

CORRESPONDENCE

Anaesthesia for telescopic procedures in the thorax

Editor,—We read with interest the article by Plummer, Hartley and Vaughan¹ and would like to raise some points for discussion. There is increasing danger from a medico-legal viewpoint of stating in a review article that a certain course of action is “mandatory” as it might subsequently be construed that failure to perform the suggested practice is negligent. Such statements may often reflect local practice and not that of a wider body of opinion. Review articles should perhaps discuss common practice and attempt to avoid a didactic approach.

On a more specific note, mediastinoscopy certainly carries a risk of unintended vascular biopsy and an adequate venous cannula is important, but is it really “mandatory” to cannulate a leg vein in all patients presenting for mediastinoscopy, as they suggest (rather than only those patients presenting with superior vena cava obstruction)? It is common practice for our surgeons to “test” unclear areas with a fine needle before biopsy. This decreases (but does not eliminate) the risk of vascular damage.

Plummer, Hartley and Vaughan state that a reinforced tracheal tube is necessary during oesophagoscopy. There must be serious loss of attention on the part of both the anaesthetist and surgeon for the tracheal tube to be compressed by the oesophagoscope. We would suggest that proper observation and communication between medical staff should prevent the need for a reinforced tracheal tube.

Is it “mandatory” to have a CT scan before anaesthetizing a patient with a pharyngeal pouch? It may well be that the origin of the pouch has been identified with a barium swallow. What is wrong with allowing the patient to sit up on the trolley during induction of general anaesthesia?

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1. Plummer S, Hartley M, Vaughan RS. Anaesthesia for telescopic procedures in the thorax. *British Journal of Anaesthesia* 1998; **80**: 223–234.

Editor,—Thank you for the opportunity to reply to Mitchell and Shaw. We would like to comment on each of the points raised.

Where the word mandatory is used, it is used advisedly, as we believe it enhances patient safety. If the latter is the key concern, then expert witnesses and lawyers would find it very difficult to criticize.

As Mitchell and Shaw state, the classical surgical approach is aspirate first and biopsy second. They freely admit that such an action decreases, but does not eliminate, the risk of vascular damage. Yet the British and American literature state that the major complication is haemorrhage, occasionally torrential. Perhaps the testing is not that good. What cost therefore is an extra venous cannula in the leg?

We take strong exception to the third point. Classical teaching, where intubation is required for head and neck surgery, is to use a reinforced tube. The review also recommends continuous monitoring, particularly of inflation pressure. We would be interested to know how else compression of a tracheal tube could be observed. Again, why take an additional risk when it can be eliminated beforehand by using a reinforced tube?

Most surgeons would like to know where the mouth of the pharyngeal pouch is located, and inevitably would have performed a CT scan. The use of barium is acceptable but apparently somewhat old fashioned, and it tends to remain in the pouch for some time. Lastly, these patients are usually in the older age group and induction of anaesthesia in the head-up position has certain disadvantages.

Finally, one of these three original authors has been in thoracic practice for nearly 30 yr. He has followed these recommendations all his professional life and they have most certainly been instrumental in enhancing patient safety. Is anything else acceptable?

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Editor,—We read with interest the review article by Plummer, Hartley and Vaughan¹ which detailed the anaesthetic considerations of telescopic procedures in the thorax. It was a comprehensive review regarding adult patients but we were disappointed by their scant references to the paediatric population.

While some of the techniques described, such as mediastinoscopy, are seldom performed in children, others such as thoracoscopic procedures are increasingly important.² Oesophagoscopy, gastroscopy and bronchoscopy are also commonly indicated, yet, in discussing these, the authors have referred to children only briefly. The reader is left to assume either that there are no other problems associated with the use of the technique in this group or that the technique is not indicated in children. Furthermore, some of the authors' assertions regarding these procedures, which apply largely to the adult population, are not qualified with respect to children. In particular, the difficulties with one-lung ventilation in young children receives no mention.

With regard to thoracoscopy, there may be benefits in terms of reduced pain and decreased morbidity in children, as in adults, undergoing diagnostic procedures.³ However, we have suggested previously that this may not be the case in operative procedures, where children may suffer considerable postoperative pain.⁴ The multiple wounds associated with these procedures may approximate to the size of a thoracotomy incision in young children, and the fact that the incisions involve several sensory dermatomes makes the provision of effective postoperative analgesia more difficult. Other authors appear unconvinced of the best form of analgesia and have advocated prospective studies.⁵ We found that the majority of postoperative pain was associated with the chest drain site, and thoracoscopic procedures do not necessarily decrease the duration of chest intubation in children.⁶

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1. Plummer S, Hartley M, Vaughan RS. Anaesthesia for telescopic procedures in the thorax. *British Journal of Anaesthesia* 1998; **80**: 223–234.
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5. Mulder DS. Pain management principles in anesthesia techniques for thoracoscopy. *Annals of Thoracic Surgery* 1993; **56**: 630–632.
6. Gomolo A, Gall O, Larroquet M, Constant I, Balquet P, Murat I. Video-assisted thoracoscopic surgery for right middle lobectomy in children. *Paediatric Anaesthesia* 1997; **7**: 215–220.

Editor,—Armstrong and Martin are correct in that the article was originally commissioned for the anaesthetic management of *adult patients* for these investigations.

However, where procedures are performed in children, with the main exception of thoracoscopy, the development of both modern equipment and techniques means that the main principles set out for adults are not too far removed for those in children, except that in children they are performed under general anaesthesia.

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A case made for automated anaesthetic record keeping?

Editor,—The results of the study of Byrne, Sellen and Jones¹ made interesting reading. They questioned the accuracy of anaesthetic

record charts when recording critical incidents. They observed that the more stressful the situation, the less likely the chart is to be accurate. They concluded that, "... thus, the relevance of anaesthetic chart accuracy to the legal process must be questioned." This is a well conducted study, which confirms a long standing suspicion.

What is the validity of manual record in a *res ipsa loquitur* case, where decision might rest on such record?² There seems to be a case to be made for automated record keeping. In major emergencies such as ruptured aortic aneurysm repair, few anaesthetists would be so tenacious or perhaps paranoid enough to divide time between priorities such as rapid blood replacement and contemporaneous record keeping, until the patient is relatively stable, which may be an hour or more into surgery. It is almost inevitable that to varying degrees, such records are made up, except when there are so many anaesthetists available that it is possible to spare one for the sole purpose of accurate contemporaneous record keeping. This is now less likely.

Even protagonists of manual record keeping, who might argue that it keeps the anaesthetist in contact with the patient, might agree that in situations of mental overload the most objective recording is in the automated mode. Automated record keeping might prove to be another means of generating a spare pair of hands, in a similar way to the laryngeal mask. The claim that mental absorption and evaluation of information in the process of manual record keeping is lost during automated recording is probably no longer valid. Manual record keeping need no longer be a condition for an anaesthetist's vigilance, since the introduction of intelligent alarms technology with integrated displays.³ Similarly, the problem of artefacts in autorecords could be solved easily by collaboration with software developers involved in the manufacture of our monitoring systems. Improved software programming should provide facilities enabling the user to mark or delete artefactual items on the record.

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1. Byrne AJ, Sellen AJ, Jones JG. Errors on anaesthetic record charts as a measure of anaesthetic performance during simulated critical incidents. *British Journal of Anaesthesia* 1998; **80**: 58–62.
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Editor,—We thank Dr Adejumo for his compliments, but would like to add a note of caution. The prime purpose of our study was to measure mental workload and not to question the validity of the anaesthetic record in the courtroom. We do agree, though, that the findings lend considerable weight to the fact that such records can be of questionable accuracy, especially under conditions of high mental workload.

We do not wish to imply, however, that we are necessarily fully in favour of abandoning manual records for automated ones. Automated records have undoubted benefits. For example, in two cases known to one of us (J.G.J.), failure to oxygenate the patient was documented only by the automated record. One episode led to severe hypoxaemia and a permanently disabled patient. In neither case was there any written record of oxygen saturation levels. Without the automated records, there would have been considerable speculation in court about several aspects of both cases.

However if, as seems increasingly likely, we are driven down the road of automated records, it must be emphasized that we need to pay just as much, or more, attention to validation of these records as we do to our charts. Thus systems and procedures need to be developed to ensure that automated records have *correct* dates and times, and appropriate annotation of artefacts (e.g. arterial pressure of 240/230 mm Hg during reaming in a total hip replacement). Further, we do not share Dr Adejumo's optimism that vigilance will be unaltered by the use of automated records. There is evidence in other domains¹ that automation can have negative effects on performance when systems take humans too much "out of the loop". Whether automated record-keeping in anaesthesia would have any such effects is at this point an open question.

To sum up, we feel that some work is required to better define the place of automated records. Unless we do so, there remains

the possibility that they will only threaten us and benefit the plaintiffs.

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1. Bainbridge L. Ironies of automation. In: Rasmussen J, Duncan K, Leplat J, eds. *New Technology and Human Error*. Philadelphia: John Wiley, 1987; 271–283.

Peripheral blocks of the lower limb for repair of fractured neck of femur

Editor,—We read with interest the article on prevention of spinal anaesthesia-induced hypotension in the elderly.¹ In our opinion, there is another way to reduce hypotension in patients undergoing surgical fixation of fractured neck of femur under regional anaesthesia using a combination of lumbar and sacral plexus blocks. We describe the management of an elderly man with aortic stenosis presenting for repair of an intertrochanteric fracture.

A 76-year-old man was admitted after a fall at home. The patient had a history of tuberculosis treated by pneumothorax and mild aortic stenosis (aortic surface 0.68 cm², mean aortic gradient 42 mm Hg in a recent echocardiogram) associated with episodic arrhythmias. On physical examination, the patient was in moderate respiratory distress, heart rate was 125 beat min⁻¹ (ECG confirmed atrial fibrillation), auscultation revealed an aortic murmur 3/6° and the lungs were clear (no clinical or radiological signs of congestive heart failure). Arterial pressure was 80/40 mm Hg. Two days later, after treatment with amiodarone, the ECG showed sinus rhythm of 85 beat min⁻¹ and arterial pressure was 85/50 mm Hg. Surgery was scheduled for the afternoon.

On arrival in the anaesthetic room, arterial pressure was 97/56 mm Hg, heart rate 95 beat min⁻¹ and peripheral oxygen saturation 91% breathing spontaneously, requiring oxygen 5 litre min⁻¹ to increase it to 98%. After i.v. administration of alfentanil 0.25 mg, the patient was turned to the lateral position, with the fractured side uppermost. Arterial pressure decreased to 85/58 mm Hg before the blocks were inserted. Lumbar plexus block was performed according to Winnie's landmarks.² A 100-mm stimulating needle identified the lumbar plexus by movement of the quadriceps femoris using a stimulus less than 1.0 mA; 1.33% lidocaine 30 ml with epinephrine 1:200 000 were then injected into the lumbar plexus. Sacral plexus block was performed using the parasacral approach based on Mansour's technique³; 1.33% lidocaine 20 ml with epinephrine were injected after eliciting a foot plantar flexion with the help of a neurostimulator using a stimulus of 0.5–1.0 mA at 1 Hz. An iliac crest point block (lidocaine 7 ml was injected opposite an orifice located on the iliac crest) completed the analgesia effect.⁴ The time to complete these blocks was 25 min. Motor block was complete in the entire fractured lower limb. The contralateral lower limb was not blocked. Surgery was started 20 min after completion of the blocks. Surgery consisted of gamanail osteosynthesis performed in 28 min and was uneventful. Only 300 ml of 6% hydroxyethylstarch was required to replace blood loss and ephedrine 6 mg was added to improve haemodynamic stability without tachycardia.

The patient was fully awake and orientated throughout; further hypnotic, or analgesic drugs were not given. The patient's satisfaction was measured at 95 mm using a visual analogue scale in the post-anaesthetic care unit. The patient started to walk 3 days after surgery and left hospital for a rehabilitation unit 10 days later.

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Editor,—Thank you for the opportunity to comment on Dr de Visme's letter in which he described a case of an elderly patient with an intertrochanteric femoral fracture, whose anaesthesia was managed by a combination of unilateral regional blocks. Despite significant preoperative haemodynamic instability requiring treatment, the procedure was apparently expedited with a minimum of systemic hypotension. Dr de Visme is to be congratulated on this achievement. However, I doubt if a majority of practising anaesthetists would perform these regional techniques sufficiently regularly to maintain a high success rate and to do so within a reasonable time frame. Even in this instance, the time from commencement of blocks to surgical incision was 45 min, which I believe is longer than the average time required to instigate spinal anaesthesia (median 28 min in our data).

Moreover, this technique required a total dose of 758 mg of lidocaine with epinephrine. Although the patient's weight is not reported, it is probable that this dose exceeds the recommended maximum of 7 mg kg⁻¹ for this agent, although I am aware of anecdotal reports of individuals safely administering up to 9 mg kg⁻¹ of lidocaine with epinephrine. Finally, I note that ephedrine 6 mg was given to this patient, but it is not clear which haemodynamic or other criteria triggered its use.

In conclusion, while the advanced regional skills described by Dr de Visme have a place in the management of high-risk patients for femoral neck surgery in which acute afterload reduction must be minimized, it may not be feasible to expect a majority of anaesthetists to regularly practise them. Hence studies such as ours, seeking to define the optimum methods of minimizing hypotension after the more widely used technique of spinal anaesthesia must continue.

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Propofol and sepsis: is it safer to use the TCI technique?

Editor,—The recent description of accurate pharmacokinetic models for i.v. anaesthetic application has been followed by the development of the concept of a target concentration infusion (TCI). Development of a TCI model for propofol¹ and the successful correlation of predicted to real plasma concentrations² permitted the first patient individualized, safe, computer-assisted anaesthetic.

The propofol TCI system consists of a syringe pump (different manufacturers) incorporating TCI software³ (Diprifusor TCI subsystem, Zeneca Ltd, Macclesfield, Cheshire, UK) and a pre-filled syringe of propofol (Diprivan, Zeneca Pharmaceuticals, Macclesfield, Cheshire, UK). Each syringe includes a coded recognition tag of specific information concerning the drug and its concentration (the programmable magnetic resonance tag). After injection of an entire pre-filled syringe, the syringe pump erases this tag.

Propofol has been associated with the risk of transmission of infection,⁴ mostly as a result of not adhering to aseptic practice. Safety warnings are repeated clearly on each pre-filled syringe (“for use in one patient only”, “sepsis must be maintained” and “this syringe should not be used for drawing up any fluid”). The lock and key system of the TCI syringe pump and tagged syringe ensures the safety of the TCI technology; erasing the tag of the pre-filled syringe when it is empty enhances patient safety by preventing refilling and reuse of the same syringe.

In order to check the safety of the available TCI technology, we recovered 75 pre-filled syringe units of 1% propofol emulsion from clinical use before complete infusion of the total volume of the syringe. After starting a TCI application *ex vivo* at a rate of

2 µg ml⁻¹, we noted the volume at which the message “nearly empty” appeared. The volume at which the recognition tag was erased by the syringe pump unit (Graseby 3500 incorporating Diprifusor, Graseby Medical Limited, Watford, Herts, UK) was also noted. This latter volume was defined as the volume beyond which the syringe could not be recognized successfully by the system after a short separation and replacement manoeuvre. The warning information “nearly empty” was displayed at 7.5 ± 2.5 ml and the tag was erased at 5.0 ± 0.5 ml.

These results imply that if separation of the pre-filled syringe from the syringe pump occurs when the message “nearly empty” appears, it does *not* impair the tag recognition capabilities. Furthermore, despite the security warning “not to be used for drawing up any fluid”, it is easy to draw up the syringe, permitting the infusion of a cheaper drug while profiting from the TCI technology. Moreover, drawing up is made easy by the design of the attachment of the plastic plunger rod to the rubber stopper of the syringe as these are screwed together.

Therefore, it could be tempting to reuse pre-filled syringes, transgressing the asepsis precaution rules. Such dangerous practice has to be discouraged vigorously and newly available technology should prevent it. Despite all written warnings, we have shown that the security features of the propofol TCI system can be bypassed easily, implying that patient safety related to aseptic precautions could be improved.

We regret that the currently available Diprifusor technology does not completely prevent misuse. We believe that simple changes in the design of the plunger–stopper attachment would make such practice physically impossible.

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Ketorolac does not increase the risk of renal dysfunction after lung surgery

Editor,—The recent editorial of Myles and Power¹ raised important issues regarding the risk of postoperative renal failure induced by non-steroidal anti-inflammatory drugs (NSAID) and the most efficient methods to assess the risk–benefit ratio of perioperative administration of ketorolac, a NSAID with opioid-like potency. We agree that there is no strong scientific evidence supporting a causal link between exposure to ketorolac and subsequent renal insufficiency and that, given the low incidence of renal failure after surgery, investigations should focus on selected high-risk groups of patients or major surgical procedures, and should rely on cohort or case-control study designs.

In our institution, administration of i.v. ketorolac has been included in our multimodal pain treatment regimen for patients undergoing thoracotomy for lung resection since 1995. In addition to the standard use of i.v. or s.c. morphine and extradural local anaesthetics and/or opioids, ketorolac has been given to 151 patients at a dose of 10–30 mg tid over a median period of 2 days (range 1–4 days). In order to prevent lung oedema, we routinely limit oral and i.v. fluids for 48 h after major lung resection (pneumonectomy and double- or single lobectomy). A prospective database, including preoperative patient history, co-morbidities, laboratory results and operative and postoperative outcome data on all patients undergoing lung resection was started in March 1995 (*n* = 234); the same items have been collected retrospectively, based on medical charts from 1990 up to February 1995 (*n* = 269)

(table 1). Among the criteria of organ dysfunction, we defined postoperative renal dysfunction as a 20% increase in plasma creatinine concentration above the preoperative value during the first 5 days after surgery. As indicated in table 1, modification of our analgesic treatment was not associated with a higher incidence of renal dysfunction (3.7% in the period 1990–94 and 2.9% in 1995–97; ns) and potential risk factors such as age, preoperative creatinine concentration, diabetes, vascular disease and duration of surgery were comparable during the two periods. Episodes of renal dysfunction were mostly transient (14/17), mild-to-moderate in nature (mean increase in plasma creatinine concentration from a mean value of 98 (SD 12) to 131 (28) mg dl⁻¹) and were associated either with increased preoperative creatinine concentrations (four of 17) and/or with other complications such as infection (eight of 17), arrhythmia (three of 17) or cardiac failure (three of 17). Only two patients receiving ketorolac (for 1 and 3 days) demonstrated transient and mild postoperative increases in serum creatinine (+25% and +31% above preoperative values).

Table 1 Characteristics of 503 consecutive patients undergoing elective thoracotomy (*n* (%) or mean (SD or range)). **P*<0.05 compared with period 1990–94

	1990–94 (<i>n</i> =269)	1995–97 (<i>n</i> =234)
Postoperative renal dysfunction	10 (3.7)	7 (2.9)
Age (yr)	62 (56–70)	64 (56–72)
Preoperative creatinine (mg d ⁻¹)	91 (10)	93 (9)
Diabetes	17 (7)	19 (8)
Peripheral vascular disease	27 (10)	26 (11)
Duration of surgery (min)	117 (19)	122 (23)
Postoperative analgesia		
with extradural catheter	195 (72)	178 (80)
with ketorolac i.v.	0	151 (61)*

Thoracotomy represents a major surgical stress and restriction of fluid intake could further activate the renin-angiotensin-aldosterone system. Although renal blood flow is thought to be highly dependent on local prostaglandin synthesis during stressful periods, our data do not support the hypothesis that short-term administration of ketorolac impairs postoperative renal function. For the given effect size (incidence of renal dysfunction of 3.7% and 2.9%), sample sizes (*n*₁=269 and *n*₂=234) and alpha level (0.05, two-tailed), the statistical power of our results was 0.06; this means that the incidence of postoperative renal dysfunction was expected to be similar in the two study periods with a probability of 94%. However, several confounding factors and biases should be considered. First, this was a non-randomized observational study where two groups of patients were compared according to time periods; data were collected retrospectively from 1990 up to 1994, and prospectively thereafter. Second, extradural administration of local anaesthetics represented the mainstay of analgesic treatment in the majority of patients (72% and 80%) and blockade of renal sympathetic nerves could partly alleviate the vasoconstricting effects caused by stimulation of the renin-angiotensin system. Hence, one may question whether thoracic extradural block affords renal protection during the stressful perioperative period or whether the release of vasodilatory prostanoids (PGE₂, PGI₂) is blocked by NSAID. Third, other drugs were administered concomitantly and ketorolac was given to 61% of patients during the second period; a preoperative increase in plasma creatinine concentration and a history of gastrointestinal disorders were considered contraindications for the use of any NSAID.

In summary, we believe that ketorolac can be administered safely over a short period (2–3 days) after major surgical procedures in patients with normal renal function. Apart from results of well designed, prospective, randomized studies, analysis of local databases can guide our rational clinical decision-making.

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Editor,—We thank Licker, Höhn and Schweizer for sharing their experiences of the use of ketorolac in thoracic surgical patients. Analysis of audit data has some value in reassuring clinicians and generating new hypotheses testing. Their data demonstrated that renal dysfunction is rare after major thoracic surgery.

We wish to clarify their interpretation of the power of their observational study: a power of 6% implies a beta (type II) error of 94%. This can be re-stated as there being a 94% chance of not detecting a statistically significant difference if in fact a true difference exists. In other words, their analysis was very unlikely to demonstrate that ketorolac causes renal failure in thoracic surgical patients. As stated in our editorial, such a study would require many thousands of patients, or alternatively, studying a group of patients at much higher risk of renal failure (note: it is actually the number of end-points that determines the sample size required).¹

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Local anaesthesia for routine ocular surgery

Editor,—I read with interest the recent article by Mawer and Coombes¹ who surveyed the current practice of local anaesthesia for routine ocular surgery by consultant anaesthetists in the Wessex region using a postal questionnaire. The survey showed that peribulbar block was the most popular block, but that retrobulbar block was also used by up to 26% of respondents. I was surprised that there was no mention of sub-Tenon's block, implying that this block is not used by this group. Sub-Tenon's block is used commonly in Aberdeen, both by anaesthetists and ophthalmic surgeons, and this spurred me into doing a literature search. While references to the technique abound in ophthalmology journals, I have been able to find only *one* reference in the anaesthetic literature.² It was also mentioned briefly in a review article on techniques of orbital regional anaesthesia.³

Sub-Tenon's block is simple and usually painless to perform; it is effective and, because of the *blunt*, specialized cannulae used to perform the block, is devoid of the potentially serious side effects of the more commonly used retrobulbar or peribulbar blocks.

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1. Mawer RJ, Coombes GA. Current practice of local anaesthesia for routine ocular surgery. *British Journal of Anaesthesia* 1998; **80**: 241–242.
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Editor,—As Dr Dark observes, it is not the current practice of Wessex anaesthetists to perform the sub-Tenon's ocular block for routine ocular surgery. Since the survey was performed, however, some anaesthetists have started to gain experience with this method of providing peribulbar anaesthesia.

It is felt that caution should be observed when the anaesthetist conducts a sub-Tenon's block. As a profession, we are unfamiliar working with scissors and forceps in such close proximity to the globe. Indeed, a globe perforation caused by scissors was reported at a recent ESA meeting. Most ophthalmologists perform this block with the aid of an operating microscope; the manual and pedal dexterity needed to operate this equipment correctly is not currently taught to trainee anaesthetists in Wessex.

Despite these reservations, we are sure that a review of the current literature, describing the method and complications of sub-Tenon's block would be well received by anaesthetists. As we indicated in our short communication, we also feel that a nation-

wide review of current practice of local anaesthesia for routine ocular surgery would be appropriate.

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Pain after amputation

Editor,—We were surprised to read a recent editorial “pain after amputation: is prevention better than cure?”, by Thompson.¹ Thompson prompts the idea of perioperative extradural block in the prevention of phantom pain.

The almost classical study by Bach, Noreng and Tjélden² is quoted in which 25 patients were allocated randomly to receive extradural bupivacaine, or morphine, or both, for 72 h before amputation (not 24 h as mentioned in the editorial) (11 patients; block group) or oral analgesia (14 patients; control group). After 6 months, all patients in the block group were pain-free, while five patients in the control group had phantom pain. The findings of Bach, Noreng and Tjélden are supported by other studies. Jahangiri and colleagues³ followed prospectively two groups of patients. One group ($n=13$) received extradural infusion of bupivacaine, clonidine and diamorphine, started 24–48 h before amputation and continued for 3 days after operation. The other group ($n=11$) received on demand opioid analgesia. After 1 week, 6 months and 1 yr, the incidence of phantom pain was significantly lower in the extradural group compared with the opioid group. In a preliminary study by Shug and colleagues,⁴ only one of eight patients who received perioperative extradural block had phantom pain after 1 yr, whereas six of eight patients who received systemic analgesia had phantom pain. The limited validity of these studies (small sample sizes, no or insufficient randomization, and non-blinded assessment of pain) was recognized by Thompson and he called for further investigation.

In November last year we published a randomized, double-blind study⁵ in which 60 patients undergoing amputation of the lower limb were allocated randomly to receive 0.25% extradural bupivacaine 4–7 ml h⁻¹ and morphine 0.16–0.28 mg h⁻¹ before and during operation (29 patients; block group) or extradural saline 4–7 ml h⁻¹ and oral or i.m. morphine (31 patients; control group). Both groups underwent general anaesthesia for the amputation and all received extradural analgesics for postoperative pain management. Patients were interviewed about pain on the day before the amputation and about stump and phantom pain after 1 week, and 3, 6 and 12 months. Blindness during the study was ensured by two independent examiners. Study end-points were rate of stump and phantom pain, intensity of stump and phantom pain, and consumption of opioids.

Median duration of preoperative extradural block (block group) was 18 h and median duration of postoperative extradural pain treatment (both groups), 166 h. After 1 week, 14 (52%) patients in the block group and 15 (56%) in the control group had phantom pain. The values for the block *vs* control group were: 14 (82%) *vs* 10 at 3 months; 13 (81%) *vs* 11 at 6 months; and 9 (75%) *vs* 11 at 12 months. Intensity of stump and phantom pain, and consumption of opioids were also similar in the two groups at all four postoperative interviews. Thus we were not able to confirm the findings of the previous studies.^{2–4} In addition, we showed that preoperative extradural block had no effect on post-amputation allodynia and wind-up-like pain.⁶ These findings suggest that although sensitization plays a role in persistent pain, it is not possible to prevent classical aspects of sensitization by transient extradural block.

While extradural pain treatment may be efficient in reducing preoperative ischaemic pain and postoperative stump pain, perioperative extradural block started 18 h before amputation and continued into the postoperative period does not prevent phantom or stump pain.

Thompson is correct when he states that the use of extradural infusions is not without risk. Extradural infections must be taken into account, especially among amputees who often have infected ulcers and diabetes. Extradural catheters should not be placed with the purpose of preventing phantom pain.

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Editor,—Thank you for the opportunity to reply to Nikolajsen and colleagues. Their study¹ was not available when the editorial² was written. Those interested in the subject will read the editorial in the context of current available evidence, including that from Nikolajsen and colleagues.

Does their single study provide the definitive answer to the question of prevention of pain after amputation? There has already been discussion in the literature regarding this issue. In a commentary that accompanied Nikolajsen and colleagues' study, Katz³ recognized the value of this work. However, in view of the complexity that surrounds the subject of post-amputation pain, he called for further high calibre research to be done. McQuay, Moore and Kalso,⁴ in the subsequent correspondence, contested this notion.

How much evidence is required to provide adequate proof? Before dismissing the possibility of prevention of pain after amputation, the best evidence must be sought because for some prospective amputees the future is grim. Such best evidence should come from a body of literature of good quality and consistency from which a strong recommendation can be made on the appropriate course of action. In my view, such a body of literature does not yet exist and although the work of Nikolajsen and colleagues is an important contribution, it does not in itself provide the definitive answer.

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Stature of anaesthetic personnel and positioning of patients

Editor,—I read with interest Dr Heath's editorial on some of the problems the more “vertically challenged” anaesthetist or anaesthetic assistant may face.¹ Coming from the other side of the normal distribution (I am 197 cm tall), I feel that some important points were overlooked and deserve to be highlighted.

I have worked in operating theatres where I regularly banged my head on the operating light (or, in the case of one particular theatre, the anaesthetic gas pod suspended from the ceiling!), because the highest it could be raised was to the middle of my forehead. I prefer to perform tracheal intubation with the operating table raised above its lowest point; in the case of rapid sequence induction, I often cannot do this as my assistant may not be tall enough to apply cricoid pressure correctly in my preferred intubating position. The end result is regular lower backache at the end of the working day. Similarly, standing up stretching to try to hold a

mask on a patient's face while bagging from a Bain circuit situated less than 1 m from the floor does not help one's lower back.

More attention should be placed on the ergonomics and design of operating theatres and anaesthetic equipment. In addition to tables which can be adjusted up and down thus helping with proper patient positioning, bagging circuits which can be elevated or lowered and theatre lights which can be retracted correctly would surely help reduce long-term morbidity among anaesthetic personnel.

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Editor,—Dr McCourt's experience usefully extends recognition of the operating theatre as a hazardous environment for staff and patients. I would prefer to translate his passive tense, "more attention should be placed ..." into an active, "anaesthetists should actively involve themselves in the design of theatres and equipment, paying due attention to ergonomics". Current training ensures that anaesthetists are exposed to all surgical specialties and are thus best placed to appreciate the needs of all medical staff within the operating theatre.

I hope that my editorial has suggested the solution to at least one of Dr McCourt's problems: standing his assistant on one or more platforms should allow effective cricoid pressure with the patient at his preferred height. With regard to the other, why not sit down? Many irksome problems are regarded as too mundane for attention. Simple solutions may be identified if they are focused upon.

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Vecuronium, like rocuronium, causes pain on injection

Editor,—In their short communication, Borgeat and Kwiatkowski stated that "vecuronium has not been associated with pain on injection."¹ But in our practice, we have noticed that vecuronium given before induction of anaesthesia frequently causes pain on injection. This phenomenon has been reported previously.^{2,3}

It is believed that vecuronium causes pain on injection as a result of its low pH. Rocuronium has approximately the same pH

as vecuronium, and probably causes pain by the same mechanism. We disagree with the conclusion of the authors that "the absence of pain in patients receiving 0.9% NaCl 1 ml adjusted to pH 4" argues against low pH as the mechanism of pain. This conclusion was based on only five patients, risking a type II statistical error. Furthermore, other acidic agents are known to cause pain on injection.⁴

We also noted that the incidence of severe pain on injection of rocuronium, as reported by the authors, was much higher than in a previous study (80% *vs* 12%).⁵ Unfortunately, the authors did not offer any explanation for this difference. They administered the agents in random order, saline 1 ml and rocuronium 1 ml, 30 s apart, in the presence of "venous stasis". This can result in an increase in the osmolality of the mixture in the vein. Increased osmolality is known to cause increase in pain on injection with vecuronium³ and other drugs.⁴ Furthermore, factors such as local distension of the vein, reduction of buffering by flowing blood and longer exposure time of rocuronium to the vessel wall are encouraged by stasis. These factors have been associated with an increase in pain on injection of propofol, and may have a similar relevance for rocuronium.⁶ We postulate that these factors were responsible for the high incidence of severe pain on injection of rocuronium.

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