CORRESPONDENCE

Sir,—Thank you for allowing us to respond to Drs Heath and Sinnathamby's letter. They report a 20% incidence (23 patients) of obstruction with the reinforced laryngeal mask airway (LMA) related to Boyle Davis gap opening during anaesthesia for adenotonsillectomy. They found that the obstruction was caused by compression of the reinforced LMA between the Boyle Davis gap and the lower teeth, a problem we did not encounter. However, in only two patients did they replace the reinforced LMA with a tracheal tube.

In a similar study to ours, Webster and colleagues reported 10 patients (18%) with reinforced LMA obstruction caused by Boyle Davis gap. Three of these patients had a tracheal intubation, while in the other patients an adequate airway was achieved by temporarily releasing the gag and deepening anaesthesia. They found that the obstruction was caused by compression of the reinforced LMA between the Boyle Davis gap and the lower teeth, a problem we did not encounter. In our study, we encountered two patients with reinforced LMA obstruction occurring distally, which we thought was caused by the gag blade pushing the reinforced LMA orifice anteriorly into the tongue base or epiglottis. We also emphasized the need to ensure adequate depth of anaesthesia when the Boyle Davis gap is opened to prevent laryngeal spasm, which occurred in one patient in our study.

At the Royal National Throat, Nose and Ear Hospital, the reinforced LMA is used routinely for adenotonsillectomy by 60% of the anaesthetists and for tracheal intubation in 40%. Three of these registrars are now trained in its use. In the last 12 months, 1302 operations involving adenoidectomy, tonsillectomy or both, were performed and the reinforced LMA was used in more than 50% of patients. No major problems were encountered with its use.

Tube compression when flexed, as demonstrated by Drs Heath and Sinnathamby, occurs more readily when the reinforced LMA has been damaged by biting during recovery from anaesthesia. Biting causes disruption of the reinforcing coils and reinforced LMA that are damaged in this way should not be used. The reinforced LMA should be protected by a gauze bite block [2] inserted at the end of surgery. Biting can be minimized by avoiding unnecessary stimulation of the patient during recovery from anaesthesia. Although our study and that of Webster and colleagues used prototype reinforced LMA, they were equally prone to damage by biting.


Nitrous oxide elimination

Sir,—Einarrson and colleagues reported that "nitrous oxide elimination rates were close to those predicted previously, but there is a risk of diffusion hypoxia for at least 30 min from cessation of nitrous oxide administration in the presence of hypoventilation" [1]. The authors failed to compare hypoventilation alone with and without nitrous oxide elimination. Therefore, in terms of hypoxia that occurred during hypoventilation, the authors should not use diffusion as the sole explanation. Einarrson and colleagues used 50% reduction in tidal volume during hypoventilation, which is equivalent to much more than a 50% reduction in alveolar ventilation because of the anatomical and physiological deadspace. The hypoxemia resulting from nitrous oxide elimination which they described during hypoventilation is probably related purely to hypoventilation, but not to diffusion.

I have an alternative, if somewhat heretical, interpretation of the available evidence on uptake and elimination of nitrous oxide, which is markedly different from that expressed in this article, or in textbooks. The new interpretation is based on common sense and basic physiological principles. When we examined previously published articles on nitrous oxide uptake and elimination, all did not consider the existence of functional residual capacity (FRC) before the alveolar membrane and the alveolar membrane itself. With recognition of the rather large capacity of FRC compared with tidal volume, a given nitrous oxide concentration cannot reach that concentration just outside the alveolar membrane for some time, perhaps about 3 min. With the use of the alveolar membrane, any gases passing through the membrane must follow Fick's principle of penetration, which emphasizes the importance of a partial pressure gradient across the membrane as a major factor controlling the uptake of a given anaesthetic. The frequently cited articles of Severinghaus [2] and many others [3-5], failed to separate the amount of nitrous oxide required for FRC washin from total nitrous oxide uptake, or to separate the amount of nitrous oxide washed out from the FRC from the total nitrous oxide washout measured at the mouth piece. When these amounts are separated [6-8], as we demonstrated in our experiments, nitrous oxide uptake across the alveolar membrane starts from zero and reaches a peak at the end of FRC washin and then declines gradually. This is contrary to the uptake described previously by Severinghaus [2] and others [6], with nitrous oxide uptake approximates to 1000 rL/min (r = time in minutes). Concepts such as the second gas effect or diffusion hypoxia, explained by a large initial nitrous oxide uptake or elimination, should be removed from the textbooks.

We should not interpret figure 5 in the paper by Einarrson and colleagues [1] showing an initial fast decline of the alveolar concentration as rapid nitrous oxide elimination from the body (across the alveolar membrane). Instead, it represents mainly washout of nitrous oxide from the FRC. I have great reservations in accepting the original and authors' description of diffusion hypoxia based on differential solubility between nitrogen and nitrous oxide. The explanation again ignores the physiological principle of the existence of the alveolar membrane. Elimination of nitrous oxide across the alveolar membrane requires a partial...
pressure gradient across the membrane. The initial elimination of nitrous oxide from the body should be minimal because there is a very small partial pressure gradient across the membrane.}

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Sir,—We thank Dr Lin for his interest in our article. He suggests that gas uptake and elimination should be measured, or rather calculated, across the alveolar membrane. We have used a traditional concept of gas elimination through the mouth, i.e. including FRC in the body. Regardless of which concept is used, alveolar dilution by nitrous oxide during elimination lowers the alveolar concentration of oxygen. This is explained by the difference between elimination of nitrous oxide and uptake of nitrogen across the alveolar membrane. We used hyperventilation with end-tidal carbon dioxide concentrations of 6.7–7.4% at 30 min. During this moderate hyperventilation, end-tidal nitrous oxide concentration at 5–30 min was approximately twice the end-tidal concentration of nitrous oxide during normoventilation. As the total partial pressure of alveolar gases must be constant, any increase in carbon dioxide or nitrous oxide inevitably leads to decreased oxygen concentration, with a resultant risk of diffusion hypoxia.

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Therapeutic suggestions during general anaesthesia

Sir,—In his recent paper [1], Professor Millar reported the results of a meta-analysis conducted on two studies. The studies in question were those of Evans and Richardson [2], who found that therapeutic suggestions during surgery under general anaesthesia were beneficial, and Liu, Standen and Aitkenhead [3], who obtained a null result using the same paradigm. Millar concluded that Evans and Richardson’s result may have been an artefact because of their control patients remaining in hospital for an unusual length of time, while the postoperative stay for all other groups was comparable in the two studies.

We welcome Professor Millar’s attempt at clarifying this complex area and his suggestions for improving the methodology used in this research. We are also unhappy about some of the methodology which is used to assess the efficacy of intraoperative suggestions. In a typical study, patients hear a tape comprising several different suggestions and are then assessed on various indices of recovery (12 measures were used in the studies by Evans and Richardson, and Liu, Standen and Aitkenhead), with a significant improvement in any of these scores being taken as a positive result. This experimental design has an unacceptably high probability of producing a type I error, that is, a spuriously significant result. Interpretation of the results is particularly difficult when a significant improvement is observed with a measure which does not relate directly to the suggestions given (such as the incidence of postoperative pyrexia in the study of Evans and Richardson).

We are concerned, however, that Professor Miller’s paper may be interpreted as refuting the claim that learning can occur during anaesthesia. Other indirect memory tests, particularly those which have already been used to demonstrate learning despite organic amnesia or subliminal perception of target stimuli [4], may provide a more sensitive test of learning during anaesthesia than do therapeutic suggestions. Several researchers have used tests such as preference judgements and category generation to investigate intraoperative learning. Although the results are so far equivocal [5], there are too many positive findings to be dismissed lightly. The results of a meta-analysis (particularly studies in different centres with different anaesthetic techniques and perhaps also different postoperative stay protocols) should not be accorded greater significance than those of a single study fulfilling the standard criteria of random assignment of patients and anaesthesiologists to the experimental conditions.

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Sir,—I am grateful to Drs Andrade and Munglani for their constructive comments. My paper was not intended, nor should it be taken, as a general refutation of the claim that learning might occur during anaesthesia. In studies of positive suggestions, however, and as the two authors themselves acknowledge, the methodologies are often so flawed, and the results so discrepant, that no reliable conclusions seem possible.

One would agree absolutely with their general premise that, whilst results so far are equivocal, if learning does occur during anaesthesia, then tests of implicit or indirect memory may be the most sensitive means of assessment. My concern is, however, that such tests are often applied in the uncritical belief that they are unambiguous measures of unconscious memory, when there is evidence to the contrary [1].

I cannot, however, agree with their statement that “there are too many positive findings to be dismissed lightly.” A similar argument has been advanced for the existence of “unidentified flying objects” (UFOs) namely that there have been so many reported sightings that they must exist. I am open-minded about UFOs; they may well exist but no objective evidence for their existence has ever been produced. I am also open-minded about learning during anaesthesia; but many of the claimed “positive” findings have been comprehensively criticized on methodological and statistical grounds [2–4]. This is not dismissing the findings “lightly”; many do not bear close scrutiny. If one were to limit the argument to reliable positive findings then it would be a small number. The same criticism, of course, applies to many negative findings in this area; the methodological failings and tendency to use small sample sizes render many null results uninterpretable.