October 14th. The medicine, evidently a bad sample, had had no effect, physiological or other, though he had taken an ounce and a half for a dose. He was jumping vigorously, every muscle apparently in a state of clonic spasm, except the facial. He was given an ounce and a half of tincture of conium, but with no apparent effect. Half an ounce thrice daily of a fresh sample of conium-juice (supplied by Messrs. Harvey and Reynolds) was ordered.

October 21st. The medicine had a distinct physiological effect every time, and he had had no bad attacks—indeed, nothing but a slight occasional quivering; but he felt weak and ill. He was directed to continue the medicine twice a day.

October 28th. The shaking had entirely ceased; but he felt weak, and had pain in the back. He was ordered a tonic iron mixture, and a dose of conium occasionally if he felt any symptoms of spasm.

November 4th. He felt better, and had had no more spasms.

This case differs in many respects from the cases of saltatory spasm reported by Bamberger, Guttman, and more recently by Dr. Gowers. In previous cases, there was a history of epilepsy, chorea, hysteria, or some debilitating disease, but none of injury. These were cases of functional irritability of the spinal cord, similar to those we see in "spinal irritation" and many hysterical persons. But in the case described there were no indications of any organic disease in the shape of paralysis or affections of the sphincters; so that we must rather class it with the sequelæ of spinal concussion, among that large, vague, and unsatisfactory collection of symptoms of which we hear much in railway trials for compensation. Some light is thrown on the pathology of cases as these by experimental physiology; but the conclusions are somewhat confusing. Division of the cord (in the rabbit) just below the lumbar enlargement causes repeated flexion of the legs, whereby we may assume the presence of a co-ordinating centre for the legs in that part; but electric stimulation of the cord above the sixth cervical vertebra also caused springing movements of the legs. This may account for the cases where the neck-muscles were involved; but there was no history of injury in my case other than in the lumbar region. Again, there is reason to assume the existence of a centre for co-ordinating reflex movements of the limbs in the medulla at the junction of the lower with the middle third of the floor of the fourth ventricle—indeed, close to the respiratory centres; but the implication of this seems unlikely in the above case.

Lastly, I would point out the necessity, when conium is prescribed, of securing a specimen which shall not be wholly inert, like two of those I tried; and of giving the remedy in sufficiently large doses to produce a distinct physiological effect. This is generally a slight feeling of heaviness across the eyes, slight staggering of gait, and a desire to lie down, which ought to be produced by about half an ounce of the succus. But it is absolutely useless to prescribe this drug, wherever obtained, unless the particular sample have been tested by its physiological action. While, however, a strong man will be only slightly affected by half an ounce of a good preparation, a much smaller dose would be sufficient for feebler organisations.

ON REFLEXES AND PSEUDO-REFLEXES.

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THE question is still debated, whether the muscular contraction following the percussion of a tendon is a true reflex phenomenon or not. It is agreed on all sides that it ultimately depends upon a reflex action, since it is abolished by the destruction of the posterior roots. Those who do not consider the contraction to be a direct reflex hold that its cause is the excitation of the muscular substance itself by the sudden stretching; and explain the integrity of the reflex loop necessary for its occurrence by assuming that the excitability of muscle to this particular stimulus depends, like tonicity, upon a reflex influence from the cord, or rather is a manifestation of this tonicity. The chief reason for rejecting the direct reflex theory is that the time which elapses between the instant of percussion and that of contraction is too short to admit of a centripetal stimulus being converted in the cord in a motor impulse, on the usually adopted measurements of the rate of nervous vibration. Now, measurements of the period of latent contraction following the percussion of a tendon, and of the rate of nerve-vibration, present too wide discrepancies as given by different authors to give very accurate data for calculation; and the process itself of calculation from one set of data to another, both obtained under different conditions, opens the door to many fallacies. In order, therefore, to avoid the uncertainties attending such a method of proof, it has seemed to me desirable to obtain myographic records of the knee-jerk and the plantar reflex on the same subject and with the same

method. In this way, the results are at least comparable. The absolute measurements may not be correct; but, if so, the error is the same in both series of results, and thereby eliminated. The point upon which I would draw attention in the annexed tracings, then, is the relative rather than the absolute duration of the periods recorded.

The method I used was one taught me by my friend Dr. Augustus Waller, and is the same as that by which he made the measurements recorded in his well known paper on "Tendon-reflex" (*Brain*, 1880). This method, the adequacy of which we independently proved by various tests, consists in recording both the instant of percussion and of muscular contraction by the same apparatus. An appropriate India-rubber bag is fixed over the explored muscle, and connected with an ordinary Marey's registering apparatus. The two events (percussion and contraction) are recorded by ascents of the lever; the interval between the points of ascent indicates the lost time of the muscle.* The lost time of the apparatus itself being (as ascertained by previous chronographic measurements) the same in both cases is thereby eliminated.

Fig. 1, line 4, time-tracing; fifty vibrations a second. Line 3, plantar reflex. At *a*, stimulation of the skin; at *c*, contraction of the extensor femoris. Line 2, knee-jerk (same subject). At *a*, percussion-wave; at *b*, contraction of the same muscle.

Fig. 2, line 4, time-tracing; same frequency, slower movement of the cylinder. Line 3, plantar reflex. At *a*, stimulation of the skin; at *c*, contraction of the gastrocnemius. Line 2, ditto. At *a*, contraction of the extensor femoris. Figs. 1 and 2, line 1, tracings obtained from patient in St. Mary's Hospital (under Dr. Broadbent, in February 1882), with marked knee-jerk and skin-reflexes. At *a*, percussion; at *b*, usual knee-jerk; at *c*, further contraction, which I consider as a sensory reflex.

* In Brissaud's work (*Recherches sur la Contracture*, etc., page 96), he erroneously attributes the percussion-wave to a direct muscular contraction; the period which elapses between the electric signal and that event he interprets as Helmholtz's latent time of stimulation. But then, what becomes of the lost time in his apparatus, which, he tells us himself (page 93), amounts to .008 sec.? His "petit fait" is of physical, not physiological, origin.

Though my researches are not yet completed, I have obtained results which may appear novel or definite enough to deserve publication.

Fig. 3, medium rate of cylinder. Tracing from another patient of Dr. Broadbent's, suffering from obscure cerebro-spinal lesions; increased plantar reflex. Galvanic stimulation of the anterior tibial group of muscles (a) was followed by the usual latency and muscular contraction (b), which readily passed into a tonic spasm. If the current was sufficiently strong, there occurred, after an interval, a further contraction (c) of the anterior tibial muscle. (This tracing was obtained by Dr. Waller and myself in July 1881. All the other tracings bear the date of February 1882, and were taken in the wards of St. Mary's Hospital, with the assistance of my clerk, Mr. G. Callender.)

cation in a preliminary note. The tracings illustrate several facts.

1. The latency of the plantar reflex is about three times as long as that of the so-called "tendon-reflex." The latter, in my tracings, has the usual duration ascribed to it by most authors, viz., about .03 of a second.

2. The muscular contraction following cutaneous stimulation tends to run a more protracted course than that following percussion of the tendon.

3. In certain cases, percussion of the tendon is followed by a contraction, of which the latency and protraction correspond exactly to those characteristic of the reflex contraction excited by excitation of the skin.

4. In certain cases, the galvanic stimulation of the muscles also gives rise to a contraction occurring later, and running a more protracted course than that due to the electric stimulus.

5. In comparing the latencies of contractions of the extensor femoris, and of the gastrocnemius, following excitations at the sole of the foot, it must be borne in mind that the difference will be comparatively small, owing to the fact that the two muscles receive their nervous supply near the knee. The difference, likewise, between the latencies of knee-jerks following cutaneous excitations at the sole of the foot, and at the patellar tendon respectively, will be equal to the time taken by the transmission of the stimulus from the sole to the knee.

6. The possibility of true reflex contractions following a galvanic shock and a tap on the tendon is one of great importance in diagnosis, and deserves further consideration and elucidation.

I have investigated myographically the question of "crossed tendon-reflex." The tracings will be published on a future occasion; but I take this opportunity to say, that the latency of contraction in the opposite leg is absolutely identical with that in the leg percussed—a fact in itself sufficient to disprove the reflex nature of the phenomenon. Further, it is easily shown that a blow, given over the tibia or patella of one leg with the fleshy part of the hand, produces little or no contraction in that leg, whilst the opposite is adducted vigorously. It is plain, therefore, that the shock conveyed along the femur to the pelvis, and acting mechanically on the most favourably placed tendons, is quite sufficient to explain the facts—a view obviously forced upon us by the results of Prevost and Waller's experiments on rabbits.

In conclusion, I wish to advert to one difficulty attending the "direct-muscular" or "myotatic" theory of the knee jerk, and allied phenomena. Though its latency is too short to justify the reflex hypothesis, is it not too long to be readily reconciled with the accepted periods of latency to electrical and other direct stimuli? We know, on the other hand, that the time of latent stimulation of nerve-centres (cortical substance: Franck and Pitres, Richet, Bubnoff and Heidenhain) is much longer than of peripheral organs; whilst we have in the muscles themselves organs (end-plates), which stand, so to speak, midway, in some of their physiological properties, between a nerve-cell and a nerve-fibre. The solution of that difficulty (if it is one) might, therefore, be found in the assumption, that the excitation produced by the sudden stretching of a muscular fibre falls primarily upon its nerve-ending. None of the known facts, at least, militates against this view, which I throw out, more as a "working hypothesis" for future investigations, than as a scientific explanation of the phenomena.

Postscript.—As this communication is going through the press, my attention is called by Dr. Ferrier to a paper by Professor Westphal, in the current number of the *Archiv für Psychiatrie*. With his admirable clinical perspicacity, the author therein describes cases, in which he has observed pathological exaggerations of several of the phenomena illustrated and measured in the above tracings—such as the double knee-jerk, the longer latency, the protraction of the reflex contraction. He has, moreover, seen in a case the knee-jerk follow cutaneous excitation, when percussion of the tendon had no effect—a fact which confirms my view concerning the second contractions exhibited in tracings 1 (Plates I, II). The concordance between my results—obtained independently, and by physiological methods—and those of so high an authority as Professor Westphal, is to me welcome and gratifying.

ABSTRACT OF LECTURES

ON THE

ANATOMY, PHYSIOLOGY, AND ZOOLOGY OF THE EDENTATA.

Delivered at the Royal College of Surgeons of England.

By W. H. FLOWER, F.R.S., LL.D.,
Hunterian Professor of Comparative Anatomy.

LECTURE III.—ANATOMY OF THE BRADYPODIDÆ, OR SLOTHS (continued).

THE teeth of both genera of Sloths are nine in number—viz., five in the upper and four in the lower jaw, on each side; but, in some foetal specimens of *Bradypus*, a minute tooth has been seen in the lower jaw, anterior to those usually met with, making the number equal above and below. No traces of milk-teeth have been discovered. All the teeth are similar in structure, single-rooted, with persistent pulps, and consisting of a central axis of soft vascular dentine, surrounded by a layer of hard dentine and a thin external coating of cement. The apex of the teeth is rounded at first, but, when they are brought into use, it wears into a cupped surface, with an irregular projecting margin formed by the hard dentine, which serves the purpose of the cutting enamelled ridges of the molar teeth of the more highly developed graminivorous mammals. In *Bradypus* (three-toed sloths), although the teeth differ in size, their cutting surfaces are all on the same level; but, in both species of *Choloepus* (two-toed sloths), the anterior tooth above and below is greatly enlarged, separated from the others, and assumes much the form of the canine of animals having the heterodont type of dentition. It differs, however, in the circumstance that the lower tooth, instead of passing in front of the upper when the mouth is closed, shuts behind it—so that these teeth are not really the homologues of the canines of ordinary mammals, but special modifications of the homodont type of dentition.

The face in both genera is short, and the lips simple and not very flexible. The tongue is short and soft, presenting a marked contrast to that of some other members of the order, which will be described in future lectures. The submaxillary glands have their normal proportions, and are slightly exceeded in size by the parotids, as in most herbivorous animals. The sublingual is very small. The stomach is very large and complex, formed in both genera on the same principle, though differing in the two in the details of its structure. In *Choloepus*, it consists of three main divisions of very unequal size. 1. There is a very large subglobular paunch, divided by an imperfect longitudinal septum into two compartments, the anterior of which is prolonged at its lower end into a curved conical cæcal appendage. The posterior compartment, into which the oesophagus enters directly, is further divided by a horizontal ridge into an upper and lower pouch, the latter being the larger. 2. A very small sac is situated much as the psalterium of ruminants, and of similar form, having its convex border turned upwards or outwards; while, running along its short floor is a groove, bounded by muscular ridges, extending from the oesophagus into (3) a small elongated tubular cavity, curved into the convexity downwards, with muscular walls, and communicating by a narrow pyloric orifice with the duodenum. This corresponds in situation with the abomasum of the ruminants. Although there is this general resemblance to the stomach of the last-named group, there is a singular difference in the distribution of the hard cuticular lining. In the sloths, this extends from the oesophagus over both divisions of the posterior compartment of the paunch, but not into that which has the cæcal prolongation, and passes through the