severed from connexion with the posterior roots, "no trace of pain is produced, and all the other causes of pain are also unable to cause it when applied either to the skin or the muscles." But, whilst Brown-Séguard has arrived at the opinion that section of the posterior roots of the spinal nerves interferes very little with the animal's power of executing movements, so that these "seem to be very nearly as perfect as if no operation had been performed," other experimenters have thought that the power of movement was sensibly affected by such an operation. Panizza‡ after operations of this kind upon she-goats, came to the conclusion that, although such animals could execute movements, and although their motor power seemed to remain intact, their movements were nevertheless uncertain, and more or less badly executed. Similar results were arrived at by Stilling, and also by Schiff in experiments upon dogs and cats; and still more recently they have been confirmed by Claude Bernard§ in experiments upon frogs, and by

\* *Ueber die Verrichtung des Rückenmarksnerven*, Heidelberg, 1845; also *Brit. and For. Medical Review*, April 1845, pp. 558-575.

† *Lectures on the Central Nervous System*, 1860, p. 9.

‡ *Ricerche Sperimentale sopra i Nervi*, Pavia, 1834.

§ *Leç. sur la Physiolog. et la Patholog. du Syst. Nerveux*, Paris, 1858, t. i. p. 250.

Leyden\* and Rosenthal in experiments upon frogs and rabbits. From this evidence, it would seem pretty evident that the division of the posterior roots of the spinal nerves does exercise some influence upon the precision and suitability of muscular movements. Brown-Séquard speaks of the alteration as being very slight; other experimenters consider the defects to be graver; but all are agreed that the amount of incoordination produced is not very great. As in these experiments, the posterior roots were cut, and ordinary sensibility was destroyed, it seems pretty clear, if the impressions of the 'muscular sense' are necessary for the proper execution of complex movements, that such supposed feelings of movement must differ altogether from ordinary sensations, and that the nerve-fibres subserving this function are *not* contained, as the other sensory fibres are, in the posterior roots of the spinal nerves. Hence, as Professor Bain says, it would appear at first sight that we must select one of two alternative propositions: either (1) we must suppose, with Arnold, that certain different and peculiar nerve-fibres pertaining to the exercise of this 'muscular sense' are situated amongst the motor fibres in the anterior roots of the spinal nerves; or else (2) we must suppose that the mind is capable of discriminating the degree of energy of the motor current, or the force poured out from the brain in voluntary movement, and that the impressions of the 'muscular sense' are of this nature.† But on the whole, for the reasons about to be mentioned, Professor Bain seems to think it more probable that the latter supposition is the correct one. Ludwig also saw nothing untenable in the latter view. He said: "Whether the nerves that subserve the muscular sense, and those that induce the muscular motion, are the same, is at present difficult to decide. It is conceivable, and not unlikely, that all knowledge and discrimination arrived at through the exertion of the voluntary muscles are attained directly through the act of voluntary excitation; so that the effort of the will is at once proceeded on as a means of judgment. This opinion is supported by the fact that the movements that give us mental judgments in by far the greater number of cases do not appear as muscular sensations; in other words they are not, like the organic sensations of muscle, localised by us in the muscle, and looked upon as possessing the characters of a sensation." Then J. Müller‡ said on this same subject: "It is not certain that the idea of the force employed in muscular contraction depends solely upon a sensation. We have a very exact notion of the quantity of nerve-force starting from the brain which

\* *Die graue Degen. der hintern Rückenmarksstränge*, Berlin, 1863.

† As will be seen further on there is still another possibility—though it will simplify the argument if I make no allusion to it for the present.

‡ *Physiol.*, t. ii. p. 480. Paris, 1859, p. 195.



is necessary to produce a certain movement. . . . It would be very possible that the appreciation of the weight and pressure, in cases where we raise or resist, should be in part at least not a sensation in the muscle, but a notion of the quantity of nerve-force which the brain is excited to call into action."

Whilst, however, there is this unanimity of opinion so far between Ludwig, Müller, and Bain, as to the kind of information revealed by the action of the muscular sense, and as to the nature of the volitional act, Dr. Landry,\* who devoted much attention to this subject came to quite an opposite conclusion. He said† that the sensations spoken of were derived from the moving organs themselves; that "the *ego* has a direct consciousness of the phenomena of volition; it knows immediately that there has been a voluntary stimulus, and to what part of the body it is directed; as to the effects produced, it is only mediately informed of these, and can disregard them. . . . The nervous action which incites the movement can only, therefore, furnish to consciousness an idea of the volition, and not that of its execution; besides, if the *sensorium* has a knowledge of the excitation of the excitomotor faculties, it ignores the quantity of nerve action exerted. It is necessary that the effect of the central incitation (the contraction) should be produced in order that the brain may perceive, and then it perceives, at the same time, both the seat and the degree of contraction. The movement itself is, therefore, the source whence we derive notions of this kind." What amount of truth is contained in these opposite statements we shall be better able to appreciate hereafter, when we have analysed so far as possible, all the phenomena of voluntary movements in their psychical and physiological aspects. Meanwhile, I will go on with the consideration of the question as to the seat of origin and nature of the impressions of the 'muscular sense.'

Wundt has given what he considers perfectly adequate reasons for inducing us to reject the view that these impressions are conducted from the muscles by sensory nerve-fibres passing upwards through the *anterior* roots of the spinal nerves. If these impressions were conducted by sensory fibres of this kind, Wundt says "nothing else could be regarded as the stimulus, save the changes taking place in the muscle, the contraction, or perhaps the electrical process in nerve and muscle accompanying the contraction. Now, this process is known to keep equal pace with the energy of the muscular contraction; and we must expect that the muscular sensation would constantly increase and decrease with the amount of internal or external work done by the muscle. But this is

\* *Mém. sur la Paraly. du Sentiment d'Activité Musculaire*, Paris, 1855.

† *Traité des Paralysies*, Paris, 1859.

not the case; for the strength of the sensation is dependent only on the strength of the motive influence passing outwards from the centre, which sets on the innervation of the motor nerves." But Wundt's reasoning on this point carries no conviction of its truth to my mind, because I am sure his latter statement would be directly contradicted by the experience of any man suffering from complete paraplegia. If such a man willed to move one of his lower extremities, he would be convinced only of his utter inability to do so; and he would experience no sensation of expended energy proportional to the strength of his desire and equivalent to what he would have experienced if his muscles had responded in the natural way. This reasoning which Wundt employs, seems to shew that he also believes the theory as to the nature of the 'muscular sense' to which Müller, Ludwig, and Bain incline; but there are grave objections to this view, which I will now proceed to state, turning principally upon the real nature of an act of volition and upon the kind of sensations pertaining to it.

In the first place, we shall derive much valuable information from a consideration of the phenomena of two morbid conditions—the one described by Landry under the name of "La Paralyse du Sentiment d'Activité Musculaire," and the other the well-known disease Locomotor Ataxy. With respect to the first disease, I will say nothing of the case observed by Sir Charles Bell, which first led him\* to surmise the probable existence of a 'muscular sense,' since the record of this case is deficient in some important details, but I will call attention to the phenomena described by Landry, who has paid much attention to the subject. He says† "the individuals attacked have preserved their power of movement in all its energy, and so long as they watch, through the sense of sight, the execution of their movements, these are effected in the usual way. Tell them to extend the index-finger, they do it; the arm, the leg, they do not hesitate in the least; direct them to do something with the hand or the foot, they do it easily, and one can scarcely observe a trace of uncertainty about the movement. Certainly in these patients, the notion of the quantity of nervous action necessary for their movements, the knowledge of its destination, the volitional im-

\* In one of his most valuable historical notes, Sir William Hamilton (*Dissertations on Reid*, p. 864 *et seq.*) has said: "That the recognition of the Locomotive Faculty, or rather the recognition of the Muscular Sense as a medium of apprehension, is of recent date, and by psychologists of this country, is an opinion in both respects erroneous. As far as I am aware, this distinction was originally taken by two Italian Aristotelians, some three centuries ago." The two persons alluded to were Julius Cæsar Scaliger, a physician (1557), and another philosophical physician, Cæsalpinus of Arezzo (1569.)

† *Traité des Paralysies*, p. 194.

pulse, the faculty of coordination, are not at fault. But now, let their eyes be bandaged, and all is changed: their movements are irregular, badly sustained—too much or too little—and the patients are not properly conscious of them. Often after having executed a movement, they are unable to say whether or not they have done so, or even they think they have accomplished it simply because they have willed to do so, when it has not taken place either on account of the movement having been prevented, or because the muscular contraction has not followed the cerebral stimulus." These phenomena constitute the essence of the disease: a power of some kind seems wanting, so that the individual cannot perform muscular movements with precision, and with the ordinary degree of consciousness,\* when unaided by the sense of sight, though he does seem to recover this power and this knowledge to its full extent when he watches the limbs which are called into action. When such symptoms exist alone—without any real diminution of the motor power, and without any impairment of the ordinary modes of sensibility—there is said to be a loss or paralysis of the 'muscular sense.' Nothing very definite can be said concerning the pathology of these cases. How can we reconcile the symptoms with the view as to the cerebral origin of the feelings of the 'muscular sense'? Volition and motor power being unimpaired and called into activity, the "feelings," which are presumed to be their "concomitants," are found wanting, or are altogether disproportionate.

Although Locomotor Ataxy is now such a well-recognized and by no means uncommon disease, much doubt and difference of opinion still prevail as to the real explanations of the phenomena presented. We know positively concerning individuals suffering from this disease, that there is great incoordination and irregularity in the movements of the lower extremities even when the eyes are open, and that all this irregularity is greatly increased when the eyes are closed; that at the same time the actual motor power of the lower extremities is either altogether intact or only diminished to a very slight extent. Then, we now have sufficient data to enable us to recognize that in certain cases these symptoms are associated with a notable diminution or even absence of the ordinary modes of cutaneous sensibility, and also of the general sensibility of the muscles and of the joints; these modes of insen-

\* We certainly want some additional information on the subject as to how far the abolition of the mere 'muscular sense' entails an unconsciousness of the movements of the limbs. I feel strongly inclined to think that Landry has made this a much more prominent symptom than it really is. Holding the opinions that he did upon the 'muscular sense,' its abolition would have implied this unconsciousness as to the feelings of movement; but I shall afterwards attempt to show that the 'feelings of movement' are not at all dependent upon the activity of the 'muscular sense.'

sibility either existing alone or in conjunction with a marked diminution in the acuteness of the 'muscular sense'—that is, in the opinion of those who think that the acuteness of the latter 'sense' is to be estimated by the individual's ability to discriminate between the differences in the weight of bodies suspended from the limbs. But there can be no doubt that other cases\* do occur in which, as I should say, a loss of the 'muscular sense' alone exists, and seems to be the cause of the incoordination of movement. Then it is now an admitted fact that the pathological lesion of locomotor ataxy consists of a sclerosis of the posterior columns of the cord, and of the posterior grey cornua, in the progress of which the proper nerve-tissue is more or less destroyed—this change also subsequently invading the posterior roots of the spinal nerves.

In explanation of the incoordination of movement, different views have been advanced by different writers: some believe that the incoordination is due to the superficial and deep anæsthesia solely—a view which has been strongly advocated by Trousseau, and lately also by Dr. Clifford Allbutt†—Trousseau even doubting the very existence of the 'muscular sense;' whilst others—and amongst them Dr. Wilks—are disinclined to believe that this anæsthesia can give rise to such symptoms, and attribute the incoordination rather to an impairment of the 'muscular sense.' Others, again, think that both these causes come into operation, and lead to the production of the irregular movements. I, in common with some few other pathologists, am disposed to think that many writers are far too prone to look upon locomotor ataxy, as a definite disease, and so to frame too exclusive theories concerning it. It is undoubtedly a most variable disease, or, in fact, an assemblage of diseased conditions, so that we really associate under this name, as under a generic appellation, a number of distinct morbid states, so long as they present the common character of ataxy of movement.

Much confusion has also arisen on the subject of the degree to which the 'muscular sense' is impaired in these cases, owing to the very different means adopted by different observers for estimating its integrity. It has been stated of late by Jaccoud‡ that the best and only proper means of appreciating this is by an application of the methods which Weber adopted in his celebrated experiments for determining the delicacy of this sense in the upper extremities. Such a test has been applied to individuals suffering from locomotor ataxy by Jaccoud, Späth, and also by Dr. Bazire, so that we are in possession of definite evidence

\* Trousseau said that, out of about fifty cases of locomotor ataxy, he had seen three of this kind.

† *Brit. Med. Journal*, Feb. 20, 1869.

‡ *Les Paraplégies et l'Ataxie du Mouvement*, p. 672.

upon the subject. I will give the principal results of Jaccoud's investigation in Dr. Bazire's words. He says: "Quite recently, Dr. Jaccoud has applied Weber's method to the lower extremities, and the results of his experiments, made on 24 different individuals, tend to show that it is possible, in health, to distinguish on an average a difference of from 50 to 70 grammes (say about two ounces) between two weights successively suspended from a lower limb. In six ataxic patients to whom this test was applied, the minimum of the difference in weight appreciated by the patient was found by Dr. Jaccoud to be considerably above the healthy average, varying from 100 to 3,000 grammes (from 3½ ounces to 4 lbs.)." Späth\* also, without being aware of the experiments just cited, adopted a similar method to investigate the state of the 'muscular sense' in two cases of ataxy, and in both he found a notable diminution in the discriminative power of the individual—indeed, in one of the two cases no difference was perceived, unless the two successive weights bore the ratio to one another of 1 : 100.

One of the cases so carefully recorded by Dr. Bazire possesses the greatest interest with reference to the 'muscular sense'. It is related in detail by him† but I will quote only the part which more immediately refers to this subject. The patient was a cabman, thirty two years of age; and Dr. Bazire's notes run thus.

"*Upper Extremities.*—He has a sensation of extreme cold in both arms, more especially the right, as if he had rested his bare arms on a cold marble slab. To the touch, however, the arms do not feel colder than the rest of the body. He complains of tingling and 'pins and needles' in the tips of his fingers, and of numbness of both upper extremities. Tactile sense is not diminished, however; for, when his eyes are closed, he can tell distinctly and at once where he is touched. When tried with the compasses, he detects the two points within normal limits. The sensibility to pain and to differences of temperature is normal. His muscles contract fairly under the influence of galvanism, and he is conscious of their doing so. He can grasp firmly although he complains of a certain diminution of power in his arms. This is more imaginary than real, and arises from his ascribing to weakness a deficiency of co-ordinating power; for he always spills his tea whenever he raises his cup to his mouth."

"*Lower Extremities.*—He has the same sensations of cold and numbness in these limbs as in the arms; he complains also of tingling of the toes. The various kinds of sensibility are normal; and, when he walks, he has no sensation of a cushion or pad intervening between his feet

\* *Beiträge zur Lehre von den Tabes Dorsalis.* Tübingen, 1864.

† Trousseau's *Lectures*, transl. by Bazire, Pt. 1, Case vi, p. 193.

and the ground. He cannot walk without the help of a stick, and even then his gait is peculiarly tottering. His legs are thrown about as if he could not measure the strength required for moving them, and his heel comes down first. He cannot stand with his eyes closed, for he quickly oscillates and loses his balance. His legs look well nourished, and have not wasted in the least since he has been ill. They possess considerable power, and cannot be bent against his will. On tickling the soles of his feet, no reflex contractions are excited in the limbs. At night, however, he has involuntary jerks and spasms of the legs. When he first begins to walk, he staggers and reels more than he does after he has been walking for some little time. When walking, he has to stop short before he can turn round; otherwise he is apt to lose his balance."

When he was last seen, twelve months after the time that the above notes were taken, Dr. Bazire said: "His gait was very characteristic of locomotor ataxy. . . . Within the last few weeks, he had noticed that he could not tell the position of his limbs in bed, under the bed-clothes.\* He still complained of numbness of the legs; and yet, strangely enough, when tactile sensibility was tested with the *æsthesiometer*, it was found unimpaired; for he could feel the two points of the instrument at a distance of only two inches, when applied in front of the legs."

Now, what are we to say to the above case? Here was an individual who presented, when first seen, notably ataxic movements, yet in whom there was no diminution of any of the ordinary modes of sensibility in the lower extremities—not even a trace of plantar anæsthesia. Are we not compelled to believe that here, at all events, there must have been an impairment or deficiency in the revelations of the 'muscular sense', acting as a real cause of some of the irregular movements: we cannot say the sole cause, because, although the want of co-ordination of movement was notably increased when his eyes were shut, there was still some irregularity of movement when they were open and the moving members were watched. And this continuance of the incoordination of movement when the eyes are open seems to be the one great difference between such cases of locomotor ataxy and the instances of "paralysis of the muscular sense" alone. The failure of co-ordination brought on by closure of the eyes in Dr. Bazire's case was that, perhaps, which was more especially due to the impairment of the 'muscular sense'; whilst the remaining disorder of movement when the eyes were open, seeing that there was no diminution of the ordinary modes of sensibility

\* That is, so long as they were at rest. When he moved them, he could at once tell from the accompanying superficial sensations.

to account for it, must have been due to some third cause. And may this cause not have been, the fact that some structural damage had taken place in the spinal cord involving more especially its co-ordinating system of fibres—those by means of which complicated movements find their organic representatives in certain pre-established cell and nerve connections? I will only add, on this subject, that the evidence to which I have before alluded, as also that supplied by the extremely interesting experiments of Heyd\* on the effects of artificially produced plantar anæsthesia, show that destruction of the ordinary modes of sensibility—superficial alone,† or superficial and deep—does give an uncertainty and more or less loss of precision to the movements executed, though this cause alone seems quite incapable of producing that extreme disorder and jerking character in the movement which is met with in advanced cases of locomotor ataxy. The different results produced by mere deep anæsthesia, moreover, as compared with those resulting from what appears to be a loss of the ‘muscular sense’ alone, are well illustrated by the case of a house-painter briefly alluded to by Trousseau‡ of whom he said: “There was complete anæsthesia of the skin over his whole body; he did not feel pricking or pinching, but could distinguish differences of temperature; and, when a vase filled with cold water was placed on his thigh, he complained of very disagreeable sensations. He did not feel violent pressure of his muscles; and, when he contracted his muscles powerfully, he only knew of their acting because he had willed it, but he had no feeling of their doing so. *The sensibility of his hands and feet was nearly perfect.* Yet, in spite of this complete muscular insensibility, he walked naturally, even when his eyes were shut.”§ This case would seem to show, too, that for man at least, as opposed to the lower animals (see note†), the preservation of

\* *Der Tastsinn der Fuss-sohle als Aequilibrirungsmittel des Körpers beim Stehen*, Tübingen, 1862, and quoted by Jacoud, *loc. cit.*, p. 472.

† Probably the effects are less here than when the deep sensibility is impaired also, since Claude Bernard found (*Leçons*, vol. i. pp. 251, 254. Paris: 1858) that frogs, the four limbs of which had been deprived of their integument so as to remove their cutaneous nerves, could still jump with the same precision and regularity as before; though, as I have already stated, undoubted impairment in the power of executing muscular movements was produced in other animals in whom the posterior roots of the spinal nerves had been cut.

‡ *Loc. cit.* p. 162.

§ Trousseau adds:—“This case, which I studied with the greatest care, proves therefore that muscular insensibility, *which necessarily implies the loss of the sense of muscular activity*, does not suffice for producing locomotor ataxy;” but I shall hope to show more fully hereafter that Trousseau’s “necessarily” was not the proper word, and that his conclusion, consequently, does not at all follow, because the impressions upon which the ordinary sensibility of the muscles depends, are quite distinct from the impressions of the ‘muscular sense.’

the cutaneous plantar sensibility is of more importance, as an aid to locomotion, than the deep sensibility of the muscles and of other parts.

Probably, in many of the worst cases of locomotor ataxy, all three causes of ataxy of movement (loss of 'muscular sense,' loss of ordinary sensibility, and damage to co-ordinating system of fibres) come into operation at the same time, though to different degrees in different cases. The same combination of defects seems to occur occasionally in certain rare hemiplegic forms of paralysis. Trousseau gives the details\* of a case of this kind, in which the cutaneous tactile sensibility, the ordinary sensibility of the muscles, and also that of the articular surfaces, was completely abolished; whilst it seems probable that the muscular sense was impaired, and the co-ordinating fibres of the cord also damaged to a certain extent. A somewhat similar though less marked instance has been recorded by Dr. Bazire.†

Returning now to the question of the mode of origin of the impressions pertaining to the 'muscular sense,' it seems perfectly obvious, after the evidence we have been enabled to obtain from a consideration of the phenomena of these two diseases—"paralysis of the muscular sense" and locomotor ataxy—that the doctrines of Müller, Ludwig, Wundt, and others cannot be correct. The impressions of the 'muscular sense' cannot, as Müller suggests, be derived principally from the "notion of the quantity of nerve-force which the brain is excited to call into action;" nor can it be true, as Wundt says, that "the strength of the sensation is dependent only upon the strength of the motive influence passing outwards from the centre, which sets on the innervation of the motor nerves." We see that individuals suffering from either of these diseases do continually call their muscles into action with a certain degree of force, whilst, at the same time, they receive either no sensation at all of this contraction, or else one altogether disproportionate to the amount of force exerted, and, consequently, to the strength of the volitional effort. In fact, all the evidence that we can obtain from disease, and also, as I think, the evidence which we can obtain from the most careful examination of our own sensations, goes rather to support, so far, the opinion of Landry—that these impressions do not depend upon our notions of the quantity of nerve-force liberated during a volitional effort, or, in other words, upon the mind's consciousness of its own outgoing energy. Before it is possible to proceed further, however, we must analyse more carefully the nature of the impressions which are said to pertain to the 'muscular sense'; and here again we shall meet with much difference of opinion.

The statement which exists in Quain and Sharpey's *Anatomy* well re-

\* *Loc. cit.* p. 162. † *Loc. cit.*, p. 213.



presents the most generally received opinion amongst physiologists regarding the 'muscular sense.' By means of this endowment, it is said "we become conscious of the existing state of the muscles which are subject to the will, or rather of the condition of the limbs and other parts which are moved through means of the voluntary muscles; and we are thereby guided in directing our voluntary movements towards the end in view." Professor Bain gives a very prominent place to the "feelings connected with the movements of the body or the action of the muscles," which he says, "are now recognised as a distinct class, differing materially from the sensations of the five senses." He regards the whole of these feelings as proceeding "from a sense apart, a sixth or muscular sense." He says also, "According to the manner of the exertion, the feelings differ considerably; a dead strain is different from movement; and distinct modes of consciousness attend quick and slow movements respectively. The most general and characteristic form of muscular exercise is exemplified in a dead strain, or else in great exertion with a moderate pace of movement." Then the sensations of this class present certain specialities which are thus referred to: "Now, as regards muscular exertion, there is a notable speciality, a radical difference in kind, signified by such phrases as 'the sense of power', 'the feeling of energy put forth', 'the experience of force or resistance.' This is an ultimate phase of the human consciousness, and the most general and fundamental of all our conscious states. By this experience we body forth to ourselves a notion of resistance, force, or power, together with the great fact denominated an external world".\* In its intellectual aspects, therefore, the muscular sense aids us in "the discriminating and identifying of degrees and modes of the characteristic consciousness of expended energy;" its revelations have reference to "the great facts of the object world named resistance, force, power, velocity, space, time, etc." But, in this account, as it appears to me, Professor Bain displays only a very meagre power of analysis. Throughout his exposition, he tends to confuse his readers by bringing under one head a number of feelings as pertaining to the 'muscular sense' which assuredly have nothing to do with this, with others which are more generally admitted to depend upon its activity—though even these are, as I think, admitted into such a category upon insufficient grounds. Surely even he will not deliberately maintain that *all* the feelings connected with movement are appreciable by means of the 'muscular sense', or that the different sensations which we derive from a dead strain or from a quick or slow movement are referable solely, or even chiefly, to the impressions derived through the intervention of the 'muscular sense'.

\* *Senses and Intellect*, 3rd edit., p. 82.

impressions which he considers to be peculiar 'active' modes of sensibility. Yet we feel sure that most people, who did not know to the contrary, would gather this to be Professor Bain's meaning, so little does he speak of the real manner in which we are rendered conscious of the different movements of our limbs and of the states of our muscles by the ordinary, or, as they have been termed by way of distinction, by the 'passive' modes of sensibility. Are we not conscious of cutaneous sensations, and of sensations emanating from our joints during the movements of our limbs—of passive impressions proceeding from skin, from bone, and possibly from ligament? Nay, are we not also, to some extent, rendered conscious of different sensations emanating from our muscles during different states of contraction—sensations dependent as much upon a passive mode of sensibility as are those impressions which we derive from our articular surfaces? And, if we do derive knowledge of this kind, as seems to be absolutely certain, would it not have been better if Professor Bain had directed more prominent attention to these facts, and if he had let us know definitely how far, in his opinion, our knowledge of movements and of muscular tensions was dependent upon passive modes of sensibility, as contradistinguished from those presumed active modes of sensibility to which he attaches so much importance and which alone, as he elsewhere admits, fall within the sphere of the 'muscular sense,' and can be considered as "concomitants of the outgoing current"?

But we can, perhaps, throw some additional light upon Professor Bain's views. Although he seems to indicate his acceptance (at p. 62) of Dr. Sharpey's statement that by means of the 'muscular sense' "we become conscious of the existing state of the muscles which are subject to the will, or rather of the condition of the limbs and other parts which are moved through means of the voluntary muscles," yet in a note (appended to p. 98,) after having spoken of the three modes (strength, continuance, and rapidity) of muscular action which we are conscious of exercising in different degrees, he proceeds to speak of a fourth variety of muscular discrimination, namely, "the sense of the amount of contraction of a muscle, and of the position of the limb in consequence." And then we find that, whilst he does include under the head of the 'muscular sense' many impressions which others would not include, that which most people have regarded as the primary or typical form of the 'muscular sense' manifestation is only regarded by Mr. Bain as having a subordinate importance and a secondary mode of origin. Speaking of the discrimination (the sense of the amount of contraction of a muscle,) he says: "Now, the discrimination must no doubt be an original fact—one cannot see how it could be acquired; but

the meaning given to it, the interpretation of the position of the limb and of the magnitudes embraced between two outstretched parts, is wholly acquired. We must learn by experience what movements correspond to the transition from one mode of contraction to the other; extension must be measured first by movement." Now with these opinions I am inclined entirely to agree: our knowledge of the position of a limb is dependent upon inferences, and extension must be measured first by movement. But then there is still the original question, How are we conscious of these movements?—through passive modes of sensibility, or through so-called 'active' modes? Now, I am prepared to maintain that this conscious impression, which we sometimes derive from a muscle in a state of contraction, is the only one (of the many conscious impressions whence we derive our knowledge of our own movements) that could by any possibility be considered as "a concomitant of the outgoing current," as an active mode of sensibility. If, in Mr. Bain's opinion, such a conscious discrimination is not the essential work of the 'muscular sense,' then it seems to me undoubtedly to follow that everything which he includes under the prominent head of "Feelings of Movement" must be resolved into the mere passive forms of sensibility to which I have just been alluding, and to which he scarcely refers at all. If it is not through our discrimination "of the state of the muscle's contraction" that we get evidence, as it might possibly be argued,\* through a so-called 'active' form of sensibility, as to the nature, extent, and duration of muscular movements, then I maintain that no form of sensation would remain by which we could become conscious of our own movements, save certain passive modes of sensibility derived from our articular surfaces, our muscles, and our skin, and certain associations which have been built up between these and previous visual impressions. If, in quick movements and in slow movements, in powerful muscular contraction and in weak muscular contraction, we do not derive our knowledge concerning these movements from some knowledge, either mediate or direct, of the actual modes of muscular contraction themselves (apart from the mind's appreciation of the volitional effort) then I maintain that there is nothing left, save various modes of passive sensibility, through which it is possible for us to obtain cognizance of such movements. For has it not been previously shown that the doctrine as to the impressions of the 'muscular sense' being concomitants

\* Though I feel convinced myself that this sensibility of muscle is as much a passive mode of sensibility as any other that could be named. And yet in one sense, too, it is a "concomitant of the outgoing current." It is a mode of sensibility which is always awakened by an outgoing current. This current throws the muscle into certain organic states, which produce corresponding impressions upon the ordinary sensory nerve-fibres distributed to the muscle.

of the volitional act, and dependent upon the mind's appreciation of its own outgoing energy, is one altogether untenable, by reason of the direct contradictions to this view which certain diseased conditions present? How repugnant such a conclusion would be to Professor Bain, I am fully aware; for does he not say† that, if the feelings of movement are due to impressions passing to the brain by ordinary afferent or sensitive nerves, "the most vital distinction within the sphere of mind is bereft of all physiological support"? Perhaps, however, the case is not quite so desperate as Professor Bain imagines. The antithesis of movement and of sensation is real and fundamental, and is not vitally affected by its being proved that we derive our knowledge of the former in this or in that way.

But, in order to judge for ourselves whether there are really any impressions of which we are *conscious* derivable through the exercise of the so-called 'muscular sense,' it is desirable that we should inquire into the true nature of the impressions in what is considered by all preceding writers to be the purest form of the exercise of the 'muscular sense': I allude to the estimation of weight. In the estimation of weights, whether relatively or absolutely, our muscles are subject to a dead strain—that strain which helps to give us our notion of Resistance; and since the time when Weber made his celebrated experiments, it has been believed by most physiologists that our appreciation of weight was due mainly to the exercise of the 'muscular sense,' and that our judgment was aided only by tactile impressions. As I have already stated Jaccoud and others regard such a test as this to be the only one that is valid in order to ascertain whether, in certain diseased conditions, there is or is not any impairment of the 'muscular sense'. Now, for my own part, I look upon even this conclusion as one which ought not to be allowed to pass unchallenged; and for these reasons. In the first place, the experiments of Weber do not seem to me to be anything like so conclusive as Professor Bain and others appear think them. They are thus described by Todd and Bowman:\* "Weber performed experiments to ascertain how far we are capable of judging of weight by the mere sense of contact. He found that, when two equal weights, every way similar, are placed on corresponding parts of the skin, we may add to or subtract from one of them a certain quantity without the person being able to appreciate the change; and that, when the parts bearing the weights, as the hands, are inactively resting upon a table, a much greater alteration may be made in the relative amount of the weights without his perceiving it, than when the same parts are allowed free motion. For example, thirty-two ounces may thus be altered by from

\* p. 77. † *Physiological Anatomy*, p. 431.

eight to twelve when the hand is motionless and supported, but only by from one and a half to four when the muscles are in action; and this difference is in spite of the greater surface affected (by the counter pressure against the support) in the former than in the latter case. Weber infers that the measure of weight by the mere touch of the skin is more than doubled by the play of the muscles. We believe this estimate to be rather under than over the mark." This, I believe, is the strongest evidence that has ever been brought forward to show that there is a distinct power of appreciating weight by muscular action, and that the discriminating power so called into play is even greater than we are able to exercise with the aid of the tactile sense. Before criticising these experiments, however, I will call particular attention to some of the results which have been arrived at by Bazire and Jaccoud in the examination of persons suffering from locomotor ataxy. Bazire examined eight patients suffering from this disease—as to their ability to appreciate differences of weight; and, he says, "in two alone (Nos. 6 and 7) was the result of the test of weight doubtful; in the other six, there was decidedly an impairment of this faculty, both in the upper and in the lower extremities." But then, as he has previously stated, and as appears from his description, Nos. 6\* and 7 are the very two cases also out of the whole eight "in which the *sensibility to touch* was not appreciably impaired." Then, looking to the six cases which were examined by Jaccoud, it was found that the degree of diminution in the ability to discriminate between different weights was different in different cases; but Jaccoud also adds: "In these six patients, the cutaneous sensibility was diminished in different degrees; in No. 2, the notion of temperature was alone preserved." Thus we find that the tactile sensibility was entirely abolished in one case; and, when we look to its details, we find that this case also is the one in which there was the most extreme inability to appreciate differences of weight.† Surely this looks very much like concomitant variation. And Dr. Bazire's cases do supply most valuable and positive information. In these two individuals there was no motor weakness, and no diminution of sensibility; yet there was notable ataxy and irregularity in the movements, aggravated when the eyes were shut, so as almost to force us to come to the opinion that they had lost some means of guidance ordinarily made use of in the execution of voluntary movements—that they were suffering, in fact from a paralysis of the 'muscular sense.' And yet, when these patients

\* The details of this case I have partially quoted.

† Whereas healthy individuals can distinguish on an average from 50 to 70 *grammes*, this individual could only distinguish a difference of 3000 *grammes* by means of his right leg, and 2800 *grammes* by means of his left.

are tested, after the most approved method, in order to ascertain the amount of this, it is found that they suffer from no appreciable diminution of that ability to discriminate between different weights, which has been supposed to be the index of the integrity of this sense. Their power of appreciating differences in weights could not be said to have been diminished; that is to say, the diminution, if any, was so slight as to make its very existence doubtful. Surely evidence such as this should make us look a little more closely into the grounds of the opinion that would include the estimation of weight as one of the notions derivable through a 'muscular sense.' Now, in spite of what had been previously said as to the superiority of the muscular sense over the sense of touch in discriminating differences of weight, Todd and Bowman added on the next page the following statement: "The relative power of different parts to estimate weight corresponds very nearly with their relative capacities of touch. Weber discovered that the lips are better estimators of weight than any other part, as we might have anticipated, from their delicate sense of touch, and from their extreme mobility. The fingers and toes are also very delicate instruments of this description. The palms and soles possess this power in a very remarkable degree, especially over the head of the metacarpal and the metatarsal bones; while the back, occiput, thorax, abdomen, shoulders, arms, and legs have very little capacity of estimating weight." Now, I would ask, how it is reconcilable, if the muscular sense is two or three times as acute as the tactile sense, that, nevertheless, the ability of different parts of the body to appreciate weight does depend so very closely upon their relative capacities of touch? Why should the weaker faculty appear the dominating one? Could there have been any mistake about the original conclusion? In my opinion, the experiments, from the results of which the conclusion was drawn, were neither crucial nor satisfactory. Surely, when the *back of the hand is placed upon a table*, and a weight is placed upon its palm, the person is not in a favourable condition for exercising a critical discrimination by means of the sense of touch as to the weight applied. Instead of thinking that the counterpressure against the support should have tended to increase the activity of the tactile discrimination, I think we should come to just the opposite conclusion: that the case was not a fair trial, because the person did not appreciate the mere sensation produced by the weight as emanating from the surface pressed upon, but also received in his sensorium at the same time a number of impressions emanating from the back of his hand, which, so far from aiding him in his judgment, would be a source of mental distraction. Then I would say also that, in estimating such heavy weights as Weber employed, the person would almost certainly have experienced sensations

emanating from the muscles themselves; and there is really no reason for believing that these sensations, derivable from a muscle which is contracting somewhat strongly, do not reveal themselves to us by means of the ordinary sensory nerves of the muscles. I believe they are results of the common sensibility of the muscle testifying to its own organic conditions, and that these sensations may well be part-components of the set of impressions by which we are enabled to judge of differences of weight. And, if this be the case, then it was only to be expected, as I think, that there should have been a much greater power of discriminating differences of weight when this common sensibility of muscle was allowed to act in conjunction with the tactile impressions, and when these latter were appreciated apart from all disturbing influences. Taking these things into consideration, in conjunction with the facts recorded by Bazire, it seems to me that Weber's conclusion from these particular experiments should be rejected, and that we should be bound to admit that this last stronghold for the supposed conscious impressions of the 'muscular sense' as 'active' modes of sensibility must be surrendered.\*

But there is another consideration which deserves our attention in this place, and which of itself also militates against the view of Professor Bain and others that the estimation of weight or resistance is arrived at principally through a 'muscular sense' or active mode of sensibility. It is this. It is generally admitted that we can in some way acquire permanent standards of weight which shall be recoverable in idea at will—a fact which Professor Bain well expresses in this way :† “Absolute weight implies a permanent standard, and a permanent impression of that standard. When I lift a weight, and pronounce it to be seven pounds, I make a comparison between the present feeling and the impression acquired by handling the standard weight of seven pounds or things equivalent thereto. This absolute comparison, therefore, implies the enduring and recoverable sensibility to impressions of resistance, which is also a fact of the human constitution. . . . A receiver of posted letters contracts an ingrained sensibility to half an ounce, and can say, of any letter put into his hand, whether it produces a sensibility equal to or under the standard.” Now, seeing it to be admitted that precisely the same parts of the brain, the same nerve-cells and the same nerve-fibres, are always concerned in the ideal recall, as were concerned in the elaboration of the original impression; and seeing that Professor Bain maintains that these impressions of resistance were originally con-

\* The case of the house-painter examined by Trousseau, which I have already quoted, seems to lend additional confirmation to this view.

† *Loc. cit.*, p. 93.

comitants of that out going motor current which excited the muscles to act,—we may well ask whether it would be possible that the mere concomitant could be revived alone, without the muscular action of which it is supposed to be the sign. We know that when the ideal recall of a past feeling of resistance is brought about, the muscular action which originally took place does not recur, and therefore we cannot believe that the molecular agitation occasioning its ideal recall has occurred in precisely the same circles that were concerned in the production of the original impression. But this would be a conclusion which I am sure Professor Bain would not be prepared to admit. It is one of the fundamental positions of physiological psychology which he has strongly enforced,\* that a renewed feeling or idea must depend upon the action of precisely the same parts as the original feeling. Therefore it seems to me, since the ideal recall could never be supposed to comply with these conditions if the original sense of resistance had been derived from the motor nerves by means of a 'muscular sense,' even this method of reasoning would compel us to reject Professor Bain's conclusions on this subject,† if there were not the other points of evidence tending in the same

\* He says (*loc. cit.*, p. 338):—"What is the manner of occupation of the brain with a resuscitated feeling of resistance, a smell, or a sound? There is only one answer that seems admissible. *The renewed feeling occupies the very same parts, and in the same manner, as the original feeling, and in no other parts, nor in any other assignable manner.* I imagine that if our present knowledge of the brain had been present to the earliest speculators this is the only hypothesis that would have occurred to them. For where should a past feeling be re-embodied if not in the same organs as the feeling when present? It is only in this way that its identity can be preserved; a feeling differently embodied would be a different feeling."

† Sir Wm. Hamilton's doctrine was somewhat different, though also, as I think, untenable. It is fully set forth in a note appended to p. 864 of his *Dissertations on Reid*. We are in accord so far, that the "feelings of movement" are derivable from purely passive modes of sensibility; only, he maintains, that in addition to these notions of a subjective kind thence derivable, we get our objective notion of "resistance" from our consciousness of expended energy in volitional action (in the same way, in fact, as Müller, Ludwig, Wundt, and Bain, imagine that we derive our impressions pertaining to the 'muscular sense'). Hamilton's view which is nearer the truth, I think, than that of Professor Bain is, in part, stated in this way:—"The consciousness of the mental motive energy, and of the greater or less intensity of such energy requisite in different circumstances to accomplish our intention, would of itself always enable us to perceive the fact, and in some degree to measure the amount of any *resistance* to our voluntary movement." Now the simplest way of testing this statement is to ask a man, whose lower extremities are completely paralysed, owing to a disease of the spinal cord, whether, when he ineffectually wills to move either of these limbs, he is conscious of an expenditure of energy in any degree proportionate to that which he would have experienced if his muscles had naturally responded to his volition. He will tell us rather that he has a sense only of his own utter powerlessness, and that his volition is a mere mental act, carrying with it no feelings of expended energy, such as he is accustomed to experience when his muscles are in powerful action, and from which action and its consequences alone, as I think, he can derive any adequate notion of resistance.



direction. But, if we assume that our perceptions of resistance have been acquired in the way I have indicated—that they are partly made up of tactile impressions, partly of passive sensations emanating from our muscles and joints, and of inferences founded upon these—then all difficulties vanish. These passive sensations being dependent upon the activity of afferent nerves and their central connections can, of course, be easily recalled in idea, without any recurrence of the muscular actions by which they were originally occasioned; so that the possibility of forming an ingrained standard of weight, which shall be *recoverable* alone and without the recurrence of the muscular action, is perfectly conceivable. We experience certain feelings of pressure combined with certain sensations in the muscles and in the joints, and we gradually come to associate certain combinations of these with the sensations produced by handling certain standard weights. The notion of “expended energy” may well be one which is subsequently superadded to the original sensation; and, of course, any intense state of consciousness, as a mere state of consciousness, does involve an expenditure of energy.

I am not singular in the opinions I have just expressed as to the nature and origin of our “feelings of movement;” for much doubt has arisen in the minds of others who have carefully considered the question, as to the very existence of any conscious impressions which can be said to be appreciable only by means of a ‘muscular sense.’ Trousseau has been one of the most prominent objectors to the recognition of this so-called sixth sense, and on these grounds. He says: “When, after shutting our eyes, we execute without effort a pretty extensive movement, we are unable, even on paying the strictest attention, to feel the contraction of our muscles, although we may feel the movement communicated to the levers by the contracted muscles. This fact is so true that, when we ask an intelligent person, who knows nothing of anatomy and physiology, which is the seat of the movements through which the fingers are flexed and extended, he immediately points to the hand, and never to the forearm. It is only when the muscular effort is considerable, or is kept up for a long time, that it is perceived where the contraction really occurs. Normally, then, we have no consciousness of muscular activity, but merely the consciousness of the movement itself, which is a perfectly different thing.”\* Then again, *the feelings during passive movements of a limb are almost precisely similar to those which occur during weak voluntary movements of the same limb.* Our consciousness of the movements in the latter case depends partly upon an inference, and

\* *Loc. cit.*, p. 159.

partly upon actual local sensations of the passive kind. He says, further on: "If we shut our eyes whilst we move one hand, for instance, we feel in the palm, and in the palmar surface of the fingers, a sensation of dragging when the hand is opened wide, and a sensation of relaxation when the hand is closed, in addition to a special sensation in the joints themselves, which latter is always striking, and even painful, when we wake up from sleep. As to muscular sensations, they only exist when the contraction is extreme, or when the muscle is in a painful condition, as after a contusion or great fatigue, for instance." I must confess that it seems to me very difficult to understand how persons competent to judge of the evidence can come to any other conclusion; and, practically, very many physiologists are agreed upon this point, up to a certain extent. Müller, Wundt, and Ludwig, all admit (and in this respect Professor Bain is opposed to them, although his utterances are somewhat contradictory) that the impressions of the 'muscular sense' do not reveal themselves in consciousness as feelings derivable from the movement itself. They maintain, rather, that these sensations are in some way an appanage of the volitional act which occasions the movement; that they are due to the mind's consciousness of its own outgoing energy. And, when Ludwig says of them, "They are not like the organic sensations of muscle, localised by us in the muscle, and looked upon as possessing the characters of sensation," Professor Bain adds,\* "Ludwig thus appeals to our consciousness as presenting the feeling of muscular energy in a characteristic form, and distinct from the feeling of muscular pains. And in this he seems to be right; for, if consciousness be a safe guide in the matter, we should say that, in the case of a voluntary effort, the feeling is as of power going out of us, and not as of a surface of sense stimulated by an external agent, and transmitting an impression inwards to the nerve-centres.† Notwithstanding this, however, it does appear, from the note at page 99, that Professor Bain believes there is some feeling of the amount of contraction of a muscle "bound up with our sensibility to movement."

What, then, are we to think of Professor Bain's exposition of the "feelings of movement," as a distinct class of sensations differing materially from the sensations of the five senses, and corresponding with those which "are often regarded as proceeding from a sense apart, a

\* *Loc. cit.*, p. 79.

† But, whilst Professor Bain expresses this opinion in the note at page 79, and there apparently agrees with Ludwig and Wundt, in the body of the work, as we have seen, he almost uniformly speaks of the "feelings of movement" also as belonging to a distinct class, corresponding with what has been spoken of as pertaining to a sixth or 'muscular sense.'

sixth, or muscular sense"? We can only say, after a careful analysis, it seems absolutely certain that he has included under this one heading, things which it behoved him to have kept distinct and separate; that very many of those sensations which he seems to consider derivable through an 'active' mode of sensibility or 'muscular sense,' are obviously not so, and can be resolved at once into modes of passive sensibility; and, therefore, that the philosophical theories which he hangs upon his supposed facts—on the supposition that those are active modes of sensibility which we have shown to be passive—having lost their former props, must find other support, if they are not to fall to the ground. Professor Bain wishes to bring out prominently the "great antithesis of movement and sensation throughout the whole mental system" and he dwells much upon "the total opposition of nature between states of energy exerted and states of passive stimulation." If, therefore, he could have shown that this psychological distinction had its root in some physiological difference, that our feelings of movement were even totally different from our passive sensibilities; that they were what we may call 'active' modes of sensibility, whose existence was dependent rather upon the immediate action of the motor nerves than upon that of sensory nerves,—then his theories would have seemed to derive an additional value and completeness. But we trust Professor Bain will see that he has not been adopting the most approved scientific methods, when he includes under one head positive sensations which are undoubtedly purely passive, with other sensations presumed to be different and of a so-called 'active nature; when he treats almost entirely of these ordinary passive sensibilities, though he gives his readers reason to believe that he is speaking of a peculiar kind of 'active' sensibility; and then, applying the knowledge derivable in great part, if not entirely, through our passive modes of sensibility, on the erroneous supposition that it has been derived through the supposed 'active' mode of sensibility, he comes to the conclusion that there are two distinct states of consciousness—an *object* attitude, in which "we are not self-conscious, but are engaged in knowing certain purely object facts, called force, extension, etc.;" and a *subject* attitude, answering to consciousness in its ordinary acceptation. If there be good philosophical reasons entitling Professor Bain to make some such distinction, I venture to think he must do it upon different grounds from those which he has brought forward.

I maintain, therefore, that *all* the information which Professor Bain considers to have been revealed to us by the activity of our locomotive organs through an 'active' mode of sensibility, has been derived from modes of passive sensibility (occasioned, it is true, by this activity), and from inferences which have gradually been built up upon these. But,

as it is desirable that there should be as little doubt as possible upon this important point, I will endeavour to show what additional evidence may be obtained from an inquiry into the nature of the stages by which a complex volitional movement is brought about.

In voluntary movements, whether simple or complex, there are two distinct classes of nervous action involved. There is the cerebral nervous action, and the spinal nervous action; and we must determine accurately what is the province of each. Speaking generally, under the first head comes (1) the cerebral incitation to the movement—the *volition*; and, under the second head, the immediate execution of the movement by means of properly co-ordinated muscular action; this execution being supposed to be essentially dependent upon (2) the co-ordinating functions of the cord and (3) the guiding influence of the 'muscular sense' and other modes of sensibility.

1. In our inquiries upon this point, we must bear in mind that in Voluntary acts we have not merely to account for the production of a movement, but of a movement of a certain kind, in which the action of each set of muscles brought into play is duly regulated so as to lead to the exact result desired. And, seeing that the volition or desire to bring about such and such movements is purely cerebral in its origin, so we are bound to admit that such qualities of movement as are contained under this head must also depend upon cerebral influence. Hence the strength, the continuance, the rapidity, and the direction of movements are variable according to the precise nature of the cerebral incitation or volition. As Jaccoud says, "The strength of the movement is regulated by the strength of the initial motor impulse; the extent depends in reality upon the same influence; the rapidity results from the more or less rapid succession of voluntary impulses; and the direction is determined by the voluntary localisation of the incitation upon certain groups of muscles." Then it is important to recollect also that, in the execution of a complex movement, any alteration that we may desire to bring about, in respect of any one of these volitional qualities, is, by the mere change in the volition itself, immediately effected with reference to the movement as a complex whole. Jaccoud illustrates this as follows: "I am walking, and then I wish to walk more quickly. Hardly have I conceived the desire before the mode of walking is changed; it has become, in short, more rapid. This intervention of my will manifests itself, therefore, by a change in the movement of locomotion as a whole. This change is the final result of a series of partial modifications which have been brought about in the original movement: this is incontestable. But I have not needed a parallel series of volitional acts; a single volition has sufficed. I have willed to walk more quickly, and

and I have then walked more quickly, without knowing anything, without even requiring to know anything, of these intermediate modifications."\* In the same way, alterations in any one of the other volitional qualities of a complex movement are found to have direct bearings upon the movement as a whole. These seem to be the principal facts that should be mentioned with regard to the mere volitional act; and I am quite disposed to agree with Landry when he says, in opposition to the views of several physiologists previously mentioned, that the volition itself includes only an incitation to a specific kind of movement, and that we must execute this movement in order to become acquainted (inferentially) with the quantity of nervous action brought into play. The feeling of "expended energy," therefore by which we obtain our ideas of resistance, and of an external world, is not contained, as we think, in the volitional act itself, but is derived through impressions emanating from the moving organs themselves during the actual accomplishment of movements. But here Landry and I part company. He says that the feeling of expended energy and the knowledge of the degree of contraction of the muscles is the peculiar and special revelation of the muscular sense; whereas I say that we are not *conscious* of the 'muscular sense' impressions, and that this feeling of "expended energy" is dependent upon our purely passive sensibilities, and upon inferences which have been gradually wrought up with them.

2. But besides these *qualities* of the movement which are determined by the volition, we have to consider the execution of the movement itself as a compound of several simple movements, brought about by the simultaneous and successive contractions of different muscles, which are perfectly harmonious and constant in their mode of action. This machine-like precision of action—the result of what is usually known by the name of *co-ordination*—is now admitted by most physiologists to depend upon certain pre-established, though gradually-acquired, nerve-connections between the different elements of the spinal cord. The movements are machine-like, inasmuch as they depend upon certain organic combinations in the spinal cord, and after these connections have once been fully formed the will has little more to do with them; the movements ordinarily take place in a definite manner, the will qualifying their mode of execution only. It requires a special exercise of mental power in order to execute certain complex movements in a way different from that which has become habitual. But in these cases, as well as when we are learning to execute a new complex movement, we give no attention whatever to the muscles by which the movement is effected—the

\* *Loc. cit.* p. 594.

states of these do not fall within the scope of our consciousness; in our tentative efforts we think only of the movements themselves—how to combine the more simple, so as to produce the more complex. The kind of movement produced is evidently dependent upon the *distribution*, in the medulla and spinal cord, of the volitional impulse; and, in a complicated motor act, its incidence upon particular groups of cells, which are the organic representatives of certain potentialities for simple movements, gives rise to the production of the complex movement. The more frequently such a complex movement has been executed, the more completely may we suppose these various groups to become bound together into one system by connecting fibres, and the more possible is it for the movements which they represent to occur in the most thoroughly automatic manner, and without the least need of cerebral intervention. The mechanism of co-ordination is, therefore, purely spinal. This fact was fully recognised by Volkmann\* even five and twenty years ago, as appears from the following quotation: "The physiological accomplishment of movements has nothing to do either with consciousness or with unconsciousness, for the mind has not the least notion of the details of this operation; and even in voluntary movements it knows nothing concerning the nerves or the muscles by the intervention of which the process is accomplished. In fact, the mind does nothing in this case but give the incitation; and if this incitation has for an effect any co-ordinated movement, it is altogether simply because the organ which receives the incitation is arranged in such a fashion that it necessarily produces a co-ordinated movement." Precisely the same views as these were also expressed, at about the same time, by Arnold and Müller.

3. Thus the work of co-ordination is entirely spinal and organic; the movement itself depends upon the spinal cord, though its particular *qualities* of force, rapidity, etc., are dependent upon the cerebral or volitional influence. The cerebrum may, therefore, also be said to exercise a kind of co-ordination—it co-ordinates or adapts the movement which is organically represented in the spinal-cord, so as to make it accord qualitatively with the aim conceived. But in order that the cerebrum may exercise this power, it seems perfectly obvious that it should be instructed from moment to moment as to the qualities of the movement actually produced, so that it may know whether to continue in its present mode of action, or whether to vary the quality of its volition, in order better to attain the desired end. Now, according to Jaccoud, whose treatment of this subject is most excellent, the cerebrum

\* Art. "Nervenphysiologie" in Wagner's *Handwörterbuch*, 1844.

obtains these necessary guiding impressions from different sources. He says: "These indispensable notions the brain obtains directly through the sense of sight; or, instead it deduces them indirectly from the instructions which reach it as to the situation of the parts which move and the condition of the contractile organs which move them; these instructions are furnished by impressions through the muscular sense, and through the sense of touch.\*" The sense of sight is what we chiefly rely upon in early years, and in acquiring new movements generally; and we all know how long it is before a person learning to play upon a musical instrument can do without the aid of this guiding sense. At last, however, his tactile sense, and perhaps also his 'muscular sense', has become so educated that he is able to do without guidance from the sense of sight; and, as a rule, he does without this primary aid as soon as the execution of the movement has become perfectly easy. Jaccoud calls the second mode of appreciation indirect, because it does not, as in the case of the sense of sight, depend upon a simple perception of transmitted sensations, but upon an interpretation of sensations. As he says:† "The sensorium requires, as a preliminary, to have learned the relations which unite the various conditions of the muscles or of the tactile organs to the different sensations perceived; it is only at the termination of this apprenticeship that it can conclude from the sensation perceived as to the statical or dynamical conditions of the parts whence the sensation springs. This education proceeds correctly, by means of the direct appreciation through the sense of sight, thanks to which the individual can compare at each instant the movement effected with the sensation perceived." After this preliminary education has been finished, the knowledge so acquired, though inferential, becomes as available and as efficacious as that which is more directly derived through the sense of sight. It can be brought into action also with just as much rapidity and exactness, so that the sense of sight is no longer needed to inform us as to the position of our limbs, and as to the nature and degree of their movemets.‡

\* In its widest sense.

† Page 601.

‡ When we commence a movement, we initiate it with certain predetermined qualities of force and extent; and this, of course, is simply a result of our past experience and education. I know that certain objects have hitherto given me certain impressions of weight when I have previously handled them, and therefore my previous education now enables me, when I see such an object again and desire to handle it, to give the volitional act its necessary qualifications. This power has been variously termed "*l'instinct locomoteur*", "*conscience musculaire*", and "*l'aptitude motrice indépendante de la vue*", and has been made the subject of much mystification, though it is a simple result of education, and should not be confounded with the functions of the 'muscular sense'. When I see a simple bundle of wool on a table, as a result of previous education I can at once nearly accurately determine what ought to be the quality

Jaccoud, therefore, thinks that the power which healthy persons enjoy of performing, with facility, movements that are at the same time habitual and complex depends, in the first place, upon the integrity of their 'muscular sense', and in the second, upon that of their sense of touch. Impressions are derived through both these channels, whereby the brain becomes informed as to the amount and kind of movement which it has called into action; and so it learns whether, to obtain the end in view, it should continue with the same volition, or should qualify it in any way. Jaccoud, in common with other physiologists, thinks that we are *conscious* of the impressions of the 'muscular sense' and, as we have previously seen, that our power of appreciating differences in weight is dependent upon the exercise of this sense. I have already stated my reasons for rejecting this opinion, and have endeavoured to show how, in reality, these estimations are inferences based upon previous sensory impressions of the passive kind—upon impressions emanating from the skin, from the joints, and from the muscles themselves. In my opinion, there are *no* conscious impressions derivable through the 'muscular sense'. This, as I think, is not to be considered as an appanage of the intellect, but rather as an unconscious organic guide in the performance of voluntary movements. Why, it may be asked, do I not, as Trousseau has already done, deny its existence altogether? To this I should reply by saying,—although there is no evidence to lead us to believe that we derive any conscious impressions through the intervention of this so-called 'muscular sense', there *is* evidence to shew that the brain is assisted in the execution of voluntary movements by guiding impressions of some kind, which whilst they differ in mode of origin from the impressions derivable by means of the ordinary cutaneous and deep sensibility, may differ still further from these, owing to the fact of their not being revealed in consciousness. How unless we grant something of this kind, are we to account for some of the phenomena of disease? How can we account for the phenomena observed in such a case as that recorded by Dr. Bazire, and in many others of a similar kind, where all the modes of

of the volition necessary to enable me to raise it. As a result of this education, I am enabled to initiate such a movement as I deem appropriate. But supposing in the case just cited, that the supposed simple bundle of wool was not a simple bundle, that it contained a heavy leaden weight in its centre, then my initial volition would have been inadequate, I should have been deceived, and the impressions that I then received from my sense of touch, from my muscles, and from my motor nerve-apparatus (concerning these latter I am now about to speak) would have instructed me that a stronger volitional effort was necessary; and it is in this way always that the sense of touch and the 'muscular sense' are supposed to intervene. By this supposed locomotive instinct, we know, as Jaccoud puts it, *what force we ought to employ*, whilst by ordinary sensibility and the 'muscular sense' we are taught *what force we have employed*.



ordinary sensibility being unimpaired, the patient is unable to execute voluntary movements when the eyes are closed, but can execute them as perfectly as ever under the guiding supervision of the sense of sight? There is clearly a loss of something in these cases, of a something which serves as a guide in the execution of voluntary movements, but whose absence can be compensated for by the supervision of the visual sense; and this is, in great part the function which some physiologists attach to the 'muscular sense'. So much, I think, we are compelled to grant; but my position is that these impressions of the muscular sense, whose existence we are thus obliged to postulate, are *unconscious* impressions, and that the conscious impressions which have usually been stated to fall within its province are really derivable through modes of ordinary cutaneous and deep sensibility.

These unconscious impressions of the 'muscular sense,' such as I assume, travelling to the cerebellum or the cerebrum, would be almost exactly comparable with other unconscious impressions which pass from one point of the spinal cord to another—such impressions, as in all reflex acts depending alone upon the spinal cord, must pass from certain sensory nerve-cells in the posterior cornua (whose afferent fibres emanate from the portion of the skin excited) along what has been termed "an intermediate system" of fibres\* to certain motor nerve-cells, the excitation of which, by this unconscious impression, is capable of bringing about the accustomed reflex action. But, as we have seen, the 'muscular sense' is called into operation as an aid to the process of cerebral co-ordination, and is not concerned in the work of spinal co-ordination. The 'muscular sense' furnishes those unconscious impressions by which the quality and maintenance of the volitional act is determined with the view to the accomplishment of a definite end, so that the anatomical structures concerned with these impressions are related to the process of Cerebral co-ordination of movements in precisely the same way that the system of intermediate fibres are related to the purely reflex co-ordinated movements which take place through the instrumentality of the Spinal Cord alone.

In the process of cerebral co-ordination, therefore, one set of impressions, those of the 'muscular sense,' are completely monopolised, whilst the impressions of touch or common sensibility are made use of in the process of spinal as well as in that of cerebral co-ordination, only with this difference, that the impressions of touch which enter into the evolution of purely reflex spinal co-ordinated movements are impressions impinging upon the spinal cells, and therefore such as do not arouse our consciousness, whereas those similar impressions of touch which, in other

\* See *Jaccoud*, p. 100.

cases, pass onwards from the spinal cells to the brain do reveal themselves as conscious states. In order that that education should take place which I have before spoken of as growing out of the associations built up between the visual and tactile impressions derivable from our various movements, it was essential (if desirable that we should be able to execute movements without the constant supervision of the sense of sight) that some of the ordinary impressions which are to serve as the monitors for the regulation of our volitions should be conscious impressions; whilst it would seem also more than desirable that the principal guides which are to be made use of throughout life should be impressions which did not reveal themselves in consciousness, and which, therefore, did not allow all the details connected with the action of our muscles to disturb our processes of thought. Precisely such unconscious impressions are those which, as I conceive, pertain to the activity of the so-called 'muscular sense.'

How far are these views in accordance with anatomical and pathological data? We know that in cases of locomotor ataxy, where, as we have seen, the 'muscular sense' seems to be so frequently impaired, the stress of the pathological lesions falls upon the posterior columns of the cord; and, to a minor extent, as Lockhart Clarke has shown, upon the posterior grey cornua, and the posterior roots of the spinal nerves. Then the posterior columns of the cord are now believed, on good anatomical and pathological evidence, to contain at least two sets of fibres—one kind passing from any given part of the spinal cord to various other parts above and below (these constituting the commissural fibres concerned in spinal co-ordination), and another kind passing upwards from the different regions of the cord to some parts of the encephalon, which have not yet been fully determined.\* Now we know also from experimental evidence that these fibres of the posterior columns do not convey impressions to the brain which reveal themselves in consciousness, so it seems to me very possible that the latter kind of fibres may transmit to the encephalon the unconscious impressions of the 'muscular sense.' And having regard to the experimental evidence adduced in the early part of this paper (p. 6), showing the effects of section of the posterior roots of the spinal nerves, I am disposed to think that these fibres have not previously emanated from the muscles or passed through the roots of the spinal nerves either anterior or posterior, but that they have proceeded more or less directly from *posterior prolongations of the ganglion cells in the anterior or motor regions of the cord*. There must, of course, be some constant proportional relationship existing between the cerebral

\* See a paper on Secondary Degenerations of the Spinal Cord, by the author, in *Med. Chir. Transact.* 1867.

volition, the changes in the intervening spinal motor cells, and the functional activity of the particular muscles which are excited by these latter; therefore, it seems quite possible that impressions produced by changes in the spinal motor cells might well convey to the brain symbolic notions as to the functionally related changes occurring in the muscles themselves. Thus I assume it to be possible that when molecular changes are excited in certain spinal motor cells as a result of a volitional impulse, proportional recurrent impressions may be carried along certain fibres taking origin from the motor cells, and ascending in the posterior columns of the cord; so that there may be conveyed to the brain definite impressions (not revealing themselves in consciousness) referrible to the changes induced in the state of the various muscles or sets of muscles of a limb.

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Now, then, it is easy for me to answer the question which immediately preceded this inquiry into the nature of the 'muscular sense.' We cannot recall ideally the impressions derivable through the 'muscular sense,' for the simple reason that such impressions, in their actuality, never did reveal themselves in consciousness. Therefore, also, I am bound to come to the still further conclusion that, if words are ever revived primarily as "suppressed articulations" the only form in which these could be recoverable as such in idea would be as remembered feelings of the movements of our organs of articulation. How vague and comparatively indefinite these are any thoughtful person will soon be able to satisfy himself; and it would certainly seem that such vague sensations would be very poor and shadowy substitutes for the definite and readily recoverable symbols with which auditory and visual impressions supply us.

Having arrived at these conclusions from a consideration of physiological facts, and from a careful examination of our conscious states, we are in a better position for proceeding with our special inquiry into the physiology of thinking, with the view of explaining various phenomena met with in the "Aphasic" and other allied conditions.

I have elsewhere\* pointed out that as the first stage in the acquirement of language, during infant-life, "we find the mere association of a particular sound with a particular set of attributes belonging to some external object, and this association becoming one of considerable constancy, so that the name invariably recalls to the child's mind the idea of the object; and the presentation of the object as promptly calls up the memory of the sound with which it has become mentally associated.

\* *Fortnightly Review*, Jan. 1869; Art. "On Physiology of Thinking."

Here, therefore, a *remembered sound*, or a perceptive impression revived in those portions of the cerebral hemispheres which are in remote connection with the ganglionic centres of the auditory nerve, must have been the symbols of thought made use of by the child if language of any kind entered into its thinking processes. But afterwards, as we have seen, when the child begins to make articulatory efforts, a secondary association is gradually established; viz., one between the remembered sound of the object, and the articulatory effort which the child makes in order to utter this sound for itself." To this was added the following sentence:—"Let it be granted that after a time this secondary association becomes as perfect and recurs as easily as the remembered sound, which at first was the only linguistic symbol, then the question arises, Do we, when we are, as it is termed, thinking to ourselves, still make use of the old and original remembered sounds of words as the vehicle for our thoughts—our mental symbols—or do we cast these aside, get at primarily, and make use of as our vehicle, the remembered acts of articulation necessary for the pronunciation of the said sounds?" Professor Bain, in commenting upon these views\* whilst he in part admits the truth of what is said concerning the period in the child's life previous to the ability to articulate—that at this time "the ideas of language must consist solely of sounds"—then goes on to say, although it is perfectly evident that certain name-sounds do recall to the child's mind the objects with which these have been previously associated, "Dr. Bastian too hastily assumes the inverted operation—objects recalling names a possible process, no doubt implied in the fact of association, yet at that stage serving no end, and giving no evidence of its existence. There is neither proof nor probability that the child indulges in recollections of sounds by themselves; trains of auditory ideas ending in nothing. None of its operations require such trains; and there is the greatest unlikelihood that they are indulged in previous to the stage when they can be reproduced in speech, or become a guide to action." It seems to me somewhat surprising that Professor Bain should have made these remarks. It can scarcely be questioned that in the great majority of cases, at least, where an association, such as I have assumed, does exist between certain sensory impressions—between impressions of sight and impressions of hearing, for instance—that such an association is as perfect in the one direction as in the other; that the visual impression is as capable of recalling the auditory, as the auditory impression is of summoning up the visual. I was, therefore, perfectly justified, in the absence of evidence to the contrary, in assuming that the same equal relationship existed

\* *Fortnightly Review*, April 1869, p. 494.

between the sights and sounds impressing themselves upon the infant's organism. If Professor Bain really thinks that such is not the case, he is wishing to establish an exception to a general rule, and therefore it lies with him to adduce the evidence in support of his opinions. When he says "there is neither proof nor probability that the child indulges in recollections of sounds by themselves," I would ask him whether there is also no proof or probability that the child indulges in recollections of sights by themselves: there certainly is just as much ground for denying the one as for denying the other; and if we deny the one we may deny them both, and thus theoretically reduce the mental life of the child which is still unable to articulate to the lowest possible level—to a hypothetical level, in fact, the utter erroneousness of which cannot fail to strike those who have watched young children in an unprejudiced manner. But as a matter of fact, I never did suppose that the child indulged in "recollections of sounds *by themselves*, trains of auditory ideas ending in nothing:" I expressly stated, "It would seem pretty obvious that, so far as the infant thinks by means of language, it does so by the *remembered sounds* of words; these are its linguistic symbols of thought, which must, however, be mixed up inextricably in its mind with other sense impressions, and more especially with those of sight."\* Professor Bain then goes on to state that none of the child's operations require such trains of auditory ideas; but with the understanding just mentioned, that in the immature mind of the child such trains are probably even more intermixed with visual remembrances than they are in the adult, we are bound to admit that if the child can think at all, however simply, it must necessarily make use in its thought of these very trains of which Professor Bain says it has no need.

When we now come to consider the later periods—the time when the child begins to, and has acquired the power of speaking in articulate language—we have to return to the question which I have formerly stated—"Do words recur to the mind engaged in silent thought in their passive or in their active forms, as remembered impressions derived through the auditory ganglia, or as remembered combinations of muscular movement?" I have been induced to answer this question in favour of the passive forms of sensibility. I think words are primarily revived in the cerebral hemispheres as remembered sound impressions; that these afterwards, in the case of speaking aloud, automatically incite the necessary combined movements for the acts of articulation, but that a remembrance of muscular movements never does inter-

\* Page 60.

vene as the idea of speech.\* I have come to these conclusions for the following reasons:—

1. I have just shown that the details of articulatory acts are never revealed in consciousness, and, therefore, obviously they could not be capable of ideal recall. All that we are conscious of is certain more or less vague and ill-defined feelings, resulting from the mere movements of our organs of articulation. These, therefore, are the only forms under which articulations can be revived in thought. The so-called 'active' modes of sensation have in reality no existence as conscious impressions.

2. Then, if we turn our attention to the phenomena presented in a well marked case of Aphasia, it will be found that very valuable evidence is supplied in proof of the views which I have been advocating. We do commonly find aphasic individuals suffering from no paralysis of articulation, and unable to speak more than perhaps one or two words, who are yet able think quite well, and whose mental faculties do not seem appreciably impaired. If such an individual thinks to a great extent by means of remembered articulations, how would it be possible when his power of accomplishing the actuality was abolished that he could still produce a word ideally, if this reproduction de-

\* This was the definite position that I endeavoured to establish in my previous paper; it did not really concern me to show whether in Professor Bain's opinion "suppressed articulations" formed our only "ideas" of speech. I thought they never did occur as "ideas" of speech; and hence I was quite justified in criticising a passage in Professor Bain's book, in which he put this mode of revival forward as a prominent one. His charge of unfairness therefore was, I think, wholly uncalled for; and I can assure him that I did not quote the passage referred to in order to "fasten upon him the opinion that there was no other form of verbal recollection but the articulations." I was criticising the most prominent statement which Professor Bain had made upon the subject—one which has stood in all three editions of his work, and which has been emphasised by the use of italics; it was sufficient for me therefore to show, in that paper, that I believed such a mode of revival impossible. The misrepresentation is, I think, on his side. In order to convince his readers that I had been dealing unfairly with him, he puts prominently forward (*Fortnightly Review*, April 1869, p. 493) a passage to which he does express opposite views; and although he says he has "uniformly expressed this opinion", the passage exists only in his recently published third edition, whilst the other which I quoted had existed in all three editions. Then with the effect, at all events, of apparently still further convicting me of misrepresentation, Professor Bain employs italics to emphasise this recently introduced passage (without saying that they do not exist in his book), and gives only an imperfect and maimed reproduction of the passage I had quoted, and now suppresses the italics by which he had always emphasised this statement in his work. Thus altogether an erroneous impression is conveyed; and worse still, there is an imputation rather than a confession of unfairness. Of course, I am glad to find from Professor Bain that there is more agreement between us than I had imagined from my study of the earlier editions of his work—though the statements at present existing in the third edition are somewhat contradictory and confusing.

pended, as Professor Bain thinks, upon the action of the motor nerves, through the medium of the 'muscular sense'? He has himself said: "It is not very easy to show, on the other hand, that a series of actions can be repeated mentally and not bodily; for, as the mental actions are performed in the same circles, it usually needs only a volition, often the removal of a restraint merely, to bring them to the full length of actuating the muscles.\*" But I have also shown it to be almost impossible to conceive that an ideal recall of such impressions could occur alone and without the muscular actions of which, in Professor Bain's view, they were the concomitants. The facts of aphasia, therefore, tend still further to strengthen the view which I have been advocating, as I think Professor Bain must perforce admit; so that we are bound to believe that aphasic individuals, like other people who are not deaf, must either revive verbal symbols (1) out of the auditory perceptive centres, or else (2) they must employ such indefinite counters as the mere remembered passive sensations of the movements occurring in the articulation of different words. I can only say that, in my opinion, influenced by the considerations which follow, the choice is not a doubtful one between these two possible methods of verbal recall.

3. Bearing in mind, therefore, what alone can be the real nature of an "articulate recollection" such as Professor Bain speaks of, I think it will not be difficult to show, in the first place, that what he considers to be the purest exercise of this sort of recollection is not such in reality. Professor Bain says†:—"I will now give the instances that seem more or less to exclude auditory ideas, and to throw the stress of the acquisition on the articulate ideas. There are some strong facts of this character. Take the case of the child conning its lessons without being allowed to speak aloud. Here the only mental effects allowed, in the first instance at least, are upon the eye and upon the articulating organs. What, then, is the actual course described? The child looks at the sentence to be committed to memory, and then repeats it in a suppressed articulation: if there be a break, it refers to the book, and again repeats articulately till the string of words becomes coherent. Now, in such circumstances, one would say that the coherence takes place either in the visual circles or in the articulate, or most probably in both." Professor Bain also adduces the case of the "student who derives his knowledge from books not read aloud"; but, he says, "still more decisive than these is the fact that the deaf have been taught to speak." These it appears, are the strongest cases which Professor Bain can adduce as tending to throw the stress of our acquisition of words upon remembered articulations.

\* *Senses and Intellect*, third ed., p. 346.

† *Fortnightly Review*, April, 1869, p. 496.

And it is especially worthy of note here that, in the first two instances cited, even Professor Bain is obliged to admit that the visual impressions have a very great deal to do with the assimilation of the words. So far, I entirely agree with him; only I think that in these very instances the auditory perceptive centres would have been also called into play; whilst, he says, there is "neither proof nor presumption" that they would be. The boy who merely conns his lesson-book without speaking aloud, receives in his visual centres certain impressions which are then translated into Perceptions of the words seen, and one almost certain result of this elaboration would be, as I maintain, the revival of activity in certain functionally related cells of the auditory perceptive centres.\* In fact, the boy is now, for the most part, only reversing the association by which he was taught the meaning of the visual symbols. And in the case of words with which he has been previously unacquainted, it seems to me almost certain that he would have to conceive some suitable sound-combination to represent them, and that then he would subsequently make his articulatory efforts in accordance with the sound-conceptions. A close investigation of actual and recorded cases of "Aphasia" has convinced me that many of the phenomena which they present are explicable only upon the assumption that in the state of health the most complete interaction and functional relationship exists between the visual and the auditory perceptive centres, but that the acts of speech are prompted almost entirely by incitations emanating from the latter. As evidence, I would call especial attention to the case of Dr. Hertz, quoted in one of my previous papers,† though it must be confessed that a consideration of this case, and more especially of that recorded by Dr. Hunn, which I have quoted in the next page, seems to justify the opinion that the motor stimuli inciting the acts of articulation during the process of *reading aloud* may pass off directly from the visual centre.

\* The boy would almost certainly derive an advantage from articulating the words of his lessons, instead of merely conning them. He thus supplements the visual impressions by conscious auditory impressions, and at the same time allows these sense impressions to take a firmer root, as it were, in his system, by strengthening the organic connections between his auditory and visual perceptive centres and the parts upon which the motor acts depend. But since articulation of some kind is so generally associated with reading, and since after we have once acquired the faculty of speech, we do usually embody our thoughts in motor acts, and not stop at the mere primary sense symbols, it does not seem to me so wonderful that we should be unable to remember "a passage read, either purely as a printed page seen by the eye, or purely as suggested ideas of how it would sound if pronounced." But, notwithstanding this, under what form does Professor Bain think Aphasics are enabled to recollect what they have heard or read?

† *Brit. and For. Med.-Chir. Rev.* Jan., p. 219; and see also April No. pp. 481-487 inclusive.



But the instances which Professor Bain has cited, although in his view they might be conceived to show that auditory impressions had nothing to do with the recall, do not in the least establish what he wishes to prove—namely, that we may recollect mere trains of articulations only.\* The instances mentioned would simply go to establish, if his view were correct, that articulations in these cases were automatically incited by revived visual rather than by auditory impressions—a view which I should be much more inclined to admit, than that we recollected words as remembered articulatory movements simply.

And I will now endeavour to show that, in fact, in the case of the deaf who are taught to speak—the instances which Professor Bain thinks most confirmatory of his theory—precisely such a relation between visual signs and articulatory acts does exist as in ordinary individuals obtains between auditory signs and articulatory acts. So that these cases, instead of favouring his view, are really strongly confirmatory of mine. The deaf having lost the use of their sense of hearing, the sense of sight takes its place; so that in them revived sight-impressions are the verbal symbols which automatically incite the muscles of articulation, as they have previously done those of the hands and fingers. I have lately visited the London Asylum for the Deaf and Dumb, and, through the courtesy of the Principal, Mr. James H. Watson, I was made acquainted with the method of tuition, and heard many very interesting results of it. The pupil must, as a preliminary, be able to interchange ideas with his teacher by means of the finger-language; and the pupil is also mostly able to write himself, and to read the writing of others. There are perfect means of communication, therefore, through visual symbols. The pupil understands that he is to be taught an additional mode of communicating his own thoughts, and of receiving those of other people. In order to utter a given sound, he is instructed (through the sense of sight, by watching his teacher) how to place his organs of articulation, lips, tongue, etc., in order himself to produce such a sound, which he is led to understand corresponds with a certain previously known and well understood visual symbol. This is the essence of the process—the building up of new combinations be-

\* In his article (*loc. cit.*) at p. 495, Professor Bain is contending with a shadow. He seems to suppose that I have denied the possibility of an adhesion between muscular acts—a possibility which I never doubted in the least. I only denied that we were *conscious* of all the details of muscular action by which an utterance was effected; of course, in the articulate utterance of any moderately long word, I know that cell-connections permitting “truly muscular adhesive groupings” must have been formed. But that is a very different thing from believing that whole strings of these combinations can, as it were, call themselves into being, and ideally constitute our verbal symbols.

tween known and commonly used visual impressions and certain muscular acts which the pupil is taught to consider as the equivalents of these. The pupil's education is carried on, of course, on *phonetic* principles; so that he has to be instructed that, although certain words are spelled quite differently, and have different meanings, they must be articulated in precisely the same way. In this manner the education is slowly and laboriously proceeded with, until the most perfect adhesion exists between the several visual symbols with which the deaf child thinks and the muscular acts necessary for their articulation. The education, however, is much slower and more labourious than that of an ordinary child, where articulatory acts are incited by auditory impressions. And why is this so? Simply because the child lacks the auditory sense which would enable him easily to modulate or correct the sounds produced, and possesses instead, as his only means of knowing what he has achieved and of guidance, nothing but those much more vague and indefinite feelings of movement emanating from his articulatory organs. Thus the difference in facility of educating a deaf child, and of educating one who is equally intelligent but not deaf, comes to be a sort of gauge of the relative superiority of auditory impressions over these other more indefinite symbols derived from the mere feelings of movement.

Then a very convincing reply can be made also, from an observation of deaf persons, to the argument adduced by Professor Bain, to the effect that, because in thinking we do often mutter or make use of suppressed articulations, this is of itself an evidence that such articulations, ideal or actual, are an essential part of the process. The reply is this: Where, as in the case of the deaf, the mental symbol is more dissociated from the muscular act destined to give the word a physical expression (because the two are not so frequently and so habitually conjoined), then the processes of thought do seem to go on entirely by means of the visual symbols, and it is quite a rare thing for the mere thought to give rise unconsciously to either of its accustomed physical manifestations: it is neither associated with finger-movements nor with muttered articulations. This is perfectly in accordance with opinions which I have previously expressed concerning the significance of the half suppressed articulations which occasionally accompany thought, and of the finger-movements that were seen during dreams in the case of Laura Bridgeman. These are to be regarded as accidental, and not as necessary accompaniments.

Thus Professor Bain's most decisive example in favour of "articulate" modes of verbal recollection in reality affords no proof of his hypothesis

whatever—visual\* symbols simply take the place of auditory symbols, and the articulations themselves are still secondary and automatic.

But yet another argument against the view that words are recalled and retained in memory as “suppressed articulations” is to be drawn from the case of the deaf. It is now a well known and undisputed fact that, if a child of five or six years old becomes absolutely deaf, it soon loses the power of speaking altogether, however good the power of speech may have been before. Mr. Watson assured me that such a result would inevitably follow, if no special education were resorted to. And then the child is reduced practically to the same condition as the child who has been deaf from birth, and he can be reinstructed to speak only in precisely the same way as the congenitally deaf child. Now this fact is quite in harmony with my view of the physiology of speech; but it does not seem very easy to make it accord with the doctrine that words are remembered even in part as articulations. The capacity for receiving impressions of the “feelings of movement” remains unimpaired; these must, moreover, be capable of ideal recall as much as they ever were; and yet the child gradually ceases to talk, precisely as it would do if these feelings were not at all concerned in the revival of the verbal symbols. And then it cannot be supposed that this gradually diminishing power of speech is owing to the lack of guidance in the acts of articulation usually derivable through the ear, because, when these same individuals have afterwards been instructed to build up their articulatory efforts upon sense-stimuli of a different kind—those of sight—they do succeed then in speaking through the sole guidance of these feelings, which would have been supposed to be inadequate for the purpose.

Such are the reasons against the presumed articulate mode of recur-

\* After the opinions which I have already expressed, it is almost needless for me to say that I do not accept Professor Bain's views concerning the sense of sight. He says:—“In all that regards visible movement and visible form, the muscular consciousness, it is now contended, is the indispensable element; the optical sensations merely guiding the movements. Naked outlines, as the diagrams of Euclid and the alphabetical characters, are, to say the least of it, three parts muscular and one part optical; their retention is supposed to depend upon the adhesive property of the ocular muscles and their nerve-centres, and not upon purely optical circles.” In his larger work, Professor Bain has fully expounded these views. I think he lays far too much stress upon this “muscular consciousness”; that this exists only to a very small extent; and that the impressions so derived are passive, and not “active,” as he supposes. Much of the information which he attributes to “muscular consciousness” is, I think, only mediately derived through the activity of the muscles. The activity of the ocular muscles does enable us to derive notably more varied sense-impressions of a purely optical character; and all these, more or less associated with vague passive sensations, emanating directly from the muscles, and knit together by the multitude of inferences which we learn to draw instinctively during the education of this sense, give us, I think, that knowledge which Professor Bain seems to suppose we derive directly through our “muscular consciousness.”

rence of verbal symbols, and I will now give those which I think tend to strengthen the supposition that words are recalled in thought as revived auditory symbols.

4. Auditory impressions do possess in the highest degree those characters of definiteness and intellectual retentiveness which would eminently fit them for such a function; whilst the very reverse is to be said concerning our mere feelings derived from articulatory movements.

5. Then I think it is another argument in favour of the same view, that, if we suppose the auditory impressions to be the forms under which words are revived for thought, we do suppose the continuance of a method which alone was possible (so far as words are concerned) during the first twelve months of the child's life. I am more inclined to believe that fresh acquirements after this date would take the form of additional and superadded associations, rather than that the early modes of thought should be reversed or superseded. I shall certainly not be dissuaded from this view if Professor Bain is unable to find any better argument against it than the stigmatising of such a continuance as an instance of "blind conservatism."

6. Lastly, it is no argument against the views I have been advocating, to say that we have, ordinarily, no *consciousness* of these auditory impressions which I presume to be revived.\* If we analyse carefully what takes place when a person *improvises* music on a piano, and consider all the previous education which has been gone through, we shall be almost bound to come to the conclusion, as I have elsewhere stated, that, "by dint of long practice and habit, there must have grown up in the person that sort of knowledge, which now seems intuitive, as to how, when, and in what manner, to touch the various notes, so as to make the instrument produce the desired sounds. These muscular movements follow as rapidly, and with as much precision and delicate adjustment, as do the muscular movements of the larynx, lips, and tongue, in the acts of articulate speech. Both alike follow automatically; and both sets of muscular movements, as it seems to me, are set a-going by the agency of revived impressions in the perceptive centres pertaining to the sense of hearing. So that as quickly as the person improvising music conjures up in imagination the idea of the several parts composing the piece, this translates itself into the suitable muscular movements necessary for educing the sounds; the different steps of the process following so rapidly upon one another, that the performer himself is perhaps conscious of giving no separate attention to the ideas of the several sounds as they become nascent, and which are so immediately trans-

\* See Professor Bain's paper in *Fortnightly Review*, p. 497. Does Professor Bain know that Aphasics can add, subtract, and multiply, without articulating?

lated into muscular movements of the greatest precision. Still it would seem that such a succession as this must occur—that the cerebral hemispheres must be necessary for such a process; and yet here, too, there is involved a reflex action precisely analogous to that occurring in the process of speaking. If it can occur in the one case, therefore it can occur in the other. Here, however, the ideal sound-combination is, so to speak, the subject of thought itself, and therefore should arouse consciousness more as its successive parts become nascent and act as stimuli, inciting to the rapid and precise secondarily automatic movements performed by the hands and fingers.\* If, however, in these cases, the several parts of the ideal sound-combination do cause such a slight and transient mental impression as to be almost unnoticed as distinct phases of consciousness, can it be wondered at that, in the process of speech, there should be so little evidence of the consciousness of words becoming nascent as remembered sounds?"

\* More than in the case of speech, where the words, when revived in the auditory centres, only come up as the *symbols* of thought, and would, therefore, naturally occupy an even smaller amount of attention, inasmuch as the person is engrossed with the thought itself. But in the case of the person improvising music, the several ideal sounds are at once both thought and symbol.