duce it into European practice was M. Cauvin, who, in THE LANCET for Aug. 10th, 1861, relates the case of a French lady who was for twenty-five years a martyr to severe attacks of spasmodic asthma, for the relief of which bleeding, belladonna, stramonium, and other remedies had been vainly tried. This lady commenced by inhaling a quarter of a grain of arsenious acid in a cigarette, and by degrees got to use as much as three grains. She always swallowed the smoke, and the relief to her asthma was speedy and permanent.

I have had opportunity of trying nitrated paper cigarettes made by Martindale of New Cavendish-street after the formula given by Troussseau, so that each cigarette contains three-quarters of a grain of the arseniate of soda, and find them very satisfactory. The patient should inspire deeply three or four times from the cigarette, then wait awhile, and see how he feels. I may say that I have never known any unpleasant effect to be caused by the careful administration in any form of arsenical remedies.

The headache and distressing after-feelings that so often follow the use of fuming inhalations of stramonium, belladonna, or opium cigarettes and powders, usually make persons averse to their employment, unless the asthma come on with great force. Tobacco-smoking, however, is a general and approved source of relief.

Internal remedies.—Space permits me to mention but a few of these, and among them I would give the first place to the preparations of arsenious acid. The acid may be given in a granule or pill in commencing dose of one-fiftieth of a grain three times daily one hour after food. Over both the catarrh and the bronchial spasm its effect will be found marked and decided, while gastric irritation and tendency to sickness often are much relieved, making one think the remedy may have some special action over the pneumogastric nerve.

The solution of arseniate of soda, in dose of two to five drops in water, may agree better than the arsenious acid in some cases; but if neither succeeds, then it is well worth while to try the following combination:—Fowler's solution, two minims; potass. iodid., two grains; liquor ammoniac, five minims; water, one ounce; three times a day. It will succeed where arsenic and iodide of potassium separately fail to benefit. Troublesome œdema about the face and eyes, remaining after the more severe symptoms of asthma are gone, I have found to give way speedily to tincture of cantharides in dose of five minims with twenty of spirit of nitrous ether in water three times daily. A remedy, strongly recommended by Dr. Gream in 1850, is tincture of nux vomica; and there is no question as to its value in old-standing asthma with flatulent dyspepsia and irregular weak action of the heart. In these cases it goes well in dose of five to ten minims with dilute phosphoric acid and tincture of ginger. When Gream employed it in summer catarrh he also used acetate of lead ointment applied high up the nostrils, a remedy which, like snuff of bismuth subnitrate, would undoubtedly do much to relieve local irritation and congestion.

Another remedy, somewhat similar in action to nux vomica, inasmuch as it agrees remarkably well in cases of asthma with feeble cardiac power, is the citrate of caffeine. It is doubtful if the salt be a definite citrate, but it is now largely manufactured in Germany and can be easily had in this country. Two or three grains of the citrate dissolved in water or in warm coffee are by no means an unpleasant dose, and in such cases as those just indicated, it is unquestionably of considerable value. Where the citrate of caffeine fails to give decided relief to the asthma it nevertheless, as patients have informed me, greatly mitigates the exhaustion and depression that had been previously experienced after the severity of the attack had passed away.

Welbeck-street.

ON "TENDON-REFLEX."

BY AUGUSTUS WALLER, M.D.

DR. BUZZARD'S recent articles in THE LANCET on the diagnostic use of "tendon-reflex" will doubtless help to popularise the routine employment of this test. It is so simple, so significant as a diagnostic guide, that it ought not to be treated as a refinement, but received in practice as a coarse and useful item. Probably by this time its value as an early sign of locomotor ataxy is recognised in the practice of all who discriminate that disease; less universally, perhaps, its absence is recognised as significant in the discrimination of infantile paralysis, of acute paralysis of the adult, and of pseudo-hypertrophic paralysis (anterior polio-myelitis). On the other hand, it is generally known that "tendon-reflex" is exaggerated in paraplegic interruptions of the cord, that it is not appreciably altered at first in cases of cerebral lesion, and that at a later period of cerebral hemiplegia its exaggeration on the affected side indicates the imminence or onset of late rigidity. These are its chief practical claims. Its physiological interpretation is a different matter, but is of some interest to the physician, if not directly, at least indirectly, as explaining the reason, and furnishing his reference or basis of observation; it is still more important to the physiologist as an article of his neurological creed. Is this so-called "tendon-reflex" in reality an action *sui generis* of which nerves from tendon to spinal centre and thence nerves to muscle are the actual mechanism; or is it no such thing? The affirmative is, with slight variations, the accepted theory of "tendon-reflex."

Seeing that the reaction is abolished by destruction of the spinal centre, or by "faults" in the up and down nerve paths (to use Dr. Buzzard's happy expression), it was supposed, and shortly asserted, that the contraction is excited by an impression reflected from the spinal cord—*non sequitur*. The observation was confirmed experimentally by the artificial production of the above lesions; the reaction was found to fail after destruction of its segmental centre, or after section of motor or of sensory channel, even if tonicity were maintained by moderate faradisation (Tschiriew). Still, *non sequitur*.

I believe¹ that the so-called "tendon-reflex" contraction is not reflex, but direct (as was supposed by Westphal when he first drew attention to the phenomenon), for its interval is that of a direct contraction, and unmistakably different from that of a spinal reflex. Unlike faradaic contraction, it fails after section of nerve or destruction of centre, because the adjustment of irritability and stimulus is a fine one, depending on a muscular tone, which requires the integrity of spinal centre and of afferent and efferent nerves, and which has not been imitated by "moderate faradisation." This very delicacy of adjustment constitutes its value as an early clinical sign.

I would reply to the criticism as to the reliability of measurements, that it is not a question of difference between 2 *versus* 3, or 3 *versus* 4 hundredths of a second, but one between 2 or 3 *versus* 10 or 12 hundredths (the time of a spinal reflex in man), and that this question is susceptible of positive answer, though I am aware that Dr. Gowers' estimate of the interval ($\frac{1}{1000}$ to $\frac{1}{100}$ sec.) seems to contradict this. I would base nothing on differences between 2 and 3 or between 3 and 4 hundredths; in fact, the conditions of this experiment on man, as compared with animals, are necessarily rough, and admit of error between these numbers. Also the latency may vary considerably (i.e., 1 to 2 hundredths) with intensity of stimulation; and, finally, I may add that the direct latency varies greatly (reaching to $\frac{1}{100}$ sec.) with differences in the nature of stimulation. I would, therefore, lay little stress on the identity of "tendon-reflex" latency for muscles at different distances from the centre. Seeing, however, that the latency for "tendon-reflex" is not sensibly different from the latency of a direct electrical stimulus, or of a blow directly on the muscle, and sensibly different from that of an undoubted spinal reflex, I conclude that the "tendon-reflex" mechanism is direct. I find that muscular contraction follows percussion of tendon at an interval of about $\frac{1}{100}$ sec.; that muscular contraction follows muscular percussion at an interval also of about $\frac{1}{100}$

BEQUESTS AND DONATIONS TO MEDICAL CHARITIES. Mr. C. R. Craddock, late of Gray's-inn-square, bequeathed £1000 each to the London Hospital, St. George's Hospital, the Seamen's Hospital Society. £500 each to Middlesex, St. Mary's, Charing-cross, King's College, University College, and the Lock Hospitals. £200 each to the Hospital for Women, Soho-square, the Hospital for Sick Children, the Hospital for Diseases of the Nervous System, Portland-terrace, St. John's Hospital for Skin Diseases, the Central London Ophthalmic Hospital, the Great Northern and the Cancer Hospitals.

¹ See Brain, July, 1880.

sec.; that muscular contraction follows percussion to a nerve trunk at an interval of about $\frac{2}{100}$ sec.; that muscular contraction follows an electrical stimulus to muscle or nerve at an interval of about $\frac{2}{100}$ sec. Conversely I find that the sensori-motor contraction to skin stimuli in paraplegia is above $\frac{1}{100}$ sec., and that the abdominal skin reflex is also above $\frac{1}{100}$ sec.

The numerical argument may be formulated as follows. 1. Phenomena known to be direct ordinarily have a latent period between $\frac{1}{100}$ and $\frac{2}{100}$ sec. 2. Phenomena known to be reflex require an interval of at least $\frac{1}{100}$ sec. 3. Certain phenomena which can be proved to be direct have a latent period as high as $\frac{6}{100}$ sec.—i.e., the latent period varies with the mode of direct excitation. 4. The ordinary period of the "tendon-reflex" is about $\frac{3}{100}$ sec. Conclusion: the so-called "tendon-reflex" belongs to direct phenomena, not to reflex phenomena.

Two facts, in addition to the ordinary clinical argument, are opposed to this conclusion, and must therefore be mentioned.

First, with regard to the paradox of increased contraction to direct percussion with absent contraction to tendon percussion (Buzzard), I have only to say that I cannot explain it, and that I need not. It is comparable with the paradox of increased galvanic excitability with diminished or absent faradaic excitability; but just as in this case, the fact is no argument that the effect of faradaism is reflex, so in the former case the fact does not argue that the contraction of tendon-percussion is reflex.

A more direct objection is that made by Dr. Prévost, of Geneva,² who urges the crossed reflex (which, he says, can be unmistakably seen in experiments on animals), as proof of the spinal nature of the phenomenon. I think it necessary to mention this objection, for it is by far the most weighty, and I cannot meet it at present. I await his continuation of the subject, and admit before hand that the objection must prove fatal unless it can be met.

That "tendon-reflex" is a clinical test of spinal conditions I in no wise deny. A spinal influence is necessary to the normal state of muscle. The testing of a muscle by percussion of its substance or of its tendon is a peripheral test of muscular state, and significant of spinal condition in precisely the sense that electrical stimuli are muscular tests significant of spinal condition. This spinal influence varies with the state of cord; may be abolished with its destruction, or enhanced with its emancipation from superior influence; it may be cut off by section of motor roots, and apparently it is cut off by section of posterior roots; if so, it is reflex—namely, the influence is that known as the reflex tonic influence of the spinal cord. But the individual contraction of "tendon-reflex" is not reflex. This statement is in contradiction with the current clinical theory, and so long as that theory be adhered to, it appears to be in contradiction with clinical facts. But this is only apparent; dismissing the notion of the contraction being a reflex, and viewing it as a peripheral test only, there is no difficulty in explaining our clinical facts. Reflex spinal influence being a condition of the reaction, "faults" on the paths of that influence, or lesions of its centre, or above its centre, will play the same part in the alterations of the reaction as if the reaction were itself a reflex.

It is probable that we shall continue to use the name "tendon-reflex" long after we shall have lost faith in the theory which gave it; but we shall understand that "tendon-reflex" is only one among a class of muscular reactions to mechanical stimuli, in the same category with muscular reactions to electrical stimuli—viz., the category of peripheral muscular tests from which we infer spinal condition.

P.S. (July 12th).—Since the above was written (February) I have had the opportunity of examining, with Dr. Prévost, the question of crossed reflex. We found on rabbits that enervation of one limb, by section of the sciatic and anterior crural, supplemented by section of the posterior roots, did not abolish the crossed movement.³ Percussion of the tendon on the injured side, causing, of course, no contraction on that side, gave on the opposite side a contraction at least as vigorous as before the injury. There is here no physiological transmission, therefore no "crossed reflex"; the effect must be due to the physical diffusion of vibration.

Upper Gloucester-place.

A CLINICAL THERMOGRAPH

FOR RECORDING ON PERMANENT DIAGRAMMS THE VARIATIONS OF TEMPERATURE OCCURRING IN A PATIENT DURING TWENTY-FOUR HOURS.

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IN this instrument the changing pressure of a confined fluid under varying temperatures is used for the purpose of obtaining a motion for registering those variations on the following theorem: that the expansion of a given liquid under the influence of an increase of temperature is proportionate to that increase, and that, its free expansion being limited, the consequent pressure is also proportionate.

The portions of the instrument directly influenced by thermal changes consist of a flat circular vessel constructed of metal, of about three inches in diameter and three-eighths of an inch in thickness; in connexion with this, seen towards the outer edge of the engraving, is a hollow metallic

FIG. 1.

spring or curved tube of an elliptical section similar to that in use, although much smaller, in the Bourdon steam-gauge, and known after its inventor as the Bourdon tube. One end of this tube is fixed to, and communicates with, the chamber or vessel; the other is closed, and actuates a simple lever movement, as seen in the illustration. Now, the vessel and tube being filled with liquid (ether) and hermetically sealed, any increase of temperature causing the contained liquid to expand will produce a pressure acting upon the interior of the tube, or hollow spring, so as to cause the end in connexion with the lever to recede from its position of rest. The lever increasing the first movement some three times moves radially across the face of a disc. This disc is caused, by watchwork occupying the centre of the instrument, to revolve once in twenty-four hours, and is divided by concentric circles into degrees of temperature, and by twenty-four radial lines, into as many hours. The extremity of the lever is armed with an arrangement for marking an ink line on the disc, upon which it lightly bears. So that during the disc's revolution a line is drawn whose position in relation to the concentric circles marks the temperatures, and in relation to the radial lines the times of those temperatures. The discs are made of thin cardboard, and are exchanged after each tracing, the lever permitting of being lifted to allow of their removal. The graduation is effected in a water bath, by comparison with a mercurial thermometer. The upper or working parts are protected from injury by a vulcanite case. Owing to the incompressibility of liquids, the instrument is not appreciably affected by barometrical changes; nor does its use require the movements of the wearer to be in any degree restrained. The principle of the construction permits of considerable variation in size, design, &c.

As described it is applied to the abdomen, the flat metallic under-surface being in contact with the skin, where

² Rev. Méd. de la Suisse Romande, Feb. 1881.

³ Ibid, June, 1881.