

conceivable when it is realized that the part of the articular process which bars the way to reduction is only about six millimetres in height, perhaps less.

The advisability of using ether in all attempts at reduction is too apparent to require mention.

With regard to the method usually advised and followed, Liddell says (*loc cit.*) p. 723 :

"Had I this case now to treat, I should, as soon as it became clear that the man would not recover under an expectant line of treatment, that is, on the second morning after the accident, relax his muscles completely by administering an anæsthetic, and then by carefully made *extension and rotation, etc.*,²⁶ try to restore the dislocated bone to its normal position."

He quotes Ashhurst (p. 729) as saying: "In the treatment of dislocation of the cervical region, the mortality has been nearly four times greater when constitutional or general treatment has been relied on exclusively, than when attempts have been made to reduce the dislocation by *extension, rotation, etc.*"²⁶ Liddell continues: "It seems to me that the inference is fairly warranted from the foregoing considerations that *extension*²⁶ (combined, of course, with rotation or pressure, as required) should be employed in every case of spinal dislocation or of spinal fracture with dislocation where the spinal functions are disturbed.

Looking more closely into the order of procedure, we find that one plan seems to have been to first rotate in the direction in which the head is already turned, then to extend and rotate the head back to place (Maxson,²⁷ Wyeth²⁸); another method has consisted of extension followed by rotation (Rathburn,²⁹ Gray,³⁰ Perisot, Berthold,³¹ Lucas³²); another, suspension of the head and rotation of the body (Morton³³); another, as extension and manipulation (Van Walther). In bilateral dislocation steady extension, followed by direct replacement without rotation, has been adopted (Ayers³⁴); with slight rotary movement (Stout); extension with rocking movement, and finger in the pharynx (Carter³⁵).

With regard to the degree of force used in extension, this seems to have been in most cases very considerable: Suspension of the head between the hands (by Morton); strong extension (by Wyeth); a considerable amount of force — "all, I believe, I was capable of exerting" (by Carter); chin and occiput held by the hands of the operator covered by those of another, strong traction being employed with counter-extension by folded sheets around the shoulders, a third physician placing the hands under those of the other two, to aid replacement (Ayers); "steady pulling, gradually increasing" (Thon³⁶); three pull the head and one the shoulders with the whole weight (Schranth); place the knees on the shoulders, drawing the head, then turning it into position (Erichsen); a hand on the forehead and on the occiput covered by those of an assistant, counter-extension being firmly maintained (Gray).

Even the degree of force indicated has not always proved successful, as in the case of Warren, in which

subsequent spontaneous reduction pointed the way to direction rather than force as the key to the situation.

It is certainly in the line of advance (notably illustrated by Bigelow's brilliant exposition of hip-joint reduction) in other dislocations to do away with this excessive force, and to follow backwards as nearly as possible the movements made in dislocation — an object which we are here aided in accomplishing by the use of the fulcrum.

The question naturally presents itself: After how long a time shall we advise operation? Gray's case was successful after four months. The spontaneous reduction in Warren's case, already alluded to, took place after nearly as long a period. Guerin reduced a case after seven months. We should err on the side of caution when any question of fracture is present, or where great force is to be used, even in simple dislocation; but when we consider how little force is necessary by the method advocated, as shown by Beach's case, there seems little or no risk in attempting reduction after a considerably longer period than four months has elapsed, though the exact length of that period it would seem premature to determine without further experience. Attempts should be abandoned if the displacement is not easily reduced.

SALINES IN APPENDICITIS.¹

BY S. C. GORDON, M.D., PORTLAND, ME.

DR. M. H. RICHARDSON, in his article published in the JOURNAL of September 28th, objects very strongly to the use of salines in appendicitis, especially in the early stages; and inasmuch as this is the time when in my opinion they are the most required and will do the most good, I cannot allow some of his positions to pass without comment, particularly as I am one who has "advocated" the prompt and free use of this method of treatment.

In the first place, I think Dr. Richardson does not fully state the case when he says: "The theoretical action of cathartics in peritonitis, as given by various men, consists in an absorption and removal by intestinal drainage of the toxic products of certain micro-organisms which, multiplying in or near the peritoneal cavity, endanger life." Now while this is true, it does not express the whole truth. I am just old-fashioned enough to believe that general bleeding oftentimes does an immense deal of good in the sthenic form of inflammation, not by removing "toxic products" alone, but diminishing the volume of blood in the general circulation, thus unloading the distended vessels in the part inflamed, and facilitating absorption of the liquid exudate, before any purulent degeneration takes place in it. I believe and know from "very considerable experience" that leeches will almost invariably stop the inflammatory process in orchitis and many other local inflammations, for the same reason. It is not simply the removal of "toxic products," but in the case of appendicitis it is more the removal of exudate while in a liquid state, in order to prevent the "toxic" elements from the intestines poisoning it and thus producing pus.

When we are called to the case there is a beginning or well-advanced peritonitis, caused by septic matter

¹ A reply to Dr. Richardson's article of September 28, 1893.

²⁶ The italics are mine.

²⁷ Buffalo Medical Journal, January, 1857, p. 479.

²⁸ Hospital Gazette, New York, June 28, 1879, p. 275.

²⁹ Peoria Medical Monthly, 1839, ix, p. 280.

³⁰ *Loc. cit.*

³¹ Monthly Abstract Medical Science, June, 1875.

³² Medical Gazette, Sydney, 1884-5, iv, p. 41.

³³ Medical Record, October 4, 1879.

³⁴ New York Medical Journal, January, 1858, p. 13.

³⁵ New York Medical Record, 1885, xxviii, p. 257.

³⁶ Austral. Medical Gazette, Sydney, 1883-5, iv, p. 82.

that has escaped from the appendix (in the cases where perforation has occurred), all the blood-vessels in the vicinity are enormously distended, and exudate is being poured out in the intervascular spaces. The prompt and free use of salines unloads these distended vessels, and promotes absorption of the exudate, before it is converted into pus. By thus promptly relieving the congested vessels, and removing pressure, pain and fever are both relieved, and in nine cases in ten, in my "considerable experience" the case is practically ended for that time. Other and similar attacks may follow soon or late, unless operation for removal of the diseased organ is made in the interval, which I always advise.

Again, Dr. Richardson says: "I do not object to carrying out this theory after the appendix has been securely tied, or after it is clear that there is no danger of rapid extravasation, but in the first forty-eight hours of appendicitis, I look upon the administration of salines as extremely dangerous, and as a not infrequent cause of general peritonitis and death. The reasons for this lie in the pathological conditions that exist in a very considerable percentage of cases. If in a given case there is a perforation in an appendix of large lumen, salines by liquefying the feces and increasing peristalsis, will cause an immediate and almost invariably fatal extravasation."

My answer to this is: First, the septic material has already escaped from the appendix and produced the peritonitis, and removing the "shell" which is left does not remove the cause, for that has already "gotten in its work" and done all the harm it can. It is the condition we are called upon to treat, and not the "tenement" that once held the toxic element. Removing the former habitat of the cause does not remove the cause or the effect, but it does add traumatism to an already serious matter.

Again, in my "very considerable experience," I have never seen a case, with one exception, where any fecal matter has existed in the "lumen" of the appendix, no matter how "large" the lumen. The effect of inflammation is to close the end nearest to the cæcum, so that no material escapes from the bowel. Salines do not, therefore, cause "extravasation" from the intestine. In the single exception alluded to, there was simply the color of fecal matter, and not any substance, that had escaped, and the proximal end was closed, so that no more could escape.

Dr. Richardson says in a subsequent paragraph, "There is the same objection to the use of salines in gunshot wounds of the intestines, in perforation of typhoid fever, or in perforating ulcers of the intestinal tract generally." I agree that there is a most decided objection to the use of salines in the pathological conditions named, but I fail to see wherein the analogy lies between these conditions and those usually found in appendicitis. In the latter there is a perforation through the appendix only, which allows some septic material to escape, while the opening to the bowel is practically shut up. While in the "gunshot wounds, etc.," there is a direct opening into the bowel itself. While the "same objection" may apply in the latter condition it fails to apply in the former, simply from the fact that the conditions fail to be the same. In "catarrhal appendicitis without perforation" I agree fully with Dr. Richardson that it is the "mildest of diseases" (of the kind), if you always know it when you see it. But even that is attended with a certain

amount of peritonitis: and salines can do no harm, but can, and do accomplish much good by terminating the case by rapid resolution, thus preserving the integrity of the tissues, so that perforation is much less liable to occur, and "general spreading peritonitis" is prevented. I am fully in accord with Dr. Richardson when he says that these cases of "general spreading peritonitis, far from being curable by salines, are, in my opinion and experience, beyond relief even by the most radical surgical measures, except in very rare instances." I know of several instances, however. I do believe also if the rule should be established in the profession, that at the very earliest symptoms of appendicitis salines should be promptly and effectually used, a large number of these cases could be prevented.

When we consider the percentage of natural cures reported, we should hesitate long before operating in the acute stage. Ranvers reports 2,000 cases in the Prussian army with 96 per cent. of cures without operation; Charité Hospital, Berlin, 54 cases with three deaths; Nothnagel's Clinic, Vienna, 65 cases and three deaths, etc. But I do believe strongly in operation after a well-marked attack is convalescent. There is always danger of relapses, and the danger of operation in the interval is, in my opinion, very small — one or two per cent. possibly.

Since June 14, 1892, I have operated nine times in the interval of attacks (none less than two, and one eight attacks). Seven of the nine I treated through the acute stage by salines. During that time I have also treated four others without operating afterwards. All recovered without any serious symptoms following the operation. No one of the four has yet had relapse. Some of these cases I did not see until pus had formed (as the operation afterward showed), and yet I preferred to risk medical rather than surgical treatment until convalescence was established. I think the result proved the wisdom of my course. Results are, after all, what we work for; and while statistics are often fallacious, yet we are governed by them in a measure in our own line of work.

I am not prepared to say that I should refuse to operate in the acute stage, where there is clearly an abscess to be drained, or a "general spreading suppurating peritonitis"; but I shall in the future be governed by my "very considerable experience," and treat all cases in the early stages by salines.

THE SYMPTOMS OF SENILITY IN PARALYSIS AGITANS.¹

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THE recent investigations of Dana² and Ketscher,³ throw a flood of light on the morbid changes which produce the symptom-complex known as paralysis agitans. These changes, confined chiefly to the cord, consist in dilatation of the blood-vessels, infiltration of round cells with marked degeneration and pigmentation of the nerve cells. Ketscher found miliary aneurisms and hæmorrhages throughout the cord. Both these authorities unite in saying that the primary change occurs in

¹ Read before the Pittsburg Academy of Medicine, December 4, 1893.

² Journal of Nervous and Mental Diseases, May 1, 1893, pp. 359, 360.

³ Neurolog. Centbl., No. 5, 1893 (quoted in Journal of Nervous and Mental Diseases).