

state of the heart and lungs in the above condition of the patient, a condition which, in short, is syncopal asphyxia. In this state we have long had abundant evidence to prove that the right side of the heart is full of blood, and that the lungs are congested. I shall not here inquire which event is the primary one. It could easily be proved that the two events are differently related in different cases, but that in all cases the one increases the other. A feeble heart does not send the blood with sufficient force through the lungs, and so permits congestion to take place in them; while the lungs, if congested from any cause, resist the influx of blood from the heart, and therefore cause it to be gorged on the right side; and this engorgement, again, greatly impedes and enfeebles the action of the heart. As might *a priori* have been expected, the same condition of the heart and lungs has been found by all experimenters with chloroform. Thus Professor M'Kendrick and his coadjutors, in their report to the British Medical Association (p. 30), writes as follows: "One of the most striking effects of anæsthetic agents is the engorgement of the right side of the heart and large veins near it. This has been directly observed by the committee, and is well known." They then proceed to discuss the share taken in this engorgement by the heart itself, which is known to be enfeebled by anæsthetics, especially chloroform, and by certain changes in the lungs which they found to be much congested in all cases of deep chloroformisation. Into this discussion we need not at present follow them; I only wish to direct attention to the fact that in a state of deep anæsthesia from inhalation of chloroform there are always coexistent (1) a feebly acting heart, (2) an engorged state of the right side of the heart, and (3) a congested state of the lungs. In the suddenly alarming cases where syncope occurs it cannot be doubted that all these conditions are exaggerated in an extreme degree, and the object of treatment certainly ought to be the restoration of the organs to a normal state as speedily as possible.

Now in all writings and text-books on this subject one of the recommendations to the surgeon, in order to accomplish this end, is to invert the body of the patient by raising his lower extremities above the level of the head and the upper part of the body. This practice is said to have had its origin in an experiment performed by Nélaton, and it generally goes by his name. His great character as a surgeon has given it a wide currency, a currency which, so far as I know, was not warranted by either the accuracy or the conclusiveness of his experiment. It is now blindly followed, I think, without consideration of the effect of the proceeding, or with very loose notions of its effect. Thus, in an otherwise good paper by Mr. Samuel Osborn, chloroformist to St. Thomas's Hospital, I find these words (p. 15):—"Inversion of the body should be always tried if stoppage of the heart's action occur. The head being lowered and the legs elevated, the blood is sent to the upper part of the body." Of course it can only be the blood in the veins of the lower part which can be so passed to the upper part of the body, and everyone knows that it must pass first through the right side of the heart, and then through the lungs, before it can be sent to the upper part of the body. But the evil against which we are supposed to be contending is that there is too much blood already in the right side of the heart and lungs, and that it cannot get on. To send more venous blood to these organs just then is surely to aggravate the mischief, especially where the heart's force is greatly diminished, and the respiration is inefficiently performed. Even although the latter function be carried on artificially it cannot be sufficient of itself to renew the pulmonary circulation while the heart remains feeble, and one great cause of its continuing feeble is the weight and paralysing influence of engorgement of its right cavities. It seems to me, then, that this inversion of the body has been recommended in these cases without due consideration, and that it is not suitable to them. If, in spite of the valves in the veins, the venous blood from the neck and arms did get to the brain by depressing the head and neck, as is sometimes done, it could only deepen the coma and increase the evil from the side of the nerve centres; and, as I have shown above, the venous blood from the lower part of the body, if forced upon that already filling the right side of the heart and lungs, could only embarrass the circulation, and still farther add to the danger of the patient.

The best position for such patients, as for all in syncope, is the prone position, because in it a feeble heart has least hindrance to its work in sending, not venous blood to the lungs, but arterial blood, both to its own substance and to

the brain. No doubt these two results of cardiac action take place simultaneously, but it is on the latter—viz., the sending forth of arterial blood, that our hopes for the patient mainly depend, and the importance of artificial respiration in assisting this is obvious to all who rightly consider the matter. The *prone position, and not more than prone*, along with artificial respiration, is that which favours most the reinforcement of the heart, and this is the one object of hope in all such cases. But the practice of inversion of the body has been so constantly recommended that no one at present thinks he does his duty to his patient unless he has recourse to it, and I believe some operating tables are now made with a lever so as to enable the surgeon readily to invert the table and the patient upon it. Now this, I submit, is both an unnecessary complication of the table and a practice really hurtful to the patient, because in so far as it succeeds in sending venous blood of the lower part of the body to the heart it tends to embarrass and weaken that organ, already labouring under influences formerly explained, against which the best remedy actually is to diminish the blood, which stagnates in the right heart and lungs; and this we attempt to do by artificial respiration with the patient in the prone position more physiologically, and therefore more hopefully, than by inversion.

I have no doubt on several occasions seen inversion performed in cases where, during chloroformisation, there was a temporary cessation of the pulse or the respiration or both—in fact, where syncope had occurred and in which recovery took place; but I believe the recovery was in spite of, rather than because of, the inversion of the patient. In fully as many cases of the kind I have not followed this practice, and yet the recovery was as rapid and as complete as in the others. It is in no spirit of boasting, but rather of thankfulness, that I state, notwithstanding my long experience of chloroform, now extending to more than twenty years of hospital and private practice, I have never yet seen a patient die of that anæsthetic.

In all cases where I have been present, the result has been uniformly fortunate, whether Nélaton's method was used or not, and therefore it may perhaps be said that my experience proves nothing one way or other; but undoubtedly there have been a good many, only too many, unfortunate cases, both recorded and unrecorded, in which the patients died in the chloroform syncope, and I believe in almost all of them inversion was performed, but did no good; and, if my view of the matter be correct, it rather did harm. The surgeons in these cases certainly did their duty in performing inversion, because it is at present the recognised practice; but now, when it is asserted to be contrary to sound views of the physiology and pathology of the case, it may cease to be any longer recommended or practised. At all events, it will surely come to be more accurately discussed, and either established on a sound basis, if that be possible, or authoritatively discarded altogether, as I think it ought to be. With this object I now write, and appeal to my surgical brethren, especially to those of them who can look at the matter from the side of physiology. If I am convinced that my opinion is wrong, I shall be very ready to confess it; but if I am right, I may have assisted in simplifying the treatment of some very alarming cases, in which one would wish to do everything that is possible towards their recovery.

Glasgow.

ON A CASE OF TOTAL SUPPRESSION OF URINE LASTING SEVENTY-FIVE HOURS; RECOVERY WITHOUT A BAD SYMPTOM.

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ON Wednesday evening, Dec. 27th, I was asked to visit J. C—, who was said to be suffering from retention of urine, not having passed any water for twenty-four hours previously.

The patient, who was an engine-driver by occupation, forty-two years of age, married, and had four healthy children, gave me the following history:—"I have always been a healthy and vigorous man, accustomed to take plenty of exercise, and never had any sickness as long as I can

remember, with the exception of an attack of syphilis about twenty years since, which gave me very little trouble. About three years ago a rash came out on my body, some of which still remains." This was found to be psoriasis, patches of which were found on his stomach, legs, and arms. "Eight months ago I was treated for dyspepsia, which was probably produced by alcohol (which I have been in the habit of taking, but not to any great excess), but more likely came on from a shock caused by a break down to my engine. I do not feel so lively in myself since that happened, but there is no actual impairment in my health. I had been taking whisky rather freely for the past four or five days and not eating much. On the morning of the 26th December I felt my water a little troublesome, having to pass it more frequently, and very little coming at a time. The last I passed was at one o'clock P.M. on that day, on the journey down to Cork: about a wineglassful of a very high colour. Since then I have made several ineffectual attempts to pass water, though not feeling any particular desire to do so." He had taken a teaspoonful of spirits of nitre in the evening, to try to start the water, and had a glass of punch on going to bed, to make him sleep, but did not succeed with either. His bowels had not been opened for three days. When I saw him he made no complaint of being sick in any way; he had no headache or pain in his back—the only trouble he had was the inability to pass water, and a consequent wakefulness. His expression was good, tongue furred and slightly red at the tip, pulse 80, skin dry, temperature normal, no fulness or pain on pressure over the bladder or kidneys. I passed a No. 8 gum elastic catheter, which produced a slight sensation of nausea on going through the urethra, and of pain in the bladder. No water came—not even was a drop detected in the eye of the instrument. When it was withdrawn, he had a slight desire to urinate, but without success. I ordered him a mixture of jalap, sulphate of magnesia, and senna, with cream of tartar and mucilaginous drinks.—Dec. 28th, 11 A.M.: Slept about an hour during the night, and vomited once some sour green matter; bowels opened twice, solid discharges; no water. Asked me not to pass the catheter as he felt there was no water in his bladder; feeling a little thirsty but by no means uncomfortable. Ordered to continue his mixture and have his loins poulticed with linseed meal, the poultices to be made with infusion of digitalis and applied every four hours. 6 P.M.: Skin dry; no appearance of urine; bowels acted once loosely; no vomiting. Ordered a jalap draught and a mixture containing squills, digitalis, and spirits of nitre; to have a hot bath and be wrapped in blankets.—29th, 9 A.M.: Slept for two hours. Skin acted well after the bath. No water passed for sixty-eight hours. Feels quite well; has no headache or pain in his back; no hiccough or eructations; stomach retentive; pupils normal; pulse 80; tongue somewhat cleaner; thirst continuing; no urinous odour from skin or breath. Says he would be all right if his kidneys acted. I passed a catheter into the bladder, which I found very much contracted and perfectly empty. The same fruitless desire to pass water came on when the catheter was withdrawn. Ordered to sit in a tub of hot water for an hour, to be dry cupped over the loins, the poultices and mixtures to be continued, and, as his thirst was troublesome, I gave him permission to drink a couple of pints of warm ale. At 1 o'clock P.M. the patient was in the same condition; felt somewhat refreshed after the bath and had taken the ale with great relish. At 4 o'clock P.M., after seventy-five hours' total suppression, about two ounces of high-coloured urine was voided, giving him a scalding sensation in the urethra. At 5 P.M. his bowels acted freely, and shortly after he passed about twelve ounces of high-coloured urine, lighter in shade than that which he had previously passed, having a sp. gr. of 1010, and not containing a trace of albumen. From that time the secretion became perfectly re-established. He complained of being rather weak in the morning, was kept in bed for four days on fluid diet, and in four days afterwards was on duty, feeling as well as ever. I regret I did not make a microscopical examination of the urine that was first passed, but it was thrown away before I could do so.

Total suppression of urine is of very rare occurrence, and comes on most frequently as the attendant or result of other diseases, but in this instance the suppression must, I think, be considered as the disease itself, and a question arises as to the cause of it. Was it from obstruction? or did it proceed from renal congestion? If due to obstruction in the ureters or pelves of the kidneys, we would have had symptoms of nephralgia, the passage of bloody urine,

calculi, &c., which were absent, and to produce complete suppression from obstruction both sides should be simultaneously blocked, a very unlikely occurrence, or we should take for granted the absence, or previous destruction from disease, of one or other of the kidneys, which we have no ground for doing in this case. I am inclined to consider it one of those rare idiopathic cases described by Sir Henry Hallford as paralysis of the kidneys, depending most likely on extreme vascular congestion of those organs, overwhelming, as it were, or paralysing their secreting powers, and due here most likely to alcoholic irritation and exposure to cold as an exciting cause. I shall be glad to learn if any of the readers of THE LANCET have met with a similar case.

Cork.

TRACTION IN HIP DISEASE, AND THE CONSEQUENCES THAT MAY FOLLOW.

By JOHN JONES, M.R.C.S.

Two years have now elapsed since my former communication on this rare complication appeared in THE LANCET (Feb. 12th, 1881), and the time has now arrived when it may not be inopportune to add some further particulars as to the sequel. In any remarks I may have to make, I trust it may not be inferred that I have any wish to detract from the value of traction *per se* in hip disease. On the contrary, in witnessing it for the first time in this case, I was much struck with its simplicity and its adaptation to the purpose for which it was intended. To account for the novelty to me of the traction method, I should add that I retired in 1865, after thirty-eight years' experience of a very laborious country practice (probably before the introduction of the traction method), and I confess from that time I have taken but little interest in what has been going on in the medical world.

The patient at the present time is in excellent health, though pallid (as he ever has been), and backward in his physical development, as a consequence, it may be, of extraordinary mental aptitude. The lameness may be approximately estimated from a shortening in the aggregate of two inches, or but little less than under the old system of treatment. He is equal to any amount of walking exercise with comparative ease, and has no pain or uneasiness in the act or consequent upon it.

I will here give the comparative measurements of the right and left limbs in inches:—

	RIGHT.	LEFT.
Inches.	Inches.	
From the anterior superior process of the ilium to the extreme point of the malleolus internus ...	31	33
From the same point of the ilium to the centre of the patella ...	17½	18
From the centre of the patella to the extreme point of the malleolus ...	13½	15
The fibulae are precisely the same length on either side—viz., ...	14	14

From these measurements it will be seen that three-fourths of the shortening is below the knee, and due to the separation—a striking example of the arrest of the growth of a bone on being separated from its epiphysis. As to the immediate cause of the separation, in the absence of injury or disease to account for it, I think there can be but one opinion, and however rare or remote such a result as happened in this case may appear to be, yet it clearly proves it to be within the range of possibility.

There may have been less power of resistance in the tissues in this case than in others of a kindred idiosyncrasy, but there was nothing in the aspect of the patient to indicate this. Under these circumstances the question suggests itself whether all the advantages of traction might not be equally secured by placing the fulcrum on the femur, immediately above the condyles, instead of the ankle; it might be thought advisable also in such a case to attach the foot to the cord to prevent inversion.

I have reason to believe that the separation took place at a very early stage of the treatment, for on my first undertaking the charge of the case (owing to the serious illness of the gentleman in attendance) which was in Nov. 1879, just twelve months after the patient had been under treatment, I noticed an enlargement at the head of the fibula. I was perplexed, and feared there might be some mischief going on in the tibia, but there was neither heat, swelling,