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An audit of hospital mortality after urgent and emergency surgery in the elderly

The 'risk factor score' proposed by the authors (*Annals*, September 1997, vol 79, p361) lacks objectivity, since definitions for the majority of criteria in it (eg poor general condition, dehydration/hypovolaemia, malnutrition, marked obesity, septicaemia, acute respiratory disease) have not been standardised. This may explain why such a scoring system failed to be more sensitive or specific than a simple 'end of the bed' assessment by the clinician.

The authors mention in the discussion that the findings of this paper produced a change in the priority given to emergency surgery in their hospital. However, it is their own conclusion from this audit that the mortality rates for their patients were not altered by the time of day the operation was performed, nor by the presence of senior surgeons or anaesthetists. Were there any other findings which might have prompted the change?

Comparing the mortality between general surgical and orthopaedic patients is misleading, since it is obvious from their data that they are two different groups in many respects, notably in the ASA grading and preoperative risk factors. For comparable ASA groups, there is no difference in mortality.

Table IV is very confusing. The authors are calculating the validity tests for predicting mortality rather than survival. In other words, they assessed whether the clinician's preoperative prediction that a particular patient would *die*, is true or not. If that is the case, the preoperative specificity (or true negative rate) is 89% (32/36) and not 11% as shown in the above table. Four patients, who were preoperatively deemed not to survive, did do so. Though it is only a small number, it would be interesting to see the factors which led to the survival of these four patients, despite a negative prediction by the anaesthetist and the surgeon.

If calculations are made for prediction of survival rather than death, the positive predictive value of *survival* (not death) becomes 87% (32/37). To aim for a positive predictive value much higher than this would probably result in some potentially salvageable patients not being given the chance of an operation to save their lives. This merely augments the view that a simple 'end of the bed assessment' by a senior clinician is a very good measure of the patient's preoperative condition and postoperative outcome.

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Laparoscopic cholecystectomy without operative cholangiogram: 2038 cases over a 5-year period in two district general hospitals

The large series of laparoscopic cholecystectomies from Whipps Cross and Hull (*Annals*, September 1997, vol 79,

p376), in which the incidence of retained common duct stone was only 0.7% despite no imaging of the bile ducts in 81%, is very impressive.

In my own series of 576 laparoscopic cholecystectomies there were 447 patients with normal common duct width, normal tests of liver function, and who did not present with jaundice, pancreatitis or cholangitis. Of these 447, 26 (4.5% of the whole series) had a proven common duct stone. These stones were treated largely by laparoscopic exploration, but also by open operation or ERCP.

What is the explanation for the difference? Of many possibilities, the most likely is that the majority of occult common duct stones (ie those that have not caused jaundice, cholangitis or pancreatitis, and have not given duct obstruction as shown by dilated ducts on ultrasound or abnormal tests of liver function) pass spontaneously over the postoperative period. However, the possibility remains that perhaps 3.8% of the Hull and Whipps Cross patients still harbour their stones, and a controlled trial would take many years to show whether this was so.

We suggest a multicentre study of all patients without preoperative indicators of common duct calculi. These patients would have routine operative cholangiography, but occult stones discovered would not be treated. All the patients with occult stones would have spiral CT after infusion cholangiography at 6 months, thus showing definitively whether the majority of stones pass spontaneously.

Is making the diagnosis of common duct stones without treating any found, an ethical proposition?

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Chronic leg ulcers: beware the 'wolf in sheep's clothing!'

(*Annals*, September 1997, vol 79, p381)

The authors appear to be under the misapprehension that non-Hodgkin's lymphoma and lymphocyte-depleted Hodgkin's disease are the same entity. I am surprised that this passed peer review. Perhaps remedial exercises in basic pathology would be advisable for all involved in this paper. This is possibly one of the only 'simple but important lessons', as stated in their introduction, that can be learnt from this publication!

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The recent case report by Mr Boyce and Dr Harding (*Annals*, September 1997, vol 79, p381) highlights the importance of considering non-Hodgkin's lymphoma in atypical chronic leg ulcers. Their discussion, however, states that non-Hodgkin's lymphoma is otherwise known as 'lymphocyte-depleted' Hodgkin's disease. This is incorrect. Non-Hodgkin's lymphoma is the collective name for a number of neoplasms, other than Hodgkin's disease, that arise in the lymphoreticular system. Hodgkin's disease has been recognised as a separate