the motor centres of the cerebrum has led to lesions in the anterior cornua of the cord. (Pitres, *Société de Biologie*, 1878. MacDonnell, Dublin Pathological Society, 1878.)

Immediately after the subsidence of the febrile symptoms, the keenness of the sensibility seems to be somewhat blunted, as though the nervous influence had been exhausted by the previous excitement; but it speedily recovers, and afterwards remains unchanged. Kennedy gave an account of certain cases in which complete recovery took place; but Heine and Duchenne both denied that these were true examples of this affection, and placed them in a separate category, under the title of "Temporary Paralysis." I am, however, satisfied that there are cases, probably much more frequent than physicians imagine, in which the regression is complete; and I have given instances, which differ in no respect from those which go on to atrophy and deformity, except in the slighter degree of the initial disturbance, and the fact that, after a brief period of paralysis, the regressive movement is carried to its full extent, and the child recovers, without any permanent traces of the attack. It is to be remarked that in all those cases where there existed facial paralysis, or paralysis of the muscles of the neck, these parts were the first to recover their power; and the same holds good of the muscles of the trunk, when they are affected together with the lower extremities; and that in all those cases some groups of muscles remained paralysed, and eventually became atrophied, while others recovered, and that they presented the electrical reaction peculiar to this form of palsy.

Heine asserted (and he has been followed by several other writers) that the true hemiplegic form of palsy never occurred in this disease; but I have seen it in thirteen cases, where the other diagnostic marks were distinct and incontestable. But, as explanatory of the cause of error, I may mention that it was persistent in both limbs of one side in only two cases; in the other it retired, leaving only one limb permanently affected. The crossed form, where the arm and leg of opposite sides are affected, is the most common form when two limbs are paralysed definitely, and was called by Heine spinal hemiplegia. It is the result of an attack affecting primarily either three, or it may be all four limbs (which occurred eighteen times), and which has receded from one or two, leaving the other on opposite sides definitely paralysed.* Of this I have five of the eighteen, the remainder ending in palsy of one leg only. I have not met with any case where there was a progressive increase of paralysis, or where an attack has recurred.

With regard to treatment and its results, I must confine myself here to stating, that hopefulness and perseverance will meet with a reward often exceeding expectation, especially if the treatment be commenced early, and the child's volition be called in to aid the efforts of art.

In conclusion, I may venture to offer a few remarks as to the manner in which the paralysis is brought about. It has been proved by Brown-Séquard, Tiesler, and others, that irritation of a nerve will produce lesion of the spinal cord; while the nerve-trunk, between the seat of irritation and the cord, presents no trace of any irritant action; so that it is quite easy to comprehend how any irritant impression upon a large surface, supplied by peripheral nerves, may cause a profound impression upon the centres to which they lead, just as we know that similar irritants affecting other peripheral nerves will cause convulsions, which are in young children often the expression of irritation of the motor centres (cortical) of the cerebrum.

That the great ganglion-cells of the anterior cornua are connected with special and peculiar nerve-fibres is probable, nay, almost certain; and, if that be so, then the explanation of the facts and peculiarities of this form of palsy is easy. The picking out of single muscles, or groups of muscles, for definitive palsy and atrophy, coincides with the picking out of certain ganglion-cells for destruction, amid others left sound; just as the groups of those vanished cells correspond with the enlargements from which the nerves to supply the wasted limb are given off. And an irritative inflammation, commencing in the cell as the focus of irritation, would, though it might spread readily enough to the surrounding parenchyma, still leave its greatest effect and most lasting traces upon the cell itself. Probably, the first effect of an irritation is to produce changes in the circulation of the nerve-centre itself, and thus involve the surrounding parts, to which the vessels so altered are distributed. At any rate, it appears from *post mortem* records,† that the parenchyma is largely involved early in the process; and, that it is through the instrumentality of the circulation is probable, not only from analogy, but also from the enlarged condition of the circumvascular spaces, which is a common characteristic of the *post mortem* conditions found in these cases.

Lastly, I would urge that the time has come when a greater degree

of generalisation may be profitably admitted in the study of this disease, which has hitherto been almost entirely subjected to analytical examination, a process so essentially needful for the distinction and verification of facts, in a new and unexplored field; but which, when once a sufficient number of facts and observations have been collected, serves rather to cramp and fetter the mind of the inquirer, who should then carry his researches into the wider fields of synthetical study, and ask what are the relations of the facts already known to other forms and manifestations of a different character.

A CASE OF SALTATORY AND GENERAL CLONIC SPASM TREATED WITH CONIUM.*

By E. H. JACOB, M.D.,

Physician to the Leeds Dispensary; late Resident Physician to the Leeds Infirmary.

THE patient whose case is described below has been under my notice twice; the first time, in the Leeds Infirmary (under the care of Dr. Eddison); the second, under my own care at the Dispensary.

J. E. A., aged 27, an iron-moulder, was admitted to the Leeds Infirmary on August 13th, 1878, and gave the following history. Four years ago, his foot was crushed by a stone. The next day, he was seized with a general shaking of limbs and body, which lasted about an hour. This was followed by similar attacks at intervals; but he was generally relieved by resting for a day or two. Eighteen months ago, he received a severe blow in the lumbar region from a crane, which was immediately followed by an attack of shaking. He continued to work, however, though the attacks became much more severe, till about four months before admission. For the last few days, he had had from one to four attacks a day.

On admission, he was fairly nourished, but looked pale. He complained of nothing but the attacks of trembling. These attacks appeared to begin with a slight quivering of one leg; this spread until the muscles of both legs were in rapid vibration. The abdominal muscles were tense, but not quivering. Generally the arms were involved, and occasionally the head. The shaking stopped temporarily if the limb were gently moved. The attacks came on occasionally at night, sometimes without awaking him. There was slight tenderness over the lumbar spines. His appetite was good; his bowels regular. The day before admission, he had two attacks, lasting one hour and half an hour respectively. Sometimes they lasted four or five hours; but he occasionally went eight to fifteen days free from them. He was kept in bed, and ordered half an ounce of conium-juice three times a day.

August 15th. He felt but little effect from the medicine, but had but one attack yesterday, lasting twenty minutes only (the shortest attack hitherto had been about fifteen minutes). He slept well. On August 16th, he had no more spasms. On September 7th, he was discharged, having had no more attacks. The succus conii had been continued in doses of six drachms.

I heard no more of this patient till the end of September last, when he was sent to the Dispensary, to be under my care, by Mr. Stamp Taylor, to whom he had applied for advice. He was then in a far worse state than when I had seen him before. He was pale and wasted, and was just beginning a spasmodic attack similar to those he had in the Infirmary. For the last month, he said, he had suffered from them from one to four times a day.

Beginning with tremor of the legs, the spasm grew more violent, till the patient fairly leaped into the air about eighty or ninety times a minute. His arms then became affected, but to a slighter extent than his legs; and sometimes his respiratory muscles joined in the general spasm, so that he breathed in short painful jerks. He declined to lie down, saying he had rather stand and jump if he had anything soft to stand on, as when he lay down "it shook him to pieces". He had been curious enough to count his leaps, and told me he had had thirty-one thousand in the last month. I gave him, on the spot, two drachms of tincture of conium, the only preparation which happened to be at hand; and ordered him half an ounce of conium-juice three times a day. The tincture of conium had no effect; but the attack passed off in about twenty minutes, leaving him much exhausted. There was some tenderness over the last two lumbar spines, and pressure there gave him a "sensation of cold". He did not know anything in particular which started the spasms, and no pressure-point could be detected.

October 7th. The medicine had had no effect. He had had one or more attacks every day. During the visit, the right arm and leg were shaking vigorously; but there was no leaping. He was ordered to increase the dose of conium till he felt the characteristic effects with which he was familiar, from his previous use of the drug.

* When the hemiplegic form persists, it is probable that it is also produced by regression from one limb where three have been originally involved.

† See Report of meeting of Pathological Society of London, February 4th, 1879.

* Read before the West Riding Medico-Chirurgical Society.

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.

By A. DE WATTEVILLE, M.A., M.R.C.S.ENG.,
Electro-therapeutist to St. Mary's Hospital.

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 1, 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.