

TRANSACTIONS
OF THE
PHILADELPHIA ACADEMY OF SURGERY

Stated Meeting Held December 6, 1926

The President, DR. CHARLES F. MITCHELL, in the Chair

ETHYL CHLORIDE IN LOCAL ANÆSTHESIA

DR. JOHN B. CARNETT demonstrated a method of securing satisfactory local anæsthesia with ethyl chloride. The skin and subcutaneous fat overlying a superficial abscess can be completely anæsthetized by spraying with ethyl chloride but the inflamed hypersensitive tissues beneath the abscess are not influenced by this anæsthetic. The usual practices of making an incision either by a simple heavy stroke of the knife or by transfixion and cutting from within outward, transmit pressure or pull through the abscess to the adjacent non-anæsthetized tissues causing great pain. By using a thin-bladed sharp knife and making pressure, too gentle to be transmitted to the depths, all pain is avoided. This may require two to six gentle knife strokes to cut through the perfectly anæsthetized tissues overlying the abscess.

The most satisfactory container for ethyl chloride is the one having a screw valve to regulate the size of the stream. The nozzle of such a container can be held within two or three inches of the abscess and by adjusting it to a very fine spray, anæsthesia results very promptly. The stream from a valveless container is so coarse that even when sprayed from a distance of 16 to 18 inches the ethyl chloride does not evaporate properly, hence anæsthesia is delayed, solution is wasted, unevaporated solution tends to flow into undesired situations as into the eye, and the area to be anæsthetized cannot be so accurately circumscribed. Manufacturers of valveless sprays should be condemned for their practice of sending printed instructions to blow on the surface to expedite evaporation of the solution. Blowing tends to contaminate the surface and succeeding wound with a variety of pathogenic germs from the surgeon's mouth.

Four or five years ago he adopted ethyl chloride as the preferred local anæsthetic for taking biopsies from ulcers suspected of being cancerous, the danger of embolism of cancer cells is increased by the injection of anæsthetic solution but is minimized by freezing which tends to hold the cells *in situ* during the incision.

CHRONIC STRAIN OF THE LUMBAR SPINE AND SACRO-ILIAC JOINTS

DR. JOHN B. CARNETT read a paper with the above title.

DR. DAMON B. PFEIFFER remarked that Doctor Carnett's theory is a very ingenious and possibly satisfactory explanation for certain cases of the ten-

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derness which is frequently elicited in the right and left iliac fossæ. Some years ago Robert Morris maintained that tenderness in this same location was an important sign of chronic appendicitis, his explanation being that chronic disease of the appendix gave rise to a state of irritation and tenderness over the lumbar ganglia.

At the last meeting of the American College of Surgeons in Philadelphia, the speaker had occasion to participate in a consultation with Sir Arbuthnot Lane in a case of carcinoma of the transverse colon. The condition was sufficiently obvious, but he was especially interested in Sir Arbuthnot's method of abdominal examination. He laid great stress on tenderness in the same locations pointed out by Doctor Carnett. His interpretation of the matter was that carcinoma was a sequence of intestinal stasis and this in turn by causing strain on supporting intestinal ligaments, gave rise to the obvious tenderness in the right and left iliac fossæ.

In the presence of such differences of opinions the matter cannot be regarded as settled. There is no question that many appendices are being removed largely on the evidence of tenderness in the right iliac fossa, and it is well to realize that there are other pitfalls in the diagnosis than the well-known sources of error such as renal and ureteral disease, pelvic disease, lymphatic glandular inflammation and other rarer conditions.

RESULTS IN GALL-BLADDER SURGERY

DRS. E. L. ELIASON and L. K. FERGUSON read a paper with the above title.

DR. GEORGE P. MULLER remarked that in this subject the primary mortality is of first importance and he was pleased to note that Eliason and Ferguson had grouped their cases according to pathology and not according to operation, cholecystectomy or cholecystostomy as is so often done. It makes little difference which operation is done in chronic gall-bladder disease, with or without stones. The mortality is negligible and dependent upon other factors than the type of operation. In acute empyema of the gall-bladder the case is different and here the mortality from cholecystectomy is higher than cholecystostomy. There is need for discrimination even if a secondary operation is necessary later.

In the common duct group he was somewhat at a loss to find the reason for his high mortality. He delays operation and practices careful pre-operative care. Perhaps it would be better to tap the gall-bladder if it will drain or the common duct if that is necessary and leave the removal of the stone, as suggested by Crile, to a later time. It certainly is a mistake radically to "spoon" the ducts in a sick patient. These patients often have a pancreatitis and extensive liver involvement and correct post-operative treatment is essential. Blood transfusion and glucose intravenously are routine on his service.

End result studies are not always easy. In his own hospital the follow-up is highly efficient but the patients are not followed for a sufficient length of

POST-OPERATIVE WATER METABOLISM

time. In a paper from the Mayo Clinic published last summer, it was noted that cases considered as cures came back many years later with recurrence of the gall-stones. In the series which he reported last year 85 per cent. of the chronic gall-bladder group with stones and 70 per cent. of the simple cholecystitis claimed perfect health. The difference probably represents the margin of failure to cure the complete pathology. He had been working on this last group lately. It seemed to make no difference whether or not the appendix was removed concurrently, nor was age a factor. The end results are apparently a matter of correct diagnosis, precise operating, and the avoidance of drainage whenever possible. However, he had 90 per cent. of perfect results in the empyema group and 76 per cent. of cures in the common duct cases, both of which were extensively drained.

In regard to anæsthesia, he now uses local anæsthesia and ethylene. Impressed by the use of posterior splanchnic anæsthesia in the hands of Eliason and Ferguson, he tried it in a few cases, but the marked fall of blood-pressure was very alarming, even though all of his cases recovered.

DOCTOR ELIASON remarked that the alarming fall in blood-pressure which accompanied splanchnic anæsthesia had been obviated by reducing the amount of adrenalin used. He now uses only half the amount which he formerly employed.

INTRADERMAL TEST FOR THE DETERMINATION OF POST-OPERATIVE WATER METABOLISM

DRS. KENNETH E. APPEL and SELLING BRILL read a paper with the above title.

DR. GEORGE P. MULLER said that this test seems at first a trifling one, the producing of a wheal and the watching of the wheal disappear. But it has given rise to the most interesting amount of speculation about acidosis and water metabolism. He thought when they first started this work that local acidosis would explain the phenomenon. In one case of diabetic gangrene they were interested to know whether to amputate below or above the knee. The disappearance time was 40 above the ankle, 50 in the calf, and 60 at the knee; according to this test he could safely amputate below the knee. This was done even though he knew how dangerous it is in the average case. The stump has done well.

According to the later theory, however, the test seems to relate to an entirely different thing and has nothing to do with acidosis. As a means of determining whether or not the patient is in need of water, the test may hold promising possibilities.

DOCTOR BRILL said that they were not at all certain of the value of this test in interpreting post-operative water metabolism; the mechanism and the disappearance of the wheal is not at all understood. Originally, workers with this intradermal wheal attempted to explain it on the basis of the work of Martin Fischer, who pointed out that colloids imbibed water when their acidity was increased. Fischer further believed that there was a local

acidosis of the tissues, and this caused an imbibition of water. On this basis he attempted to explain all types of oedema. That there may be a local acidosis has been recently confirmed by the work of Rous at the Rockefeller Foundation.

However, especially since the publication of Loeb on "Proteins and the Theory of Colloidal Behavior" in 1922, this theory of Fischer's has been discredited because Loeb showed that this imbibition of water with increased acidity took place at a pH below the isoelectric point, and that there was also a swelling of the colloid with increased alkalinity on the alkaline side of the isoelectric point. The isoelectric point of most of the body proteins is in the neighborhood of a pH of 4.7 to 5. Physiological processes in the body take place at a pH of 7 to 7.4. Therefore increased acidity would be expected to cause a decrease rather than an increase in the absorption of water by the body colloids at this pH. As a matter of fact there is no doubt that a shift of water between cells, lymph, and serum takes place with a change in pH. However, in which direction this shift takes place it is premature to attempt to state at this time.

The disappearance of the wheal post-operatively might perhaps be explained by considering the relation between the capillary pressure and the osmotic pressure of the blood. Post-operatively, there is perhaps a dehydration which causes an increase in the osmotic pressure of the blood and therefore a more rapid disappearance of the wheal. This is in line with the explanation given for oedema by Starling in his book on *Human Physiology*. They are now at work on this problem.

NOTE.—Since the presentation of this paper, further work, under direction of Dr. J. H. Austin, has shown that the above explanation is probably incorrect.

USE OF THE RUSSELL APPARATUS IN THE TREATMENT OF FRACTURE OF THE SHAFT OF THE FEMUR

DR. THOMAS J. RYAN read a paper with the above title.

DR. DAMON B. PFEIFFER said that this method is a valuable addition to the treatment of fractures. It employs the principle of balanced traction in a most ingenious manner and if general experience bears out the satisfactory character of the results as reported here, it should be widely adopted. Apropos of the fact that quite a number of the cases reported in this paper were children, he would mention two features which are valuable to remember.

The first is, that it is comparatively easy to "set" a transverse fracture of the femur in children. Of course it is useless to attempt to replace spiral or oblique fractures by manipulation, but in the case of transverse fractures, by a combination of traction and manipulation, it is nearly always possible so to lock the fragments as to make the after-treatment a very simple matter. The most effective manoeuvre in bringing about this end-to-end apposition is to angulate the bones sharply with traction on the distal fragment and then, holding the bones in alignment in the plane of angulation to bring them

FRACTURE OF THE SHAFT OF THE FEMUR

slowly up to their normal position. Even when the fracture has existed for some days and overriding is extreme so that direct traction will not succeed in reducing the shortening, it is usually possible to lock the fragments in this manner. Recurrence of deformity may be prevented readily by coaptation splints and fixing the leg in suspension with just enough traction to effect immobilization. He was not referring, of course, to supracondylar fractures or those of the upper end of the femur, but only to those which occupy a position somewhere within the middle two-fourths of a long bone.

The second important consideration in the treatment of fractures in children is the fact well-known to those who have had experience in the treatment of acute fractures during the early years of life, that growing bones have a truly marvelous ability to correct malpositions and even shortening due to lack of correct anatomical reposition after fracture. Ashhurst has called attention to the way in which function and muscle balance shape the skeleton in childhood. Truesdell, Speed, and others have shown vividly that misalignment and shortening may be almost disregarded in early childhood. A fractured long bone is stimulated to increased growth by the fracture, and if shortening is present it soon overtakes its fellow on the opposite side. Angulations and overriding are so compensated that it is often difficult after the lapse of a year or two to discover either by examination or by the X-ray where the site of fracture has been. He ventured to speak of this at some length because these factors are not generally appreciated. About two years ago in taking over a surgical service from one of his colleagues in a hospital in this city, he found in the children's surgical ward a small boy about five years old who had had a fracture of the middle of the femur. He had been treated by the overhead suspension method of Bryant. The alignment was good and the shortening not more than a half inch. Callus had already formed and the fracture was fairly stable. Relying on the facts just stated he felt that the end result would be not only good, but perfect, and in due time arranged to send the child home. Unfortunately in this case one of the members of the family happened to be a nurse from another city and she asked to see the X-ray and was horrified to find the position of the fragments as he had just described them. He explained to her the prospects, but it developed that with a true layman's horror of X-ray deformities she took the child to see a well-known surgeon in this city, whose work is not with acute fractures but with chronic bone conditions, and to the speaker's great surprise he sent the child back to the hospital, broke up the union, and put on a plate. A better appreciation of the behavior of fractures in children would be a safeguard against the perpetration of bad surgery of this sort.

DR. J. TORRANCE RUGH remarked that every one of these cases presents its own problems. The results shown by Doctor Ryan are certainly good and yet when one tries extension in other cases and after a most thorough application of the extension for a period of ten days or perhaps two weeks, one fails to get apposition and fails to overcome the overlapping—then one wonders how it is that the extension always succeeds in the cases reported.

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It is his practice to operate, if after ten days, apposition of the fragments cannot be secured.

DR. A. P. C. ASHHURST said that it is a pleasure to see surgeons take an interest in fractures of the femur, even if it requires a new and complicated apparatus to interest them. In all probability, the same results can be obtained by simpler means, but if it takes a complicated apparatus and a newer method to interest them, let us have it. Doctor Ashhurst agrees with Doctor Pfeiffer in that he never saw a fracture of the shaft of the femur in a child which did not give a good result, no matter what the treatment. To operate for malunion, in a case such as he has described, would seem to border on malpractice.

DR. GEORGE P. MULLER said that this method of treating fractures of the femur is not new in principle but has brought out several interesting points. The results obtained are probably no better than those which Doctor Ashhurst reported to the American Surgical Association as having been secured by Buck's extension, but the dressing is exceedingly uncomfortable for the patient and the results speak for themselves. It is true that they plated several of the cases, but then the method is not infallible and there will be occasional failures. In addition to the fractures of the shaft reported, they had several cases of fracture of the neck of the femur with good results and there was one pathologic fracture of the femur secondary to carcinoma of the prostate which healed in perfect position. They had one case of peroneal palsy in which a simple sling was used under the knee and with the leg on a pillow. This apparatus is more comfortable than a plaster case and patients seem to like it better than the Buck's extension.

Replying to Doctor Rugh's remark, Doctor Muller said that he operated on three of the twenty patients; one without reason, and one maybe without reason, and the other because he could not get reduction. Patients require attention with the Russell apparatus. If shortening recurs when the weight is removed, it means interposition of the tissue between the fragments. In the cases reported he secured perfect apposition.

CHANGE OF EDITORIAL ADDRESS

The office of the Editor of the *Annals of Surgery* has been changed to 489 Washington Avenue, Brooklyn, New York. All contributions for publication, Books for Review, and Exchanges should be sent to this address.

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