CORRESPONDENCE

THE LARYNGEAL MASK FOR INTRAOCULAR SURGERY

Sir,—Ripart, Cohendy and Eledjam [1], in commenting upon the article by Lamb, James and Janicki [2], described two untoward incidents associated with the use of the laryngeal mask airway (LMA) for controlled ventilation during intraocular surgery. As a result, they have abandoned the technique. They have done so without relating the incidents to their total experience of the LMA and without quantifying the complications associated with tracheal intubation.

Our own clinical experience of LMA use with controlled ventilation in intraocular surgery during the past 2 years supports the original contentions of Lamb, James and Janicki [2]. Our audit data show that, in 593 uses, there were six difficult placements (five were eventually successful, while in the sixth the trachea was subsequently intubated); one LMA became displaced during the procedure but was repositioned quickly and successfully and there was one case of gastric distension because of malplacement which was corrected before surgery commenced. Only the last two incidents presented a significant clinical problem (0.3% of the total use). Neither resulted in morbidity. Over the same period this compared with a 1.6% incidence of serious problems with the use of tracheal tubes (three of 187 uses): one case of laryngeal spasm, one difficult intubation and one episode of pulmonary oedema at extubation.

For all our surgical specialties, 135 incidents were reported in 3974 tracheal intubations (3.4%): 120 difficult placements and 15 episodes of laryngeal spasm. There were 52 incidents in 5655 LMA uses (0.9%): 45 difficult placements and seven episodes of laryngeal spasm (chi-square = 73.93; P < 0.001).

With sufficient experience, use of the LMA can provide improved operating conditions for intraocular surgery and a smooth recovery with a small incidence of serious problems. Contrary to the anecdotal comments of Ripart, Cohendy and Eledjam, our data support use of the LMA for controlled ventilation in intraocular surgery.

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Sir,—I read with interest the article of Van Elstraete and colleagues on tracheal tube cuff inflation as an aid to blind nasotracheal intubation [1]. Inflation of the tracheal cuff in the oropharynx is assumed to centre the tip of the tube and to direct it anteriorly towards the larynx. The authors showed a high success rate when the technique was used in anaesthetized and paralysed patients with a normal airway, whose head and neck can be manipulated safely to place the head in the "sniffing" position before tracheal intubation. However, it remains to be established if the technique improves the success rate of blind nasotracheal intubation in cases of difficult intubation.

I have recently tried the technique for awake blind nasotracheal intubation in a 26-yr-old male patient who suffered cervical spine fracture. The spinal cord was intact and the patient was brought to the operating room in traction for cervical spine fixation. Traction or manual in-line stabilization, although reducing cervical spine movement, places the oral, pharyngeal and laryngeal axes out of alignment and makes orotracheal intubation more difficult, therefore, it was planned to proceed with awake blind nasotracheal intubation before positioning the patient and induction of general anaesthesia. After topical anaesthesia was applied, a size 8.0 mm polyvinylchlorideuffed tracheal tube was introduced via the right nostril into the oropharynx. While the cuff of the tube was deflated, blind tracheal intubation was attempted. However, these attempts failed to achieve intubation. The procedure was abandoned, and it was decided to try tracheal tube cuff inflation as an aid to blind nasotracheal intubation. The cuff of the tube was inflated in the oropharynx with 20 ml of air, and the tube was advanced gently while the patient was breathing spontaneously. Slight resistance was felt, while free air movement continued through the tube, denoting entry of the tip of the tube into the glottis and contact of its inflated cuff with the vocal cords. At that time, the cuff was deflated, the tracheal tube was advanced into the trachea and the cuff was reinflated.

This experience demonstrated that the cuff inflation technique can facilitate awake blind nasotracheal intubation in cases of cervical spine injury which may not allow or may even contraindicate manipulation of the head and neck to achieve the "sniffing" position.

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Sir,—We agree that this technique may be of use in such patients. Although patients in our study were anaesthetized and paralysed...
and the cervical spine was in the "sniffing" position, we are encouraged by the case report regarding an awake patient. We have noted that the technique appears equally successful when the cervical spine is immobilized in a neutral position. Awake tracheal intubation in cases of cervical spine injury allows neurological assessment during and after the procedure and it is claimed, although without supporting data, that preservation of muscle tone may provide some protection. Further studies are required in this patient population, using the technique of cuff inflation to define success rates, amount of cervical spine movement and effects on patient outcome. Studies comparing this technique with others such as awake fiberoptic intubation would also be of interest.

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USE OF ALFENTANIL AND PROPOFOL FOR DIFFICULT TRACHEAL INTUBATION

Sir,—In the letter from Coghlan, McDonald and Csepregi [1], it was suggested that the use of alfentanil and propofol for tracheal intubation [2] may be useful for management of the difficult airway. We have had the opportunity to use this technique successfully for such a case.

A 25-year-old female presented 5 days after extraction of her wisdom teeth under local anaesthetic. She had developed a large right parapharyngeal abscess which severely restricted mouth opening to a maximum of 1 cm between upper and lower incisors. We elected to induce anaesthesia with halothane in oxygen and perform nasotracheal intubation using a fiberoptic laryngoscope. Unfortunately, because of the large parapharyngeal swelling and subsequent distorted anatomy, we were unsuccessful. Attempts were made at blind nasal intubation, but these were also unsuccessful because of the nasotracheal tube being deflected by the parapharyngeal swelling. Laryngoscopy was impossible under deep inhalation anaesthesia, as the mouth would still not open more than 1 cm.

We were able to maintain an airway while the patient was breathing spontaneously, but had difficulty ventilating the lungs using a face mask and hence were reluctant to use a neuromuscular blocker for fear of losing control of the airway. We therefore decided to use the combination of alfentanil and propofol, with the ability to antagonize the effects of the alfentanil rapidly using naloxone, if this should be necessary. We gave alfentanil 25 μg kg⁻¹ followed by propofol 1 mg kg⁻¹. This enabled us to open the mouth 2 cm and perform laryngoscopy. We were able to visualize the tip of the epiglottis and intubate the trachea with the aid of a bougie.

We feel that this is a useful alternative technique, when used by experienced operators, for managing a patient with a difficult airway.

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2. Coghlan SFE, McDonald PF, Csepregi G. Use of alfentanil with propofol for nasotracheal intubation without neuromuscular block. British Journal of Anaesthesia 1993; 70: 89-91.

THE P < 0.05 CRITERION IS NOT INVIOABLE

Sir,—It is interesting to read that single stimuli every 10 s and train-of-four stimulation every 10 s yield different estimations of the potency of mivacurium. Maddineni and colleagues [1] reported that the leading twitch (T1) of the train-of-four exhibited a deeper block than the single twitch (T). The authors suggested, plausibly, that the more intense train-of-four stimulation itself and the secondary improvement in blood flow relative to the muscle hasten the onset and deepen block of T1. Another factor likely to contribute to the deeper block of T1 is that a train-of-four every 10 s allows less rest between stimulations (8.5 s to 10 s).

I wish also to comment on the authors' conclusion that T differs from T1 significantly for ED₉₅ (43 vs 35 μg kg⁻¹ (P = 0.03)), insignificantly for ED₉₀ (85 vs 66 μg kg⁻¹ (P = 0.05)), and insignificantly for slopes of the dose-response curves. It is not logical that there should be a difference in only one of the two points of the regression lines without a difference in the slope.

Many authors and reviewers inflexibly adopt the customary "P < 0.05" criterion. Some even object to the notation of P = 0.01, P = 0.1, etc. and insist that either "P < 0.05 or "*", and nothing else, be printed. Some further equate "statistically not significant" with "not different". Others—a minority to which I belong—are more wary of the danger of type II error associated with small sample size in a highly variable population [2]. They point out that the P < 0.05 criterion is artificial. Borderline P values should be revealed [1, 2]. "Statistically not significant" is not "not different", if the means are not different, t tests rarely need to be done.

Taking the example of Maddineni and colleagues' study, great individual variability in data existed, as expected. Instead of describing P = 0.051 as insignificant but P = 0.03 significant, I would call both comparisons significant to P = 0.075. This leaves a 92.5% chance that the two differences in ED values are both significant, and allows the logical conclusion that the two regression lines are parallel. In other words, I would reject the P < 0.05 criterion. The authors' overall conclusion was in favour of the difference.

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Sir,—I agree entirely with the sentiments expressed by Dr Lee. He has obviously noticed that the difference between the ED₉₀ values was also very close to being significant (P = 0.051). This was the aim of our giving the actual P value rather than simply saying that the difference was insignificant.

We are glad to see that Dr Lee agrees with the conclusions we had reached and I hope others will reach similar conclusions. I also agree with him that to describe a difference as significant at P < 0.05 is only arbitrary, although conventional.

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SPINAL NEEDLE: AN UNUSUAL COMPLICATION

Sir,—We read with interest the report on "Concorde nose" in Whitacre spinal needles in which McLeod, Carson and Banister illustrated the point that, if a Whitacre needle strikes bone, the needle should be removed and discarded [1]. The side hole in a pencil-point needle renders it weak and thus makes it more prone to bending. We recently encountered bizarre bending of the stilette of a bevelled re-usable spinal needle, presumably after striking bone.

A 37-year-old ASA I female was to undergo vaginal hysterectomy under spinal anaesthesia. A 23-gauge bevel-tipped spinal needle was introduced slowly at the L3-4 interspace with the patient in the left lateral position. Resistance was felt at a depth of about 4 cm and, on the presumption that the needle had entered the ligament flavum, a slight increase in force was made. After some resistance, the needle moved in with a jerk. Believing that the needle had by then entered the subarachnoid space, the resident attempted to remove the stilette, but could not. On a second attempt, the hub sheared off at the junction with the shaft, which stayed inside the needle. An attempt was then made to withdraw the needle and the stilette together, but although the needle was removed, the stilette (without hub) remained in situ. By increasing the flexion of the spine, it was possible eventually to remove the stilette, using some force. After removal, the tip was found to be