Post-thoracotomy epidural vs paravertebral analgesia

Editor,—I read with interest the article by Richardson and colleagues on post-thoracotomy pain.1 The authors compared thoracic paravertebral bupivacaine (bolus of 0.5% followed by infusion of 0.25%) with thoracic epidural bupivacaine (bolus of 0.25% followed by infusion of 0.25%). Both groups received PCA morphine. The paravertebral group had significantly lower pain scores, better preservation of respiratory function, lower incidence of chest infections, and less nausea, vomiting and hypotension. Thus the authors concluded that with these regimens, paravertebral block was superior to epidural bupivacaine. As many believe that thoracic epidural can provide superior analgesia, these are impressive findings.

I feel it is important to emphasize that Richardson and colleagues used 0.25% bupivacaine without opioids in the epidural group, which is an uncommon practice. A combination of local anaesthetic and opioid administered epidurally has been shown to be synergistic.2-3 Even though the optimal drug combination has yet to be determined, 91% of anaesthetists in the UK use a combination of bupivacaine with either fentanyl or diamorphine.4 Choosing to administer only local anaesthetic epidurally effectively handicapped the epidural group in comparison with the paravertebral group. It is conceivable that a combination of an opioid and dilute 0.1% bupivacaine would have resulted in a significantly better outcome in the epidural group.

The authors reasoned against using a local anaesthetic-opioid combination regimen for the epidural group on the basis that all patients returned to the general ward and they were unsure how to provide additional rescue opioid analgesia, as a combination of neuraxial and systemic opioids (for example) is known to be a major risk factor of respiratory depression. It is worth noting that 39% of respondents to a survey of thoracic epidural analgesia practice allowed patients to return to a general ward without specific additional nursing resources.4 Inadequate analgesia with a correctly placed epidural catheter can generally be managed with an epidural top-up; systemic opioids are usually not needed. The use of epidural morphine, including top-ups, has been shown to be safe on surgical wards.5

Paravertebral analgesia is an attractive alternative to epidural analgesia; it is a relatively easy technique and is free of the risk of major neurological injury. We need a study comparing the two methods of analgesia where each technique is individually optimized. Otherwise I am left wondering whether paravertebral analgesia is really as good or maybe even better than thoracic epidural.

J. Y. Y. Wang
Department of Anaesthesia
Edinburgh Royal Infirmary
Edinburgh, UK


2 Tejwani GA, Rastan AK, McDonald JS. Role of spinal opioid receptors in the antinociceptive interactions between intrathecal morphine and bupivacaine. Anesth Analg 1992; 74: 726-34


scientific meetings and I have not yet received an adequate reply. I would like to be corrected if my information is erroneous. Having made these comments, I am mindful that both of our groups needed additional opioid analgesia and I take this as a spur to continue to refine our regional analgesic techniques.

I agree that thoracic epidural analgesia can be continued on the general ward without additional nursing resources. This is indeed the practice in our own hospital where over the past 8 yr many hundreds of epidurals, using bupivacaine and morphine, have been managed without untoward respiratory incident. To restrict this form of analgesia to only those patients with two to one nursing ratios is a distortion of responsibilities. However, we felt that deviation from this (our standard) practice was necessary for the sake of our study. We had to circumvent any barrier, albeit unwittingly imposed, between the patient and additional analgesia. Additional epidural top-ups could have been given, but this would have involved delay and therefore introduced bias to the detriment of the epidural group and hence our data quality.

Obviously, high quality studies are required. The simple question, ‘can epidural opioid/local anaesthetic regimes compared to epidural opioid/local anaesthetic plus morphine improve post-thoracotomy pulmonary function or improved outcome?’ needs to be answered. Further small studies with different combinations examining pain scores and side effects are not helpful. I would be interested in taking the dose regimens from the resulting optimal study and comparing them in our own institution with our paravertebral nerve block based regimen. For ethical reasons, it is not possible to use the cough until gas leak around the mask was just prevented. With this technique, cuff volume was about 25 ml when the size 4 in females and gradually removed air from the cuff until gas leak around the mask was just prevented. With this method, cuff volume was about 25 ml when the size 4 in females and size 5 in males were used.2

Third, the authors assessed the presence/absence of gas leaks around the mask at airway pressures of 10 and 18 cm H2O. Several tests have been described to assess leaks; each differs slightly in accuracy and inter-observer reliability.2 It would be useful to know which test was used. Fourth, in the discussion, the authors indicate that the population studied was Oriental. It is a commonly held belief that smaller masks are easier to use in Oriental patients. Could the authors comment on the reason for failed placement in the three patients noted in Table 1? Were there any data on the time to successful placement between different mask sizes?

Finally, the authors imply that smaller LMA would commonly be required for tonsillectomy as the larger masks protrude into the oral cavity in 24–45% of patients. We consider this recommendation to be weak as the frequency with which the cuff protrudes into the mouth may be less with the flexible LMA (which has a smaller, more mobile tube that might be less likely to push the cuff into the oral cavity) and the presence of the cuff in the oral cavity may not interfere with surgical access to the tonsils. One of the authors (J.B.) has experience of 53 consecutive adult tonsillectomies with the flexible LMA (males size 5; females size 4) without any problems relating to surgical access.

J. Brimacombe
Department of Anaesthesia and Intensive Care
University of Queensland
Cairns Base Hospital, Australia

C. Keller
Department of Anaesthesia and Intensive Care
University of Innsbruck, Austria
J. Sartain
Department of Anaesthesia and Intensive Care
Cairns Base Hospital, Australia

Appropriate size of laryngeal mask

Editor.—We read with interest the article by Asai and colleagues re-evaluating the appropriate size of laryngeal mask airway (LMA) in males and females.3 The authors found that larger sizes have a more effective seal, but came up into the mouth more often, which could interfere with tonsillectomy and could increase the risk of sore throat or lingual nerve damage. The value of this study would be greatly improved if the authors could provide additional information.

First, the data do not necessarily support these findings as no mention is made of the volume of air used to inflate the cuff. This makes inter-size comparisons difficult as the efficacy of the seal, fibroptic position2 and (probably) the extent to which the cuff protrudes into the mouth are related to cuff volume. Second, if the cuff is seen in the oral cavity, we agree that a smaller size should be used for tonsillectomy, but we do not agree that a smaller size should be used if prolonged surgery is expected. The authors’ rationale for this latter recommendation is that the presence of the cuff in the oral cavity might increase the risk of oropharyngeal morbidity. We are unaware of any data supporting this hypothesis and are concerned that the process of multiple insertion could itself increase the risk of oropharyngeal morbidity. The authors may be able to clarify this matter if they could provide postoperative morbidity data from their study population, as the LMA was inserted into each patient on 2–3 occasions.

Having made these comments, I am mindful that both of our groups needed additional opioid analgesia and I take this as a spur to continue to refine our regional analgesic techniques.

I agree that thoracic epidural analgesia can be continued on the general ward without additional nursing resources. This is indeed the practice in our own hospital where over the past 8 yr many hundreds of epidurals, using bupivacaine and morphine, have been managed without untoward respiratory incident. To restrict this form of analgesia to only those patients with two to one nursing ratios is a distortion of responsibilities. However, we felt that deviation from this (our standard) practice was necessary for the sake of our study. We had to circumvent any barrier, albeit unwittingly imposed, between the patient and additional analgesia. Additional epidural top-ups could have been given, but this would have involved delay and therefore introduced bias to the detriment of the epidural group and hence our data quality.

Obviously, high quality studies are required. The simple question, ‘can epidural opioid/local anaesthetic regimes compared to epidural opioid/local anaesthetic plus morphine improve post-thoracotomy pulmonary function or improved outcome?’ needs to be answered. Further small studies with different combinations examining pain scores and side effects are not helpful. I would be interested in taking the dose regimens from the resulting optimal study and comparing them in our own institution with our paravertebral nerve block based regimen. For ethical reasons, it is not possible to use the cough until gas leak around the mask was just prevented. With this technique, cuff volume was about 25 ml when the size 4 in females and gradually removed air from the cuff until gas leak around the mask was just prevented. With this method, cuff volume was about 25 ml when the size 4 in females and size 5 in males were used.2

Third, the authors assessed the presence/absence of gas leaks around the mask at airway pressures of 10 and 18 cm H2O. Several tests have been described to assess leaks; each differs slightly in accuracy and inter-observer reliability.2 It would be useful to know which test was used. Fourth, in the discussion, the authors indicate that the population studied was Oriental. It is a commonly held belief that smaller masks are easier to use in Oriental patients. Could the authors comment on the reason for failed placement in the three patients noted in Table 1? Were there any data on the time to successful placement between different mask sizes?

Finally, the authors imply that smaller LMA would commonly be required for tonsillectomy as the larger masks protrude into the oral cavity in 24–45% of patients. We consider this recommendation to be weak as the frequency with which the cuff protrudes into the mouth may be less with the flexible LMA (which has a smaller, more mobile tube that might be less likely to push the cuff into the oral cavity) and the presence of the cuff in the oral cavity may not interfere with surgical access to the tonsils. One of the authors (J.B.) has experience of 53 consecutive adult tonsillectomies with the flexible LMA (males size 5; females size 4) without any problems relating to surgical access.

J. Brimacombe
Department of Anaesthesia and Intensive Care
University of Queensland
Cairns Base Hospital, Australia

C. Keller
Department of Anaesthesia and Intensive Care
University of Innsbruck, Austria
J. Sartain
Department of Anaesthesia and Intensive Care
Cairns Base Hospital, Australia


Editor.—In our study,1 we inflated the cuff initially with the recommended maximal volume of air (20 ml for size 3, 30 ml for size 4 and 40 ml for size 5) and gradually removed air from the cuff until gas leak around the mask was just prevented. With this method, cuff volume was about 25 ml when the size 4 in females and size 5 in males were used.2

Second, we did not claim that the smaller size should be used if prolonged surgery is expected. Instead, we stated that ‘when the cuff of the laryngeal mask is seen [in the oral cavity], the decision to replace the mask with one size smaller depends on several factors. For example, a small mask is preferable when . . . surgery of a long duration is expected’ (authors’ italic).2 The rationale for this claim was, as Dr Brimacombe and colleagues correctly state, ‘the presence of the cuff in the oral cavity might increase the risk of oropharyngeal morbidity’. Of course, one
should consider the increased risk of morbidity associated with multiple insertions. We did not formally study the incidence of postoperative sore throat and thus we cannot provide data regarding this factor.

Third, we assessed the presence or absence of air leak by hearing an audible noise around the mask. Fourth, the reason for failed ventilation through the laryngeal mask was the same in the three patients in Table 1—the mask came out of the mouth during inflation of the cuff. We did not measure the time taken for insertion of the mask.

Finally, the flexible and standard laryngeal masks have different tube structures, but they have masks of the same specifications. Therefore, theoretically, there should be no differences between the two types of device in the incidence of the mask being seen in the oral cavity. Dr Brimacombe's information of unproblematic use of the flexible laryngeal mask in 53 patients is useful, but it may have been that all of his patients were reasonably tall (patient height was not given in his report).

T. Asai
K. Shingu
Kumamoto Medical University
Moriyuchi City, Osaka, Japan

Local anaesthetic infiltration and natural killer cell cytotoxicity

Editor.—The study by Greisen and colleagues demonstrated that infiltration of local anaesthetic abolished the natural killer cell response to painful stimulation of abdominal wall skin. This is an observation that cannot be refuted, but I must express serious concern that, on the basis of their results, they question the beneficial effect of local anaesthetic infiltration of surgical fields, and suggest that it may impair the response against certain microbial infections and dissemination of tumour cells. The battle to improve the quality of surgical pain relief is difficult enough without such unqualified statements. I would make two specific points.

First, the experimental model used was one of acute pain 'without tissue injury'. I know of no surgical wound that is not associated with tissue injury and therefore would question the validity of the conclusions. I appreciate that a similar study with tissue injury would be unlikely to attract many volunteers, but that does not make their model clinically relevant.

Second, local anaesthetic infiltration has been used for surgical procedures for more than 100 yr, but there is no evidence that it predisposes to bacterial infection or dissemination of tumour cells. Can Greisen and colleagues provide any, and, if so, does that mean that they would have every minor surgical procedure performed under general anaesthesia instead? Perhaps they would revert to using no anaesthesia or analgesia at all!

Observation is easy. Considering the relevance of the observation is altogether more difficult.

J. A. W. Wildsmith
University Department of Anaesthesis
Dundee, UK


Editor.—We thank Dr Wildsmith for his interest in our article. We never attempted to discredit the use of local anaesthetic infiltration for intra- and postoperative pain relief. Of course our data do not allow for this. Our purpose was to examine the natural killer (NK) cell response to acute pain per se and to discover if local anaesthesia could modify the response. Thus we were investigating the pathophysiology of pain and not that of surgery.

NK cells probably play a role as a first line of defence against certain tumour cells and virus infected cells, and an increase in NK cell cytotoxicity (in the experimental situation against a specific tumour cell line) is taken to reflect an increase in the ability to damage deleterious cells. We noted that local anaesthesia blocked the NK cell response in addition to pain. Therefore, we raised the question if infiltration with local anaesthesia has a beneficial effect. The question has to be taken as one for generating hypotheses and not as a conclusion.

The fact that local anaesthesia has been used for many years is not in itself proof of its safety. To our knowledge, there are no studies in humans on the importance of local anaesthetics on tumour dissemination or susceptibility to viral infections.

J. Greisen
Department of Medicine V
E. Tonnesen
Department of Anaesthesiology
Aarhus University Hospital
Aarhus, Denmark


Double-lumen tube placement: protecting the good lung

Editor.—I congratulate Cheong and Koh for their study that not only questioned accepted practice for double-lumen tube placement but also succeeded in showing that there is, after all, a better way.

They used the fiberoptic bronchoscope in the tracheal lumen of a left-sided double-lumen tube to observe, as it was happening, passage of the tube into the left main bronchus and inflation of its bronchial cuff. The twin advantages of speed and certainty of double-lumen tube placement make this a rational approach, especially in the presence of pneumothorax or where there is a need to protect a good lung from contamination from a diseased lung. In the latter situation, however, the advantages may be lost if fiberoptic vision is impaired by the presence in the trachea of purulent secretions or blood.

In such cases, I have taught a tube placement procedure that has involved ‘blind’ initial insertion of a left-sided Robertshaw tube as the preferred double-lumen tube. I still believe that the relatively rigid anatomical shape of the Robertshaw reduces the likelihood of its entering the incorrect main bronchus during placement and, more importantly, of its becoming displaced in the course of surgery.

I was therefore keen to assess the merit of continuing with my old procedure but with the addition of the fiberoptic bronchoscope placed in the tracheal lumen of the left-sided Robertshaw tube during otherwise ‘blind’ placement. Unfortunately, I find that although the technique works very well for plastic double-lumen
At leastidr. experience with the Robertshaw double-lumen tube is limited and provided an excellent and concise review of the subject. However, and clinical evidence on which such recommendations are based. days, underwent an exploratory laparotomy and removal of a left mesoblastic nephroma. Approximately 20 kg kg$^{-1}$ produced rapid and sustained improvement in both heart rate and cardiac output. The baby made an excellent recovery and was subsequently discharged home with no signs of neurological impairment.

In his review, Dr Zidekan stated that ‘no children have survived to discharge who have received more than two doses of epinephrine’. The references used to support this statement studied the results after ‘out of hospital’ arrests where long ‘down times’ and relatively inadequate resuscitation could be confidently expected. Dr Zidekan also stated that in the ILCOR guidelines, atropine is ‘not indicated during resuscitation as the adrenergic effects of epinephrine are considered to over-ride the parasympathetic (vagal) effects of atropine’. The exact cause of this infant’s cardiac arrest is not known but was probably secondary to relative hypovolaemia with pre-existing impaired ventricular function. Before operation, this baby had been hypertensive requiring treatment with $\alpha$ and $\beta$ blockers. Residual adrenergic block may have played a role in the aetiology of the cardiac arrest, in addition to the requirements for high doses of epinephrine. The effective response to atropine with such high circulating concentrations of catecholamines is also surprising. Perhaps residual $\beta$ block was a contributory factor? The prompt recognition and treatment of this asystolic arrest was undoubtedly responsible for the eventual successful outcome. Clearly, such a short period between onset and effective treatment is highly unusual, especially in an ‘out of hospital’ situation and only adds strength to the emphasis placed on the early recognition and treatment of the critically ill child, as taught in resuscitation courses (e.g. Paediatric Advanced Life Support and Advanced Paediatric Life Support).

The guidelines recommended by the ILCOR and Resuscitation Council (UK) should be used but not rigidly adhered to without consideration of each individual case. Indeed, within the current guidelines, flexibility in therapy is ‘allowed’ when you ‘consider and correct reversible causes’. Of course, this also requires flexibility of thought on the part of those leading the resuscitation. The potentially huge disparity in outcome between individual resuscitation cases depending on their aetiology and location, as illustrated by the case report here, must surely encourage a more cautionary approach to the use of such sweeping statements when associated with such widely read and important guidelines.

H. Hack
Royal Manchester Childrens Hospital
Manchester, UK

Paediatric Resuscitation Guidelines

Editor,—Dr Zidekan’s article concerning paediatric resuscitation and the ILCOR guidelines, including the current experimental and clinical evidence on which such recommendations are based, provided an excellent and concise review of the subject. However, a recent case has led me to question two statements made in support of the current guidelines. A 3.8 kg male baby, aged 5 days, underwent an exploratory laparotomy and removal of a left suprarenal tumour. Histology subsequently showed it to be a mesoblastic nephroma. Approximately 20 min after removal of the tumour, the baby suddenly became profoundly bradycardic and hypotensive (arterial saturation remaining normal) and then rapidly suffered an asystolic cardiac arrest. Multiple doses of epinephrine, volume replacement (colloid and blood), sodium bicarbonate and calcium chloride were given (according to currently accepted ILCOR and Resuscitation Council (UK) guidelines). After four doses of epinephrine, ventricular fibrillation developed which proved very resistant to treatment. Eventually, after the sixth attempt at defibrillation and following a fifth dose of epinephrine, a slow, broad complex rhythm associated with a spontaneous cardiac output returned. Atropine 20 µg kg$^{-1}$ produced rapid and sustained improvement in both heart rate and cardiac output. The baby made an excellent recovery and was subsequently discharged home with no signs of neurological impairment.

In his review, Dr Zidekan stated that ‘no children have survived to discharge who have received more than two doses of epinephrine’. The references used to support this statement studied the results after ‘out of hospital’ arrests where long ‘down times’ and relatively inadequate resuscitation could be confidently expected. Dr Zidekan also stated that in the ILCOR guidelines, atropine is ‘not indicated during resuscitation as the adrenergic effects of epinephrine are considered to over-ride the parasympathetic (vagal) effects of atropine’. The exact cause of this infant’s cardiac arrest is not known but was probably secondary to relative hypovolaemia with pre-existing impaired ventricular function. Before operation, this baby had been hypertensive requiring treatment with $\alpha$ and $\beta$ blockers. Residual adrenergic block may have played a role in the aetiology of the cardiac arrest, in addition to the requirements for high doses of epinephrine. The effective response to atropine with such high circulating concentrations of catecholamines is also surprising. Perhaps residual $\beta$ block was a contributory factor? The prompt recognition and treatment of this asystolic arrest was undoubtedly responsible for the eventual successful outcome. Clearly, such a short period between onset and effective treatment is highly unusual, especially in an ‘out of hospital’ situation and only adds strength to the emphasis placed on the early recognition and treatment of the critically ill child, as taught in resuscitation courses (e.g. Paediatric Advanced Life Support and Advanced Paediatric Life Support).

The guidelines recommended by the ILCOR and Resuscitation Council (UK) should be used but not rigidly adhered to without consideration of each individual case. Indeed, within the current guidelines, flexibility in therapy is ‘allowed’ when you ‘consider and correct reversible causes’. Of course, this also requires flexibility of thought on the part of those leading the resuscitation. The potentially huge disparity in outcome between individual resuscitation cases depending on their aetiology and location, as illustrated by the case report here, must surely encourage a more cautionary approach to the use of such sweeping statements when associated with such widely read and important guidelines.
publication, has therefore added to the data source and the results that he has reported would be considered, together with those reported via published paediatric Utstein template reports, at the next review.

The paediatric resuscitation guidelines should not be considered as rigid protocols. I note that in his report, Dr Hack adhered to the recommended guidelines initially. In some events, especially when the cause is treatable and there is little or no monitored hypoxia, it is appropriate to continue resuscitation and to use any reasonable resource or treatment available. In this case, multiple administrations of epinephrine (dose not reported) were successful in restoring a spontaneous circulation. The use of atropine in the treatment of the resulting peri-arrest bradycardia was appropriate, despite the anomaly of the previous multiple doses of epinephrine. Finally, may I suggest that the success of this event was a result of sustained maintenance of tissue oxygenation. It cannot be emphasized sufficiently that sustained delivery of oxygen to this infant’s tissues was critical to the successful outcome.

I would hope that the publication of this letter will encourage others to report their paediatric resuscitation events, either as individual case reports or using the Utstein template, so that we can continue to improve the practice of paediatric life support.

D. A. Zideman
Department of Anaesthesia
Hammersmith Hospital
London, UK

Which is better in children: edrophonium or neostigmine?

Editor.—We read with interest the review article by Fisher on neuromuscular blocking agents in paediatric anaesthesia. It was a concise summary of the use of these agents in paediatric practice today. The author’s preference for edrophonium over neostigmine, however, did not seem to be a true reflection of what is known about antagonism of neuromuscular block in children.

There have been few comparative studies in children of the speed of action of edrophonium and neostigmine. In comparable mg per kg doses, recovery from an intense atracurium-induced neuromuscular block in children is faster after neostigmine than edrophonium. In adults, it has been shown that in the reversal of profound block produced by vecuronium or atracurium, neostigmine is more effective than edrophonium and its maximal effect is reached more quickly, even though edrophonium is faster in its initial onset. Monitoring of the depth of neuromuscular block in infants and children is technically more difficult and not perhaps as widespread as in Dr Fisher’s department. This suggests that the chance of profound neuromuscular block at the end of surgery is greater in paediatric anaesthetic practice. Neostigmine would therefore be a better choice than edrophonium.

In his article, Fisher stated that less neostigmine is needed in children than in adults, and quoted Fisher and colleagues. Quoting the same article, he then stated that the ED95 of neostigmine for antagonism was greater for children than for adults. It is possible that this is a typing error and that the author means to suggest that edrophonium has a higher ED95 in children than in adults. For this reason, the author suggests the use of higher doses of edrophonium for antagonism in infants and children. These studies, however, were carried out under steady-state infusion of tubocurarine and not during the recovery phase from the newer non-depolarizing agents. Moreover, there was no significant difference in the dose of edrophonium required to antagonize tubocurarine-induced neuromuscular block in children and adults. In contrast, several studies have shown that neostigmine antagonizes residual non-depolarizing neuromuscular block more effectively in children than in adults. Debaene, Meistelman and d’Hollander showed that, when twitch height recovered to 10% of control after vecuronium, neostigmine 30 μg kg⁻¹ had a more rapid onset in children than in adults, and that a TOF of 0.7 was obtained in less than 10 min in all patients, including infants. The dose of neostigmine to effectively antagonize 90% block produced by rocuronium is indeed smaller in children (mean 7 μg kg⁻¹) than in adults (56 μg kg⁻¹). The effects of 2 × ED95 of rocuronium could effectively be antagonized in infants with neostigmine 20 μg kg⁻¹.

With mivacurium there may be an argument for the use of edrophonium as it slows the hydrolysis of mivacurium by plasma cholinesterase less than neostigmine. However, until this issue is resolved, we agree with the author’s recommendation of not antagonizing profound mivacurium-induced block.

In summary, we feel that there are little compelling scientific data to prefer edrophonium to neostigmine in paediatric patients. Indeed, with the new shorter acting neuromuscular blocking agents, there may be some advantage to the use of neostigmine over edrophonium.

J. J. Driessen
E. N. Robertson
L. H. D. J. Booij
Department of Anaesthesiology
Academic Hospital Nijmegen
Nijmegen. The Netherlands

References

12 Leuwer M, Motsch J, Schleidt U, et al. Dose–response, time course of
Editor.—Driessen, Robertson and Booij challenge my preference for edrophonium over neostigmine. The first issue that they explore is profound block. My review acknowledges that neostigmine is preferred in that setting. Even if they are correct that “monitoring of infants is technically more difficult,” I hope that neuromuscular block is monitored routinely in infants. I also expect that clinicians are able to distinguish between profound twitch depression at the time of antagonism (no response to train-of-four stimulation) from the more typical level of recovery (presence of more than one twitch in response to train-of-four stimulation). Under these usual conditions, is there evidence to refute my claim that edrophonium works faster?

Driessen, Robertson and Booij detected a typographical error that I wish to correct. The ED₉₀ for edrophonium (not for neostigmine) is larger in children (233 μg kg⁻¹) than in adults (128 μg kg⁻¹). Although the difference was not significant, the larger value and larger variability in children than in adults led to our recommendation ‘that paediatric patients receive larger doses of edrophonium (e.g., 1.0 mg kg⁻¹) than adults.’

Driessen, Robertson and Booij then claimed that three studies demonstrated that neostigmine antagonizes residual non-depolarizing neuromuscular block more effectively in children than in adults. Unfortunately, they mis-state the conclusions of those studies. Debaene, Meistelman and d’Hollander found no difference between infants, children and adults for recovery of twitch at any time or in the train-of-four response before 10 min. They attribute the improved late train-of-four recovery in children to ‘the faster rate of spontaneous recovery from vecuronium’, rather than an effect of the antagonist. The results of Abdulatif and colleagues also can be explained by the more rapid spontaneous recovery in children than in adults (as evidenced by their control groups). Leuwer and colleagues’ abstract reported that neostigmine 20 μg kg⁻¹, given at 25% recovery, improved the time to train-of-four >0.7 in infants (no adults were studied) compared with a group given no antagonist; however, variability in recovery was quite large (so exceeded the mean) and no other neostigmine doses were evaluated.

Driessen, Robertson and Booij concluded that ‘there is little convincing scientific data to prefer edrophonium to neostigmine in paediatric patients’ and suggest that ‘with the new shorter acting neuromuscular blocking agents there may be some advantage to the use of neostigmine over edrophonium’. In contrast, studies in children and adults demonstrate that edrophonium antagonizes 90% block faster than neostigmine. Hence, whenever block is not intense and recovery time is important (as is increasingly true in clinical anaesthesia), I prefer edrophonium in children.

D. M. Fisher
Department of Anaesthesia
University of California
San Francisco, CA, USA


Bacterial contamination of needles used for spinal and epidural anaesthesia

Editor.—We read with interest the article by Raedler and colleagues regarding contamination of needles used during central neural block. It is cause for concern that almost 18% of the needles showed evidence of bacterial contamination, even though no patient developed infective complications. There are points we would like to make with regard to the aseptic technique which may explain the high incidence of bacterial colonization.

First, it is not clear if the 10% polyvidone–iodine (PI) used for skin preparation came from single or multiple-use containers. There is evidence to show that multiple-use PI bottles in normal use may become contaminated by bacteria and also that the solution is less effective than PI from previously unopened bottles. Also, alcohol chlorhexidine is a commonly used alternative to PI for skin disinfection and has been shown to reduce the rate of central venous catheter colonization and catheter-related sepsis compared with PI. There are no studies comparing the two solutions for skin disinfection before central neural block, but we would suggest that it should be used in preference to PI.

Second, the authors refer to the use of sterile gloves and drapes, but no mention is made as to whether face masks were worn. While we accept that the use of face masks has not been shown to decrease the incidence of postoperative surgical wound infection, there is evidence to suggest that they decrease bacterial dispersion and that their use can reduce the incidence of catheter-related infections during insertion.

We suggest that the use of alcohol chlorhexidine instead of 10% PI and wearing of face masks for central neural block may reduce the incidence of bacterial contamination of spinal and epidural needles.

J. Whiteside
J. A. W. Wildsmith
University Department of Anaesthesia
Dundee, UK

4 Legras A, Cattier B, Dequin P, Boulain T, Perrotin D. Prospective randomised trial for prevention of vascular catheter infection: Alcohol...
Editor.—Raedler and colleagues1 described an incidence of 17.9% bacterial contamination of spinal and epidural needles after lumbar puncture of the subarachnoid or epidural space. The authors make the important point that despite using an aseptic technique, lumbar puncture may contaminate spinal and epidural needles. Their important point that despite using an aseptic technique, lumbar puncture may contaminate spinal and epidural needles. Their study recommends single-use containers. Hence they suggest single-use bottles for skin disinfection. Small bottles of PI for single use might be more effective in this respect.

The advantages of using alcohol rather than aqueous based solutions are further demonstrated by in vitro studies. Sato, Sakuragi and Dan5 excised skin specimens from 60 patients undergoing elective back operations after preparation with either aqueous 10% PI or 0.5% chlorhexidine in 80% ethanol, and cultured staphylococcal species in 32.4% vs 5.7%, respectively.

A further problem, raised by Birnbach and colleagues, concerns the frequency of bacterial contamination of previously opened multiple-use bottles of aqueous 10% PI. Bacteria were found only in previously opened bottles of which 40% were contaminated. Hence the study recommends single-use containers.

PI in an alcoholic solution overcomes the disadvantages of aqueous solution. A comparison of the efficacy of 1% PI ethanol solution and 0.5% chlorhexidine ethanol solution7 showed no significant difference in decreasing skin bacterial counts (bacterial reduction rate 95% vs 93.5%).

Complete eradication of indigenous bacteria is impossible using the currently available skin disinfecting methods. However, to reduce the risk of infectious complications of epidural anaesthesia, attention should focus on any means of prevention, including optimal skin disinfection. Raedler and colleagues do not state if their PI solution was aqueous based or came from previously opened multi-use bottles. They recommend further improvement of hygienic measures but do not include optimization of skin disinfection with an alcoholic solution of 10% PI or 0.5% chlorhexidine.

E. Chopowska
French Hospital
Bristol, UK

6 Birnbach DJ, Stein DJ, Murray O, Thys DM, Sordillo EM. Povidone iodine and skin disinfection before initiation of epidural anesthesia. Anesthesiology 1998; 88: 668–72

Editor.—Our aim was to study our practice of administering spinal or epidural anaesthesia; our technique was guided by generally recognized rules. We used aqueous povidone-iodine (PI). Face masks were not used in all cases, but there was no correlation between the use of face masks and needle contamination in our study. We are aware of the study published by Birnbach and colleagues3 who found 40% of multiple-use PI bottles to be microbiologically contaminated compared with 5% contamination rate in bottles used for the first time. Hence they suggest single-use bottles for skin disinfection. Small bottles of PI for single use are not available in our country. Moreover, Birnbach and colleagues stated that none of the organisms isolated from the sponge sticks might have been the reason for their findings.

Another point raised was that alcoholic PI might be superior to aqueous solutions. We found our antimicrobial solutions eradicated bacteria after weeks of use. But our results demonstrated that standards and guidelines must be subject to continuous evaluation. Despite using an aseptic technique, lumbar puncture may contaminate spinal or epidural needles. In line with Birnbach and co-workers, we conclude that indigenous bacterial skin flora is most likely the source of this contamination. Alcoholic solutions may be more effective in this respect.

C. Raedler
Department of Anaesthesia
University Hospital Innsbruck
Innsbruck, Austria

1 Birnbach DJ, Stein DJ, Murray O, Thys DM, Sordillo EM. Povidone iodine and skin disinfection before initiation of epidural anesthesia. Anesthesiology 1998; 88: 668–72