CORRESPONDENCE

Propofol and postoperative nausea and vomiting

Editor,—I read with interest the article by Bree and colleagues1 on combining propofol with morphine patient-controlled analgesia to prevent postoperative nausea and vomiting (PONV). They showed that addition of propofol did not decrease the incidence of PONV. I would like to suggest some explanations for their results.

The authors correctly pointed out that the plasma concentration of propofol for 50% reduction in nausea scores in a group of postoperative patients was 343 ng ml $^-1$. This concentration may be achieved with a bolus dose of propofol 10 mg followed by continuous infusion of 1 mg kg $^-1$ h $^-1$. Therefore, a 70-kg patient would receive 70 mg h $^-1$. In Bree and colleagues’ study, the propofol treated group used a mean dose of propofol 9.8 (95% CI 7.5–12.1) mg h $^-1$ for the first 24 h and 2.6 (95% CI 0.9–4.2) mg h $^-1$ for the next 24 h. Indeed, the maximum dose of propofol used (in one patient) was 180 mg in a 4-h period (45 mg h $^-1$). Hence, it is likely that their patients received sub-therapeutic doses of propofol for antiemetic effects.

We have recently reported the use of patient-administered propofol for the treatment of PONV.1 In a randomized, double-blind design, patients self administered propofol 20 mg or Intralipid (placebo) with a 5-min lockout interval, where no propofol could be administered. The propofol group had significant reductions in the degree of nausea (25% less nausea vs placebo), incidence of vomiting (12% vs 56%) and need for rescue antiemetic (17% vs 70%) compared with placebo. Placebo patients had a nine-fold increase in the risk of subsequent vomiting and a 10-fold increase in the likelihood of using rescue antiemetics. The propofol group received a mean dose of propofol 100 ± 60 mg during the study (2 h). This was not associated with an increase in sedation compared with placebo. Propofol treated patients were also more satisfied with their control of PONV.

When propofol is used for its antiemetic effects in comparative studies, it is important that therapeutic dose ranges are administered.

T. J. GAN
Department of Anaesthesia
Duke University Medical Centre
Durham, NC, USA


Anaesthesia in third world countries

Editor,—When asked to join a group of surgeons on a trip to rural Guinea, West Africa, we were faced with the challenge of providing safe general anaesthesia in a setting with certain limitations to anaesthetic practice. First, an unreliable electricity supply ruled out the use of any drug needing storage in controlled temperatures (e.g. propofol) and mains-operated equipment. Second, the lack of any gas-sampling system made inhalation agents an inappropriate choice. Third, because of the shortage of trained staff, we had to ensure an awake patient at the end of each operation. Unfortunately, during their training, both authors had only gathered theoretical knowledge of the EMO inhaler, which is thought by many to be the ideal method of anaesthesia in third world countries.

Remifentanil, with its unique pharmacological profile, is used in different combinations to provide safe and easily titratable anaesthesia in many settings.1–3 Methohexital is used frequently in our institution for awake sedation and as propofol or inhalation agents were not thought to be an option for the reasons stated above, we decided to use the combination of remifentanil and methohexital as a simple i.v. anaesthetic regimen. The following technique was used successfully for general anaesthesia in 39 patients without any major complications.

After i.v. atropine 0.5 mg to block ketamine-induced secretions and to prevent bradycardia caused by remifentanil, patients received i.v. sedation with low-dose midazolam 0.05 mg kg $^-1$ and ketamine 0.5 mg kg $^-1$. Anaesthesia was induced with a bolus dose of methohexital 1 mg kg $^-1$ and an i.v. infusion was started containing methohexital 1 mg ml $^-1$ and remifentanil 0.03 mg ml $^-1$, the initial rate being 1 ml kg $^-1$ h $^-1$. After mask ventilation was achieved, patients were given vecuronium 0.05–0.08 mg kg $^-1$ and the trachea was intubated about 3–5 min. Patients underwent manual ventilation with 40% oxygen in air and the methohexital–remifentanil infusion was titrated to required effect during operation. Mean rate of infusion was 1.38 (range 0.7–1.95) mg kg $^-1$ h $^-1$. The drip was turned off approximately 5 min before the end of operation and the majority of patients were awake and cooperative within 5 min of the end of the procedure. Postoperative analgesia was provided with an NSAID, alone or in combination with i.v. tramadol.

Details of the surgical procedures are given in table 1. Table 2 lists the choice of anaesthetic, its duration, time to extubation and complications encountered.

Table 1 Surgical procedures performed under general anaesthesia

<table>
<thead>
<tr>
<th>Diagnosis</th>
<th>No. of anaesthesics</th>
</tr>
</thead>
<tbody>
<tr>
<td>Hernia (inguinal, scrotal)</td>
<td>13</td>
</tr>
<tr>
<td>Goitre</td>
<td>9</td>
</tr>
<tr>
<td>Gun shot</td>
<td>5</td>
</tr>
<tr>
<td>Vesiculovaginal fistula</td>
<td>4</td>
</tr>
<tr>
<td>Utero-vaginal prolapse</td>
<td>4</td>
</tr>
<tr>
<td>Tubal abscess</td>
<td>2</td>
</tr>
<tr>
<td>Fixed tongue band</td>
<td>1</td>
</tr>
<tr>
<td>Appendicitis</td>
<td>1</td>
</tr>
<tr>
<td>Fractured elbow</td>
<td>1</td>
</tr>
<tr>
<td>Gluteal abscess</td>
<td>1</td>
</tr>
<tr>
<td>Lipoma</td>
<td>1</td>
</tr>
<tr>
<td>Perforated colon</td>
<td>1</td>
</tr>
<tr>
<td>Recto-vaginal fistula</td>
<td>1</td>
</tr>
<tr>
<td>Revision of vesicovaginal fistula</td>
<td>1</td>
</tr>
</tbody>
</table>
Table 2  Duration of anaesthesia, time to extubation and complications encountered

<table>
<thead>
<tr>
<th>Duration of anaesthesia (min)</th>
<th>Complications</th>
</tr>
</thead>
<tbody>
<tr>
<td>Less than 30</td>
<td>6</td>
</tr>
<tr>
<td>30–60</td>
<td>4</td>
</tr>
<tr>
<td>60–90</td>
<td>15</td>
</tr>
<tr>
<td>90–120</td>
<td>4</td>
</tr>
<tr>
<td>120–180</td>
<td>6</td>
</tr>
<tr>
<td>Longer than 180</td>
<td>2</td>
</tr>
</tbody>
</table>

Time to extubation after stopping remifentanil–methohexital infusion (min)

<table>
<thead>
<tr>
<th>Duration of anaesthesia</th>
<th>Complications</th>
</tr>
</thead>
<tbody>
<tr>
<td>Less than 5</td>
<td>25</td>
</tr>
<tr>
<td>5–10</td>
<td>7</td>
</tr>
<tr>
<td>Longer than 10</td>
<td>5</td>
</tr>
<tr>
<td>Missing data</td>
<td>2</td>
</tr>
</tbody>
</table>

Respiratory arrest with i.v. sedation

<table>
<thead>
<tr>
<th>Complications</th>
</tr>
</thead>
<tbody>
<tr>
<td>Residual neuromuscular block</td>
</tr>
<tr>
<td>Nausea/vomiting</td>
</tr>
<tr>
<td>Respiratory arrest with i.v. sedation</td>
</tr>
</tbody>
</table>

There were no major problems with this technique. One critical anaesthetic incident occurred in a young woman who had received ketamine–midazolam sedation for revision of her gunshot wound, and experienced respiratory arrest when given another 20 mg of methohexital. The trachea was intubated and the patient recovered uneventfully, proving that a battery-powered pulse oximeter is a mandatory piece of equipment even in the most basic settings.

The cost of the general anaesthetic using remifentanil–methohexital is approximately £15 ($22) per hour of anaesthesia for a patient of 70 kg body weight. This relatively high cost is counterbalanced by the fact that the capital cost of an anaesthetic machine is not incurred.

We conclude that infusion of a fixed combination of remifentanil and methohexital provided safe and simple general anaesthesia. While not the method of choice in third world countries in standard hospital settings, we found it cost-effective and safe in an environment where anaesthesia needs to be provided with little equipment.

The mission to Guinea was organized by MANGO (Medical Actions in Guinea), a charity based in Frankfurt, Germany. All drugs were donated by the respective manufacturers.

C. OSMER
G. HEINBUCH
Abt. f.Anaesthesiologie und Operative Intensivmedizin
Klinikum der Justus-Liebig-Universität
Giessen, Germany


Current practice of local anaesthesia for ocular surgery

Editor,—As anaesthetists at the King Khaled Eye Specialist Hospital (KKESH), we read with particular interest the survey by Mawer and Coombes.1 KKESH has completed 55 000 procedures under local anaesthesia and another 45 000 under general anaesthesia since its opening in 1984. We wish to endorse the recommendations of the Joint Working Party on Anaesthesia in Ophthalmic Surgery2 on the management and care of patients undergoing ophthalmic surgery and briefly outline our standard practice.

At KKESH, as is usual in ophthalmic practice, patients present at the extremes of age, often with diabetes, hypertension, ischaemic heart disease or congenital problems. It is our practice to evaluate fully all of our patients. All patients are subjected to a thorough medical evaluation and appropriate special investigations. Patients considered suitable are encouraged to have their procedures performed under peribulbar anaesthesia, which is our preferred local technique because of its good record. All patients presenting for cataract surgery have axial length determined by ultrasound.

Resuscitation equipment is available in the preoperative holding area and all staff are regularly certified in basic life support. An anaesthetist is always present during performance of blocks. All patients, with the exception of babies and small children, have i.v. access established on arrival in the preoperative holding area. Electrocardiogram and arterial pressure monitoring are started before proceeding with the local block, which is administered by a consultant anaesthetist. Monitoring of electrocardiogram, arterial pressure and oxygen saturation is continued during the procedure by anaesthetic personnel who are present throughout. Patients are returned to the wards when they are considered stable.

A small percentage of peribulbar blocks are performed by ophthalmology registrars under anaesthetic supervision. Registrars are not permitted to administer blocks to patients who have one eye an axial length longer than 25 mm or staphyloma, or vision in the other eye of less than 20/200; they are not permitted to do “top-ups.” Registrars are required to perform 50 peribulbar blocks under supervision during their training. Four of six consultants use 25-mm sharp 25-gauge needles and two use 38-mm sharp 25-gauge needles. All anaesthetists use a standard mixture of lidocaine, bupivacaine and hyaluronidase.

Problems encountered during peribulbar blocks, although uncommon, may be severe and even life threatening, such as central spread of the local anaesthetic agent or activation of the ocular cardiac reflex.3 Patients with treated hypertension or unrecognized labile hypertension may present a problem in the preoperative holding area, particularly after the administration of phenylephrine eye drops.

Although we have experience of all of these complications, and even pulmonary oedema, they were always diagnosed promptly and treated immediately. We have never had cause to regret our cautious approach. It has stood the unit in good stead and has resulted in an impeccable anaesthetic outcome. We believe that departure from the above practice represents substandard practice.

K. R. EDGE
S. J. KRIGE
Y. K. BOSMAN
P. W. DU TOIT
F. MONTOYA
J. I. NEWSTEAD
A. V D BERG
King Khaled Eye Specialist Hospital
Riyadh


Editor,—Thank you for the opportunity to respond to Dr Edge and colleagues. The practice at the King Khaled Eye Specialist Hospital (KKESH) appears exemplary. There is no doubt that the Working Party recommendations which they endorse represent very safe anaesthetic practice.

It would be interesting to learn if patients are admitted as day cases, having been assessed by an ophthalmology doctor at a preoperative clinic or whether the anaesthetists have the opportunity to see the patients, either on the ward or in an anaesthetic preclerkling clinic before the day of surgery. I am sure this may reflect on what appears to be an ideal preoperative work-up, as yet not always available in the NHS. The training available for inserting peribulbar blocks appears well thought out, yet there is no mention of training for trainee anaesthetists. It is an interesting comparison that in the Kingdom of Saudi Arabia, consultant anaesthetists teach the trainee ophthalmologists, whereas in the UK the reverse is often the case.

K. R. EDGE
S. J. KRIGE
Y. K. BOSMAN
P. W. DU TOIT
F. MONTOYA
J. I. NEWSTEAD
A. V D BERG
King Khaled Eye Specialist Hospital
Riyadh

Kingdom of Saudi Arabia
Practical airway assessment

Editor,—Contrary to the conclusions of Arné and colleagues,1 I believe that multivariate indexes that are designed to help “predict” difficult intubation are of little value to anaesthetists in the real world. Investigations have focused on the relationship of several factors alone or in combination (e.g. mouth opening, dentition, Mallampati score, neck extension) to the degree of difficulty in seeing the vocal cords with direct laryngoscopy or to “difficulty”2 associated with actual tracheal intubation. However, I am not aware of any study that has examined the relationship of bedside airway assessment and “difficulty with mask ventilation.” What we are really interested in when making clinical decisions is the potential for morbidity (from all possible courses of action) or mortality. A logical analysis predicts that morbidity and mortality result because of difficulty with oxygenation-ventilation, aspiration or trauma related to the chosen method of airway intervention. The ASA airway algorithm3 implies that unanticipated “difficult airway scenarios” (by whatever definition) will continue to occur despite bedside airway assessment (either because the tests are inherently imperfect4 and/or because anaesthetists do not know what to do with the information).1

It is time we took a more pragmatic approach to bedside airway assessment and airway management until such time that a bedside airway test(s) is developed that can be relied upon to a degree that allows the construction of an algorithm for airway management based on bedside assessment. I try to teach anaesthesia residents to think of bedside airway management in the following practical terms. First, recognizing that traditional bedside tests are of some, albeit limited, value, I suggest classifying the airway as obviously normal, obviously abnormal or somewhere in between. Then identify any factors that may put the patient at increased risk should there be a brief problem with tracheal intubation (e.g. full stomach, intracranial disease, suspected cervical spine instability, ischaemic heart disease, anterior mediastinal mass, etc.). Consider if there are any alternatives to general anaesthesia or, more specifically, general anaesthesia with a tracheal tube. Recognize that it is extremely unusual to encounter a patient that cannot be ventilated via a face mask (or a laryngeal mask) —although our ability to predict “difficult ventilation with a face mask” remains unquantifiable! Be familiar with the ASA airway algorithm and comfortable with its implementation (which assumes competency with all of its elements) and ensure that the necessary equipment and personnel are immediately available and in “working order”. Recognize that techniques designed for the management of the potentially difficult airway are not always benign. Consider giving the patient a choice. Perhaps most importantly, ask yourself what you would want done.

S. A. LANG
Department of Anaesthesia
University of Calgary
Calgary, Alberta, Canada


Editor,—We read with interest Dr Lang’s letter regarding the use of predictive scores for preoperative detection of difficult intubation1 and are grateful for the opportunity to reply.

We agree that the most important problem is to identify “difficulty with mask ventilation” and that the primary goal of bedside evaluation would be to allow construction of an algorithm for airway management. However, as pointed out, the incidence of difficulties in face mask ventilation seems to be very low: El-Ganzouri and colleagues found an incidence of 0.08% in a population of 10 507 patients, and Rose and Cohen found an incidence of 0.01% among 18 500 patients. This low incidence explains why no study to date has determined a bedside multifactorial score of prediction. Inclusion of more than 500 000 patients would be necessary for a multifactorial evaluation. Moreover, construction of such an algorithm must also include situations where difficulties in both ventilation and intubation occur. The work of Rose and Cohen failed to identify even one case, despite studying 18 500 patients. Thus a very large epidemiological study, including several million patients, would be required. Bedside prediction of difficult intubation is a more pragmatic approach. If a score predicts difficult intubation with a minimal false negative risk, a positive prediction suggests the patient should be allowed to breathe spontaneously thus avoiding potential difficulties with oxygenation and ventilation.

We disagree with the idea that airway management could be only a matter of common sense. In fact, common sense works only when the airway is obviously normal or abnormal. It does not work in intermediate situations, which are the most difficult to predict. The main use of a predictive score is for these intermediate situations where several minor difficulties are associated (e.g. a Mallampati class 2 associated with limitation of neck movement in the 90–100° range). In these cases, using only common sense leads to an erroneous prediction in approximately 50% of patients. When the risk index is applied, the probability of a wrong prediction is reduced to 7%, probably because associated minor signs cannot be missed or forgotten. Dr Lang suggests that sufficient knowledge of an airway algorithm and competency with all the substitutive techniques is more important than an erroneous prediction. We feel that fibreoptic tracheal intubation is easier to perform in spontaneously breathing patients than in apnoeic and paralysed patients. How do you plan spontaneous ventilation without a preoperative prediction? However, we agree that knowledge of an airway algorithm is always required. The cornerstone of the debate seems to be is it safer to prevent or to treat the problem? Our opinion is that in the field of the difficult airway, as in the more general field of anaesthesia practice, we should prevent and anticipate events rather than treat them.

J. ARNÉ
J. FUSCIARDI
Department of Anaesthesia and Surgical Critical Care
University of Potters
Potters, France

Anasthetic for telescopic procedures in the thorax

Editor,—I was interested to read the recent review of anaesthesia for telescopic procedures in the thorax, by Plummer, Hartley and Vaughan,1 but question some of their assertions about oesophagoscopy.

First, they state that “a fiberoptic oesophagoscope is generally
used for inspection and therapeutic procedures” Most ENT surgeons with whom I have worked use the rigid instrument routinely. Second, their use of reinforced tracheal tubes (presumably those containing a steel spiral within the wall) is at variance with the practice of many anaesthetists, who use pre-formed PVC (“RAE”) tubes, and have no problems with tube compression. It is incorrect to assert that “no suitably sized reinforced tracheal tubes [are] available” for use in children, as a full range of such tubes (both paediatric and adult) is available in the UK, manufactured by “Sheridan”.

A. TAYLOR
Department of Anaesthesia, Sunderland District General Hospital Sunderland

1. Plummer S, Hartley M, Vaughan RS. Anaesthesia for tele-neck is performed, a suitable “Sheridan” reinforced tube is used. When any surgical operation around the head and reinforced tubes. However, it would seem logical, given this information, that when any surgical operation around the head and neck is performed, a suitable “Sheridan” reinforced tube is used.

R. VAUGHAN
Directorate of Anaesthesiology University Hospital of Wales Heath Park, Cardiff

Appropriate size of the laryngeal mask airway in adults

Editor,—I read with interest the article of Asai and colleagues1 on the selection of an appropriate size of laryngeal mask airway (LMA). In this study there are some references to my work, as we have some common conclusions.2 But as there are some misconceptions about our study design, clarification is needed.

Asai and colleagues compared two different sex-related formulae (SRF) for selection of an appropriate size of LMA, but they did not compare their findings with weight-related formulae (WRF).3 The latter have been proposed by the inventor of the LMA and are widely accepted in anaesthesia. In the study of Voyagis, Batzioulis and Secha-Doussaitou,4 they recommended using size 5 for all males and size 4 for all females, and using a smaller size (No. 4 in males, No. 3 in females) if placement of the LMA was inappropriate. This SRF was compared with the WRF suggested by the inventor in that time period (pre-1996). In our study, the age of the patients was not used as a predictor of LMA size (which was reported incorrectly by Asai and colleagues). When applying the SRF, the size of an LMA No. 5 was eight times more common, as it was used in many non-obese male patients. Mean body weight in these patients was 20 kg lower than that of the control group (SRF: 76 (61 16) kg vs WRF: 96 (6) kg; P < 0.0001). Brimacombe and colleagues5 replied to our study by suggesting that further evidence is needed before moving away from weight-based recommendations. However, in October 1996, Brimacombe and Brain revised the quantitative limits of their initial WRF. The new limits are 20 kg lower than the initial ones (i.e. more than 70 kg, size 5; 50–70 kg, size 4; and less than 50 kg, size 3). This new WRF is not significantly different from the SRF suggested previously,6 as the majority of female adults have a body weight of 50–70 kg and the majority of males have a body weight of more than 70 kg.

Although our study is reported by Asai and colleagues as the only one in which an attempt was made to determine the appropriate size of LMA, in the references, the letter of Brimacombe and colleagues7 is not reported as a reply comment. Additionally, in the introduction, the wrong impression is given that Brimacombe and colleagues recommended the use of an LMA as predicted by gender, but the opposite is true. Asai and colleagues found that 13% of males (four of 30) and 17% of females (five of 30) had unacceptable security of the airway when LMA No. 5 and No. 4 were used, respectively. Improvement in air-tightness was not reported when a smaller size LMA was used in these patients. In our study, the respective frequencies of inappropriate airleak were 16% (11 of 67) in males and 12% (nine of 77) in females, being statistically equal to those of Asai and colleagues. Whenever our patients had unacceptable airleak with the larger sizes of LMA, the use of smaller ones resulted in optimal airtightness.

G. S. VOYAGIS
Department of Anaesthesiology Sotiria General Hospital Athens, Greece

References


Editor,—It was my mistake to state that “Voyagis, Batzioulis and Secha-Doussaitou attempted to decide if the patient’s age or weight was a better indicator for selecting the appropriate size of the laryngeal mask” (italics added)—in fact, they studied the effect of patient sex, not age.2 However, it should be apparent that my statement was a mistake, since I summarized in the subsequent sentences that they compared sex- and weight-based selection methods.3

Dr Voyagis claims that selection of the appropriate size of the laryngeal mask based on the patient’s weight is widely accepted in everyday practice, but it may not be so. The majority of anaesthetists I work with decide the size primarily based on the patient’s sex. I cited Brimacombe and colleagues’ statement that “… based on a common misconception that the size 3 is for females and the size 4 for males” to indicate that it is a common practice among anaesthetists to select the size based on sex, whether or not Brimacombe and colleagues recommend a weight-based selection method is irrelevant here. In fact, I believe that they carefully avoided claiming that size should be selected based on the patient’s weight rather than on sex, by stating that “further conclusive trials are required before the manufacturer’s weight-based recommendations are altered”.3

The main purpose of our study was to show that the size 4 is usually not too large to insert in females, and may even be a better choice than the size 3 in terms of incidence of airleak. I did not suggest that an appropriate size of mask should be selected based on the patient’s sex rather than on weight. In this respect, I support Brimacombe and colleagues’ statement that “judging the correct size of the laryngeal mask can be difficult since the relationship between gender, weight, height and upper airway geometry appears inconsistent”.2

There are similar difficulties in selecting the appropriate size of a tracheal tube. Nevertheless, the size of a tracheal tube is usually selected based on sex. However, other factors are taken into consideration: patient age, height, weight and size of the skeletal structure, and incidence of complications (such as postoperative sore throat).2

I noticed recently that in some apparently large women, the size 5 laryngeal mask provided a better seal than the size 3 or 4 without producing higher pressures on the pharynx, indicating the limitation of the selection of an appropriate size based solely on the patient’s sex. Therefore, I support fully the claims made by Voyagis, Batzioulis and Secha-Doussaitou that “the overall suggested sex-related formulae do not work in all cases, but might be considered as a general principle”.2 I also believe that neither their2 or our4 study contradicts the recommendation in the latest instruction manual that “as large a size of mask as possible should be used”.

T. ASAI
Department of Anaesthesiology, Kansai Medical University Osaka, Japan


Editor,—We were interested in the article by Asai and colleagues1 who assessed the most suitable size of laryngeal mask airway (LMA) by determining the competence of the seal under general anaesthesia. Our attempt, examining instead the incidence of sore throat in 200 patients following the use of a size above and a size below those commonly used, came to a different conclusion.2

Frequently used sizes are LMA 4 in men and LMA 3 in women, and we had determined previously the incidence of sore throat using these sizes in a study of 839 patients.3 Sizes LMA 5 and 3 in men, LMA 4 and 2.5 in women were also tested. A size larger than usual might have been expected to require a lower cuff pressure or alternatively a size smaller might have reduced the surface contact area. The outcome of cuff volume adjustment until leakage of respiratory gases was just prevented was also studied, equivalent to the “just-seal” conditions of Asai and colleagues’ fourth group. We found that the incidence of sore throat was significantly lower in men than in women if the cuff was inflated normally, an observation which has been reported elsewhere4; it was lower or the same in all patients when the cuff was adjusted to just prevent leakage, and was increased with both the next sizes up (5 and 4, respectively) or the next sizes down (3 and 2.5). In effect, the LMA 4 in men and LMA 3 in women resulted in the lowest incidence of soreness and these would appear to be appropriate sizes.

M. R. NOTT
R. P. HILL
Department of Anaesthesia
St Richard’s Hospital
Chichester

Editor,—It may not be surprising that Drs Nott and Hill reached a different conclusion from us as they looked at different end-points from ours to decide an appropriate size of laryngeal mask. They made their decision based on the incidence of postoperative sore throat,1,2 whereas we did so mainly on the incidence of airleak during manual ventilation.3 Despite a difference in conclusion, I believe that their results1,2 and ours3 are generally consistent.

First, in Drs Nott and Hill’s studies,1,2 when the cuff volume of the laryngeal mask was not adjusted (i.e. the mask being inflated with the recommended maximum volume of air), the incidence of sore throat was higher when a larger (size 4 in females and size 5 in males) than a smaller (size 3 in females and size 4 in males) mask was used. Similarly, in our study,3 when the mask was inflated maximally, the pressure exerted on the pharynx was greater with a larger than with a smaller mask.

Second, they found that the incidence of postoperative sore throat was reduced by adjusting the cuff volume of the laryngeal mask to the minimum effective volume (“just-seal” condition).1,2 These results are consistent with ours3; removal of air from the maximally inflated cuff to the minimum effective volume significantly decreased the pressure exerted by the mask on the pharynx.

Third, in their study, when air was removed from the mask to the effective minimum volume, there was no marked difference in the incidence of sore throat between sizes 4 and 5 in males.1,2 This is also consistent with our findings that there was no significant difference between the pharyngeal pressures produced by the two sizes in males.3

The last finding of Drs Nott and Hill, however, is apparently not consistent with ours: they claimed that the incidence of sore throat associated with the use of the size 4 was greater (approximately 20%) than that for the size 3 (they report 3.9%, but it is in fact 7.3% (16 of 219 patients)) in females.1 In contrast, in our study, there was no significant difference in pharyngeal pressure between sizes 3 and 4.1 There are two possible explanations for this inconsistency. First, the difference in the incidence of postoperative sore throat in their report might not be significant if a statistical analysis was applied for this difference, although there is difficulty in doing so as historical controls were used.3,5 The second possibility is that size 4 causes a higher incidence of sore throat than size 3, by producing abrasion of a wider area of the oropharynx during insertion.

Taken together, I believe that the following two conclusions can be drawn from Drs Nott and Hill’s study and ours. First, the pressure exerted by the laryngeal mask on the pharynx and incidence of postoperative sore throat can be reduced by removing air from the maximally inflated cuff to the minimum effective volume (or “just-seal” condition). Second, intracuff pressure, the pressure exerted on the pharynx and the incidence of postoperative sore throat are greatest when a large laryngeal mask is used without adjusting cuff volume; therefore, cuff volume should be adjusted to the minimum effective volume, particularly when a large mask is used.

T. ASAI
Department of Anaesthesiology
Kansai Medical University
Osaka, Japan

**Paddle size in defibrillation**

Editor,—The success of defibrillation is determined by transmyocardial current which comprises a proportion of the total transthoracic current. Transthoracic current is inversely proportional to transthoracic impedance (TTI). Low TTI has therefore been equated with generating higher transmyocardial current, and by implication, improved success from defibrillation. For example, the low TTI achieved when using adult defibrillation paddles on children has been assumed to improve outcome from defibrillation.6

Although a decrease in TTI increases overall transthoracic current, it does not necessarily increase total transmyocardial current. Above an optimal paddle size, some current traverses extracardiac pathways through the thorax, bypassing the heart,7 and results in reduced transmyocardial current. This has been demonstrated in canine studies in which larger paddles reduced TTI but greatly reduced defibrillation success rate (33% vs 83%).8 Failure to improve transmyocardial current with increasing paddle size may also explain why some studies have failed to show any benefit in cardioversion success rates using larger paddles.9 Therefore, measurement of TTI alone cannot establish if paddle size is optimal.

Current guidelines quoting 13 cm as the optimum paddle size in adults10 are not based on studies of transmyocardial current or defibrillation success at different pad sizes. Since optimum paddle size in dogs is approximately 13.0 cm and canine hearts are smaller than human hearts, it has been suggested that optimum size in humans may actually be larger than 13 cm.11 The optimum paddle size in humans remains to be determined.

C. D. DEAKIN
Southampton


Editor.—The point made by Dr Deakin is correct. As with many other aspects of resuscitation, practice is guided by convention, tradition and extrapolation from animal experience, but scientific evidence is missing. This applies to electrode size: optimal electrode size in humans has still to be determined. Many of the investigations are limited to studying paddles of manual defibrillators, but the problem is at least as important for self-adhesive electrodes of automated external defibrillators.

Although sudden cardiac death from ventricular fibrillation is the single most frequent cause of death in the industrialized world and defibrillation is recognized as the only effective treatment for ventricular fibrillation, our knowledge and understanding of the mechanisms of defibrillation in humans are still limited. It is well known that the effect of defibrillation is very much dependent on the various technical characteristics of the components of the “defibrillator–paddle–couplant–chest complex”. Little scientific evidence is available on the absolute and time-dependent influence of these various components on transthoracic impedance, transhilaric and transmyocardial current or the influence of paddle size, shape and couplet on the characteristics and duration of the behaviour of the electrocardiographic surface signal immediately after delivery of an electric shock. Little scientific evidence is available relating to the optimal energy of the first and subsequent shocks, the optimal waveform for defibrillation or optimal electrode size and shape.

Since the introduction of automated external defibrillators, using self-adhesive pads, interest in optimal technology, including electrode technology, has increased suddenly. Resuscitation councils such as the European Resuscitation Council have repeatedly insisted on the need for standardization for electrodes and connectors for automated external defibrillators. This plea for standardization could be an incentive for the defibrillator industry to focus their research on these aspects and to share their knowledge and research programmes with the clinical investigators.

L. BOSSAERT

An extrapyramidal reaction to ondansetron

Editor.—A healthy, 37-year-old female presented for evacuation of retained products of conception after intrauterine death at 18 weeks’ gestation. She gave a history of severe postoperative nausea and vomiting which had previously been treated successfully with metoclopramide. Therefore, it was decided to administer ondansetron 4 mg i.v. before induction of anaesthesia.

A few minutes after administration of ondansetron, she began to complain of blurred vision and dizziness. Induction was continued and, after pre-oxygenation, rapid sequence induction was performed with thiopental 375 mg and succinylcholine 75 mg followed by tracheal intubation. Anaesthesia was maintained with 1.2% inspired isoflurane and 70% nitrous oxide in oxygen, and supplemented with alfentanil 500 μg, with the patient breathing spontaneously.

Recovery after the procedure was punctuated by a brief episode of laryngospasm on extubation, but was otherwise uneventful and rapid. The patient had no postoperative nausea or vomiting and experienced very little pain. However, she complained of blurred vision and had uncontrolled movements of her eyes, face, and arms. On examination, she had marked horizontal nystagmus, involuntary eye movements and mild choreiform movements of her arms. She was observed for 30 min, during which time there was no improvement in her symptoms. She was given procyclidine 5 mg i.v. and her symptoms stabilized within 5 min. She was observed for 2 h during which time there were no further episodes. She was transferred to the ward where the rest of her recovery was uneventful.

Extrapyramidal reactions to ondansetron have been reported when ondansetron has been used in higher and repeated doses of 8–30 mg 8-hourly, for prevention of nausea and vomiting during chemotherapy. As far as I am aware, extrapyramidal reactions to ondansetron have not been reported in the anaesthetic literature. This case suggests extrapyramidal reactions may be caused after a single, small i.v. dose of ondansetron.

C. STONELL
Department of Anaesthesia Yeovil District Hospital Yeovil


Postdural puncture headache after combined spinal–epidural analgesia and anaesthesia

Editor.—Combined spinal–epidural (CSE) analgesia1 has become increasingly popular for analgesia in labour. However, several authors have questioned the desirability of routine dural puncture. Among other criticisms of the technique it has been suggested that CSE analgesia is associated with a greater risk of post-dural puncture headache (PDPH) than epidural analgesia.2

We have examined retrospectively the reported incidence of headaches in 2086 consecutive obstetric patients after epidural block, subarachnoid block or CSE. These were performed for elective Caesarean section (“elective” CS), in labour for analgesia or for emergency CS (“emergency”). The reported incidence of headache after receiving regional analgesia or anaesthesia is shown in table 1.

<table>
<thead>
<tr>
<th>Procedure</th>
<th>No. of women reporting</th>
<th>Obvious dural puncture</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Epidural</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(16-gauge Tuohy)</td>
<td>13</td>
<td>0</td>
</tr>
<tr>
<td>Elective</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Emergency</td>
<td>1219</td>
<td>21 (1.7%)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>5 (0.4%)</td>
</tr>
<tr>
<td>Subarachnoid</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(25-gauge polyanic)</td>
<td>107</td>
<td>0</td>
</tr>
<tr>
<td>Elective</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>112</td>
<td>0</td>
</tr>
<tr>
<td></td>
<td></td>
<td>1 (0.9%)*</td>
</tr>
<tr>
<td>CSE</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(25-gauge through</td>
<td>246</td>
<td>1 (0.4%)</td>
</tr>
<tr>
<td>16-gauge Tuohy)</td>
<td></td>
<td>2 (0.8%)</td>
</tr>
<tr>
<td>Elective</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>389</td>
<td>10 (2.6%)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>5 (1.3%)</td>
</tr>
</tbody>
</table>

Of the small number of patients receiving elective epidural anaesthesia, none complained of headaches. Of the 1219 patients receiving epidural analgesia–anaesthesia in labour, 21 complained of a headache after delivery. Only five (0.4%) of these had identified dural punctures. All others were complaints of minor headaches not typical of PDPH which resolved with simple analgesia or no treatment.

Of the group of patients receiving subarachnoid anaesthesia, none of the elective or emergency patients complained of headaches, despite one of the patients having a dural puncture from the 20-gauge spinal introducer needle.

Of the 635 patients receiving CSE, 246 had an elective proce-
dure for a planned LSCS; two (0.8%) of these patients had an identified dural puncture with a 16-gauge Tuohy needle. Only one of these complained of a headache and required an extradural blood patch for treatment. A total of 389 patients received CSE for analgesia in labour or for emergency LSCS. Ten (2.6%) of these patients complained of headache, five (1.3%) of whom had identified dural punctures with a 16-gauge Tuohy needle; four of these required an epidural blood patch to alleviate the PDPH. The others had minor headaches not typical of PDPH, all of which resolved after treatment with simple analgesia.

These data would support the hypothesis that CSE analgesia is not associated with a clinically significant increased incidence of severe headache. The majority of severe headaches in our audit were caused by dural puncture with the Tuohy needle in emergency cases. This is probably a reflection of our practice of performing CSE analgesia in women who are in severe pain in late labour or before emergency Caesarean section because of the previously reported advantages of speed of onset and better sacral dermatomal analgesia.

C. ROWLEY
I. JAYAPALAN
C. D. ELTON
Department of Anaesthesia
Leicester General Hospital
Leicester

Pharmacokinetics of propofol during conscious sedation using target-controlled infusion

Editor,—I read with interest the study by Oei-Lim and colleagues (1) is similar to the clearance calculated using the PKOPT model (0.144 min⁻¹) in the table) and third, the value of Kc for the PKOPT model should be 0.144 rather than 0.114 min⁻¹. Consequently, this means that the calculated clearance for the PKOPT model is 25.64 ml kg⁻¹ min⁻¹ and that for the Gepts’ model is 28.7 ml kg⁻¹ min⁻¹. We apologize for these errors which arose in the publishing process but were not subsequently detected when we read the proofs.

Because the clearance values of the two models are similar, the two models will, as Dr Lim states, ultimately predict similar concentra-tions at steady state when the peripheral compartments are equilibrated with the central compartment of the models. However, for propofol, this steady state situation takes approximately 6 h to attain. In the first part of our study, the procedures were completed within 2 h and we therefore suggest that Dr Lim’s remarks concerning steady state are not relevant to our study.

Notwithstanding the above-mentioned errors, the simulation in figure 6 is correct. During the first 60 min of infusion, there was a substantial difference in propofol concentrations predicted by the two models and we have shown that the PKOPT model optimally described the observed data both in our study and the Gepts’ model, which systematically over predicted the observed data. We confirmed this finding in our prospective study. The simulation we have performed in figure 6 demonstrates that, according to the PKOPT model (1) which optimally described the population data that we obtained in our study, the Gepts’ model, when used as the delivery model in a TCI system, resulted in a steadily increasing plasma concentration of propofol and, presumably, a deepening level of sedation during the time span of our study. When the PKOPT model parameters were used to deliver propofol in a modified TCI delivery system, an improved model performance was obtained.

It should be borne in mind that both the Gepts’ and PKOPT models are “averaged” population models. The performance of each model will vary from patient to patient and also for each individual with time. The performance of a given model for an individual patient is relatively unimportant in relation to how the population as a whole behaves. In figure 6, the data points from an individual patient were over predicted by both models. However, the prediction error was less for the PKOPT than for Gepts’ model. It was not stated that the patient was a “best fit” for the PKOPT model. In this graph for this particular individual, the PKOPT model described the data better than Gepts’ model. The PKOPT model described the population optimally but not necessarily any one individual.

T. A. LIM
Clinical Sciences Section
International Medical College
Malaysia