Letters and comments

Contributors to this section are asked to make their comments brief and to the point. Letters should comply with the instructions to authors at <http://www.rcseng.ac.uk/publications/annals/authorinstructions.html>. Tables and figures should only be included if absolutely essential and no more than five references should be given. The editor reserves the right to shorten letters and subedit contributions to ensure clarity.

COMMENTS ON

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Postoperative ventilation in the recovery area

MJ FORSHAW¹, AZ KHAN¹, AR DAVIES¹, DC STRAUSS¹, A PEARCE², RC MASON¹

Departments of ¹General Surgery and ²Anaesthesia, Guy's and St Thomas's NHS Foundation Trust, St Thomas's Hospital, London, UK

CORRESPONDENCE TO

Robert Mason, Consultant Surgeon, St Thomas's Hospital, Lambeth Palace Road, London SE1 7EH, UK

T: +44 (0)20 7188 2563; F: +44 (0)20 7188 0883;

E: Robert.mason@gstt.nhs.uk

We note with concern that Robertson and colleagues advocate against postoperative ventilation following oesophageal resection, yet their study was associated with an overall in-hospital mortality rate of 8.2% which is higher than published results from other centralised UK centres.^{1,2}

In our unit, we have utilised a similar care pathway, the only difference being that patients are routinely kept intubated and ventilated in the recovery area until the following morning (about 18 h). Between 2000 and 2003, we performed 168 oesophageal resections (57% transhiatal, 43% transthoracic) with a similar ASA categorisation and unplanned ICU admission rate (15.5%) to Robertson *et al.* but with an in-hospital mortality of 1.3%.²

Postoperative ventilation has a number of benefits. It allows time for the vital functions and oxygenation to be optimised and efficient endobronchial suction and chest physiotherapy to be performed without distress. Recent evidence that early postoperative elevated cytokine levels may predict the occurrence of subsequent septic complications suggests that aggressive treatments such as continued ventilation may be beneficial in a subset of patients.³ It has also been our experience that delays often occur in the intubation of postoperative deteriorating patients. Arguments such as muscle wasting and weaning problems do not apply when the duration of ventilation is kept short. We would argue that Robertson *et al.* have not made a clear case for abandoning routine postoperative ventilation.

References

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AUTHOR'S RESPONSE

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SIMON PATERSON-BROWN

Department of Surgery, The Royal Infirmary of Edinburgh, Edinburgh, UK

CORRESPONDENCE TO

Simon Paterson-Brown, Clinical Director, Lothian Directorate of General, Vascular and Urological Surgery, The Royal Infirmary of Edinburgh, 51 Little France Crescent, Edinburgh EH16 4SA, UK
E: simon.paterson-brown@luht.nhs.uk

On behalf of my co-authors I thank you for the opportunity to respond to the letter from Forshaw *et al.* Their results between 2000 and 2003 of 168 oesophageal resections associated with an in-hospital mortality of 1.3% are indeed impressive and one can understand their reluctance to