
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. STACKS
M. C. BELLAMY
Department of Anaesthesia
St James's University Hospital
Leeds


**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 Dore [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 Dore 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
Murtawillumbah Hospital
NSW, Australia


**Heparin and platelet function**

Sir,—I read with interest the paper by Boldt and colleagues [1] on heparin reversal. 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.

D. SAVVA
Riyadh Armed Forces Hospital
Riyadh
Saudi Arabia

Without adequate confirmation of complete reversal such as thrombin time [6], I feel their conclusions on platelet function and postoperative blood loss must be questioned.

S. N. GOWER
Anaesthetic Department
Leeds General Infirmary
Leeds


Sir,—I appreciate Dr Gower’s comments on our study which dealt with different anticoagulation regimens and platelet function in patients undergoing cardiac surgery operations. His major criticism was that heparin reversal was not adequately confirmed in our present study and that excess heparin was responsible for the more pronounced postoperative blood loss in the patients who received higher doses of heparin. Dr Gower is correct when he states that the activated clotting time (ACT) as a method for assessing heparin reversal with protamine has some limitations. However, I am convinced that it is not correct that “...ACT is not suitable for the determination of the completeness of heparin reversal.” Several studies in the clinical setting which focus on optimizing coagulation in cardiac surgery patients are based on monitoring ACT. In our study we also measured activated partial thromboplastin time (APTT) in the postoperative period (on the intensive care unit) from which, in addition to ACT, excessive heparin plasma concentrations can be excluded. There were no significant differences in ACT or APTT between the groups. Moreover, in patients who received heparin 600 u kg-1 with aprotinin (group 4), postoperative blood loss was significantly lower than in the patients who received only heparin 600 u kg-1 (group 3). Total doses of heparin in these two groups were not significantly different (group 3: 51 400 ± 8050 u; group 4: 59 000 ± 7500 u), although group 3 had a significantly higher blood loss and increased need for transfusion of homologous blood and blood products. Thus we do not agree that our conclusion on heparin, platelet function and postoperative blood loss must be questioned. However, Dr Gower is correct that direct measurements of heparin plasma concentrations (e.g. by a chromogenic method) may be helpful to exclude excessive heparin more accurately.

J. BOLDT
Department of Anaesthesia and Intensive Care
Juttus-Liebig University
Gießen

Effects of oral nizatidine on preoperative gastric fluid pH and volume in children

Sir,—Mikawa and colleagues [1] recently published a paper on oral nizatidine, a new potent H2 receptor antagonist for prophylaxis against pulmonary aspiration in children undergoing surgery. In addition to being wary of these new, more expensive, long-acting drugs used to treat a non-problem, I find it particularly upsetting that the standard for risk of aspiration still being used is the paper by Roberts and Shirley, published in 1974 [2]. I must admit that I too am guilty of quoting this paper [3], however, that was before a more recent paper by Raidoo and colleagues was published [4].

The basic defect in the study of Roberts and Shirley is that the information was never published in a peer reviewed journal for the conditions necessary to create aspiration pneumonitis in Rhesus monkeys. Raidoo and co-workers demonstrated that at least twice the volume (i.e. 0.8 ml kg-1) with pH 1.0, was necessary to reproduce the results that Roberts and Shirley claimed they found in their laboratory. This markedly changes the population of children or adults who might be at potential risk for pulmonary aspiration of gastric contents [5].

Certainly, if the number of patients who came to the operating room as being “at risk” were as high as previously published, again, including my own study [6], the incidence of aspiration would be much higher than the documented rate of about 5 per 10000 anesthetics in children of physical status ASA I and II. The risk of aspiration is clearly related to gastric residual volume but the value of 0.4 ml kg-1 is flawed [5].

C. J. COTÉ
Department of Pediatric Anesthesia
Children’s Memorial Hospital
Northwestern University Medical School
Chicago, IL, USA


Viscosities of commonly infused substances

Sir,—In anaesthetic practice, a wide range of drugs and solutions are infused. Poiseuille’s law states that for a given driving pressure, fluid flow is inversely proportional to the viscosity. Design of infusion pumps must take into account the viscosity of the infusion to determine the appropriate pressure generating capability. In the same way, viscosity determines the flow rate in gravity fed infusions for a preset reservoir height. Together with surface tension and orifice size and shape, it also determines the size of drop produced. Therefore, pumps or infusion controllers based on drop counts to deliver a required flow must be recalibrated for solutions of different viscosities and most manufacturers of these devices incorporate some means to accomplish this. In spite of this, the accuracy of volumetric controllers can only be expected to be ±10% [1]. To date, the literature has directed little attention to the role that viscosity of infuses plays in the performance of an infusion system.

Analysis of pressure—flow relationships of solutions yields an indication of the effective viscosity as it pertains to the performance of infusion systems. Resistance (Res) equals the ratio of pressure change (AP) to flow change (AF), when flow is changed from one rate to another (Res = AP/AF), and according to Poiseuille’s law, is proportional to the viscosity of the infusion. By comparing the individual values of Res for infusible substances, the relative viscosity of an infusion can be determined.

We have studied the effective viscosity of some commonly infused solutions relative to normal saline, using an IVAC model 560 volumetric infusion pump (IVAC Corp, San Diego, CA, USA), a constant fluid-flow source that provides continuous inline pressure measurements with an accuracy of ±2 mm Hg [2]. First, saline was infused through device-specific infusion tubing.