

Correspondence

Long term consequences of Roux-en-Y

Sir

Mr Earlam is to be congratulated on his extensive review of bile reflux and Roux-en-Y anastomosis (*Br. J. Surg.* 1983; **70**: 393-7). He has wisely refrained from discussing the results of Roux-en-Y since they are difficult to assess and there are few controlled clinical trials. I have never performed a Roux-en-Y anastomosis for post-gastric surgery complications or severe persistent symptoms but I have had referred, either for assessment or further remedial surgery, 12 patients who had this operation or its even worse modification, the Tanner's roux 19 operation (1). The long term consequences (5-10 years) of these patients were as follows: marked weight loss (mean 35 kg) in all; bacterial overgrowth, 12/12; steatorrhoea, 9/12; hypoproteinaemia with a serum albumin <3.59/dl, 11/12; anaemia with a Hb <10g, 12/12; and bone disease, 2/12. It is indeed correct that they had all stopped vomiting bile but the long term nutritional consequences were such as to be life threatening in at least 5 of these unfortunate patients. My experience, such as it is, has convinced me that this operation is simple and attractive to surgeons in the anatomical sense. Physiologically it is unsound. It is ulcerogenic, promotes inadequate mixing of biliary secretions with food and invariably induces mal-absorption. These considerations assume clinical importance since the majority of these patients by virtue of their age and benign disease have a long life expectancy and thus the long term consequences cannot and should not be ignored.

A. CUSCHIERI

1. Tanner N.: Surgery of peptic ulceration and its complications. *Postgrad. Med J.* 1954; **30**: 448-65.

Sir

In his interesting article (*Br. J. Surg.* 1983; **70**: 393-7) Mr Earlam refers to William Beaumont MD as a Canadian. Beaumont was a surgeon in the United States Army. Alexis St Martin, the subject of his experiments, was a Canadian voyageur. Furthermore, Beaumont's famous work (1) was published in Plattsburgh, NY, rather than in Pittsburgh Pennsylvania. To the best of the writer's knowledge no medical work except this so important one has been published in Plattsburgh.

Beaumont's work demonstrates how much can be accomplished under less than ideal conditions by the prepared and dedicated observer.

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1. Beaumont W.: *Experiments and Observations on the Gastric Juice and the Physiology of Digestion*. Plattsburgh: Allen, 1833.

Sir

In my article I stated 'the decision to do a Roux-en-Y anastomosis must not be taken lightly... subsequent complications consist of those that can normally follow a partial gastrectomy and those that may occur after operations on the small bowel. These over-simplified statements concerning the possible side effects of this operation hide the mass of physiological changes which follow bile diversion. Therefore Professor Cuschieri's letter is important for its emphasis that this operation has potential complications which must in every instance be balanced against the symptoms that the patient wishes to lose. Most of the operations I do are in my opinion salvage procedures for 'gastric or oesophageal cripples'; I perform them rarely and only as a last resort but never as a primary procedure because of these possible complications.

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In situ femoral, popliteal and distal bypass for limb salvage

Sir

After carefully reading the paper by Denton et al. (*Br. J. Surg.* 1983; **70**: 358-61) we think that some comment is required. In view of our own extensive experience in this area, it was disturbing to see the reported 28 per cent overall thrombosis rate and specifically a 40 per cent thrombosis rate for bypasses to the tibial position in this series of cases. Since 1976, we have accumulated 427 in situ bypasses of which 259 (61 per cent) have been placed in the tibial vessels for limb salvage. The 1-month cumulative patency rate for the tibial bypasses is 94.2 per cent and the 4-year cumulative patency rate is 73.6 per cent (1).

One of the major advantages of the in situ technique is the ability to use veins which are significantly less than 4 mm in diameter. In fact, 60

per cent of the entire series and 77 per cent of the tibial bypasses themselves have been done with veins which are smaller than 3.5 mm when accurately measured by callipers. Denton et al. have reported a 44 per cent failure rate with these veins but our experience is that vein size has little or no effect on the performance of these bypasses. Comparison of life table plots of bypasses done with distal diameters 4 mm and greater and those with vein sizes 3.5 mm and smaller show very little difference over the 5-year interval. One likely explanation for Denton et al.'s high thrombosis rate may be the endothelial trauma caused by the repeated passage of a rigid intraluminal instrument. The literature indeed presents a variety of techniques for rendering valves incompetent when performing in situ bypass. However, to our knowledge, the published results show that the most successful technique for rendering the valves incompetent has been the method of valve incision (2). In any event, the most important detail to be observed during the performance of valve defunctioning is the avoidance of intraluminal shearing forces to which the endothelium is exquisitely sensitive. With respect to the segment of vein which exists below the knee joint, we have found that the retrograde valvulotome is the only safe instrument for use in this area and only after the vein has been fully dilated either by arterial or controlled hydrostatic pressure (3).

With respect to the failures which occurred in the grade III (poorer) outflow tracts, we have demonstrated that such outflow tracts have little to do with patency of the in situ bypass because of its ability to function adequately even at very low flow rates. In fact, we now have a total of 15 bypasses carried out to discontinuous tibial vessels which have functioned equally as well as those which were placed in more anatomically favourable situations. The overriding factor which we have correlated with the success of the in situ bypass is meticulous avoidance of venous endothelial injury.

Although we continue to use the in situ saphenous vein to the distal popliteal position when this is the area of insertion of choice, we also believe it is a grave mistake for an individual surgeon to abandon the reversed vein bypass graft in favour of the in situ bypass unless the in situ technique which is used is one that is known to produce results which are superior to those achieved with the reversed vein. Unfortunately, such results are not apparent in this paper.

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1. Leather R. P., Shah D. M. and Karmody A. M.: Infrapopliteal arterial bypass for limb salvage: Increased patency and utilization of the saphenous vein used 'in-situ'. *Surgery* 1980; **90**: 1000-8.
2. Karmody A. M., Shah D. M. and Leather R. P.: A five year consecutive study of the saphenous vein used "in-situ" for arterial bypass. In S. Stipa and A. Cavallaro (ed.) *Proceedings of the Sero Symposium*, Volume 44, *Peripheral Arterial Disease: Medical and Surgical Problems*. London: Academic Press, 1982.
3. Corson J. D., Leather R. P., Shah D. M. et al. Retrograde valve incision for in-situ bypasses utilizing a valvulotome. *Ann. R. Coll. Surg.* 1983. (In the press.)

Sir

While acknowledging the extensive experience of in situ vein grafting of Karmody and his group in Albany, I think the comments of Corson et al. call for clarification.

It is meaningless to criticize our failure rate of 40 per cent femoro-distal bypasses when we are only discussing 10 such cases, and with added experience, our success rate may well improve. Other authors, e.g. Gruss et al. (1) in Heidelberg, with a reported experience of 285 in situ bypasses, state that their results are considerably worse when the distal anastomosis is made to a single artery. I would agree that autogenous vein will often function in low flow rate situations, but this, unfortunately, will not always salvage the limb and we have had to amputate at least two legs despite a functioning vein bypass.

I would not agree that the passage of the Hall disrupter causes significant endothelial trauma—we have examined veins in the post-mortem room after experimental valve disruption, and have yet to see any endothelial damage, although electron microscopy might show this. We have not used the retrograde valvulotome, but conclusions regarding its overriding superiority would be best reached by a controlled trial between the two methods of valve disruption; Corson has not done this.

To compare results between different series and techniques is not very helpful unless the cases are carefully matched, and patient selection is all important in the field of bypass grafting as every vascular surgeon knows.

What our paper attempted to show was *not* that the *in situ* method gave better results than the standard reversed graft with anastomosis to the popliteal artery, but that it could be used in situations where it was difficult or even impossible to use a reversed graft, and this is still our contention.

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1. Gruss J. D., Bartels D., Machado J. L. et al.: *In situ* vein bypass. In: Greenhalgh R. (ed.): *Femoro-distal Bypass*. London, Pitman; 1981.

Marlex mesh abdominal rectopexy for rectal prolapse

Sir

The latest in a series of papers on rectal prolapse by Mr Keighley et al.: *Br. J. Surg.* 1983; **70**: 229–32) was read with considerable disappointment. It contains a number of major deficiencies.

(a) It is stated that 100 consecutive patients with rectal prolapse had a Marlex mesh rectopexy and yet in a previous publication in 1980(1), 17 patients in the same series had a Teflon mesh rectopexy. By 1981(2), the series had grown to 39 patients and the reference cited in that paper suggests that an Ivalon sponge rectopexy was performed. To further add to the confusion, a second paper published in 1981(3) using a similar series of patients, indicates that Marlex mesh was also in vogue. It behoves the authors to clarify exactly what materials were used in their consecutive series of papers, and possibly reduce their latest collection of Marlex rectopexies by at least 17.

(b) No reason is given for their measurement of anal canal pressures but it subsequently becomes evident in the discussion that the aim was to assess the effect of rectopexy on anal canal pressures. The conclusion that 'pressures are not influenced by operation', is not surprising as it can be found in any of their previous papers (1–3). To then look at the predictive value of anal manometry, particularly in incontinent patients, with the knowledge that rectopexy has no effect on anal canal pressure is surely a worthless exercise.

(c) A specific deficiency in the anal manometry results, as shown in Tables II, III and IV is the failure to state what the numbers represent. Their use of the paired Student's *t* test to analyse the observations leads to the conclusion that they must be mean \pm standard deviation (s.d.). In using the *t* test, an assumption is made that the data are normally distributed. In half of the data in these tables, the mean \pm 2 s.d. would lead to a negative anal pressure. It would be reasonable to predict with such negative anal pressures that the rectal prolapse might cure itself! The data are obviously not normally distributed and the use of the *t* test is therefore inappropriate.

(d) The conclusion by the authors that their results 'speak for themselves' is hardly tenable. The results of anal manometry cannot be analysed statistically using the data published in any of the aforementioned papers and are therefore effectively 'mute'. The results of the authors' treatment of rectal prolapse await proper operative details and no doubt the authors would agree that an independent assessment of their results would help eliminate bias.

At a time when there is much debate about improving the standard of surgical publications, it is disconcerting to see a paper with major deficiencies appearing in print. Furthermore, the benefit to surgical progress of repeated small studies with minor variations in patient numbers (1–3) is obscure. Reviewers might question such benefit by asking, 'Is this a new contribution to the subject, or has it all been done before?' (4), as stated recently by Mr Keighley himself.

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1. Keighley M. R. B., Makuria T., Alexander-Williams J. et al. Clinical and manometric evaluation of rectal prolapse and incontinence. *Br. J. Surg.* 1980; **67**: 54–6.
2. Matheson D. M. and Keighley M. R. B.: Manometric evaluation of rectal prolapse and faecal incontinence. *Gut* 1981; **22**: 126–9.
3. Keighley M. R. B. and Matheson D. M.: Results of treatment for rectal prolapse and faecal incontinence. *Dis. Colon Rectum* 1981; **24**: 449–53.
4. Keighley M. R. B.: The management of the perforated appendix; a controlled clinical trial (Letter). *Br. J. Surg.* 1983; **70**: 188.

Sir

We are grateful for the opportunity of commenting on Mr D. MacLellan's letter. We are sorry that our paper caused him so much difficulty and disappointment. We hope that we can alleviate some of his difficulties.

Certain critics might choose to be confused because, in our earlier papers, not concerned with precise operative technique, we refer to work on rectopexy by others who use different materials. They might also choose to quibble because in other earlier work we used a general descriptive term for an inert mesh rather than using a proprietary name as we did later. The principle is that a biologically inert mesh is used in the repair. The purpose of our communication has been to describe, in some detail, this surgical procedure and its clinical results. Our paper represents one of the largest clinical series and with an almost complete follow-up. Furthermore, to report no deaths and no recurrences in 100 consecutive patients is, we believe, something to be proud of.

We felt that we should accompany our technical description of the procedure by further details of our manometric findings. The numbers are now considerably larger than those previously reported and allow us to determine whether we could predict, on the basis of preoperative pressures, the likelihood of return of continence following rectopexy. We accept Mr MacLellan's criticism regarding the use of the paired Student's *t* tests if the data are not normally distributed. We accept this criticism and a review of our data indicates that the pressure results were not normally distributed since there were a substantial number of extremely elderly patients with a lax anal canal who had no recordable anal pressure. The large standard deviations reflect the wide scatter of anal pressure recordings in this population whose age ranged over seven decades. It is for this reason that we feel that anal manometry is not of predictive value in these patients.

The earlier papers from our Unit were concerned with anal pressure measurements and the effect of different types of therapy, including operation, on these measurements. The recent paper that so disappointed Mr MacLellan was the first and only detailed account of the technique of our operation used for rectal prolapse. We are understandably pleased with the results of this technique and feel, as did the reviewers of the paper, that it should receive publicity in the *British Journal of Surgery*.

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Aortocaval fistula with aortic aneurysm

Sir

Burke and Jamieson are to be congratulated on their excellent results in 7 cases of aortocaval fistula (*Br. J. Surg.* 1983; **70**: 431–3). This rare condition is infrequently recognized preoperatively.

However, we feel they rather overplay the interpretation of the laboratory findings. The median value for haemoglobin appears to be incorrectly marked in Fig. 1. While a haemoglobin of 12.5 g/dl may be within 'the normal range', it is not the usual finding in patients with peripheral vascular disease, in whom both the haematocrit and haemoglobin are generally raised (1, 2). The authors claim that the ratio of haemoglobin to haematocrit is disturbed by haemodilution in this condition. However, this statement can only be supported by comparison of these values in comparable healthy controls and patients who have a ruptured aortic aneurysm not involving the cava.

Two important signs of aortocaval rupture have been omitted. Scrotal oedema is at least as common as priapism and the finding of venous pulsation in the lower limbs is diagnostic of a major arteriovenous fistula. The authors mention that the anaesthetist may detect a raised CVP in the presence of cardiac failure. It is increasingly recognized that monitoring of critically ill patients with circulatory instability is better performed using a Swan-Ganz catheter, rather than a CVP line. Finally, it is worthy of mention that some authorities advocate preoperative aortography, when the condition of the patient permits; since this allows better planning of the operative procedure. This can be rapidly performed using Digital Subtraction Angiography.

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2. Bouhoutsos J., Morris T., Chavatzos D. et al.: The influence of haemoglobin and platelet levels on the results of arterial surgery. *Br. J. Surg.* 1974; **61**: 984–6.