

loss of weight of several years' duration. She was a poorly nourished, anæmic, tired-looking woman. In the epigastric region there was marked splashing and less marked splashing in the cæcal region, with slight pain on pressure in the latter. The stools were constipated and foul, and the urine contained many catarrhal cells and a few pus cells. Examination with the X rays showed a slightly enlarged and proptosed stomach, and retention of bismuth in the duodenum (Fig. 4), pronounced ileal stasis, as well as marked cæcal and colon stasis.

A course of medical treatment, including careful dieting, vibratory massage, enemas and lavage, was carried out for over six weeks, with very slight benefit, the symptoms returning as soon as the patient began to get about. An operation was decided on, preference being given to the short-circuiting operation.

At the operation, performed by Mr. Alexis Thomson, the stomach was found to be slightly enlarged, the pylorus was distinctly narrowed, the second and third parts of the duodenum were very markedly dilated, and numerous firm adhesions were found involving the terminal coils of the ileum, while bands connected the abdominal wall to the upper part of the ascending colon and to the first part of the descending colon, these findings amply confirming the X ray diagnosis recorded above. In view of the severity of the adhesions it was thought possible that the correction of these and anchoring the cæcum might suffice to give the patient relief. Accordingly the cicatricial bands in relation to the ileum and ascending colon were divided, the raw surfaces that resulted from this were covered with adjacent peritoneum, and the cæcum was anchored by the method of Wilms; all the raw surfaces in the affected areas were smeared with vaseline. The result of the operation, however, has not been satisfactory, it being doubtful whether the patient has derived, or is going to derive, any benefit from it. It is probable that a later short-circuiting operation or colectomy will be called for. Three months later the report is more satisfactory, the patient has benefited considerably from the operation, her condition, however, not being entirely satisfactory.

The results of operative treatment in this case, and I have seen several others with similar results, rather go to support Lane's teaching that the adhesions are more a result than a cause, and therefore treatment directed simply to the correction of adhesions will in most cases prove of little value.

General Remarks on the Operative Treatment of Advanced Cases of Intestinal Toxæmia.

Our views on the value of operative treatment in this group of cases must at present be tentative. The operative procedures requiring consideration are: (1) the treatment of any adhesions or pathological kinks which may be found on examination; (2) reducing the size of the cæcum and anchoring it *in situ*; (3) short circuiting; and (4) colectomy.

With regard to the treatment of adhesions Case 4 indicates a practical difficulty in so far as in some of these cases the benefit following the operation is slight. This must be explained either by the reformation of fresh adhesions or by accepting Lane's view that these adhesions are a result and not a cause of the primary condition. With regard to the operation of plication of the cæcum I have had in the course of the past year a series of seven cases in private practice, in which Mr. Henry Wade has operated and reduced the size of the cæcum, anchored it to the abdominal wall, and severed any adhesions involving the terminal ileum and colon. Great benefit has followed the operation in four cases, the others being much less satisfactory. In the successful cases, the extent to which the benefit was due to the modification of the cæcum or to the correction of the adhesions is unknown. In one case in which the operation was unsuccessful the X ray examination yielded strong evidence which suggested that the stitches plicating the cæcum had given way. On the whole, I am less satisfied with

the result of this operation than I was with my first two cases. There is, I believe, much to be said in favour of the wider application of the short-circuiting operation as recommended by Lane in cases that are specially selected. The number and type of cases likely to be benefited by short circuiting can only be determined after a much fuller and more extended experience of that operation in the hands of the skilled general surgeon. The operations of short circuiting and colectomy are open to grave abuse unless they are undertaken only after careful expert diagnosis of the condition and the previous application of a thorough course of medical treatment.

The general conclusions to which I draw attention are as follows.

1. The diseases under consideration are essentially medical conditions, capable of being cured by appropriate treatment if thoroughly carried out in the earlier stages; the question of surgical treatment should therefore seldom arise.

2. There are at present many advanced cases of these diseases in which prolonged medical treatment on the most modern lines produces little or no permanent benefit, and in which benefit may be looked for from appropriate surgical treatment, which effectively reduces the septic absorption from the large bowel. Examination by means of the X rays gives information of very great value in these cases; special attention should be directed to the cæcum, ileum (terminal part), and duodenum, and the results should be studied along with the other clinical findings.

3. The operation of short circuiting as recommended and performed by Lane is a simple one, but its nature is such as may readily lead to unsatisfactory results in less experienced hands.

4. Before any operation is decided upon a prolonged course of medical treatment, carried out with great attention to detail, is indicated.

5. When these medical measures have failed to relieve the operation of short circuiting, possibly combined with colectomy, partial or complete, as recommended by Lane, should be performed.

I have pleasure in expressing my indebtedness to Dr. W. Hope Fowler for the time and trouble he has taken in the investigation of the numerous cases forming the basis of my paper.

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THE DRAINAGE-TUBE IN ABDOMINAL SURGERY.

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OF recent years the value of drainage in the treatment of peritoneal infections has been very seriously called in question, and in an admirable paper published in THE LANCET¹ Cuthbert Wallace gives it as his opinion that "drainage save in true intraperitoneal abscess is useless." He speaks from an extensive experience of every type of intraperitoneal infection, and states further that the surgeon's duty has been performed when the focus of infection is removed and the abdomen closed. This view implies that the faith of the operating surgeon must be placed in the bactericidal properties of the peritoneum rather than in a temporary exit to noxious and infective fluid from the peritoneal cavity. Such an attitude is certainly strengthened by recent investigations

¹ THE LANCET, June 15th, 1912, p. 1603.

into the bacteriology of the infected peritoneal cavity, though there are still surgeons who advocate multiple drainage in septic peritonitis, and even drainage of non-infected areas to lessen the risk of spread from an infective focus. Wallace considers that the operation in acute abdominal diseases enables the surgeon to deal with the focus of infection and relieve intra-abdominal tension, and whilst I am in the main heartily in accord with his views, I think some consideration of the pathological aspects of the case may be of value.

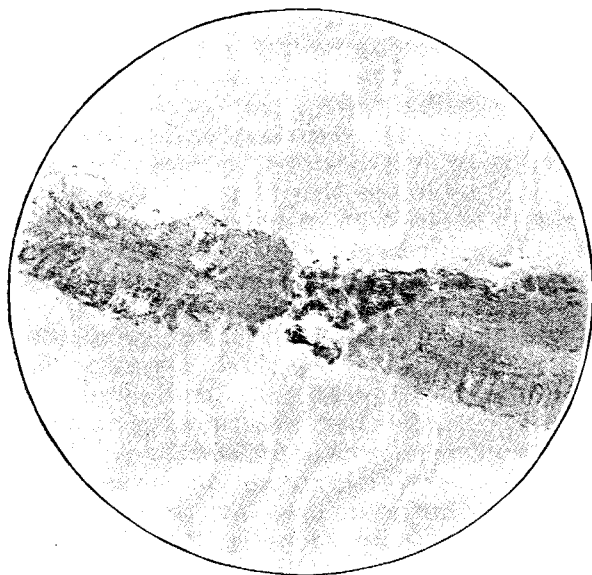
In a recent work on "Acute Abdominal Diseases"² Cassidy and I have pointed out that the resistance of the tissues of the abdominal wall is inferior to that of the peritoneum, and therefore we have advised drainage of the abdominal wall in such operations as those for the removal of an acutely inflamed appendix, and my own experience of such drainage in preference to placing a tube in the peritoneal cavity has been most encouraging. In a good many operations for acute, gangrenous, or suppurative appendicitis, both in adults and children, where a few years back a tube would have been left from the cæcum to the exterior, such tube drainage of the abdominal wall has been entirely successful and reopening of the wound has not once been called for. Wallace admits that complete closure of the abdomen in these septic cases is at times followed by virulent suppuration in the planes of the abdominal wall, and that the patient may die from such septic poisoning and not from peritonitis. Surely, such a catastrophe can be avoided by instituting free drainage of these muscular and fascial planes, and such a step appears to me to be called for in the "relief of tension," for dangerous tension is not merely that of the peritoneal cavity but of the abdominal wall also. This procedure is in keeping with the treatment of diffuse cellulitis, where suppuration is threatened, by multiple free incisions.

On the other hand, there is much truth in the saying that it is better to drain than not to drain, and it is manifestly wrong to close an acute intra-peritoneal abscess; the difficulty lies in the distinction between a true abscess cavity within the peritoneum and an area infected with virulent organisms; if there is no definite space to drain it is probably better not to insert a drainage-tube. The following experimental observations throw some light on this question, but it is abundantly clear that the surgeon requires a knowledge of the bacteriology of his operations before he can truly decide where lie the uses and abuses of the drainage-tube.

To ascertain the changes induced by the presence of a rubber tube in the normal non-infected peritoneal cavity, I introduced a sterile tube into the abdomen of several rabbits; the tubes were passed into the peritoneal cavity amidst the coils of intestine and sutured at one end to the abdominal wall, which was then closed. This does not represent accurately the manner in which they are used in human beings, but the results correspond to the actual changes brought about by the presence of such tubes. I found that in a very few hours lateral openings in the tubes were closed by plugs of inflammatory exudate, and in 24 hours the whole of the tube was plastered over with newly formed fibrin, with numbers of red blood corpuscles and degenerated finely granular leucocytes. Cultures were taken before the introduction of the tubes

and at the time of their removal; in all the experiments they remained sterile. The factors which determined these peritoneal changes were: (1) the presence of the foreign body, and (2) the pressure exerted by it; the latter was attributable to the fact of the tube being anchored to the abdominal wall, for in some other experimental cases where similar tubes were placed free in the peritoneal cavity little or no change occurred around them, and they could be easily withdrawn at the end of four days.

In the presence of organisms changes similar to these occur with greater rapidity, but the inflammatory processes do not stop at fibrinous closure of the tubes. One of the known effects of organisms is the liquefaction of the tissues, and therefore tubes in infected areas may be expected to yield fluid exudate. To reproduce in experimental animals the conditions obtaining in the operative treatment of human peritoneal infection is practically impossible, therefore in this direction I have investigated the effect produced by the presence of small infected cotton-wool sponges inoculated with 24-hour cultures of *staphylococcus aureus*. By analogy I think it is fair to infer that these results represent to some extent changes due to the pressure of foreign bodies in the presence of infection. In two cases out of seven where such infected sponges were used second cultures obtained by removing the sponge and dropping it into a broth tube showed the presence of bacilli as well as cocci; in one case the *bacillus coli*, and in the other the *bacillus proteus*, was so recovered, and in both the *staphylococcus aureus* was still present. The only possible source of these bacilli must have been the lumen of the gut, and the accompanying figure, which shows microscopically the formation



Section of intestine over sponge infected with 0.25 c.c. of broth emulsion of 24-hour agar culture of *staphylococcus aureus*. The sponge had remained in position for one month. Shows almost complete destruction of the intestinal wall proceeding from the serous to the mucous coat, the gap being filled up with inflammatory cells. This represents an early stage of faecal fistula. Objective $\frac{1}{2}$ inch.

of a faecal fistula, readily explains the secondary infection of an embedded sponge. Macroscopically the gut over the sponge appeared thickened and inflamed, but stained sections reveal the fact that a portion of the wall is composed almost entirely of fibrin and inflammatory cells. In this connexion I may, perhaps, refer to a short note by Mr. A. E. Maylard,³ in which he describes the way in which an

² Baillière, Tindall, and Cox, London, 1913.

³ THE LANCET, Feb. 28th, 1914, p. 608.

iodoform gauze plug placed in the abdomen during a gall-stone operation ulcerated its way into the bowel and caused symptoms of obstruction. This was an operation performed in the presence of infection and the passage of the plug into the lumen of the gut was clearly due to infective ulceration from its peritoneal aspect, the site of entry being healed over and protected by adhesions. It was successfully removed some 14 days after the gall-stone operation.

The consideration of the secondary infections of drained wounds would be incomplete without reference to aerial contamination, and whereas such infection may be held to be very rare during the course of an operation it is well known that any wound, abdominal or otherwise, treated by tube drainage for any length of time is apt to yield cultures of such organisms as the bacillus proteus in addition to the original causative bacterium, or this latter may be entirely outgrown by such a contaminating bacillus. It is clear, therefore, that in the case of persistently drained abdominal wounds the primary infection may have added to it organisms from without or within; the former may be derived from the external air or implanted during the dressing of the open wound, the latter may come from the lumen of the bowel as the result of pressure on the bowel wall.

From these experimental and clinical observations the following conclusions appear to me to be justified. 1. That owing to the adhesions which rapidly form around drainage-tubes their value is distinctly limited in the treatment of peritoneal infections. 2. That since the tissues of the abdominal wall possess a much lower degree of resistance than the peritoneum, drainage of the former may frequently be called for where drainage of the peritoneum is not necessary. 3. That the presence and pressure of a drainage-tube in peritonitis may determine the transudation of organisms from the lumen of the gut to the peritoneal cavity.

With regard to the last observation this secondary infection is of less serious importance than might be supposed owing to the fact that the wound area has usually been shut off by surrounding adhesions before this occurs. To prevent adhesions between drainage-tubes and adjacent structures the tube should always be rotated whenever the dressing is done, and its gradual withdrawal from the wound should be begun from the third day after operation. I have said nothing about so-called gauze drains because I believe they are more properly termed gauze dams, except when used in the form of a small wick down the lumen of a tube as a "cigarette drain," and there the capillary action of the gauze may be of value in securing the escape of thin inflammatory fluid. In the case of thick pus they more often act as a cork at the orifice of the wound than as an external drain.

As to the value of the Fowler position and the pelvic drainage-tube there is still some difference of opinion. I think there can be no doubt that the former represents the ideal attitude of the patient both for the surgeon and the nurse during the early days of abdominal convalescence, and it seems clear that the complications of subphrenic, perisplenic, and other residual abscesses have been greatly reduced in frequency by its adoption. Pelvic drainage-tubes do not appear to form adhesions quite as readily as those in other situations, and when the tube is correctly placed so that its deep end lies about half an inch from

the bottom of the pelvic basin drainage even in this uphill fashion can be secured for the first few days of convalescence.

At the present time it seems totally irrational to be a slavish adherent to the policy of either always draining or never draining, but it appears to me that much of the drainage of the peritoneal cavity now practised would be better limited in scope to the abdominal wall alone.

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THE WILD MONKEY AS A RESERVOIR FOR THE VIRUS OF YELLOW FEVER.

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THE possibility of the wild monkey acting in certain places as a reservoir for the virus of yellow fever is a very interesting one. On my way to the West Indies I was told by a lady, Mrs. Randolph Rust, who has been long resident in Trinidad, that the old negroes there say they can always tell when there is going to be an epidemic of yellow fever in the island owing to the fact that prior to its appearance the red howler monkeys are found dying and dead in the High Woods. In view of Manson's injunction to search for a reservoir of the virus amongst some of the lower animals this negro belief struck me as curious and interesting, though naturally one did not place overmuch credence in it. Still it seems worthy of investigation, and Mr. Randolph Rust, the chief pioneer of the petroleum oil industry in Trinidad, whose land is in the south of the island at a place where monkeys abound, kindly promised to make further inquiries.

When I finally reached Port-of-Spain this question of the monkey assumed fresh interest and importance. Dr. H. L. Clare, the Surgeon-General, gave me particulars regarding the recent small outbreak of yellow fever at Brighton near the pitch lake in the south-west of Trinidad. He told me that the origin of the infection was shrouded in mystery, for there was no question of importation from the mainland or elsewhere, and, so far as could be told, there had been no mild cases of fever or illness amongst the coloured employees of the American company which is boring for oil in this vicinity. Further, the two men first attacked were employed during the day on an oil bore situated at the end of a road which had recently been cut for eight miles into the dense virgin forest. In this locality the red howlers occur, though they are only seen as a rule in the early morning. I told Dr. Clare the story about the negro belief. He had not heard of the latter, but was much interested, and agreed that it merited careful inquiry and investigation. At a later period I found several people who knew all about the negro view, but it had not struck them as deserving special attention. Mr. Rust got hold of his chief hunter, who confirmed the negro statement in every particular, and said that before the last large epidemic of yellow fever in Trinidad the red howlers were found dying and dead in large numbers. Further confirmation was forthcoming from Mr. F. W. Urich, entomologist to the Board of Agriculture, an acute and trained observer, who told me that he himself had seen the monkeys lying dead, and at the time thought they had probably perished from the effects of a protozoal