REMARKS

ON

THE "MUSCULAR SENSE", AND ON THE
PHYSIOLOGY OF THINKING.*

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ONE of the cases carefully recorded by Dr. Bazire possesses the greatest interest with reference to the "muscular sense". It is related in detail by him (Trousseau's *Lectures*, transl. by Bazire, Pt. I, Case VI, p. 193), 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 coordinating 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 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 is 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 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 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 (*Der Tastsinn der Fuss-sole als Aquilibrationsmittel des Körpers beim Stehen*, Tübingen, 1862, and quoted by Jaccoud, *loc. cit.*, p. 472) 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 of movement and jerking character which is met with in advanced cases of locomotor ataxy. The different results produced by mere cutaneous and deep anæsthesia, 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 (*loc. cit.*, p. 162), 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 (see note *), as opposed to the lower animals, the preservation of 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 (*loc. cit.*, p. 162) 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 coordinating fibres of the cord also damaged to a certain extent. A somewhat similar though less marked instance has been recorded by Dr. Bazire (*loc. cit.*, p. 213.)

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

* Probably the effects are less here than when the deep sensibility is impaired also, since Claude Bernard found (*Léçons*, vol. i, pp. 251, 254. Paris: 1858) that frogs, the four limbs of which had been skinned, so as to deprive them of 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.

† 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.

* Continued from page 396 of number for May 1st.

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

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 represents 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" (p. 82). 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 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, which he thinks is concerned with 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 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 mere passive modes of sensibility, as contradistinguished from those presumed active modes of sensibility 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 this 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*." With this latter sentiment I entirely agree: 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 nothing would remain by which we could become conscious of our own muscular 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 cerebrum'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 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 (p. 77) 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 occasioned by 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 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

* 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 this 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.

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 Mr. Bain and others appear to think them. They are thus described by Todd and Bowman (p. 431): "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 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 are tested 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 is it 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 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 due to the common sensibility of the muscle testifying to its own organic conditions, and that these sensations may well be part components of the 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, than when these latter were appreciated alone and apart from all disturbing influences. Taking these considerations, 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.*

[To be continued.]

THERAPEUTICAL MEMORANDA.

[Under this head, we shall publish from time to time, as materials accumulate, short records of remarkable cases in practice which are sufficiently rare, interesting, or instructive, to deserve record, but do not call for lengthened statement or comment. Brevity and point should be the valuable characteristics of cases forwarded for this column.]

NOTE ON THE DOSE OF IODIDE OF POTASSIUM IN SYPHILIS.

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IN sypphilis of long standing large doses of iodide of potassium are, I believe, imperatively necessary. In America, where this drug is looked upon as almost a specific for sypphilis, it is customary to give it in very much larger doses than we have any idea of, and often with the utmost benefit. Two cases have recently come under my notice, in which I have given it in large doses with great effect, after it had been used in small quantities for some time. In the first, sypphilis had been of two years' standing, and there was considerable eruption and ulceration both of throat and legs. The patient had been under treatment since the first appearance of the disease, and was, as the date at which I saw him, taking three grains of iodide of potassium thrice daily. I put him at once on one drachm daily in scruple doses, the improvement was most marked and rapid, and was continued for about six weeks; he has since continued perfectly well. In the second case, sypphilis was of seven years' standing; and, when I saw the patient last August, there was most extensive necrosis of the floor of the nose and vomer, ulceration perforating the soft palate, and a complete prostration of vital power, the patient being anæmic and very low in flesh. He had for months together taken iodide of potassium in small doses, also mercury, both inwardly and in baths, tonics, etc. In this case, I began with half-drachm doses, carried up to a drachm and a half, thrice daily, in the course of a month. Under this treatment, in less than two months, he gained nearly twenty-eight pounds in weight, his general health improved so much that he was able to take horse-exercise, the necrosed bones separated, and the ulceration healed entirely. In December he was able to return to his duties, though still continuing the iodide of potassium.

* 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.

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