great practical experience of R. W. Smith led him to hold the exactly opposite opinion. But Smith left the matter with this suggestive remark: "The question is still open to investigation." For my own part, I would submit that these rare cases of inversion are not to be explained by any theory of muscular action. Indeed, Smith appears to me to have exploded that hypothesis by showing that when the deformity has been removed and the limb restored to its natural length by extension, as soon as the extending force ceases to act, though the limb is again shortened, the foot will be found to remain everted. Nor need we attempt to arbitrate between Dupuytren and Smith in their antagonistic theories concerning the relative position of the acetabular and crural fragments as explaining the rare inversion; for there is a much simpler solution of the problem. It is this: If the external violence which broke the neck of the bone did its work when the limb was inverted, inversion may persist. Inspection of Smith's six cases of inversion goes far towards proving the correctness of this theory. Comminution or impaction is described as having obtained in five out of the six instances. The egg will not stand upon the table; but rap its end down upon the smooth surface with enough violence to fracture and cause some comminution of the shell, and a condition of stable equilibrium is at once obtained.

But to return to the question of the cause of the eversion of the limb: Diagram C represents an apparatus by means of which I have been

DIAGRAM C.

enabled to test the relative power of the muscles of external and internal rotation of the thigh, and with this result, that the muscles of internal rotation are considerably stronger than their opponents. A is a strong spring balance, which is connected with a flanged foot-plate B. The balance is fixed upon a board twenty inches by fifteen, and the board is secured against the wall and rests upon the floor. For experimentation, the man lies supine, and with his heel upon the floor, the sole of the foot is lodged within the foot-plate. Now, keeping the knee straight, he to his utmost inverts or everts the thigh, as the case may be, and the central rod of the balance is drawn out, the pounds' force being registered upon a dial D by means of an indicator C, which is connected with the rod of the balance by a slender cord of silk. With the board in this position, inversion of the right limb and eversion of the left could be tested; and to try eversion of the right and inversion of the left, the board would have to be turned upside down. I repeat that I find in this way that the muscles of inversion are stronger than those of eversion. The centre of gravity of the lower limb lies far to the outer side of a straight line running from the centre of the hip-joint to the middle of the heel. Moreover, when a man is lying supine upon a firm and flat surface, the weight of the limb is transmitted to that surface by the back of the pelvis, by the calf, and by the heel; the back of the thigh does not rest upon the plane of support, because the femur arches boldly forwards in its course from the hip to the knee-joint. So, in the supine subject, the centre of gravity of the thigh is

elevated, as well as being to the outer side of the theoretical axis of the limb. During sleep, the centre of gravity assumes the lowest possible convenient position by eversion of the limb. Then the front of the capsular ligament becomes tense and sets all muscles at rest. If the thigh be now somewhat flexed, so as to temporarily slacken the ligament, further eversion is permitted; and if in the mortuary the ligament be divided, still further eversion is obtainable. Moreover, if the neck of the femur be divided inside the capsule by means of a chisel, eversion to its full and characteristic extent is insured. For then the weight of the limb can no longer be received from the head of the femur and transmitted by the pelvis. The limb sinks and rolls outwards in search of stable equilibrium. But granting, for the sake of argument, that the eversion of the limb after fracture of the neck of the femur is due to the action of the pyriformis and company, how are we to explain the outward rolling of the limb which is so characteristic a feature of fracture of the shaft of the femur? "Rotation outwards of the lower fragment", says Mr. Bryant, "is found in nearly every case."

In different British and foreign museums, I have examined and taken notes of one hundred and fifty-two specimens of union after fracture of the femur below the lesser trochanter. Some of the bones have been picked up in graveyards, some had been removed from mortuaries. Some of the patients had been treated, I suppose, *secundum artem*, and many, if we may be allowed to judge from results, had not. Of these bones, only fourteen showed inversion of the lower fragment. In some of these cases, the inversion was but slight; and in most of them the malposition, comminution, and much new osseous cement were noticeable. Of the other preparations, about 50 per cent. showed eversion of the lower fragment, whilst 50 per cent. were femora united with neither inversion nor eversion. Those fourteen exceptional specimens were instances, I take it, of the violence which caused the fracture leaving the limb pounded in a state of secure inversion. So the broken ends of the bone had remained crossed or interlocked, or imbedded in the surrounding and retentive tissues. To attempt to account for the eversion of the limb after the neck or shaft of the thigh-bone has been broken, by the hypothesis of muscular contraction, is to wander from substantial reality to shadowy theorising.

And as an ending to this imperfect paper, I will ask this pertinent question: When administering an anæsthetic for the painless dealing with a broken neck of the femur, has ever a surgeon observed the everted limb to take up of itself a position of inversion or even to right itself?

ON EVERSION IN FRACTURE OF THE NECK OF THE FEMUR, ETC.

By RUSHTON PARKER, M.B., B.S., F.R.C.S.,

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Now that this discussion has been reopened, it may be as well to finish it while it is fresh in the memory. With Mr. Edmund Owen, I perfectly agree, that the eversion of the lower limb in sleep, death, paralysis, and fractured femur, is equally due to the unresisted rolling outwards of the limb, owing to the suspension of the ordinary muscular functions. We are strengthened by the testimony of Professor Spence, in the form of an ancient conviction, and by that of Mr. Cooper Forster in his recent conversion. To me, this was once for all fully proved by a friend, when we were resident colleagues in University College Hospital ten years ago. The fact cannot, I think, be disputed, is probably widely known, and should be recollected in differential diagnosis; though, as Mr. Spence truly says, it needs no profound vindication. Eversion is present in most fractures of the pelvis no less than of the femur, including the neck, and is due in all to the natural falling outwards of the uncontrollable limb, provided the fracture of the neck be unimpacted.

But what is the cause of eversion in impacted fracture of the neck of the femur? This is a point of some interest, because inseparable from a complete discussion, though unnoticed by all the gentlemen who have shared in it, including Mr. Davies-Colley; but one which has been clearly expounded by Bigelow.

Impacted fracture of the neck he has clearly shown to be everted owing to impaction of the posterior wall only, with a mere bending of the anterior wall, or at any rate its non-displacement if actually broken; due to the greater strength of the anterior wall, as anyone may see on examining these fractures in dissection, and on preparing sections of the femur according to Bigelow's directions and engravings.

The rare occurrence of inversion in these fractures he equally clearly explains as due to a greater force, sufficient to overcome and impact the anterior wall as well as, and more than, the posterior (The Hip, pp. 118-138).

The notion of muscular contraction having anything to do with impacted fracture of any part of the neck is obviously untenable; and the shape, direction, and deformity in the unimpacted fractures are perfectly explicable without it. We never need to take it into account in any fractures except those obviously due to no other cause, such as cross-break of the patella, or olecranon. For instance, the notion of muscular action in Colles' fracture has been conclusively disposed of by Gordon, in his admirable exposition of the cross-breaking action at the lower end of the radius, due to over-extension of the wrist restrained by the inextensible flexor tendons until the bone itself gives way; and I made a perfect Colles' fracture at a first attempt on the dead body on following Gordon's directions (Fractures of Radius and Clavicle, etc., 1875).

I quite agree, then, with Mr. Owen, that "muscular action has nothing to do with it", and think the fact that this is so in impacted fracture of the femoral neck helps to justify the notion, which to many is self-evident, that in unimpacted cases it is equally true. I do not think, however, that it is worth arguing about to any elaborate extent with those who do not admit it.

But a word with Mr. Davies-Colley on another matter. In fracture of the neck, shortening is by no means a necessity, as I have had several good opportunities of knowing, either in impacted or in unimpacted fracture. On this point, Bigelow says, "This fracture" (impacted, of neck) "is characterised by shortening and eversion of the limb, sometimes so considerable that we are obliged to accept a diagnosis based upon an almost imperceptible eversion, and a shortening of half an inch or less, by careful measurement".

When I say shortening, I mean "recognisable shortening", whether by careful measurement, or, what I think better, by squaring the anterior iliac spines with the middle line of the trunk, and placing symmetrically the axes of the lower limbs, whether parallel or not. The shortening can then be made out at a glance. So persistently do surgeons expect to find obvious shortening, that I have known the diagnosis suspended, I had almost said declined, for several days, in an unmistakable case, by two men of great experience and undoubted ability.

The diagnosis is really very easy to the surgically minded, especially after a little experience, and most particularly after having failed to recognise the fracture in even a single case. This injury is a very common one, not only in elderly people, but during and before middle life. But to put the exact method of diagnosis into words is hardly more easy than useful, further than to say that, in immediate disablement through a fall on the trochanter, it may be always suspected and generally found. The snapping-off of the head by indirect violence, such as turning in bed, I equally include among those easily diagnosed.

The most important item of all is the treatment, for which Thomas's hip-joint splint is the splint *par excellence*; and the bed must be soft. In firmly impacted fracture, the cure is painless, or nearly so; but the bed must be soft (this must be insisted on). In unimpacted fracture, with shortening, extension should be added, and the shortening will be reduced, and even abolished.

When practicable (which it seldom is), the splint should be put on before moving the patient from the place where he falls; as, in the act of transportation, a limb previously without shortening may be shortened. In fracture near the head, this splint is particularly important (the so-called intracapsular, the separate recognition of which, and its distinction from, so-called extracapsular, I accept Bigelow's opinion in considering an unprofitable attempt at useless refinement). In a few weeks, the patient can get up on crutches, wearing the splint, with a high boot or clog on the sound foot; the supposed inevitable non-union in fracture near the head being prevented, or having the best chance of being prevented.

If non-union should nevertheless occur, a modification of Thomas's knee-splint, as an ischiatic crutch with a single stem, will enable the patient to get about on his lower limbs, without the help of his arms. (See *Hip, Knee, and Ankle*, by H. D. Thomas. 1878.)

SURGICAL MEMORANDA.

THE TREATMENT OF SPINAL CURVATURE BY THE PLASTER-OF-PARIS JACKET.

AFTER an extensive trial of this method of treating spine-disease, I am inclined to regard it as one of the most important additions that have been made to surgical therapeutics for many years. It is not to be wondered at that there are a few who object both to the principle as well as the practice. Dr. Ormsby's experience, however, cannot be an unusual one, for he says (JOURNAL, October 11th) "those attempting to cure spinal curvature by this treatment must be ready to expect

serious and at times alarming consequences, besides numerous disappointments". No doubt, there will be some disappointments; but, as to the "serious and alarming consequences" to which he alludes, I am bound to say that I cannot imagine what they can be. I have a large number of children at the hospital undergoing this plan of treatment—cases of lateral and of angular curvature, as well as general debility, where the spine seems to be weak and yielding. There is one point on which I join issue with Professor Sayre's teaching. I heard his demonstrations at Cork, and have read his remarks at Amsterdam. In applying the jacket, he advocates the use of an under-vest that fits like the skin; and, when the plaster bandages are being applied, he recommends that they shall be well rubbed in and made to fit into the intercostal spaces and all other irregularities in the chest-wall. In this I cannot agree with him. Even if during the extension and while the chest is fully expanded the jacket fit into all these inequalities, after extension has been left off, it must become comparatively loose. I quite agree that the jacket must be accurately moulded to the fixed points, and that it should not fit loosely anywhere; but I think that any attempt to make it fit like a glove would be unphysiological, and that it would lead to "alarming consequences". Unfortunately for the subjects of spine-disease, the chest has movable walls; but undue interference with these movements, such as would result from encasing it within an unyielding plaster-jacket, that fits well into all the intercostal spaces, would add rather than otherwise to the patient's discomfort. I have been greatly pleased with the result of this mode of treatment; and my success has been considerable, though I have not carried out Professor Sayre's instructions in the particular point just alluded to.

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AN UNUSUAL CAUSE OF TYMPANIC ABSCESS.

TYMPANIC abscess with rupture of the membrane so frequently occurs at all ages, without any pain or antecedent symptoms, and without any discoverable cause, that any agency or exciting influence bearing on the subject is worthy of note, as by increased knowledge such accidents may possibly be diminished in the future. I therefore draw attention to the following case, as I have met with no similar history amongst numerous instances of abscess and perforation coming under my notice.

A gentleman about seventy years of age consulted me recently, concerning a discharge from one ear and deafness on that side. He stated that, a month previously, he had been seized with a violent attack of epistaxis. The nares were plugged at first anteriorly; but, this proving insufficient, the posterior nares were also plugged. The posterior plug, however, only remained *in situ* twenty-four hours, and then he discovered that he was totally deaf on the right side. He suffered no pain in the ear; but in three weeks a copious discharge suddenly commenced. On examination, a watch, which he could hear at a distance of six inches from the left ear, was only audible on close contact with the right side, where a perforation was discovered, through which pulsation could be observed in the tympanum, accompanied by tinnitus. A week afterwards, he also a sharp attack of inflammation of the meatus, which yielded to treatment in a few days.

As to the causation of this accident, it may be explained in several ways. At the time when the epistaxis occurred, an effusion of blood may have simultaneously taken place in the tympanum; but I incline to the belief that the abscess was due either to blood being forced into the Eustachian tube or tympanum, or, more probably, that the orifice of the Eustachian tube and adjacent parts were bruised by the manipulation of the plug; and that mischief in the tympanum was set up sympathetically, which would certainly, in many respects, be an interesting pathological fact to aurists, as having a direct bearing upon the question of operations in the postnasal region. Finally, I may remark that, in cases of otorrhoea and perforation, many surgeons would be surprised to find, on inquiry, how rarely any history can be obtained, except when scarlet fever or measles is the known agent.

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UNREDUCED DISLOCATION OF THE THIGH INTO THE FORAMEN OVALE.

WILLIAM HENRY R., aged 1 $\frac{3}{4}$ years, was brought to the Lynn Hospital on August 23rd, as the mother said, because the child had lost the use of his left leg. No satisfactory history could be obtained as to how the accident occurred. Until August 1878, the child was quite well and walked about. On returning from school one day in August of last year, the mother noticed that the left leg appeared to hang loose, and the child complained of pain and inability to get about. She did