ON THE
IRRITABILITY OF THE MUSCULAR FIBRE
IN
PARALYTIC LIMBS.

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THE Society did me the honour to publish a paper on the subject of the present communication in the Twenty-second volume of its Transactions.

In the last volume, or the Twelfth of the New Series, a paper on the same question is published, from the pen of Dr. Todd.

The conclusions to which Dr. Todd has arrived are at variance with my own. On first discovering this, I hastened to ascertain what could be the cause of the discrepancy between us, confident as I felt that my experiments and observations had been conducted with extreme care. I soon perceived that Dr. Todd had not employed the same kind of apparatus; and a few comparative trials established the fact, that in the difference of apparatus consisted the difference between our results.

I.—*The Instrument employed.*

My object was to select such an apparatus as, being of the very *mildest* operation, should really be a *test* of the irritability of the muscular fibre of those muscles subjected to its

influence, and present the least complicated results. It was essential to avoid violence—to avoid all effects of timidity, surprise, sudden sensation, &c., evinced in starting, wincing, &c. I chose the well-known Cruikshank's battery or trough, the commonest, the simplest, the mildest, the most easily adjusted, of all. By means of this instrument I could apply a gentle galvanic influence, proceeding from ten, twenty, thirty or more plates, consisting of a simple current of low intensity; I could, in a word, apply the precise power required; viz. the lowest which would produce an obvious effect.

Dr. Todd has used the electro-dynamic or the magneto-electric apparatus. Now these are machines of great intensity; and instead of affording a simple current, inflict currents rapidly interrupted, repeated and even alternated; events, each of which presents its own peculiar effects on the muscle or on the nervous and muscular fibre.

But waiving the mere *à priori* objections, that the same instrument was not employed by Dr. Todd as by myself, it was impossible to conclude, without experiment, that the effects of instruments so different would be the same. I may now add, as the result of experiment, that, so far from being identical, the results afforded by the two instruments are *diametrically opposite*. We have, in this simple fact, the entire explanation of the discrepancies between Dr. Todd's statements and my own.

In effect, as my friend Mr. Henry Smith, to whom I am indebted for his most able assistance in all these experiments, early and acutely observed, the galvanic trough is, from its simplicity of operation and its low intensity, really a *test of the irritability* of the muscular *fibre*; whilst the electro-dynamic apparatus, from its extreme intensity and mode of action, displays the *power* of the muscular *mass*.

It may also be surmised, that whilst the simple current of low intensity acts principally on the superficial fibres, the rapidly-repeated galvanic agency, in its intense form, may penetrate to the deeper-seated *nerve*, and so display a degree of energy not observed in the other case.

And I beg here to lay before the Society an interesting fact, illustrating the effect, even upon nerves of sensation and volition, of disuse:—A frog had lost its inferior extremity, probably by some voracious fish; the divided thigh presented a stump perfectly healed. On examining and comparing the two lumbar nerves, I observed that that nerve which proceeded to the amputated limb had become atrophied. Doubtless this occurs in cases of disuse from paralysis; and, consequently, that nerve may lose more or less of its excitator power.

That Mr. Smith's view of this question is the correct one, is proved by an experiment which I have repeatedly made.

If the hands of a healthy person be subjected to the action of the electro-dynamic apparatus, they are energetically *closed*: the flexors muscles being more massive and powerful than the extensors, the former are more forcibly contracted than the latter; it is their *power* not their *irritability* which is tested.

But it is to an actual case in point that we must appeal; and I beg here to introduce an experiment, made to determine the question before us, viz. that of the results obtained by the two kinds of apparatus to which I have adverted. I give it in the words of Mr. Smith. It was made on the 10th of November 1847.

“In a case of hemiplegia of three years and a quarter's duration, in a man aged 28, the arm being slightly wasted and the hand contracted, the leg only a little enfeebled, we first applied the current from the common Cruikshank's battery, the hands being placed in salt water in one basin, the feet in another: on using a small number of plates, the muscles of the paralysed arm were found to be slightly affected by a current which did not influence those of the unaffected arm; on augmenting the number of plates, both arms were moved, but the paralytic arm more than the other.

“We now substituted Hearder's electro-dynamic apparatus for the Cruikshank's battery: both arms were moved, but the

unaffected more than the paralytic arm, the muscles being more powerfully contracted, as observed both by the sight and touch; a difference which became still more obvious as the power of the apparatus was augmented."

Having thus pointed out the source of the discrepancy between the results obtained by Dr. Todd and myself, I need not proceed any further. Still there are so many and such important questions involved in this inquiry, that I am anxious to be allowed to adduce fresh evidence of the statements made in my former paper. Most of all, I would call the attention of the Society to the great physiological principles involved in this inquiry—viz. that, whilst volition is an exhaustor of the irritability of the muscular fibre, this irritability is essentially dependent on the influence of the spinal marrow; and to the important application of galvanism as a test of that irritability, and as a diagnostic between the cases of paralysis, in which, first, the influence of the cerebrum, or, secondly, the influence of the spinal marrow, is severally removed; for both these doctrines remain in their full force.

II.—*The Terms employed.*

Before I proceed, I must also take this opportunity of defining the medical terms employed by me in this discussion.

In the first place, by *cerebral* paralysis, I mean any disease which *severs* the influence of the cerebrum, and consequently the acts of volition, from the paralysed limbs.

In the second place, by the expression *spinal* paralysis, I mean any disease which *severs* the influence of the spinal marrow from those limbs.

In my former memoir, I speak expressly of "the cases of paralysis arising from the severed influence of the spinal marrow, as distinguished from those arising from the severed influence of the cerebrum merely."

Cerebral paralysis usually depends on disease of the cerebrum, but spinal paralysis may depend on disease situated in any locality, even in the cerebrum, *so as to sever*

the influence of the spinal marrow from the paralysed muscles.

By the term paralysis, too, I mean *pure* paralysis, and I carefully distinguish such paralysis from cases in which *spasm*, "excited immediately from some disease affecting the spinal marrow" (or system) "itself," is superadded; cases which I have recently distinguished by the term *spasmo-paralysis*.

It is especially necessary to distinguish between such cases as, from involving "some disease affecting the spinal marrow itself," are attended by *early* spasmodic affection, and those cases of pure cerebral paralysis, in which spasm supervenes at a remote period, from the physiological action of the spinal marrow on muscles from which the influence of volition being withdrawn, the irritability of their fibre is augmented and their permanent contraction induced, presenting the only case, probably, in which such contraction is physiological, or pathological only in a secondary point of view.

It is true that Dr. Todd has referred the early and the tardy contractions in hemiplegia or cerebral paralysis to the operation of other causes, (see page 218,) but it is equally certain that Dr. Todd's views are erroneous; for both experiment and observation prove that no condition of the cerebrum, *limited* in its influence to the cerebrum, *can* induce spasmodic action.

An equal, or, rather, a double error is committed by Dr. Todd in reference to the action of strychnine (see page 227). It has been amply proved that strychnine acts on the spinal marrow, and only on the spinal marrow.

Throughout this inquiry it is essential, not only that our instruments should be identical, but that our terms should bear a meaning at once specific and accurate.

III.—*The question submitted to fresh experiment.*

The questions now are :—

1st. Are there cases of paralysis in which the irritability of the muscular fibre is greater in the paralytic than in the unaffected part or limb?

2nd. Are there cases of paralysis in which the reverse of this obtains?

3rd. What are these cases severally and respectively? Are the former, cases in which the influence of the cerebrum *alone* is severed? and are the latter, cases in which the influence of the spinal marrow *also* is intercepted?

4th. Is the galvanic battery or trough of Cruikshank, cautiously employed, a fair test of the irritability of the muscular fibre in paralytic limbs? and, consequently, diagnostic of these two kinds of paralysis?

In the prosecution of these questions many precautions are required.

In the first place, it will always be observed that the patient is timid on the first application of the galvanism. The effect of surprise and of sudden sensation, in wincing, starting, &c., must be carefully distinguished from that of the galvanic influence on the muscular fibre.

2. The degree of the galvanic force should never be, because it never need be, such as to give "pain," far less such as to "jerk the limb out of the basin." Such violence must complicate the phenomena and confuse the results. The galvanic force should be the *mildest* which can produce an obvious effect.

3. The first effect of a paralytic seizure is one of shock, probably of diminished irritability of the muscular fibre. *Time* must be allowed for the restoration, as well as for the augmentation, of this property. It is, therefore, only after a certain lapse of time, that it is proper, or perhaps *safe*, to apply galvanism, especially if the patient be timid.

4. Within a very few days of the paralytic seizure, there is frequently a rapid and considerable recovery of voluntary motion; afterwards the recovery is both much slower and much less marked. The former is the recovery from *shock*, the latter diminution of the actual disease.

5. Lastly, in some cases there is *no* recovery. In these cases I think it probable that there may be no augmentation, perhaps no restoration, of the irritability of the muscular

fibre. In one such case, after the lapse of seven weeks, the muscles of the paralytic arm remained less susceptible to the proper galvanic influence than the unaffected limb.

Having made these preliminary and precautionary remarks, I proceed to the detail of a series of new experiments and observations. My task will then be terminated, and I trust the Society will be convinced that it was not without due caution and care that I gathered the facts together which I ventured formerly to lay before it, and that I drew my inferences from them. This is the reward of my labour,—that I have nothing to retract, nothing even to modify.

The cases in which I have carefully tested the irritability of the muscular fibre in paralytic limbs, before competent witnesses, are now almost innumerable: I may refer to the instances recorded in my former paper, read before this Society; to shorter papers published since, in my “Observations and Suggestions,” Series I. p. 114, and Series II. p. 44; and to cases recorded in the *Lancet*. But I will not take up the time of the Society with any cases or experiments except such as I have observed *since* the publication of Dr. Todd’s paper, and made, therefore, with tenfold care, if possible, to insure accuracy.

Case I.—The first case which I shall adduce is that of Mrs. —, aged 35, affected with extreme paralysis of the right side of the face. The right eye could not be closed by the most strenuous efforts, but remained widely open; the right side of the face remained flaccid, whilst the left was drawn into forcible contraction on any attempt to speak, and under the influence of effort or emotion. The case was regarded by all present as paralysis of the facial nerve.

We applied galvanism from five, ten, fifteen and twenty plates of Cruikshank’s battery: our surprise was great to observe that, although the left or unaffected eyelid was only, or most, moved, it was the muscles of the right or paralysed side of the face which alone were thrown into a state of contraction. The current was directed from the forehead to the chin, and from temple to temple, first from right to left, and

then from left to right; still the effect was invariably the same.

Now in every case of paralysis of the facial nerve, as in the case to be next noticed, it is the muscles of the unaffected side of the face which are most susceptible to the galvanic influence. In the present instance this was the case with the orbicularis only.

On further inquiry we found that the attack came on rather suddenly, with uneasy feeling—a sense of confusion—about the head, the noise as of a cricket in the right ear, and numbness in the right side of the tongue. It became plain, that it was a case of cerebral paralysis, and although its precise nature still remained obscure, two things were obvious enough: first, that it presents an instance of pure paralytic affection, uncomplicated with spasm, in which the fibre of the paralyzed muscles is more irritable than those of the unaffected muscles; secondly, as we shall see further immediately, that it is not a case of paralysis of the facial nerve; and thirdly, that the diagnosis was made out by means of the galvanic trough. For this case I am indebted to the kindness of Mr. Byam, of Welbeck-street.

Case II.—The second case which I shall adduce is that of a gentleman aged 30, who suffered from extensive suppuration on the left side of the neck and face, from exposure to the inclemencies of the weather in Australia; it left the left side of the face completely paralytic. The left eye gapes widely under the most strenuous efforts to close it, and the face is drawn to the opposite side in speaking, laughing, &c.

We tried the effect of galvanism, as in the preceding case: it was now the muscles of the right or unparalysed side which were excited, or most excited, to contraction, according as we employed a smaller or greater number of plates.

These experiments were repeated once a-week during several months, and therefore a great number of times, and before a great number of witnesses. There could therefore be no possibility of error. The patients are still under my care; and if a doubt exist in the mind, I can readily, as I

will most gladly, repeat the experiments in any manner that may be satisfactory to the Society.

Case III.—For this case I have to thank Mr. Archer, of Montagu-street. The patient, Mr. A. B., aged 48, has had two attacks of hemiplegia, one about eighteen months ago, the second three months ago. *The right arm and hand* are left greatly paralysed, and the muscles of the former are considerably emaciated.

We placed the two hands in one basin containing a solution of common salt, the two feet in another; we then passed the current from a common galvanic trough from basin to basin, varying the number of plates from about fifteen to twenty.

For the first few minutes the *left* or unaffected hand was most agitated at each completion of the galvanic circle, obviously the effect of *timidity*.

Afterwards, when the patient was re-assured, the *right*, or paralytic hand and arm, were distinctly moved by a number of plates which did not affect the left, and much more than the left, by a number which affected both. We first observed a finger of the paralytic hand to be moved; we then observed the muscles along the paralytic fore-arm to be raised at each repetition of the galvanic current, when no such phenomena were observed in the healthy arm and hand.

We now reversed the current; the effect was still the same.

The experiment was made on the 17th of December 1847. On the 21st it was repeated, with precisely the same effects, except that, from the commencement, the muscles of the paralytic arm were more contracted than those of the unaffected limb.

Mr. Archer was present, with Mr. Henry Smith and myself, on both these occasions.

Case IV.—On January the 31st, 1848, I galvanized W. M., aged 36, at Dr. Bennett's; Dr. Bennett, Mr. Le Gros Clark, and Mr. H. Smith, being present.

The patient had had six attacks, the first sixteen, the last

ten months ago. The left arm and leg were left greatly paralysed, especially the former; the right leg was also somewhat affected. The sight of the left eye was impaired, but there was little paralysis of the face; he could, however, close both eyes together, and the right *singly*, but not the *left*.

The muscles of the left arm were considerably emaciated, the other being finely developed; the hand could with difficulty hold his stick; the thumb was more paralyzed than the fingers, and drawn slightly towards the palm of the hand.

We first tried the effect of Cruikshank's galvanic trough with fifteen plates. The *left*, or affected hand, and especially the thumb, was slightly moved when no movement was observed in the right. With twenty or twenty-five plates, movements were observed in *both* limbs, but far more in the left than in the right, the fingers being moved and the muscles along the arm seen to contract.

We now tried the electro-dynamic apparatus of Hearder. The effects were reversed, being much more strongly marked in the sound than in the paralytic limb.

To this account I beg to append the following certificate, drawn up by Mr. Le Gros Clark. I leave it entirely without comment:—

“ We have witnessed the experiments performed on Wm. Mackintosh, suffering from hemiplegia. In the first, Cruikshank's battery was used, in which there was very decided contraction of the flexors and pronators of the fore-arm of the paralysed side; the same effect was produced, but less apparently and to a more limited extent, on the sound side. In the second experiment, with Hearder's electro-dynamic machine, there was no question about the effect being much more strongly marked on the sound than on the unsound side.

(Signed) “ James R. Bennett, M.D.
 “ F. Le Gros Clark.
 “ Henry Smith.”

Case V.—My next case is as interesting, and in the same

manner, as my first. The result was unexpected, yet confirmatory of the general principle, and displays the galvanic influence once more as a corrector of a hasty and erroneous diagnosis.

The experiments were witnessed by Mr. Erichsen and Mr. Barlow, as well as by Mr. Henry Smith and myself.

The paralysis was confined to the left leg : it had come on gradually, and without any sudden seizure, fifteen months before, and had induced debility and atrophy of the glutei and of the muscles of the thigh and leg, with diminished temperature and circulation, the limb being thin, cold and livid.

I had inadvertently concluded that this case was one of partial *paraplegia*, and therefore of spinal paralysis. I expected that the muscles of the paralytic limb would be less affected by the galvanic influence than those of the unaffected leg. It was just the contrary. On applying Cruikshank's battery, as usual, first in its mildest form, and then in that of gradually augmented power, the muscles of the paralytic limb, the *tibialis anticus*, the *gastrocnemius*, &c., were observed to contract, whilst those of the unaffected limb were unmoved, or far less moved.

When the power was considerable, the muscles of the paralytic limb were forcibly contracted, whether seen or felt, whilst scarcely a tremor was observable in the healthy limb.

On carefully interrogating the patient, it now appeared that in the commencement there had been slight tingling of the left hand, a fact not elicited before. It was plain that the case was one of partial *hemiplegia*.

We now exchanged the Cruikshank's trough for Hearder's electro-dynamic apparatus. *The effects were reversed!* The muscles of the unaffected limb were now moved or permanently contracted (according as the application of the machine was momentary or continued), when scarcely a movement was observed in the paralysed limb. The unaffected limb was jerked violently when the paralytic limb was scarcely moved.

No more glaring example can be given—1. Of muscles of

paralytic limbs with augmented irritability of fibre; 2. Of the diagnostic powers of the galvanic trough; 3. Of the wide difference between the effect of this apparatus and of the electro-dynamic machine.

This experiment was performed on the 8th of November 1847. It was repeated on the 9th. The results were the same.

I believe I need not adduce more cases than those already detailed, to prove the facts propounded in my former paper.

I will conclude this paper by one or two remarks on some suggestions that have been made as to other possible modes of explaining the phenomena which have been detailed.

I. It has been supposed that, in cerebral paralysis, the healthy limb may be, however unconsciously, restrained from moving by the volition of the patient. I do not deny that there may be some foundation for this remark, which was first fully and clearly pointed out by my acute friend, Mr. W. F. Barlow. But it is only necessary to be aware of the possibility of this influence to guard against it.

In the first place, there *can be* no such influence exerted on the features; for if there were, the face would be distorted, and this would be obvious to the most cursory observer.

In the second place, the volition may be suspended by causing the patient to let his hands hang down loosely, like a pendulum.

In a word, any mistake of this kind could only result from extreme inattention.

II. It has been suggested that the greater movement of the hemiplegic muscles may be the result of a reflex action.

But these movements may be limited to a single muscle, both in the face and in a limb, and are perfectly dissimilar from all reflex movements to the experienced eye; for these are movements of groups of muscles, and of a totally different character.

The idea is also negatived by a simple experiment: in no animal are reflex nerves more distinct, in every respect, than

in the frog. Now let a frog be decapitated, to remove sensibility, and let the nerves of one inferior extremity be divided, or, as far as possible, removed altogether, and let the two limbs be subjected to the same galvanic influence; in one there might be, but in the other there cannot be, a reflex action. Yet the irritability being alike, both are moved equally. This experiment Mr. Smith and I have repeated, with another object, many times.

To determine the question in regard to the human subject, I performed the following experiment: I first passed the galvanic current through the arms of a patient in the usual manner, the hands being in one basin, the feet in another, and observed the usual marked and greater effect on the muscles of the paralysed arm and fore-arm. I now passed the same current from the fingers and hand to the *wrist*, first of one, and then of both, sides, excluding the muscles of the arm and fore-arm: there were now movements of the wrist, hand and fingers *only*.

Now, as far as reflex actions are concerned, the arm was in the same condition in both experiments. The conclusion is obvious. Reflex action has nothing to do with the phenomena.

Indeed, as I have already said, the movements of the paralytic limb have not the slightest resemblance to those of excited reflex action.

In conclusion, I have no hesitation in declaring my opinion to be, that, if performed with due attention, neither volition nor reflex action has any influence on the results in these experiments.

Dr. Todd makes some remarks in reference to the influence, 1. Of the nerve imbedded in the surrounding tissues; and, 2. Of the degree of nutrition of the muscles themselves. These are questions requiring much further and careful investigation. I think the results will be at variance with Dr. Todd's conjectures.

Postscript, July 6, 1848.—I have this day been informed, by Dr. Golding Bird, that, in a series of experiments on children, at Guy's Hospital, he has found that, in cerebral paralysis, pure water being made the medium of conduction, the affected limb is moved by a degree of galvanic power which has no influence on the unaffected limb; and that the contrary holds in spinal paralysis. I trust Dr. Golding Bird will lay the results of his trials before the Society.

I may here remark, that in my first experiments, pure water was always used. The difference in the effect, between this and a solution of salt, is very great; and if, instead of using either, metallic contact be employed (by laying discs of metal, connected by wire, on each wrist, and then completing the circle), the result has been the same, in one instance, as that of the electro-dynamic apparatus; that is, opposite to that of the current retarded by using water as a part of the conducting circle.

Particular caution is required in making such experiments as those of Dr. Golding Bird: a single galvanic shock passed through the tissues induces an *electrogenic* condition; the muscles are less susceptible to the influence of this stimulus in a second or third shock, than in the first or second. When the limbs are subjected to the trial *singly*, this fact must be carefully borne in mind.

In addition to the testimony afforded by the experiments of Dr. Golding Bird, I may state, that since the date of the foregoing paper, I have continued to devote one evening weekly, aided by Mr. H. Smith and many others, to the subject, constantly with the same results.