mates have been shown to be unreliable in themselves and in the methods by which they have been reached. It is a part of the theory of podalic extraction in contracted pelvis, that the force exerted there by the accoucheur exceeds that of the best labour-pains, and it is interesting to observe that the experiments confirm at least this part of the theory. At the same time, it is necessary to realise that the excess of force in favour of podalic extraction is truly very limited.

I am sure that most accoucheurs, not being familiar with dynamometrical experiments, will regard each his own available muscular power as being far more than he can dare to use in podalic extraction; that is, far more than a hundredweight. No doubt he is right, in so far as that he can easily lift a hundredweight, perhaps even two; but that is a very different and a very easy matter compared with applying a dragging force of a hundredweight in a special direction. Probably no ordinary man can stoop over a bed and pull in a direction across the front of his body, or in what would now be called a coronal direction, with a force of a hundredweight, and this is what many attempt and erroneously fancy they can do. Numerous failures arise from practitioners erroneously estimating the amount of force they are exerting, and still more from erroneously estimating the amount of extractive or dragging force they are exerting on the foetus, since it is common to confuse the amount of force exerted with the amount made efficient as a dragging force; and it is the direct dragging force that may be allowed to rise to about a hundredweight. If a proper judgment be formed of the great difficulty of using a dragging force of a hundredweight, except when the accoucheur is favourably placed, it will be easily understood how important it is to have the patient in a convenient position; for the practitioner must wield his great force skilfully, and gently, and easily, if he is to do his work in the best way.

In the foregoing experiments, it will be observed that the cervical part of the vertebral column invariably gave way, and the dis severed vertebrae became widely separated from one another before the whole neck yielded and decapitation was effected. While the vertebral column yielded when the weight or force was about 105 lbs., a further addition of about 15 lbs., forming a total of 120 lbs., was required to produce decapitation. From this, it is evident that at first almost the entire dragging force is exerted through the vertebral column; that the tensile strength of all the tissues of the neck is not simultaneously and equally tried, and that the vertebral column is weaker than the combined soft parts. The vertebral column yields with a snap or jerk, and immediately a marked elongation of the foetal body occurs. Of both of these events, the accoucheur can scarcely fail to be aware, even if he did not expect them, and this information is important practical direction to him, if he wish, as he almost certainly does, to avoid decapitation. The yielding of the column of bones does not restrict the force he can exert; for he can generally, not always, apply more, to the extent of about 15 lbs., without producing decapitation. But the events referred to form a distinct warning to him as to how near he has reached the amount of force available by dragging the foetal body. He cannot, when they have occurred, pull with greatly increased force, even for half a minute at a time, without incurring the imminent risk of decapitation.

Another point is clearly shown by these experiments; viz., the sufficiency of a single limb for dragging, so far as mere strength of materials is concerned; and this determination has many evident practical bearings. In the trials narrated, we uniformly attached the apparatus and its connected weights to one leg just above the ankle. The parts were protected by a piece of leather wrapped around them from the cutting sort of edge presented by the cord used to tie on the apparatus. One leg always proved stronger than the whole neck. It is interesting to remark, with reference to the leg, that, just as in the neck, the bony column was weaker than the other combined soft parts; for, during the dragging, various cracks or snaps in the limb were heard, and were subsequently found to have been produced by separation of femoral or other epiphyses. One limb always proved more than sufficient for the neck. Whatever advantages may or may not arise from using the Prague seizure, one thing is sure, that, with a view to merely having sufficient strength of materials, it is not required; one leg is, in this point of view, sufficient. It is plain that this is not exactly proved by the experiments, for in them the weight acting on the neck exceeded that acting on the ankle by the greater part of the weight of the body; but the ankle will not have this advantage, except when the body is dragged downwards vertically, or nearly so, which is not done in practice.

I have spoken incidentally of the comparison between the power that may be exerted by dragging the child in podalic extraction without compromising its life, and the power that may be exerted by dragging the child by the forceps. While the limit of the former power is easily

ascertained and has been just stated as about 100 lbs., it is no part of my present object to determine the higher limit of the latter, and it is a far more difficult matter. But there are various ways of showing that the latter is considerably higher than the former, which, as I have already said, is not the universal or even the general belief. I have made many experiments in connection with this subject, but I shall not here enter on them, satisfying myself with giving some easily appreciated evidence.

An accoucheur can decapitate a child by the efforts of podalic extraction; in other words, his available power overcomes the whole tensile strength of the foetus. Few men pulling in an advantageous manner find much difficulty in applying a dragging force of 120 lbs.; but it is quite possible to apply a forceps to a foetal head, at the brim of an actual pelvis or of one artificially constructed for experimental purposes, so as to defy the utmost efforts of the most powerful accoucheur. These iron hands can be made virtually of any strength, and the grasped spheroidal foetal head is firm enough to resist such quasi-cephalotriptic compressing or otherwise destroying force, as they exert in maintaining their hold against a dragging force greater than can be used in podalic extraction, because greater than is sufficient to rend in twain a foetal body.

A CRITICAL EXAMINATION OF DR. MUNRO'S VIEWS ON MALARIAL FEVERS AND CHOLERA.

By W. C. MACLEAN, C.B., M.D.,

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THE fourteenth volume of the *Army Medical Department Reports*, recently published, contains a paper on "Malarious Fevers and Cholera", by Deputy Surgeon-General Munro, C.B., head of the Medical Branch in the office of the Director-General, on which I feel constrained to offer some observations. It would not, perhaps, be difficult to assume the office of a critic, either anonymously or under a *nom de plume*, and thus escape thrusting myself in *propria persona* before the profession. In the present case, such a course, it strikes me, would be neither fair to Dr. Munro nor otherwise suitable. Dr. Munro does not write anonymously; he puts forward his deliberate opinions in an official publication with all the weight he derives from his official position and his deservedly high reputation in the Medical Department of the Army. Moreover, the issues he raises in his paper are not matters of merely speculative interest; they involve questions of the gravest practical importance in medicine and sanitation. Opinions coming from such a source are entitled to respectful consideration and fair and responsible criticism. To what Dr. Munro has written I have given careful consideration, and I propose, in the following observations, to discuss the issues he has raised, I hope with the courtesy and fairness the author has a right to expect, and with the fulness the importance of the subject demands.

In all controversies the first thing to be done is to understand clearly, and to state exactly, the matter in dispute. In this case there is no difficulty, as Dr. Munro has plainly put the difficult thesis he has set himself to maintain with sufficient distinctness. It is—1. That there is no such poison as malaria; 2. That so-called malarial fevers, cholera, yellow fever, and heat-apoplexy, are merely different manifestations of one and the same disease, the difference being one not of cause, but of degree; 3. That the cause of the above diseases is "certain electrical conditions" (the nature of the said conditions is not explained), which are the outcome of "heat acting on moisture".

It will be seen at once that Dr. Munro admits the action of two agents which have always been thought active in evolving malaria from the soil; but, instead of agreeing with those who believe in the existence of any poison or "telluric influence" to which the convenient name of malaria has been given, he attributes the disease-producing agent to the above named electrical conditions acting with paralysing effect on the sympathetic nervous system.*

* It is not denied that the sympathetic system of nerves is implicated in grave malarial fevers, and in specific yellow fever in particular. Many of the most notable objective symptoms in the latter point to this implication. Bellot the younger even insists that this can be demonstrated pathologically *post mortem*. The other nervous centres are implicated too. A pigmented condition of the brain is one of the most familiar of the morbid appearances in malarial fever. It is for Dr. Munro to connect the morbid appearances with the "electrical conditions" of which he writes so positively.

Lastly, whether in any case we have an intermittent or a remittent fever, or yellow fever, or cholera, or heat-apoplexy, depends on the extent to which this paralysis has taken place.

In an Introductory Lecture delivered by me at the opening of the present term at Netley, I called the attention of my audience to the fact that, in India at the present time, a school of ingenious writers has arisen, who, forsaking the fruitful but laborious method of bedside study, seek to advance practical medicine by the apparently more attractive and certainly more easy method of framing theories of disease, basing wide generalisations, and even deducing rules of practice from them of the utmost importance not to individuals only, but to whole communities. A better example of this it would be hard to find than in the thesis which Dr. Munro set himself to establish.

Turning first to the question of malaria, readers of our author's paper will be puzzled by the manner in which this nonentity is sometimes spoken of. Again and again, we are told there is no such poison as malaria, yet, at page 272, writing of ozone, the author says, "that, in excess, it is not only destructive to human life, but equally so to malarious poison". If there be no such entity as malaria, it is hard to see how ozone or anything else can destroy it. There are, indeed, throughout this paper other contradictions equally puzzling. Thus, at page 269, Dr. Munro says: "How heat, or different degrees of heat and moisture, act so as to produce disease, I do not know; whether by the production of malaria poisonous to human life, or by direct influence on the nervous system;" while, a little further on, we meet with a confident statement to the effect that the said "heat and moisture" generate "certain electrical conditions", which "conditions" paralyse the sympathetic nervous system, and so induce the hitherto deemed distinct diseases named above. Again: typhus and typhoid fevers are spoken about in the text as two distinct diseases, apparently in the sense commonly accepted by pathologists; but, when this is so, a note is added to the effect that "typhoid enteric fever is not included in this remark"; but what kind of fever the author means by typhoid, is nowhere explained.

It is, I confess, wearisome to have to discuss at this time of day the question whether or not the soil in certain places, and under certain sufficiently familiar conditions, is capable of giving off a poison capable of producing a class of fevers with phenomena so remarkable as to mark them off from all other febrile disorders by a line of demarcation so broad, clear, and distinct as to have been recognised by tolerably intelligent people in all ages; while pathologists have, in addition, with an unanimity not always observable in medical questions, traced to the same cause, even when it has operated without inducing the remarkable phenomena of paroxysmal fever in any of its degrees or forms, certain degenerations in the fluids and solids much too familiar to need mention here; and not only so, but to be traced and demonstrated in the organs of the foetus, when the mother has lived and died under the influence of this poison.

Dr. Munro, like others who share his opinion, seems to attach importance to the undeniable fact, that the existence of malaria cannot be demonstrated, I suppose as a chemist demonstrates oxygen. But of the contagium of how many diseases can this be done? We know that a child exposed to the contagion of scarlatina gets scarlatina, and not measles; we know that the two diseases have certain features in common; but, from the observation of ages, we know that the poison which causes one is incapable of generating the other. It is probable we shall never know the exact nature of, or the difference between, the two specific poisons, more than we do now; we recognise their different effects when we see them, and that is all. It is almost humiliating to have to urge considerations so familiar, but it would appear they are sometimes lost sight of.

I must be forgiven also if I venture to remind those who hold with our author, that there is no such earth-born poison as malaria, that the word carries no other meaning beyond what the term plainly implies; physicians who use it pretend to no more exact knowledge of its nature than what they gather from its effects. Those who dispute its existence do not appear to see the force of the argument derived from the accumulated experience of countless observers and sufferers of different races in all regions of the globe where paroxysmal fevers prevail. This cannot be disposed of by vague phrases and statements about electrical conditions which are not supported by evidence of a kind to satisfy scientific inquirers, and which, apart from this, would fall with little effect on the ears of those whose hard fate it is to struggle for existence with this deadly enemy of the human race.

I decline altogether to reprint what is to be found in every text-book on medicine; viz., a list of places notorious for insalubrity from malaria. It is for Dr. Munro, and those who think with him, to explain in intelligible language what it is that has given such places the bad reputation that for ages has clung to them, and, in language equally

clear, to explain the changes, be they in the "electrical conditions" or otherwise, that has rendered many such places in our own country, among others, once notorious for the prevalence of paroxysmal fevers and their sequels, now healthy. We may fairly ask those who hold the non-malaria theory to explain why the terrible remittent fever which scourged Hong Kong was unknown there until the soil was turned up and exposed for the building of the city of Victoria—a fact to which I can speak from personal knowledge; why paroxysmal fevers broke out in the environs of Paris when the ground was broken up during the construction of the fortifications in the time of Louis Philippe; how the same thing happened, as recorded by Trouseau and other observers, in the same city during the great excavations for building and other works during the Second Empire, and in other examples which might be multiplied indefinitely. If the soil did not give birth to a fever-generating poison in all or any of the above cases, what did? It is for Dr. Munro and his non-malaria friends to give us a rational explanation of the facts on their own principle.

When the authorities charged with the responsible duty of fitting out the expedition to the West Coast of Africa were devising the best means for the protection of the troops from malarious poison, Dr. Oldham, of the Bengal Army, addressed a letter to the *Times*, assuring all concerned that they were disquieting themselves in vain; they might take his word for it, there was no such thing as malaria. We have not since seen any attempt on this gentleman's part, or that of any of his followers, to explain the formidable paroxysmal fevers, with their serious complications, which the medical officers of that expedition had to treat on the spot, and with which we at Netley, in their hardly less serious sequels, had to deal; nor how it came about that many of the soldiers and seamen, and healthy fresh young men of the Army Hospital Corps, returned, after a few months' service in that pestiferous climate, looking like men who had served for twice as many years in the Peshawur valley. Will the "certain electrical conditions" of Dr. Munro, or the chill-theory of Dr. Oldham, account for it? or must we fall back on the saying of Dr. Parkes, that, when we speak of an unhealthy climate, we mean a malarious one? Certainly this old explanation better accords with a fact mentioned to me in the most spontaneous manner by many of the sufferers, both officers and men; viz., that when in the course of their fevers they sweated copiously, the towels with which they wiped their persons and the sheets in which they slept, when they had them, gave out the identical fetid odour of the malaria-charged soil.

While, on the subject of the West Coast of Africa, it is allowable to ask if such a cause as that put forward by Dr. Munro, or the chill-theory of Dr. Oldham, or both put together, can explain, to the satisfaction of any unprejudiced truth-seeker, the crowded graveyards of Sierra Leone, that place of fatal memory to our race, the mention of which fills the mind with sadness, where whole families moulder away, the average duration of whose lives in that climate hardly exceeds a year; or the castle-yard of Cape Coast Castle, described by my friend Dr. Fleming "as almost covered with grave-stones".

Nor can I resist inviting the attention of all who are sceptical as to the possibility of the soil at certain times and seasons, and under certain conditions, giving birth to a fever generating poison, to the contents of a deeply interesting letter from Colonel Gordon, C.B., R.E. (well known for the splendid service he rendered to China and to humanity in the masterly suppression of the merciless Taeping Rebellion), to his brother-in-law, Surgeon-Major Moffitt. Colonel Gordon is now engaged in completing a work begun by Sir Samuel Baker; viz., the suppression of the slave trade along the upper waters of the great river whose source has been the subject of so much speculation and such anxious search. He writes what he calls "a medical letter" from the "entrance to the Soubate", in which he describes a climate so pestiferous, that that of Cape Coast Castle appears by comparison salubrious. It is simply death to face it during what Colonel Gordon calls the "crisis" of the rains as the day declines. "The blacks, natives of the place, as the evening closes, retire to their houses, close their doors and windows, and light wood-fires, giving out abundance of smoke." "The moment night closes", adds the Colonel, "you experience a sort of indisposition, which seems inexplicable; you feel a sick sensation, which ceases if you get under cover with a wood-fire giving off a good deal of smoke". In another part of the letter, he recurs to this indispensable precaution, without which he would rather sleep in a latrine; "the stench is palpable, and your mouth in the morning is as if you dined off copper-filings". Towards morning, the sickness passes off, until the dreaded night returns. One is tempted to add, that England should be rich in officers like Gordon, when she can afford to risk his valuable life in the service, not of his country, but of a satrap of the Sultan of Turkey.

Before passing from this part of my subject, I have one more remark to make. Dr. Cornish, the Sanitary Commissioner of Madras, insists

on the fact, "that fevers in India, taking one year with another, destroy twice as many lives as small-pox and cholera, and all other epidemic diseases, put together". This being so, the cause of such fevers becomes a question, not merely of interesting and curious speculation to medical men, but one of the gravest importance to a teeming population and to the nation that has assumed the responsibility of governing them. The Government of India has at this moment a task imposed on it requiring the aid of statesmen, engineers, and sanitarians in the first rank of their respective sciences. It is nothing less than to contrive well considered measures to prevent the recurrence of famines, and to stay the progress of those fevers, the destructive agency of which Dr. Cornish has so forcibly described. I repeat, "well considered efforts", for such they would need to be; for hitherto some of the irrigation works directed to prevent the first evil have not only failed, but have, by interfering with the natural drainage of certain districts, left them so saturated with moisture as to develop malaria to an extent that threatens to depopulate them. Let us speculate as much as we please on medical questions; but let such of us as are in a position in any, the smallest degree to influence, or distract, or embarrass the Government in the work they have to do, pause before we put forth theories, which, if acted on, may indefinitely delay the progress of sanitation. If the Government of India be tempted to embrace a theory that we in this country know would be very captivating to the minds of money-saving vestrymen, that fevers are the outcome of "chill" or "certain electrical conditions" of which we know nothing, and which lie out of the domain of human control, it would obviously follow that it is vain to contend with them.

Whether it be true or false that out of the soil, under certain conditions long recognised, a poison issues, which, for mere convenience sake, we call malaria, the belief has at least been fraught with good to mankind; for, just in proportion, in all places and in all climates, as we have carried out the measures believed to be antagonistic to the genesis of this poison, so have we diminished disease and death, and promoted human happiness and progress.

[To be continued.]

ON THE LOCALISATION OF THE FUNCTIONS OF THE BRAIN.*

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IN the short time allowed to me it is not my intention to read to you any elaborate paper on the localisation of function in the brain. I only wish to make a very few remarks, in order to bring before you more objectively, than by mere written description, some of the facts which I have published elsewhere, in the *West Riding Lunatic Asylum Reports*, and in a recent paper communicated to the Royal Society; and also to point out to you some of the bearings of the results of experiments on the lower animals in regard to the pathology and diagnosis of certain forms of brain disease. I presume you are all aware of the method of experimentation which was first introduced and applied by Fritsch and Hitzig, two German physiologists who experimented on dogs, and also of a similar method which I followed on a more extensive scale, not only on dogs but on several other of the lower animals, especially monkeys. It is chiefly with regard to the experiments on monkeys that I wish to speak to-day.

The experiments which Fritsch and Hitzig commenced, and those which I have made, have been repeated in this country and abroad by several experimenters; and I think that the main facts which are elicited by galvanisation of the brain are admitted by all. The interpretation of those facts, however, is a matter on which very considerable difference of opinion exists. Both Fritsch and Hitzig differ from the explanation which I give, as also M. Dupuy, M. Carville, Brown-Séquard, and Schiff of Florence. On the application of a galvanic or electric stimulus to certain regions of the brain, certain definite motor phenomena result. Hitzig and myself attribute these movements to excitation of the cortical substance of the hemispheres. Dupuy, who has repeated the experiments, and others who follow him, admit the definiteness of the movements which are caused, but they deny that they are dependent upon excitation of the cortical centres, asserting that the cur-

rents are conducted from the cortex of the hemispheres to the basal ganglia, the corpora striata, and motor tracts. Hitzig also makes a similar objection to my method.

In reference to this, I wish to make a short criticism. It is said by Dupuy, that when you place an animal fully under the influence of chloroform, you may stimulate the brain, but no action at all is produced; but when you stimulate the motor centres as well as the corpora striata or the motor nerves, then you get movements produced as before, notwithstanding the anaesthesia; therefore, he argues, the cortex of the brain cannot be motor, because it is not affected in the same way as the other motor centres. This seems to me to bear directly against himself, and to prove more conclusively the facts which I have brought forward and the theory I hold with regard to them. I have myself shown that the cortex of the hemispheres and the motor ganglia are differently influenced by anaesthetics. If you place an animal deeply under chloroform, you may irritate the cortex of the brain and no movement is produced; but if you expose the corpus striatum, you may excite movements, and when the corpus striatum is no longer excitable, movements may still be produced by stimulation of the corpora quadrigemina. This shows that the motor centres differ in their excitability under the influence of chloroform; and, unless it be proved that chloroform destroys the conductivity of brain-tissue, these facts rather favour the idea that the movements are dependent upon the excitation of centres in the hemispheres, which now have lost a function which they formerly possessed. It is said, on the other hand, that these movements are due to conduction; that the currents used are too strong; that they are conducted down through the substance of the brain to the motor ganglia. In answer to this, I will here refer to the brain of the monkey. In the first place, there are certain regions of the brain of the monkey which do not react to any stimulus, however strongly applied; as, for instance, the antero-frontal regions and the whole of the occipital lobes, which proves that conduction, if such exist, is only within certain limited regions. On the other hand, when the fissure of Sylvius is separated, and electrodes are placed on the island of Reil, which covers the corpus striatum, no movement results under the influence of a stimulus which excites movements in regions more distant from the great motor ganglia. Therefore, conduction cannot be the explanation of these facts. Still further, the phenomena which are produced by the application of a stimulus to points closely related to each other, are so different that they can hardly be explained by conduction. Conduction ought, if it be the explanation, to call forth all these movements in a mixed manner. Instead of this, they are so definite and predictable, that I undertake, on exposing the brain of an animal, to say, on the application of the electrodes, that the animal will now do this and now do the other. These results have always followed whenever I have given any demonstration. So that, taking into consideration the definiteness of the results, their predictability and uniformity, haphazard conduction is out of the question. For instance, if I place electrodes on this part of the brain of the monkey—the upper part of the superior temporo-sphenoidal convolution—the animal pricks up its opposite ear, turns the head to the other side, opening the eyes widely, and dilating the pupils. That is invariably the action produced. If I apply the electrodes just across the fissure of Sylvius, a point not further distant than the fraction of an inch from the previously stimulated region, the animal retracts the angle of its mouth.

Let me point out some of the chief centres, stimulation of which gives definite results. I may state generally that I regard the whole of the brain as divided into a sensory and a motor portion. Roughly, a line drawn up through the fissure of Sylvius, and carried through the parieto-occipital fissure to its extreme termination, cuts off the whole motor region, while all behind and below is sensory; and the external phenomena which result from the stimulation of the brain are to be regarded as an excitation of voluntary motor centres, or of the centres of sensation. With the aid of this diagram I indicate at the upper part of the ascending parietal convolution the leg centre; movements of the leg being produced on the opposite side of the body. In the upper part of the ascending frontal or the posterior part of the superior frontal there is a centre for certain movements of the arm and the leg, chiefly those for grasping. In the lower division of the ascending parietal, we have a centre for the movements of the fist. In the ascending frontal, behind the superior frontal sulcus, is a region the stimulation of which causes the action of the biceps, the animal raising its hand to its mouth and closing the fist. A little lower down, behind the posterior extremity of the middle frontal convolution, is the centre of action of the facial muscles, the zygomatics, and those which elevate the angle of the upper lip and the ala of the nose. At the posterior extremity of the inferior frontal, corresponding to what in the human brain is conventionally termed Broca's convolution, is a centre for the various movements of articulation. The stimulation of a corresponding

* Delivered before the Medical Section at the Annual Meeting of the British Medical Association in Norwich, August 1874.