

this discussion, although I hope not altogether out of sight of its object. I fear, too, that the account I have given of these symptoms, and their use in diagnosis, may have seemed complex. I wish I could have made it more simple. But I do not think that complexity is a test of error, or simplicity a guarantee of truth. The longer I study the symptoms, the more I am convinced that their nature is complex, and that the evidence they give often requires much care rightly to interpret. How often, in the progress of science, the first impressions of simplicity have had to give way before increasing knowledge to a conviction of complexity. As facts multiply, we find that conditions vary. We have to plod patiently amid the multiplicity of facts and the variety of conditions to trace, by slow degrees, the relations that underlie the phenomena and determine their manifestations, and we have to wait until fuller knowledge can reveal the new simplicity of ordered law.

## ON REFLEX ACTIONS, KNEE-JERKS, AND MUSCULAR IRRITABILITY IN TYPHOID FEVER, PHTHISIS, AND OTHER CONTINUOUS FEVERS.

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FOR four years past I have taken almost every opportunity of investigating the state of the reflex actions, knee-jerks, and muscular irritability in various diseases, but more especially in typhoid fever and phthisis. My now extensive investigation of that phenomenon, for which the term "knee-jerk" is the best designation, makes me hold the view, which I believe is also held by my former teacher, Dr. Gowers, that absence of the knee-jerk is inconsistent with perfect health of the neuro-muscular system. I make this proposition with the fullest knowledge of what has been said by other physicians.

*Degrees of muscular irritability.*—I think we may recognise clinically several degrees of muscular irritability. That phenomenon which goes by the name of the "idio-muscular" contraction can be got in health, but not with great facility. When the idio-muscular contraction is obtained with ease, I recognise the first degree of increased muscular irritability. The second degree of augmented excitability of muscle may be said to exist when spontaneous contractions of the whole, or nearly the whole, muscle occur—e.g., subsultus tendinum. But there are other phenomena which must be classed under the head of increased muscular irritability, and of these there are also two degrees. The first degree consists in the production of fibrillary contractions by percussion of muscle, and the second is the spontaneous occurrence of fibrillary tremors. Both these degrees of muscular irritability may be witnessed in progressive muscular atrophy, lateral sclerosis, peripheral disease of nerves, and also in typhoid fever, phthisis, and other morbid processes attended with fever and wasting.

*Typhoid fever.*—In all cases of marked typhoid fever the knee-jerk is exaggerated, so much so that its equivalent (contraction of quadriceps extensor) may often be produced by drawing down the patella with the forefinger, and then percussing the straining forefinger—a method that I first learnt from Dr. Gowers. Occasionally I have observed a brief but distinct clonus of the same muscle. I also find that the muscular irritability is greatly increased in typhoid fever, and this increase shows itself in several ways. When a muscle is tapped by immediate percussion with the finger or stethoscope a contraction of the whole muscle is brought about, and at the same time a series of fibrillary contractions occur in the belly of the muscle. These conditions are best observed in a voluminous muscle like the calf, but they occur in all the muscles of the limbs. Curiously enough, the facial muscles are by no means so irritable, though occasionally percussion over or about the malar process may cause a very obvious contraction of the orbicularis palpebrarum. The tongue shows fibrillary tremors rather earlier than the trunk and limb muscles. When the knee-jerks are greatly exaggerated, as they are sometimes in typhoid fever, spontaneous fibrillary contractions of the muscles occur,

and spontaneous contractions of the whole, or nearly the whole, of the muscle are not unfrequently present; these are, of course, well known under the name of "subsultus tendinum." When this marked degree of irritability exists "ankle clonus" is usually present and easily elicited.

*Wasting of muscle.*—Side by side with the development of this muscular irritability the muscles waste, and there is some proportion between the degree of wasting and the degree of increased irritability. But the phenomena cannot be explained simply as the result of lowered vitality due to mere emaciation, for pure wasting afebrile diseases do not so markedly increase the reflex or direct excitability of muscles, as I have determined in cancer and simple stricture of the oesophagus.

*Electrical reactions.*—I have made some observations on the faradic and galvanic excitability of the muscles in typhoid fever chiefly in children. I find that the faradic excitability is increased as well as the galvanic; but the faradic excitability is rapidly exhausted. There is also a qualitative change in the galvanic reactions. The contraction with negative break occurs with a much feebler current than in health, and the contraction from positive make may occur as early as the contraction with negative make. These facts show that we have not to do with the ordinary "reaction of degeneration." They remind me of some electrical reactions that Dr. Gowers showed me in a case of chronic neuritis of the musculo-spiral nerve.

*Superficial or cutaneous reflexes.*—In typhoid fever I have observed obvious increase in the plantar, cremasteric, epigastric, abdominal, scapular, and gluteal reflexes. To sum up, it is evident that we have to do with a general increase of muscular irritability going on side by side with muscular wasting and loss of power.

*Date of onset and duration and mode of disappearance.*—In typhoid fever these exaggerated actions come on usually in the course of the second week of the fever, and gradually increase till the subsidence of the fever. The phenomena continue from two to three weeks after the fever process has ended. As the muscular strength of the patients returns, and as the weight of the body increases, the various exaggerations tend to disappear. In my experience the muscular contractions cease before the knee-jerk assumes its normal degree.

*Phthisis.*—In cases of phthisis attended with fever the same series of phenomena may be observed. Active idio-muscular contraction and subsultus tendinum, fibrillary tremors spontaneous and elicited, increased knee-jerk, and increase of other tendon reflexes, quadriceps extensor clonus, ankle clonus, facial irritability, lingual tremor, increased superficial reflexes, muscular wasting, loss of power, and similar electrical alterations. I have also observed the same phenomena in a case of pyæmia that had lasted three weeks. In two cases of rheumatic fever that had lasted over two weeks, I noted increased knee-jerks and increased activity of the idio-muscular contractions. Dr. Gowers has told me that a clonus has been described in rheumatic fever.

*Prognosis and treatment of the neuro-muscular exaggerations.*—The phenomena above referred to being regarded as expressions of a "typhoid" state of the nervous system, the indications for prognosis and treatment are the same as pertain to that state. The presence of these exaggerations is a plea for the exhibition of nervine restoratives, the best of all of which are sleep and rest, with easily digested food. Opium is the best drug, and should be employed wherever possible; it produces sleep, lessens the reflex activities, and thereby promotes the storage of nervous energy.

*Remarks.*—All the phenomena above mentioned may be ranged under the head of hyperkinesis. As to the precise pathogeny of these conditions, no certain statements can be made. In none of the cases that I have examined has there been anything akin to rigidity or tonic spasm. Tonic spasm never seems to be a sign of mere exhaustion of any part of the reflex arcs of the spinal cord. It generally occurs when disease of the pyramidal tracts exists. Most probably the energy that causes tonic spasm is derived from the motor cells of the reflex arc. And the question is, under what physiological circumstances an apparently continuous liberation of energy occurs. In many individuals unaccustomed to sharp walking a powerful tonic spasm of the muscles covering the front of the leg can be rapidly developed by hurried progression. We know that, speaking generally, the extensor muscles are inherently weaker than the flexors, and the anterior tibial muscles belong morphologically to the

extensor group. Lesions of the cerebrum and pyramidal tracts are much more often followed by rigidity of muscle than any lesion situated elsewhere. It is impossible, in the present state of our knowledge, to make any definite assertions as to the physiology of rigidity, but, speaking broadly, we know that diseases limited to the reflex arcs of the spinal cord do not *per se* produce rigidity. It seems most probable that the exaggerations of muscular action in phthisis and typhoid fever should be attributed to an increased irritability, not only of the muscular tissue itself, but also of the nervous components of the reflex arcs of the spinal cord. Since ankle clonus may appear and disappear in continued fevers, it would seem certain that the foot phenomenon is not always dependent on lateral sclerosis of the spinal cord. Indeed such a proposition, though still held by some neurologists, has long ago been rejected by others. Further, sclerotic tissue in and of itself can produce no symptoms, for it is altogether outside the nervous system, and it only produces symptoms indirectly by its action on the genuine nervous protoplasm. It would be strange indeed if that condition of the neuro-muscular protoplasm on which ankle clonus depends could not be brought about by changes other than those induced by sclerosis of the pyramidal tracts. Cases of peripheral and spinal and cerebral paralysis have been recorded as occurring during and after typhoid fever. But the changes to which I refer are to be met with in all cases of typhoid fever which have had considerable fever lasting for more than ten days. It seems to me that such deviations from the normal that I have described can only be attributed to general disturbance of the nutrition of the nervous and muscular apparatus. As has so often been pointed out by various authors since the time of Todd, "movements" are next door to paralysis. No case of hyperkinesis occurs without some weakness, and excessive action is a sign, not of strength, but of disease. "Irritable weakness" is the best name for this phenomenon, which is illustrated in delirium, chorea, and the various other forms of over-action. The excessive action of Carlyle's diction was but the *strength* of irritable weakness; and all forms of genius, however admirable, are probably nothing more than useful or profitable disease. To such strange conclusions are we led, and so are our vanities unmasked, by the frightful matter of fact of modern physiology.

*Note on a contrivance for obtaining the knee-jerk more readily than by most other means.*—This consists simply in "stirruping" the foot in the left hand, and adjusting the leg at the most convenient angle of flexion at the knee. The centre of the instep should be allowed to rest comfortably on the palmar aspect of the fingers and front part of the palm.

## HEALING BY FAITH.

MIRACLES AT LOURDES AND OTHER PLACES; DISAPPEARANCE OF TUMOURS.

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IN the magazine *Annales de Lourdes* for July there is a notice of my article on Faith-healing in THE LANCET of June 20th. The writer, while admitting that my explanation suffices to account for the sudden cure of such affections as I indicated—conditions simulating and presenting all the symptoms of disease of the spine, paralysis, and disease of the knee, and which he terms *sine materie*,—declares that nothing short of a miracle will account for the disappearance of an organic lesion, an *altération matérielle*. He continues: "We do not ask that Dr. Buchanan should go back on former cases (at Lourdes); it will be better to recount a fresh one. Mdlle. D—, aged forty-four, had a tumour the size of a hen's egg in her left breast. Dr. Teuwin of Tongres certifies that in September, 1884, he diagnosed it as a cancer, causing *fortes douleurs*. Mdlle. D— went to Lourdes, and on May 1st, 1885, used the water flowing from the grotto, on which she declared that the pain ceased, but the tumour remained. On the following day she again used the water, after which the tumour disappeared. On her return to Belgium she visited Dr. Teuwin, who certifies, 'I declare that to-day, May 9th, the tumour has completely disappeared.'" The writer of the article continues: "Have we here an organic lesion? Granted that it was not malignant; it was at least

a tumour. The cure of May 2nd cannot be explained by confidence, imagination, or any impression whatever. It is a phenomenon without analogy among those which are seen in hospitals or schools of medicine." And the writer infers that it was an instance of direct divine interposition—a miraculous cure.

The easiest way of getting out of the difficulty of believing in the above statements is to assert that there must be some error or fallacy in the narrative—that the thing is impossible, and therefore did not occur as related; it is incredible, and there must be some flaw in the story, which does not appear on the surface. I do not intend to adopt this solution.

For my own part, I hesitate to accept the evidence adduced in the *Annals* as satisfactory in a case of such importance as the credibility or incredibility of an alleged miracle. I myself would not be satisfied with anything less than a certificate as to the state of the patient immediately before and immediately after the alleged cure, given by a surgeon who, by his clinical experience and professional reputation, was recognised by the profession in his own country as an authority whose opinion could be relied on.

I distinctly disavow any intention of throwing any doubt on the sincerity of M. Teuwin. He may be a most able and skilful practitioner; but I do not know of his existence. For all that the readers of the *Annals* know, there may be no such man. If there be, none of these readers probably ever heard of him before. My geographical ignorance is such that I do not know where Tongres is, or if there is such a place. All this I state to show that evidence which might be satisfactory to others is absolutely worthless to me, and if strictly inquired into it must be equally so to almost every, if not every, reader of the case. The truth of the cure is also alleged by the reference to a *procès verbal* at Lourdes; but messieurs the missionaries at Lourdes will not blame me for not accepting that as evidence. It is also stated to be contained in a letter from M. l'Abbé Demeersman, Curé of the parish of St. John the Baptist at Tongres. But M. l'Abbé will excuse me when I state that, so far as trustworthy medical evidence is concerned, I attach less importance to his letter than to M. Teuwin's. Altogether, I assert that, so far as the report of this case is concerned, the evidence is not sufficient for me to accept its accuracy.

But supposing the case actually did occur exactly as stated in the narrative, it is not correct to say that it is without analogy among those which are seen at hospitals. On the contrary, it is just one of the many examples of tumours spontaneously disappearing, as they sometimes do, without any treatment, to the surprise of the surgeon, who is wholly at a loss to account for the phenomenon. Adenoid tumours of the breast are those which most frequently become absorbed or removed without any apparent cause. Certain physiological changes which occur during the course of female life are well known to affect, and sometimes quite suddenly, the appearance or disappearance, the increase or diminution, of tumours apparently adenoid. But independently of such physiological actions, tumours are well known to undergo all varieties of condition without any assignable cause.

It is true that in the case of Mdlle. D— the tumour is alleged to have disappeared in forty-eight hours, and this is a most unusual circumstance. Still, the element of time is not so extraordinary as the actual disappearance. Even this point is left in doubt in the report. The surgeon certifies the existence of the tumour in September 1884, but does not seem to have examined it again till May 9th, 1885, when he says he found it gone. The Curé seems to have taken the place of the medical attendant subsequently to September, 1884, and declares the tumour was in existence in April, 1885. I can scarcely believe that the Curé himself examined Mdlle. D—'s breast and gave the certificate referred to, when he could have obtained a medical opinion and report if it had been desired. But, at all events, he guarantees the correctness of a certificate given by two women who seem to have been employed instead of the surgeon. The whole question of time is left obscure, or at all events without evidence satisfactory to a medical man.

But even if I go a step further, and accept the narrative as given without reservation, I still assert that it is not unique. Examples of a similar kind occur occasionally, though rarely, within the experience of most clinical surgeons. The last of the cases which follow is equally astounding and impossible of explanation. Everyone knows the effect of the nervous system on all the functions of the body, and how