

blood flow and increase the tubular production of urine by its diuretic action but the only prospective trial to assess its effect in jaundiced patients failed to demonstrate any significant benefit and showed that postoperative deterioration in renal function was actually greater in patients receiving mannitol⁶. The routine use of mannitol in this setting is not effective and may even be detrimental if it exacerbates pre-existing hypovolaemia and hypotension due to its osmotic potential.

Clearly, further studies on the systemic and renal haemodynamics of obstructive jaundice are required. At present, we suggest caution in the routine perioperative use of mannitol in jaundiced patients as it may initially improve urine output but ultimately do harm and precipitate renal impairment.

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Computed tomography in acute left colonic diverticulitis

Sir

I read with interest the article by Ambrosetti *et al.* (*Br J Surg* 1997; 84: 532-4) concerning acute left colonic diverticulitis. In this and previous papers^{1,2} Ambrosetti *et al.* clearly demonstrated that the incidence of severe diverticulitis, as diagnosed by computed tomography (CT), was higher in patients who were operated on during hospitalization. In contrast, the incidence of mild diverticulitis as diagnosed by CT was lower in patients who were successfully treated medically. The difference was statistically significant².

The authors conclude that 'elective colectomy may be advised after a first attack of acute left diverticulitis...in patients whose initial CT scan revealed the prognostic signs of severity described' and that 'CT can predict medical treatment failure during the first acute manifestation of diverticulitis'².

In response to these papers and their conclusions I would like to make two points. First, I would have been interested to have the opinion of the late Thomas Bayes about these conclusions³.

He may have suggested not to confuse specificity, sensitivity and predictive values. In a patient hospitalized with acute left colonic diverticulitis, the data from Ambrosetti *et al.* show that the CT positive predictive value of medical treatment failure and early operation was 30 per cent². I respectfully suggest that CT is not a valuable tool for assisting the surgeon in making a decision for early resection in this condition and the best tool seems to be a 'wait and see' policy.

Second, I do not understand why they observed 19 patients diagnosed as having mild diverticulitis by CT who were operated on between October 1986 and January 1992¹ and only ten patients between October 1986 and June 1995².

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Author's reply

Sir

I am not sure I quite understand the confusion between specificity, sensitivity and predictive values used to predict medical treatment failure. This work has clearly shown that 76 per cent of patients with medical treatment failure have severe diverticulitis on initial computed tomography. If it is true that a positive predictive value to predict medical failure is 30 per cent, the same radiological investigation has a positive predictive value of 96 per cent for medical treatment success when acute diverticulitis is moderate. In this latter situation, it is a 'wait and sit' rather than a 'wait and see' policy.

The second point is correct and the text should have read nine, not 19, patients.

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Contraindication for the use of neostigmine in colonic pseudo-obstruction

Sir

An 87-year-old man was admitted as an emergency with abdominal pain, distension, vomiting and constipation. Two days previously he had been discharged from a neighbouring hospital following a 2-week admission for acute abdominal pain, for which no cause was found. He had had a duodenal ulcer diagnosed 20 years ago.

His abdomen was markedly distended; there was no tenderness and no bowel sounds present. He had a raised white cell count ($16.8 \times 10^9/l$), urea level (11.2 mmol/l) and creatinine concentration (162 $\mu\text{mol/l}$). A plain abdominal film showed dilated small and large bowel. A Gastrografin (Schering Health Care, Burgess Hill, UK) enema did not demonstrate an obstructing lesion.

A diagnosis of acute colonic pseudo-obstruction (Ogilvie's syndrome) was made, with mild renal impairment as a predisposing cause. The patient was treated with nasogastric suction and intravenous fluids. There was no improvement after 2 days and he was, therefore, given neostigmine 2.5 mg. There was a rapid response with passage of flatus and faeces and relief of abdominal distension.

Six hours later the patient had signs of peritonitis. Abdominal ultrasonography confirmed the presence of free peritoneal fluid. At laparotomy there was a large perforated duodenal ulcer and biliary peritonitis. The ulcer had been sealed by a plug of greater omentum that had been displaced by the vigorous peristaltic contractions of the transverse colon. A Pólya gastrectomy was performed, but unfortunately the patient died from septic shock 12 h after surgery.

Pseudo-obstruction of the large bowel is not uncommon in the ageing population and treatment with neostigmine has been advocated due to its parasympathomimetic action¹. In this patient the pseudo-obstruction was the result of an ileus secondary to peritonitis confined to the supracolic space. Neostigmine should not be used if there is the possibility of a recently sealed-off duodenal or colonic perforation being unplugged by strong peristalsis.

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- 1 Hutchinson R, Griffiths C. Acute colonic pseudo-obstruction: a pharmacological approach. *Ann R Coll Surg Engl* 1992; 74: 364-7.

Myths in management of colorectal malignancy

Letter 1

Sir

I read with interest the paper by Köhler *et al.* (*Br J Surg* 1997; 84: 248-51) and their attempt to dispel perceived myths about the management of colorectal malignancy.

I was particularly intrigued by the suggestion that an abdominoperineal excision would automatically include a wide mesorectal excision. This suggests some misunderstanding of the principles of total mesorectal excision, as currently practised.

Many surgeons, during an abdominoperineal excision, ligate the so-called lateral ligaments of the rectum, with the aid of large artery forceps. This inevitably leaves tissue on the side wall of the pelvis, incorporated in the ligature, which would be taken with the specimen if a total mesorectal excision were performed. At the other extreme, it would be perfectly possible to perform an abdominoperineal excision of the rectum using a technique similar to the close perimuscular dissection used by some surgeons for rectal excision for inflammatory bowel disease, which would leave the whole of the mesorectum in the pelvis.

Köhler *et al.* raise the question as to why there is no difference in recurrence rate between abdominoperineal excision of the rectum and anterior resection in previously published studies. The answer to that specific question is simple: the mesorectal dissection in both sets of patients would have been identical and neither group would have had a mesorectal excision.

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Letter 2

Sir

We enjoyed reading the article by Köhler *et al.* The points raised for debate are valid and well argued on the basis of published studies. With regard to wide mesorectal excision, however, the authors appear to assume that abdominoperineal resection (APR) 'automatically includes wide mesorectal excision' and therefore question why the local recurrence rate parallels that of anterior resection without mesorectal excision. Not only can APR be carried out within total mesorectal excision but this was routine practice until the value of complete mesorectal excision in rectal cancer was realized¹⁻³. This assumption is, therefore, unfounded and any conclusions drawn from it must be viewed with scepticism.

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Letter 3

Sir

In the February edition, Köhler *et al.* argue that the oncological standards of colorectal surgery remain far from being scientifically proven and imply that the future of surgery for colorectal cancer may be better served by laparoscopic approaches. These carry the theoretical advantage of less immunosuppression. The authors cite only those articles that support their premise and commit serious interpretive errors in evaluating articles that do not do so. There are also gaps in the principles which are at the foundation of present oncological standards. Two examples will be cited.

The authors argue against the effectiveness of the no-touch isolation technique of Turnbull *et al.*¹ and cite a 58 versus 38 per cent 5-year survival rate for patients with positive nodes. These results could be attributed to the obligatory wide mesenteric resection that follows ligation of the named arterial and venous blood supply at their origins or apices.

Coincidentally Wiggers *et al.*² disproved the value of initial vascular pedicle ligation and confirmed the value of a wide mesenteric resection, which was performed in both experimental and control groups. For node-positive patients, the 5-year survival rate of the 'no-touch' group was no different from that in the control group, in both cases exceeding 50 per cent. Köhler *et al.* failed to note that these and others' results represent a 150 per cent improvement in survival rate when compared with the widely reported 5-year survival rate of only 25-40 per cent for patients with positive nodes after conventional or segmental resection, techniques that ignore the principles of a complete mesenteric resection. They fail to cite other studies that demonstrate similar or better results³ and fail to appreciate that the studies by Wiggers *et al.* have confirmed the advantages of a complete mesenteric resection for colonic cancer, while stating