another cause, or at least appear to spring from a different origin, and be connected in some way with rheumatism and the rheumatic diathesis. They are as follows:—

CASE I. William Spencer, cotton-spinner, aged 24, was admitted under my care at the Bristol Royal Infirmary in November last. His history was as follows. Four years ago he was discharged from the 2nd regiment of foot, on account of an attack of rheumatism, for which he was in Hospital seven months. His joints were then much swollen. Three years since he felt aching pains in the left shoulder, with stiffness and inability to move it freely; these symptoms gradually increased, and, *pari passu*, the shoulder became more emaciated; he has, however, continued to work at the cotton factory until very lately.

He is now excessively emaciated all over the body as regards the muscular structures, the joints being very prominent, and the muscles feeling like mere cords. His ordinary position is with the elbows flexed to a right angle, the forearms supinated. He states that he can move his arms better at night, especially after exertion during the day; the attempt to straighten the arm gives pain. He can raise the arms without pain, but has not power to lift anything. His general health is good. The first sound of the heart is rather harsh and rough. He perspires excessively in his limbs, particularly on the thumbs, and most remarkably after the galvanism, which was tried without benefit. He left scarcely, if at all, improved.

CASE II. Charles Webber, aged 23, short, but healthy looking, and apparently well made, became an out-patient, October 1856. By trade he is a shoemaker, and always enjoyed good health. He was lately a corporal in the 17th foot, from which he was discharged on account of his present disablement. Six months since, whilst on duty at Limerick, he was exposed to very heavy rains, and continually wet; on rising one morning after exposure could not walk, on account of pain in his right hip, knee, and ankle joints, which then began to swell. He was confined to bed for fifteen days. After the first seven days the pain passed to his shoulder and side of the head, leaving his hips and legs. Soon after the pain reached his arm he found the flesh between his shoulder and elbow diminishing, on the right side first, then on the left. In two months the arms were no larger than his wrists, and he could not lift them to his head. This continued for six weeks, when they began slightly to increase again, and he felt more power in them.

*Present condition.* His arms, from the neck to the elbow, are less than half of the natural size, all the shoulder and humeral muscles being much diminished; the biceps only seeming of anything like the natural size. There is very little deltoid, and the triceps feels like a mere band. His appearance exactly coincided with that of Dr. Reade's patient, as shown in the drawing that accompanies his paper. He cannot raise the arm well, but can flex the forearm on the arm firmly. He had no particular treatment in his regiment; nothing but liquor potassæ. He had syphilis three years ago, and a few spots came out as the rheumatism appeared. He was under treatment for a month, principally with iodide of potassium, and much improved.

I should feel great diffidence in suggesting doubts as to the correctness of M. Cruveilhier's assertions, supported as they appear to be by the observation of the morbid changes, described in the *post mortem* examination of the corresponding case, alluded to by Dr. Reade, did I not conceive that the cases now reported bore me out in the supposition that the cause alleged by Cruveilhier is at least not the only cause of this curious disease; but there are many other objections to this theory to which these cases direct our attention.

It will be observed that in Dr. Reade's case, and in my own, especially the second, there are several common peculiarities.

1. The symmetry of the morbid action.

2. The localisation of it to particular groups of muscles; for example, those of the shoulder and humerus, and, to some extent, the biceps; whilst the muscles of the forearm remained unaffected.

3. The uniform commencement of the atrophy in the upper parts, and progress downwards.

4. The absence, in each case, of more paralysis than must necessarily be the result of such a degree of atrophy; in fact, it is not paralysis, but loss of power from want of muscle.

Each of these, independent of all other considerations, appears to me to afford strong objection to either of these alleged causes of the morbid actions. For,

1st. They require that we assume the anterior spinal roots to be exactly symmetrically affected on either side.

2nd. With a knowledge of the extraordinary complexity of the arrangement of the filaments forming the anterior roots of the cervical nerves, more especially the different distributions and variety of muscles supplied by branches from the fifth, sixth, seventh, and eighth roots, it is almost impossible to conceive atrophy of these anterior roots, affecting only limited groups of the muscles, to all of which these branches proceed; for instance, how can the scapular muscles be affected with paralysis and atrophy, caused by atrophy of the spinal roots, through which their nervous filaments pass, without involving also the phrenic nerve; or of the deltoid and humeral muscles without corresponding affection of those of the forearm and hand, whilst the subscapular, musculo-spiral, and circumflex nerves may be traced through the plexus to the 5th, 6th, 7th, and 8th roots, and the ulnar and median to the same.

3rd. If we allow Dr. Reade's view, that these cases depend on imperfect nutrition, the result of disease of the spinal cord, which involves the influence of the anterior roots—in other words, M. Cruveilhier's theory—we must suppose, to explain these cases, that this function of the cord is so located and circumscribed that disease of its structural instruments may produce its effect without inducing paralysis; or assume (as Dr. Reade states his case proves) that, as the result of organic lesion of the spinal marrow, "muscular atrophy may exist without paralysis, as paralysis of motion may exist without disease of sensation or the converse."

4th. The absence of complete paralysis in each case, and the proportion between the wasting and loss of power, seem to evidence that the loss of power is more fairly to be ascribed to want of nutrition and loss of substance, than to loss of nervous energy.

In fine, I cannot help thinking that the phenomena of these cases give no support to the views of M. Cruveilhier or Dr. Reade; and without considering the evidence of the two I have related, and that of Rokitansky, as sufficient to prove any connection between this curious condition and rheumatism, they do, I think, afford reason to induce us to lean to the side of analogy, and look for some more general or diathetic reason to account for such peculiarities of mal-nutrition. I should add that there was no evidence to lead to the notion of lead affection, nor, as far as I could learn, had mercury ever been largely administered in either of the cases I have reported, though "spots," probably syphilitic, appeared with the rheumatism in the case of Webber.

## THE "HYDATID FREMITUS" OF PIORRY.

By W. J. LITTLE, M.D., Senior Physician to the London Hospital.

I RESPOND, with much pleasure, to Dr. Markham's invitation to contribute my quotum of experience as to the value of the "son hydatique", or the "hydatid fremitus", of Piorry. After perusal of Piorry's treatise, entitled "*Exploration par la Percussion médiate*", I, like Dr. Markham, was inclined to consider that author's account of his *son hydatique* as a pedantic refinement of symptomatology. About 1840, however, I visited, late one evening, in consultation with Dr. Langmore, of Finsbury Square, a gentleman, aged 36, who presented, with other signs of hepatic derangement, an enormous abdominal tumour which had commenced in the right hypochondrium. A recent and sudden aggravation of symptoms occasioned the consultation. I discovered the presence of the "*son hydatique*" of Piorry, and realised the truth of Piorry's description, as quoted by Dr. Markham, "La sensation que le doigt éprouve et que accompagne le bruit (humorique) peut être entièrement comparée à celle que donne une montre frappée sur la surface opposée au verre." Upon the strength of this sign, I suggested that the tumour was probably due to hydatids. I was subsequently informed by Dr. Langmore that the same night the patient discharged by vomiting a *pailful* of them. Some months later I saw the individual, without signs of tumour, in tolerable health, although very spare in habit. I believe that he sank two years afterwards from the same disease.

With my present experience of hepatic tumours, I should, in such a case as that above related, rely much upon the great size of the more or less obscurely fluctuating mass in diagnosing its hydatid origin. I have never witnessed a perfectly similar case.

I once had under my observation an unmarried woman, aged 33, affected with a single hydatid or serous cyst of liver, causing abdominal enlargement equal in size to fully advanced



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pregnancy. She was tapped as for ovarian dropsy; unhappily it was an hepatic cyst, not united to the abdominal parietes, some of the fluid escaped into peritoneum, succeeded by death in eighteen hours. Insufficient stress had been laid upon the sufferer's statement that the swelling commenced at epigastrium.

It is probable that if the nature of the tumour had appeared doubtful, and the opening into cyst had been cautiously effected through the intervention of potassa fusa, the patient's life would have been spared. This unfortunate instance may be serviceable as a warning and as a hint in Dr. Markham's interesting case.

## CLINICAL OBSERVATIONS ON THE TREATMENT OF FRACTURES BY THE IMMOVABLE APPARATUS.

By JOSEPH SAMPSON GAMGEE, Staff-Surgeon of the First Class, and Principal Medical Officer of the British Italian Legion during the last war; late Assistant-Surgeon to the Royal Free Hospital, House Surgeon to University College Hospital, etc.

[*Continued from page* 156.]

IT was remarked in my last communication on the treatment of fractures, that the question is one of fact, and that *by fact it must be solved:* a process much more intricate and fraught with sources of fallacy, than its professed or apparent simplicity would lead one to suppose. Let any one examine the long catalogue of disputed questions in matter of fact in medicine and surgery, and he will arrive at the conclusion that their solution has been prevented, not so much by illogical reasoning, as by error in the observation of fact, and misstatement of the question at issue. To use the words of Sir Charles Bell,* "what are professionally called facts are for the most part only those notions which a man insensibly adopts in the course of his practice, and which take a colour from his education and previous studies. It is this which makes the facts of one age differ from the facts of another age; and the opinions of men differently educated to vary in what they are inconsistent enough to call matters of fact." The medical fact—I use the expression in its largest sense—is not, like the physical, a matter of simple and direct observation: it is, in the majority of instances, only arrived at by observation of several subordinate facts, by an exercise of extreme philosophic caution in excluding fallacy from inquiry into cause, and by logical severity in enunciating a statement of this mental process and its results. The laws of causation—always most intricate in the organic world—are so to a peculiar extent in all that affects deviation from the healthy standard of structure and function, wherein the relation of sequence is of itself no evidence whatever of causal affinity. The number of circumstances to be considered before a pathological or therapeutical fact can logically, and therefore with any sound hope of practical advantage, be referred to one or more causes, is so great, and requires so keen an analytical spirit, as to inspire doubt, whether many of those who specially devote themselves to medical inquiry have anything like a correct appreciation of its legitimate views, requirements, and processes. The following case of difficulty in diagnosing fracture is full of instruction in point in proportion to its simplicity.

An old man had fallen on the pavement, striking the left hip. Unable to rise, he was carried to bed; and a surgeon diagnosed fracture through the femoral neck, from the three following facts: very great difficulty and pain in raising the limb; ecchymosis and crepitation over the trochanter; shortening to half an inch. On being consulted as to the treatment to be adopted, I thus weighed the diagnostic signs. The difficulty of movement and ecchymosis, which were undoubted facts, were perfectly consistent with simple bruise. The crepitus and shortening, however, appeared an unmistakable sign of fracture; but, on examination, I found they were not facts. The crepitus was a slight superficial crackling, not a dull deep seated grating. On placing my hand over the sound trochanter, and moving it, I felt precisely the same crackling, which I consequently regarded as due on both sides to friction between the surfaces, most probably thickened and lengthened, of the old man's bursæ, the subcutaneous one on the trochanter, and the deeper one between that bony process and the insertion of the gluteus. At first the shortening seemed real; but, on placing the hips perfectly bent, and tracing down the bones, I discovered a curious congenital deformity of the internal malleolus on the sound side; it was almost twice its natural size, both in breadth and length. This fact, conjoined with the observation that the length of the two femora, as measured to the patellæ, was perfectly equal, demonstrated the fallacy of the previous measurement, as due, not to shortening of the injured member, but to congenital inordinate length of a bony process of the sound one. The grating and shortening excluded, I suspected the case might be one of simple bruise; such it proved to be, after the old man had been in bed three days, with the benefit of cold lotions.

I have related this simple case to illustrate the complexity of simple medical facts, and the care necessary to their appreciation, even in what is considered one of the most simple departments of surgery—the diagnosis of a fracture. How much more difficult is all that relates to the very beautiful but really intricate subject of therapeutics! a branch of which is the theme of my present series of communications.

To avoid what has been above referred to as the second great obstacle to the attainment of truth in medical and surgical controversy—*a wrong statement of the question*—I purpose clearly to establish the object I have in view in the publication of these clinical reflections on the treatment of fractures of the limbs by the immovable apparatus—a plan of treatment much more comprehensive than might be supposed by its designation according to the instrument employed for carrying it into effect. Its most essential features refer—1. To the principle of immediate reduction, whatever be the direction or character of the fracture; 2. To the immobilisation of the severed fragments, so as to allow movement of the body, whether for mere comfort or more cogent reason, as in the case of fractures in military practice; 3. The employment of gentle and uniform compression as the preventive and curative of the swelling which so frequently complicates fracture; 4. The plan of treatment under consideration aims at discarding the multifarious contrivances at present generally employed in the treatment of fractures, and establishing on a scientific basis general rules of treatment, so far as is warranted by known facts, and by the peculiar difficulties attending generalisation in medicine.

[*To be continued.*]

16, Upper Woburn Place, Russell Square, February 25th, 1857.

* Observations on Injuries of the Spine and of the Thigh-Bone. London: 1824. p. 73.

## LARYNGISMUS STRIDULUS: ITS PATHOLOGY AND TREATMENT.

By THOMAS PAGET, Esq., F.R.C.S., Senior Surgeon to the Leicester Infirmary, etc.

I HAVE been led to select the present subject of to-night's consideration by but slight, if any, merit of originality in the view I am about to bring forward of a very serious infantile disorder. I have not, however, under the head of asthma infantum or cynanche stridula of Parr and Miller, or laryngismus of Good and others, found the disease so systematically and clearly considered as its dangerous import to life and its complications seem to demand; nor any principle indicated in the various treatments named, which by an intelligible philosophy and a simplicity of aim would commend itself to confidence. I cannot hope that my paper will evince any other merit than that of an attempt to supply this deficiency. Others have most probably adopted the views and treatment. I shall have pronounced them.

The phenomena of laryngismus stridulus I need not describe to professional brethren in technical phraseology, much less define with didactic precision. Who has not been sitting by the infant whose mother was recounting the suffocating spasms which had torn her child for the last few days (or rather nights, for the days have been less distressed), while she had gone on hoping from day to day without sending for medical aid? Which of us, while so occupied, has not seen suddenly come a frightened, staring expression in the child?—seen its pallor of cheeks with lividity of lips and orbitar areolæ?—then noticed two or three coughs, expiratory jerks, or shrill cries short as explosions, it is difficult to say which to call them?—and witnessed the tussle that follows, when, the breath being thus jerked out, a closing of the glottis by spasm renders fresh inspiration impossible? How the poor thing writhes, struggles and stretches into opisthotonos, until want of air increases the pallor and lividity, which were at first only those of terror, to those of asphyxia, and the insensibility and relaxa-



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