putations was thus 1 in 2.3, instead of 1 in 2.9, as he states it. In country practice, out of 618 legs amputated, 81 died, or 1 in 7; or the mortality was between three and four times less in rural practice than in the practice of St. Bartholomew's Hospital.

III.—*The Surplus of Deaths after the Four Major Amputations of the Limbs in the Practice of St. Bartholomew's Hospital, compared with Rural Private Practice.*

The number of patients who died after limb-amputation in St. Bartholomew's from 1863-68 was, as we have seen, 81 out of 214 operated upon. But, according to the statistics of rural practice which I have collected, out of every 214 operated upon only 23 died.

Therefore, the SURPLUS of mortality in large hospitals, as compared with rural practice, in private habitations, and cottages, is thus 58 out of 214 operations; a difference at first sight so incredible, and involving such an appalling sacrifice of human life as to appear to me to call on all of us loudly and earnestly for the most anxious inquiry and rectification. I make this remark under the belief that this excess of mortality is mainly if not entirely traceable to hospitalism and its effects. Can Mr. Coote suggest any other rational cause or causes to account for the tremendous difference in the results?

Or let us state the problem otherwise. Out of 2,098 limb-amputations collected by me—performed in country and provincial private practice, and chiefly in the homes and cottages of the poor—227 died. But, according to the statistics of St. Bartholomew's Hospital, if these 2,098 amputations had been performed upon patients within its walls, 794 lives would have been lost among those subjected to operation, instead of only 227; or in this given quantity of limb-amputations the hospital surgeons in their practice would have a SURPLUS or excess of 567 deaths greater than the rural surgeons would have met with if they had been the operators in the same number of cases.

Mr. Coote owns that my statements respecting the high rate of mortality in large or in Metropolitan hospitals is sufficiently important to "demand either an answer, or else some change in our hospital system". I submit that Mr. Coote's first alternative has failed entirely; and I quite agree with him as the second alternative, viz., the necessity of "some change in our hospital system." If, for example, the 214 patients who were subject to limb-amputations in St. Bartholomew's, from 1863-68, had been all sent out of the rich palatial hospital in which they were placed, into villages or cottages—and perhaps from the city into the country—50 of their lives would, I believe, have probably been saved. But even if the 25, or one half only, of these human beings were preserved by such a change, are we not, by every principle of humanity and sympathy, bound to try to effect it? For our hospitals should not be charities merely in name, but charities in truth and reality.

Mr. Coote holds that there is a necessity for us to "aggregate our sick poor in large hospitals." Most assuredly there is no such necessity; and whenever the painful peril and needless sacrifice of human health and life connected with our present hospital system are sufficiently impressed upon the mind of the public and of the profession, the system itself will be altered; and that without any great difficulty. It seems to be Mr. Coote's opinion that small or detached hospitals are attended with diverse inconveniences. Latterly, in America, some hospitals, much larger than any in London, have been constructed upon the cottage or village and single-story principle, with the administrative, cooking, and other departments in their centre; and—without discussing for the present the question of their great salubrity—they are averred to be cheaper in their original construction (as there are no strong foundations and heavy supporting walls required); cheaper in their administration (as there are no higher flats to mount and carry up to); and far better ventilated, as single-storied buildings confessedly are. Besides, such hospital-forms and buildings seem to save expenditure by shortening the duration of the hospital residence of the inmates; the poor patients recovering more rapidly and steadily in them; for, instead of being continually imprisoned, as it were, in their bed-rooms or wards, they can, during their convalescence, always easily reach the ground and open air without the dire necessity of descending and again ascending weary and repulsive flights of stairs.

ARREST OF HÆMORRHAGE FOLLOWING EXTRACTION OF TEETH.—S. Lawrence, of Lowell, Massachusetts (*New York Medical Record*) usually applies persulphate of iron in excessive bleeding; and, if that do not prove successful, a piece of lint or cotton is spread out, and as much of dry plaster of Paris as will go into the cavity is placed on the cotton, so as to make a ball or pellet, which is forced into the alveolus with a blunt instrument. The finger or thumb is then placed upon it until the plaster sets and the cavity is hermetically sealed.

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

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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 revived 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 certain 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. Pre-eminent 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.

MUSCULAR SENSE.

Much speculation and inquiry has of late years been devoted to the elucidation of this most interesting and important sense-endowment. After a consideration of some of these, Professor Bain (*Senses and Intellect*, 3rd ed., p. 76) 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 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 (*Ueber die Veranlichung des Rückenmarksnerven*, Heidelberg, 1845; also *Brit. and For. Medical Review*, April 1845, pp. 558-575) 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-Séquard (*Lectures on the Central Nervous System*, 1860, p. 9) 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-Séquard 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 (*Ricerche Sperimentale sopra i Nervi*, Pavia, 1834), after operations of this kind upon certain animals, 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 (*Lec. sur la Physiolog. et la Patholog. du Syst. Nerveux*, Paris, 1858, t. i, p. 250) in experiments upon frogs, and by Leyden (*Die graue Degener. der intern Rückenmarkstränge*, Berlin, 1863) 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 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. But, on the whole, for the reasons about to be mentioned, Professor Bain seems to think it more probable that the latter supposition should be 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 (*Physiol.*, t. ii, p. 480. Paris, 1859, p. 195) 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 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 (*Mém. sur la Paraly. du Sentiment d’Activité Musculaire*, Paris, 1855), came to quite an opposite conclusion. He said (*Traité des Paralysies*, 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 con-

ducted 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 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. 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 as to the 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 this subject. He says (*Traité des Paralysies*, p. 194): "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 impulse, 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, the "feelings", which are presumed to be their "concomitants", are found wanting.

Although Locomotor Ataxy is now such a well-recognised 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 of movement in 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 recognise 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 insensibility 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 this latter sense is to be estimated by the individual's ability to discriminate between 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 anaesthesia solely—a view which has been strongly advocated by Trousseau, and lately also by Dr. Clifford Allbutt (BRIT. MED. JOURNAL, Feb. 20, 1869), Trousseau even doubting the very existence of the muscular sense; whilst others—and amongst them Dr. Wilks—are disinclined to believe that this anaesthesia 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 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 in respect to 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 (*Les Paraplégies et l'Ataxie du Mouvement*, p. 672), 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 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 (*Beitrag zur Lehre von den Tabes Dorsalis*, Tübingen, 1864), 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.

(To be concluded.)

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

* 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 Caesar Scaliger, a physician, (1557), and another philosophical physician, Cæsalpinus (1569) of Arezzo.

† 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 these "feelings" are not at all dependent upon the "muscular sense."

EAST LONDON CHILDREN'S HOSPITAL.—The annual general meeting of the East London Hospital for Children, Ratcliffe Cross, took place on Wednesday, Lord Enfield, M.P., in the chair. The report testified to the great usefulness of the charity, and to the economy with which it had been managed. It was proposed to open a ward for the isolation of contagious diseases; and it was unanimously resolved to take immediate steps to obtain sufficient funds for the purpose.

ROYAL INSTITUTION OF GREAT BRITAIN.—Dr. Michael Foster has been appointed Fullerian Professor of Physiology, *vice* Thomas Henry Huxley, LL.D. Edin., F.R.S., resigned.