

use of laryngeal mask airway versus tracheal tube. *Anesthesiology* 1994; 81: A1281.

Sir,—We thank Dr Brain and colleagues for their interest in our paper [1] and for their comments. We shall take their points in order.

(1) We confirm that a No. 3 LMA was used in female patients (60%) in our study. The study reflects common practice in anaesthesia. Standard practice in this hospital, as in many others at the time the study was carried out (1992), was to use a No. 3 LMA for adult female and a No. 4 LMA for adult male patients. While we agree that inadequate mask surface area may be a cause of malpositioning, the No. 5 LMA only became available very recently and hence was not used in our study. We cannot speculate as to what the results might have been using different mask sizes.

(2) A standard dose of vecuronium was used (0.1 mg kg^{-1}), all operations took 30–40 min and neuromuscular block was monitored with a peripheral nerve stimulator using the train-of-four method. Antagonism of neuromuscular block was affected simultaneously with discontinuing volatile agent. A difference in the depth of anaesthesia between the two groups is unlikely. We do not know of any well validated means of measuring depth of anaesthesia, but we were unaware of any clinical disparity. There is no evidence that our patients were partially antagonized. We accept that a Relaxograph would give more quantitative information and further work is indicated to clarify the relationship between degree of neuromuscular block and oesophageal pH. Whatever the physiological explanation of the phenomena, the two groups were treated identically and a significant difference was observed. It is fallacious to argue that the two groups should have been treated differently for the purposes of the study.

(3) It would seem unlikely that an oesophageal probe measuring less than 1.5 mm in diameter would have a significant effect on the upper oesophageal sphincter although the theoretical risk exists. We are not sure what methodological modification Brain and colleagues would suggest. For the LMA to even partially rely on the competence of a weak physiological sphincter is a cause for concern.

(4) Our LMA placement was by standard insertion technique.

(5) We agree there may have been theoretical differences in the ventilatory variables between groups. This may be important where high peak flow rates are used in small radius tubes. We are not aware of clinically significant pressure gradients in tracheal tubes of larger diameters at low flow rates. Though inventive, this explanation does not account for our results; rather one would expect it to obscure the inter-group difference we observed.

In our original article, we commented on the final remarks in the letter of Brain and colleagues. Aspiration has been described by several authors [2, 3]. There remains considerable doubt as to the safety of ventilation via the LMA.

J. M. J. VALENTINE

A. F. STAKES

M. C. BELLAMY

Department of Anaesthesia
St James's University Hospital
Leeds

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Prediction of difficult tracheal intubation

Sir,—In his recent study [1], Savva established by sensitivity and specificity analysis that sternomental distance, thyromental distance and modified Mallampati tests (in that order for his data) were useful individual predictors of difficult tracheal intubation. He found that the interincisor gap by itself was not related to the view on laryngoscopy. This latter finding is consistent with the data published by Bellhouse and Doré [2], where the interincisor

gap was measured on lateral radiographs of patients whose mouths were fully opened. However, Wilson and colleagues showed that the interincisor gap was significantly smaller in those patients in whom laryngoscopy was difficult [3]. Subsequent unpublished work by Bellhouse and Doré using bedside tests has shown that interincisor gap is a particularly useful test to add to modified Mallampati and an assessment of head extension on neck, providing a composite examination with good sensitivity and specificity.

Whereas most patients in my experience have an interincisor gap in excess of 4 cm, the average in Savva's patients was 2.92 cm, with 55% of his patients being less than 3 cm and only 12% 4 cm or more. One cannot help questioning either the characteristics of Savva's patients or his method of measuring and recording interincisor gap.

C. P. BELLHOUSE
Murwillumbah Hospital
NSW, Australia

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2. Bellhouse CP, Doré C. Criteria for estimating likelihood of difficulty of endotracheal intubation with the Macintosh laryngoscope. *Anaesthesia and Intensive Care* 1988; 16: 329–337.
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Sir,—The interincisor gap of all of my patients was measured with the mouth fully opened and the patient awake. Great care was taken to ensure that all measurements were accurate. As these measurements were taken from a sample ($n = 350$) of adult Saudi patients undergoing various types of surgery, they can be taken to be representative of those expected in the whole adult Saudi population. The mean interincisor gap of my patients (2.92 cm) was indeed smaller than that found by Bellhouse and Doré and by Wilson and colleagues in their studies. Perhaps this difference was caused by racial factors.

Nevertheless, I agree that interincisor gap is a useful test provided that a measurement of less than 2 cm is taken as being predictive of difficult tracheal intubation. Its specificity would thus be very high but its sensitivity would be very low. However, if it is used with sternomental distance ($\leq 12.5 \text{ cm}$) and assessment of jaw protrusion (position C) as a composite examination, the sensitivity is almost 85% and the specificity is still around 90%.

D. SAVVA
Riyadh Armed Forces Hospital
Riyadh
Saudi Arabia

Heparin and platelet function

Sir,—I read with interest the paper by Boldt and colleagues [1] on heparin and platelet function. I am concerned however that heparin reversal was not adequately confirmed.

Protamine was given in a 1 to 1 ratio with the initial dose of heparin; thus groups 1 and 2 would have received the same dose of protamine even though group 2 had received an infusion of heparin throughout the period of cardiopulmonary bypass. The authors stated that all activated clotting times (ACT) after administration of protamine were less than 150 s, thus excluding excessive circulating heparin as a cause of bleeding. However, in a review article in 1989, Aren [2] stated that the ACT method is not suitable for determination of the completeness of heparin reversal as it is insensitive to low plasma heparin concentrations. He referred to work by himself [3] and Esposito and colleagues [4]. Hooper and colleagues [5] measured residual plasma heparin concentrations after cardiopulmonary bypass, and finding no correlation with ACT measurements reached the same conclusion. Furthermore, in relying on ACT, Boldt and colleagues cannot exclude the occurrence of rebound heparinization, particularly in the high-dose groups.