

Primary restorative colectomy in malignant left-sided large bowel obstruction

I have read the above article by Dorudi, Wilson and Heddle (*Annals*, November 1990, vol 72, p393) with some alarm.

There are three principles in performing colonic anastomosis, any of which can be ignored only at the risk of a patient's life. The authors emphasise the importance of a good blood supply to the bowel ends and to the absence of tension, but imply that mechanical preparation is not necessary. But their 18 patients did have mechanical preparation, ie emptying of liquid and gaseous content by an enterotomy. In the absence of faecal masses this is surely good enough, but any attempt to suggest that an empty proximal colon is not necessary for a safe large bowel anastomosis is misguided and dangerous.

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Treatment of carcinoma of the oesophagus

I read with interest the parallel articles reopening the controversy of surgery or radiotherapy for squamous cell carcinoma of the oesophagus (*Annals*, January 1991, vol 73, pp1-12). As one who expressed support for the MRC trial when this was first conceived in the early 1980s, I would like to shed some light, derived from my own experience and that of others, on some of the reasons why I believe the trial had to be abandoned due to lack of support.

There is little doubt that the impetus for the trial originated from the deplorable results of resection as portrayed in the review by Earlam (1) a decade ago. No one would argue that an operative mortality of 29%, a 5-year survival of 4% and almost one-third of patients being operated on without undergoing resection, and therefore needlessly, represented a sad indictment of the surgical management of this condition. Had those figures been representative of results achieved in 1980 they would certainly have justified a critical appraisal of available management options. However, because the series comprised pooled data from many series published over several decades, they portrayed a vastly heterogeneous group of patients, some of whom underwent surgery up to 40 years previously.

The effects of the publication of Earlam's review were twofold. Firstly, it prompted many surgeons who knew their results were somewhat more optimistic to publish their experience and to continue to address the factors influencing operative mortality and survival (2-5). Secondly, and less fortunately, it persuaded many gastroenterologists that they could achieve just as good (or bad) results by palliative intubation, thus depriving many patients of the prospect of cure. It is now firmly established that in specialist units dealing with large numbers of patients, operative mortality is 10% or less, and because of the wide variation in results outside such units, I would entirely agree that it is in these centres where such patients should be managed.

The 1980s saw the emergence of much more meaningful and encouraging data relating to outcome after surgery, with little, if anything, to add to the historical data relating to radiotherapy. Our own unit in Lancaster received all hospital referred cases, whether for palliation or an attempt at cure, from a well-demarcated catchment area with a population of some 200 000.

This placed us in a relatively unique position enabling a broad overview of the disease and the ability to assess the proportion for whom surgery was considered appropriate, at least according to our criteria (6). This proved to be 40%, considerably higher than the 25% cited by Earlam and Cuschieri from admittedly inaccurate HAA statistics, and some might even claim that 40% is a conservative figure. The survival figures which Earlam states should be achieved of 45% at 1 year, 20% at 2 years, and 10% at 5 years were exceeded in our series of 123 resections performed between 1975 and 1988, with corresponding figures of 57%, 31% and 14%, with an overall mortality of 8.3% (7). I would entirely agree with Earlam that in order to compare the results of surgery fairly against other modalities, mortality should include that at 30 days or in hospital, whichever is the longer, and survival should relate to the total number of patients operated upon, and our results have been clearly expressed in this way (8). These results, as do most published series, relate to all oesophageal carcinomata, although it is becoming increasingly recognised that stratification of survival data according to cell type and tumour staging yields important differences. Adenocarcinoma is forming an increasing proportion of oesophageal tumours, now comprising up to 50% of many series. These are by no means all gastric carcinomas migrating upwards, but an increasing incidence of carcinoma arising in Barrett's columnar-lined oesophagus, a phenomenon reported by many workers (9, 10). If data relating to adenocarcinoma are excluded, our findings show a 5-year survival rate of 24% for squamous lesions, rising to 57% for node negative and 75% for mucosal lesions (7).

It is against the background of these and similar data that many surgeons were faced with a moral dilemma in randomising what appeared to be localised, potentially curable squamous lesions, which can now be resected with low mortality and a 'cure' rate of at least 24%. Few have been convinced by existing radiotherapy data, those of Pearson never having been repeated, not even by himself in Edmonton nor by his successors in Edinburgh. Cederqvist *et al.* (11) using a similar regimen to that of Pearson reported a 4% 5-year survival rate among those able to complete the rigours of a course of radical radiotherapy, and 9% of patients died during their course. As Khoury states, the fact that two randomised trials have shown no survival advantage by combining radiotherapy with surgery implies that considerable optimism is required to believe that radiotherapy alone can do better. Furthermore, the emphasis during the last decade on greater clearance (resulting in residual microscopic tumour at resection margins in 2% instead of 25%) and extended lymphadenectomy over a greater longitudinal field, makes it unlikely that a sufficient dose of radiotherapy can be delivered per unit volume of field by external beam.

In the context of the available data emerging in the last decade, I personally found it ethically unjustifiable to randomise those patients fulfilling the entry criteria for the MRC trial, and I know many of my colleagues felt likewise. Whilst the trial may have been appropriate in the context of the data available in 1980, and may represent a 'lost opportunity' to answer the question by means of a randomised controlled trial, I believe that the impetus behind it has helped put the results of surgery on a clearer footing, has stimulated efforts towards more careful audit and definitions and has heightened awareness of the potential hazards of resectional surgery and the

means to reduce these. In this context, I believe the considerable effort in mounting the MRC trial was not wasted.

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The role of reconstructive surgery in the management of war wounds

As surgeons in 32 Field Hospital, in Saudi Arabia and dealing with war wounds, we read with interest the article by Coupland (*Annals*, January 1991, vol 73, p21) on the role of reconstructive surgery in the management of war wounds.

The article reaffirms the author's previously stated opinion that application of an external fixator has a lower priority than wound excision and should be delayed until the first dressing change or wound closure is undertaken (1). His argument rests upon the assumption that external skeletal fixation at primary surgery will impede subsequent wound excision or future reconstructive surgery.

It is our opinion that application of an external fixator at primary surgery is essential in the management of large soft tissue wounds associated with limb fractures in war. External fixation affords immediate and optimum stabilisation of soft tissues, and assures limb length and axial alignment when bone mass is lost. Additional benefits include the protection of vascular repair and the reduction of patient discomfort during rearward evacuation.

While we acknowledge the greater potential risk of pin track infection in the casualty brought in from the battlefield, we do not feel that this outweighs the benefits of early application provided adequate predrilling of pin sites is performed with sufficient soft tissue release. In addition, the position of the pins can be altered at the time of any soft tissue reconstruction if required.

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We enjoyed reading Mr Coupland's article on reconstructive surgery in the management of war wounds (*Annals*, January 1991, vol 73, p21). We cannot, however, agree with him that external fixation of fractures should be classified under the heading of 'Delayed Primary Reconstruction' and performed as he implies at the time of delayed wound closure after 4 or 5 days.

We feel that it is important for an educational journal such as the *Annals* to reflect the current orthopaedic opinion which points convincingly to the early fixation of fractures, especially in multiple injuries. In a recent review on the topic, Phillips and Contreras (1) summarise the evidence that early fixation of major fractures within the first 24 h:

- 1 Decreases the duration of ventilatory support required;
- 2 Decreases the time spent in the intensive care unit;
- 3 Decreases the incidence of adult respiratory distress syndrome (ARDS) and fat embolism;
- 4 Reduces the incidence of multisystem organ failure and late sepsis;
- 5 Reduces the incidence of complications related to the fractures;
- 6 Decreases the length of hospitalisation;
- 7 Decreases mortality;
- 8 Probably improves fracture outcome;
- 9 Simplifies nursing care; and
- 10 Reduces the cost of medical care for these patients.

Clearly, operating conditions are far from ideal in times of war, but we feel that the principle of early fixation of long bone fractures within 24 h should at least be aimed at.

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