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J HOLLINGWORTH FRCS
Surgical Registrar
H D KAUFMAN ChM FRCS
Consultant Surgeon

Selly Oak Hospital
Birmingham

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Primary restorative colectomy in malignant left-sided large bowel obstruction

The paper by Dorudi, Wilson and Heddle (*Annals*, November 1990, vol 72, p393) clearly demonstrates that primary restorative colectomy may be performed in certain cases of malignant left-sided large bowel obstruction, without the need for on-table colonic lavage. Disappointingly, the authors do not help the reader to decide which patients would benefit from this procedure.

No details are given about the general condition of the patients, the amount of faecal loading of the colon or the degree of distension of the obstructed bowel. We are not told whether the surgeons encountered any difficulty when suturing bowel ends of greatly disparate circumferences or whether any bowel was of doubtful viability and how they dealt with this. I remain unconvinced that this procedure can be safely performed on all patients with malignant left-sided large bowel obstruction. There must surely be some cases where the patient is unfit and the additional operating time taken to perform a difficult anastomosis (particularly after a low anterior resection) would jeopardise the patient's life. In such instances the patient is better served by the formation of a proximal colostomy (and a distal mucous fistula wherever possible). There must also be cases where the caecum is so greatly distended and ischaemic that an extended right hemicolectomy with ileocolic anastomosis is more appropriate.

The authors are to be congratulated for their enviable results: a mortality rate of less than 1%, no wound infections and no clinical anastomotic leaks; commendable results indeed for emergency surgery. It must be remembered, however, that this paper reports a very small case series and I would be most interested in the results of the next 18 patients treated.

Mr Dorudi and his colleagues have raised an interesting issue and clearly large prospective randomised studies are required. Although they have successfully challenged the view that large bowel preparation is required before anastomosis, I will await further results before adopting this procedure for all patients with large bowel obstruction that I manage.

HAROUN GAJRAJ MS FRCS
Lecturer in Surgery

St Thomas' Hospital
London

Blood transfusion in total hip replacement: is it always necessary?

I read with interest the paper by Porteous and Miller (*Annals*, January 1991, vol 73, p44) concerning the necessity for blood transfusion after total hip replacement. They are to be congratulated for furthering the awareness that blood transfusion after major surgery should be based on need rather than habit. I would, however, take issue with your assessor's less than enthusiastic comments. A mean postoperative haemoglobin concentration of 10.3 g/dl at 48 h and 11.1 g/dl at 14 days hardly counts as significant anaemia. In cardiac surgery, where increasing efforts are being made to reduce the requirements for homologous blood transfusion, the criteria for postoperative homologous transfusion in haemodynamically stable patients are as low as a haemoglobin of 8.0 g/dl or a haematocrit of 25% *(1)*.

It is also entirely possible that some of the pharmacological agents, such as aprotinin, tranexamic acid and desmopressin acetate (DDAVP), attracting interest for reducing homologous blood transfusion in cardiac surgery, could be applied in this setting of elective major orthopaedic surgery.

RUSSELL MILLNER MB BS FRCS
Research Registrar in Cardiothoracic Surgery

St George's Hospital
London

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Deaths following trauma: an audit of performance

We read with interest the above paper by Phair *et al.* (*Annals*, January 1991, vol 73, p53). It was recommended that "more detailed studies and comparisons with other centres . . . are required". This can be best achieved by a national coordinating system.

We are pleased to report that the United Kingdom Major Trauma Outcome Study was established in 1989 at the North Western Injury Research Centre (NWIRC). Over 30 hospitals nationwide participate and the database now contains information on 8000 injured patients. Injury scaling is performed at NWIRC preventing intercoder variability.

Statistical feedback using the TRISS methodology *(1)* is provided on a regular basis and is used to highlight patients for interdisciplinary audit, hopefully leading to improvements in trauma care. Additional benefits include the potential for comparative studies between hospitals employing different systems of trauma care and the development of a large database which can be used to refine the scoring systems themselves.

Two further points should be clarified. Firstly, M values for less than 100 patients are not statistically reliable and consequently the Z value cannot be viewed with any confidence. Secondly, Mr Montague in his comments advocates the use of the 1990 version of the Abbreviated Injury Scaling publication in the TRISS calculations. As there are, as yet, no published regression coefficients for use with AIS90 the 1985 version should still be used.

M WOODFORD
MTOS Coordinator
S HOLLIS
Senior Medical Statistician

Hope Hospital
Salford

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McBurney's point—fact or fiction?

I have read with interest the letters on the siting of the appendix and incisions for its removal (*Annals*, January 1991, vol 73, p65). I wonder if I might be allowed to make a few comments based on embryology, anatomy and clinical observations. Many