Anaesthetists be’ware . . . and alarmed

Editor—Congratulations to Errando and colleagues on their excellent work surveying the problem of awareness under anaesthesia. Their finding of so high an incidence (1%) in a cohort from which some high-risk patients had been excluded is sobering, especially given the exclusion of cases in which a neuromuscular blocker had been administered in error before induction.

One particular strength of the survey is their definition of awareness based on the patient’s view. It is too glib to dismiss patient reports as being confused or simply wrong—disproving the patient’s account is a Pyrrhic victory when they remain certain of being aware ‘under’ your anaesthetic!

Finally, the fact that nearly 75% of ‘aware patients’ had not discussed the issue with the responsible anaesthetist highlights our own susceptibility not to be aware of the scope of the problem.

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Editor—Thank you for your comments. Although our ‘crude’ incidence of awareness with recall (AWR) during general anaesthesia is definitely high (1%), we prefer to take into account the ‘real’ incidence of 0.6% in elective procedures. Owing to space constraints, our discussion needed to be considerably shortened, so could not be more extensively discussed. However, the 1% figure is not so different from that of other published work as the definition of AWR can significantly modify the true incidence. As in other aspects of anaesthetic procedures, the patient perception should prevail. An important lesson learned from our work, as Dr Nielsen notes, is that we lose important information and feedback from our patients. The availability of a recovery room offers a possibility to pick up some of these cases, but this is not always possible. Another important question is whether patients should be informed about AWR at the preoperative visit. If this is done, maybe this would be the time when we should remind the patient or their relatives that if AWR occurs, they should inform their anaesthetist after the procedure. In this way, the preoperative interview could have the preventive action on the psychological after-effects.

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Safety and jet ventilation

Editor—Cook and Alexander confirm that the practice of jet ventilation may be dangerous when applied without control of the driving pressure and tracheal pressure. However, we would like first to point out that we are not the authors of the following sentence ‘caution against use of TTJV when ventilation from supraglottic or subglottic catheters can be used, as risk of iatrogenic injury is too high’. In fact, our opinion is the exact opposite: laryngeal surgery, and especially laryngeal endoscopy, is optimally performed under transtracheal high-frequency jet ventilation. The reason is, as stated by Cook and Alexander, ‘the main advantage of transtracheal techniques is that it provides the surgeon with operating conditions unhindered by anaesthetic equipment or the need for the surgeon to maintain ventilation’. In a nationwide survey on anaesthetic management of laryngeal laser surgery, Cozine and colleagues did not show any difference between jet ventilation and tracheal intubation, but demonstrated that rate of complications was directly related to the number of surgeries performed under jet ventilation each year. In other words, we do well what we do often! Most laryngeal invasive endoscopic procedures are planned and simply cannot be performed without adequate equipment, particularly a jet ventilator which automatically cuts off ventilation when end-expiratory pressure exceeds a preset level.

Finally, we want to stress that every anaesthetist should be comfortable with transtracheal ventilation/oxygenation since they may, one day, face a cannot ventilate, cannot intubate situation. Laryngeal surgery is thus the ideal field for training in inserting a transtracheal cannula, even more so as this technique offers perfect operative conditions in invasive ENT endoscopic surgery. Furthermore, we strongly agree that guidelines and regular training in this particular field would be most welcome.

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doi:10.1093/bja/aen252
Editor—Many thanks to Dr Bourgain and colleagues for their interest in our article. First, we would like to apologize for mis-attributing the statement to the authors. The correct reference in our article was to 11 not 1 and it is not a direct quotation.4

We believe our survey provides sufficient evidence to raise concerns over the use of transtracheal ventilation using a high pressure source without control of airway pressure, but a survey lacking denominators cannot confirm or refute its lack of safety. In the absence of large randomized controlled trials comparing techniques, properly designed prospective cohort data collection may be sufficient. It is possible that the 4th National Audit Project of the Royal College of Anaesthetists5 which will, from September 2008, prospectively identify major airway complications throughout the UK, will provide such information.

What our survey does illustrate is that the reality in the UK is that in the majority of hospitals, transtracheal procedures can only be performed using manual techniques. Assuming these techniques are only used for laryngeal surgery, we conclude that only 15% of respondents performed transtracheal techniques electively and only 7% have access to high-frequency jet ventilation. We agree the manual technique is ‘suboptimal’. However, if our experience is typical, it is likely that financial pressures prevent purchase of the more expensive equipment and suboptimal techniques are likely to be the norm in the UK for some time to come.

As inferred in our paper, we agree that all anaesthetists should be familiar with transtracheal ventilation techniques. Our survey suggests, at least in the UK, that experience is unlikely to be achieved during elective ENT lists. Other solutions are necessary.

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doi:10.1093/bja/aen253

Effects of glyceryl trinitrate on cerebrovascular autoregulation

Editor—We read the article by Moppett and colleagues with great interest. There is a paucity of data on cerebrovascular effects of vasoactive drugs in awake patients without intracranial pathology and we are grateful to the authors for providing such data. However, we would like to highlight two aspects regarding the glyceryl trinitrate (GTN) part of their study.

First, the CO₂ reactivities measured in this study are very high. We would expect reactivities in the range of 15–30% per kPa.2 Such high CO₂ reactivity makes the study of changes in blood flow velocity in response to changes in arterial pressure very difficult, as small changes in CO₂ will have marked effects on the flow velocities. Table 1 suggests that a shift to a lower end-tidal CO₂ occurred between the measurements performed before and during infusion of GTN. This would have led to improved autoregulation. Secondly, assuming intact autoregulation, the decrease in flow velocity in response to the arterial pressure decrease induced by GTN is surprisingly large. If the static rate of autoregulation3 is calculated based on the median given in Table 1, the result is 37%, that is, in contrast to the measurements of dynamic autoregulation performed by the authors, static autoregulation is clearly disturbed. This discrepancy is unexpected in the investigated group of patients.

In our opinion, based on the presented data, it cannot be concluded that GTN did not affect cerebrovascular autoregulation in this study.

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Editor—We thank Dr Steiner and colleagues for their interest and thoughtful comments on our study.¹

In response to the end-tidal CO₂ values, the individual changes in ‘resting’ end-tidal values with and without GTN were mostly <0.3 kPa, and occurred in both directions. We cannot exclude undetected changes in Pao₂ due to GTN effects on pulmonary blood flow. We would therefore suggest that, within the limits of the study, any effects of changes in end-tidal CO₂ were small and unlikely to affect autoregulation significantly.

We agree with Dr Steiner that the decrease in flow velocity in parallel with a GTN-induced decrease in arterial pressure can be interpreted as a reduction in static