Anaesthesia and the competence revolution

K. R. MYERSON  
Department of Anaesthetics  
Eastbourne Hospitals NHS Trust  
Eastbourne

CORRESPONDENCE

Editor,—In the recent editorial on anaesthesia and the competence revolution,1 Dr Greaves summarized clearly the problems involved in the assessment of doctors, and warned of the dangers inherent in following a vocational approach to the assessment of anaesthetists. However, as Dr Greaves writes, practical skills, behaviour and personal qualities are important requisites for an effective anaesthetist. So how best can we assess these aspects? It is clear that a “competency” approach, dividing the process of assessing an individual into a series of single tasks, the performance of which is marked and collated, is not appropriate. Such an approach trivializes professional behaviour, with its complex underlying reasoning, and makes no effort to assess these deeper aspects.

It is clearly vital to examine performance in the workplace. The Standing Committee on Postgraduate Medical and Dental Education (SCOPME), whose role includes advising the Secretary of State on such matters, recognizes the need for service-based assessment. It suggests that such assessment should be a formal process with right of appeal, and should be fair, but not too bureaucratic or legalistic. Furthermore, it should be part of the learning process.2 Standards remain a problem. Using specific laid down criteria for each aspect of anaesthetic performance (criterion referencing) is one possibility. However, such an approach would be over-complicated, requiring frequent revision of criteria used, and would not assess those deeper aspects of behaviour that we would wish to examine. Relating performance to that of others, regardless of the actual standards achieved (peer or norm referencing) is another, albeit unsatisfactory, approach.

However, a third method, relating the standard of performance to the assessor’s expert understanding of the minimum required for safety (limen referencing) could provide the basis for a workplace-based assessment standard.3 Such an approach has been found to be useful and reliable.4 Although it is difficult to quantify the element of professional judgement involved, there is currently no better method of assessing performance of the whole task.4 Consensus opinion on standards is difficult to attain, but the main underlying issue must be one of ensuring the relative safety of the clinical practice of the trainee given that person’s stage of training and degree of supervision. Competence assessors are well aware of what constitutes safe practice. By a continuing dialogue with the trainee, they are able to get a good idea of the underlying thought processes, attitudes, skills and knowledge, and provide feedback to the trainee. With appropriate safeguards, such judgmental elements within an array of assessments and assessor strengths will strengthen rather than weaken the accuracy of the picture.4 Such safeguards might include full documentation, openness and right of appeal. It is helpful to avoid the use of graded assessments, because such an approach is highly subjective and lends a spurious objectivity to the assessment.5

To write, as Dr Greaves has in his concluding paragraph, that “until the time that research is done, measurements made in the workplace should not be used either to monitor the progress of trainees or make decisions in relation to their careers” is clearly wrong. After all, there is also a relative lack of quantitative research-based knowledge supporting the effectiveness of the medical education we provide, yet we would certainly not choose to stop providing any education on this basis.

There is no single ideal assessment method, and it is unlikely that there ever will be. Each method used will have different advantages, disadvantages, reliability, validity and feasibility. A range of methods must therefore be used, in the knowledge of each one’s strengths and weaknesses. These will develop and become refined in the light of experience.

The South Thames (E) Assessment Scheme is an example of one such scheme in which is evolving in the full knowledge of the problems associated with workplace-based assessment, providing valuable feedback to trainee and trainer alike. The real danger lies in not performing assessments in the workplace. Without them, the Record of In-Training Assessment (RITA) system of Annual Review, upon which our whole system of specialist accreditation relies, becomes a worthless paper exercise.

Editor,—I read with interest Dr Greaves’ editorial on the shift towards competence-based education.1 While this educational strategy has many obvious attractions he highlights some of the problems likely to be encountered if the approach is adopted more widely. In particular he alludes to the difficulties associated with competence-based assessment. I agree fully with his assertion that our understanding of the tools and assessment process currently available are inadequate to justify their introduction and would like to comment further.

The fundamental role of assessment in the learning process is often not fully appreciated. While assessment in the form of traditional examinations has often been used primarily as a means of arbitrarily discriminating between candidates, a more enlightened approach to assessment views it as a crucial and inherent part of the learning process itself. For instance it is not surprising that what students choose to learn is largely assessment-driven—candidates concentrate on areas in which they expect to be examined. Assessment can also pinpoint weak points allowing remedial work. Crucially the assessment process can act as an appraisal of the teaching system itself. A major difference likely to occur with the use of competence-based assessment is the use of criterion-referenced marking strategy. Traditional examinations have often been perceived as being norm-referenced with the overall performance of an individual related not only to his own performance but also to that of his peers. Criterion-referenced marking strategy has many obvious attractions he highlights some of the problems likely to be encountered if the approach is adopted more widely. While any assessment process requires considerable forethought, design and planning, competence-based assessment is particularly complex. Madsen and colleagues2 have described the clinical competency criteria they use for assessment of anaesthetic trainees; these fall into four broad categories—knowledge, case management, technical skills and oral skills. They also describe “essential character attributes” which include ethics, reliability and appropriate response to stressful situations.

Gonczi3 has described the process needed to design the more exhaustive competency-based assessment used by the Royal Australian College of General Practitioners. Briefly the first step was to analyse the competencies needed by a GP; these included knowledge, skills and attitudes relevant to the medical areas and problems to which they were directed. It was necessary to assess knowledge, interpretative skills, problem solving attitudes and interpersonal and communicative skills, and perceptual abilities. These areas are not assessed in isolation. Instead the use of these competencies in solving a variety of problems in different situations is considered. The different areas also receive different weightings in the assessment. In order to carry out this assessment
of candidates' competence, eight different assessment methods were needed—case commentaries, MCQs (including simple completion sequential and relationship analysis questions), clinical interpretation tests, computerized case studies, physical examination, diagnostic interview, management interview and in-practice assessment. These examples illustrate not only the complex nature of competence-based assessment but the need for a broad range of methods of assessment to increase both the validity and reliability of the process.

The design of effective competence-based assessment in anaesthesia undoubtedly needs further research resource allocation and probably the involvement of professional educators. There is perhaps a danger of being swept along by the tide of change brought by the medical education revolution. However, to adopt newer approaches without the resources research, and experience needed to properly implement them would be ill advised.

M. FORSTER
Singapore General Hospital Singapore


Autologous blood transfusion

Editor,—We read with interest the commentary on the guidelines for autologous transfusion.1 In particular it states that blood containing amniotic fluid should not be salvaged because of the risk of embolism and disseminated intravascular coagulation (DIC). We agree with this. However, the risk–benefit ratio of transfusing salvaged blood that might contain small amounts of amniotic fluid has to be considered.

A 26-yr-old Jehovah's Witness developed progressive haemorrhage shock secondary to acute haemoperitoneum, 6–8 h after emergency Caesarean section under general anaesthesia for fetal distress. At Caesarean section the obstetrician had experienced moderate difficulty in separating the placenta from the uterine wall and blood loss had been estimated at 800 ml. As resuscitation commenced with oxygen, i.v. crystalloid and synthetic colloids, extensive discussion took place with the patient as to what form of transfusion (if any) would be acceptable to her. She concluded that transfusion of autologous blood was acceptable (even if it was temporarily out of circulation) but that homologous transfusion (whether red cells, platelets or fresh frozen plasma) was not.

The haemoglobin concentration before Caesarean section and after initial resuscitation decreased from 12.9 to 6.6 g dl⁻¹. Laparotomy was performed 9 h after Caesarean section at which 1400 ml of blood were salvaged from the peritoneal cavity. Blood was prepared using a red cell salvage centrifugal processor (Cell Saver 3 Plus Haemonetics) suspended in 0.9% saline solution and re-infused. Immediately after the second operation haemoglobin concentration was 5.9 g dl⁻¹ but this had decreased to 3.7 g dl⁻¹ within 12 h with further bleeding. Another 200 ml collected from the abdominal drains over this time were reinfused with an increase in haemoglobin concentration from 3.8 to 7.7 g dl⁻¹, but this was transient, decreasing to 4.0 g dl⁻¹ in 6 h.

Coagulopathy and thrombocytopenia developed before laparotomy and peaked 12 h after operation (INR 2.5, APTT ratio 2.5, platelets 53 × 10⁹ litre⁻¹). Tissue hypoxia was manifested as lactic acidosis (base deficit 8.1 mmol litre⁻¹ 12 h after operation) and oliguric renal failure (urea 16 mmol litre⁻¹, creatinine 0.31 mmol litre⁻¹, 2 days after operation).

Methods used to minimize blood loss and maximize oxygen delivery included: sedation and controlled ventilation for 36 h; central venous and direct arterial monitoring (during and after operation) to guide volume expansion; maintenance of arterial PaO₂ greater than 25 kPa; aprotinin; small infrequent blood samples; early enteral nutrition, iron, folic acid and vitamin B₁₂ supplements; and recombinant human erythropoietin (4000 u s.c once daily for 5 days).

The patient was discharged from the intensive care after 6 days. Haemoglobin concentration at a stage when oxygen delivery was critical. However, whether the reinfused red cell suspension contained a clinically significant amount of amniotic fluid is unknown. This may have been one of several factors contributing to DIC. Would it have been possible to analyse the salvaged blood–red cells for amniotic fluid both qualitatively and quantitatively before reinfusion and if so could this have aided management?

S. G. O. REES
N. O. BOHEIMER
Department of Anaesthesiology
Torbay Hospital
Devon


Propofol and electrophysiological variables during emergence from anaesthesia

Editor,—We read with interest the article by Doi and colleagues1 describing relationships between calculated concentrations of propofol and several variables. However, the original data are not described in enough detail to enable a knowledgeable reader to verify the reported results. From the article it appears that two major flaws: (i) linear relationships were computed using repeated measurements in 10 patients leading to a medley of 349 points; (ii) linear prediction bands were shown as straight lines.

The first misuse of simple linear regression was to mix repeated measures in just a few patients. When simple linear regression is used to model the data, hypotheses behind the model must be kept in mind (i.e. the independence and Gaussian distribution of the error terms). In other words pooling repeated measures which are obviously non-independent is misleading. Some techniques have been developed to cope with the pooling of independent data from several individuals with non-independent repeated measures in the same individual.2

The second misuse of statistical tools was to draw linear prediction bands which are curvilinear in essence.3 Moreover, the prediction band is usually used for prediction for an individual and the computation made by the authors based on 349 measures in 10 patients is misleading because of the non-zero covariance between the repeated measures made in each individual.

As pointed out by Shann,4 the moral of this story is that we should never use a statistical technique unless we are completely familiar with it.

C. MÉLOT
F. CANTRAINE
Department of Computer Science
School of Medicine
Free University of Brussels
Brussels, Belgium

considered to be completely useless. Even one which was 99% successful would fail to meet the unique requirements of anaesthetists for total success. The same type of reliability must be applied to a technique of measuring the effect of anaesthetics on patients during surgery. A system which could reliably detect transition from unconsciousness to consciousness successfully in 99% of patients would have the possibility of providing incorrect information in one of every 100 patients who received an anaesthetic. Even one which was unsuccessful in only one of 1000 patients would be of little use.

The prediction bands did not demonstrate confidence intervals which would have been curvilinear but were calculated from residual variance from the regression line and were presented to demonstrate the distribution of the EEG variables. Had the 95% confidence intervals been calculated before regression, they would have been curvilinear. Each figure contained data from identical patients and the essential point of the study was that a real difference was detected in the performance of the monitoring systems during the recovery phase. It may be true that Poon’s statistical method could be more valid for computing linear relationships from repeated non-independent measurements in multiple subjects. Poon’s method can reputedly detect mild non-linearities. However, if our data were subjected to Poon’s analysis the results would not be much different as experimental conditions were similar among subjects and linear regression analysis probably dealt with our pooled, non-independent measurements appropriately. Additionally, our data were Gaussian in distribution as analyses of other data from our studies showed the AEPI and BIS to be normally distributed. Therefore, inter-individual variability would have had far less effect on the analysis. In any case we believe this criticism in no way affected our conclusions about the AEPI as we found it to be the worst predictor of propofol concentration.

While we agree that a statistically puristic approach may have been to adopt the analysis suggested by our statistical colleagues, we considered that our approach would be a greater test of the systems under evaluation and may yield a higher level of confidence in the use of the different EEG variables. It is important that we do not accept a system to assess the level of anaesthesia which appears to be statistically sound if this does not provide the necessary extremely high level of clinical reliability. Figure 1 clearly demonstrated the considerable overlap in BIS values recorded during the transition from unconsciousness to consciousness. This was repeated with all variables except AEPImid which showed almost total separation between values recorded before and after awakening. The use of curvilinear 95% confidence intervals would have included a wider range of data points at the extremes, would have added no additional value to the analysis and may have been less stringent in presenting the data. The moral of this story is that statisticians may contribute less to an understanding of medical matters than they realize.

Bronchospasm during inhalation of nebulized midazolam

Editor,—Nebulized midazolam has been described previously in a pilot study of adult volunteers in which it was administered nasally.1 We would like to report a complication of nebulized midazolam administered orally via a jet nebulizer.

During the course of an investigation into the sedative effects of nebulized midazolam, two of 10 subjects complained of chest tightness. Both were healthy volunteers although one gave a history of childhood asthma. The preparation of midazolam 5 mg ml−1 i.e. (Hypnovel, Roche) was administered in a dose of 0.2 mg kg−1 via a jet nebulizer (Pari LC Plus, Pari Medical Limited). Changes in the ratio of forced expiratory volume in 1 s (FEV1) to forced vital capacity (FVC) were measured and were greater in these subjects than in the remaining volunteers (table 1). Oxygen saturation values were measured throughout the study and decreased with increasing sedation; the lowest value for the 10 volunteers was 93%, occurring in one subject who remained asymptomatic and in one who developed chest tightness.

Alternative routes of administration of anaesthetics and anaesthetic adjuvants are constantly being sought with the aim of increasing convenience, safety, bioavailability and effective drug action while decreasing side effects and cost.2 Intranasal midazolam has been used successfully for sedation of children,3 but causes nasal burning, irritation and lacrimation during instillation into the nares.4 Nebulized midazolam may offer a more comfortable route of administration but its effects on lung function have not been reported in human subjects. It is known that acidic aerosols may cause bronchoconstriction in asthmatic subjects, particularly if the aerosol is hypotonic or hypertonic,4 or in the presence of inflamed and hyper-responsive airways.5 The pH of the preparation of midazolam we used was 3.0 and it is possible that bronchoconstriction could be avoided if a buffered solution were used.

The concept of a nebulized sedative may at first seem an attractive alternative to the parenteral route, especially in children, and a nebulizer may be well tolerated, particularly by an asthmatic child. However, we would advise caution in the use of midazolam by this route, especially where a history of asthma is given, until further studies have evaluated this mode of administration.

A. S. M. McCORMICK
Department of Anaesthetics
Poole General Hospital
Poole, Dorset

V. L. THOMAS
Shackleton Department of Anaesthetics
Southampton University NHS Trust
Southampton


Table 1  Forced expiratory volume in 1 s (FEV1) as a percentage of forced vital capacity (FVC) before and 10 min after administration of nebulized midazolam in 10 volunteers

<table>
<thead>
<tr>
<th>Subject No.</th>
<th>Symptoms of chest tightness</th>
<th>FEV1/FVC (%) before nebulizer</th>
<th>FEV1/FVC (%) after nebulizer</th>
<th>Percentage change in FEV1/FVC (%)</th>
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<tbody>
<tr>
<td>1</td>
<td>No</td>
<td>81.3</td>
<td>84.4</td>
<td>3.93</td>
</tr>
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<td>2</td>
<td>No</td>
<td>93.0</td>
<td>92.9</td>
<td>−0.15</td>
</tr>
<tr>
<td>3</td>
<td>No</td>
<td>78.2</td>
<td>76.9</td>
<td>−1.61</td>
</tr>
<tr>
<td>4</td>
<td>No</td>
<td>92.3</td>
<td>92.1</td>
<td>−0.22</td>
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<tr>
<td>5</td>
<td>No</td>
<td>81.8</td>
<td>83.0</td>
<td>0.40</td>
</tr>
<tr>
<td>6</td>
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<td>7</td>
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<td>88.2</td>
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<td>−4.37</td>
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<td>9</td>
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<td>60.0</td>
<td>−14.29</td>
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<tr>
<td>10</td>
<td>Yes</td>
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<td>72.2</td>
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<tr>
<td>Mean</td>
<td></td>
<td>83.8</td>
<td>80.8</td>
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<tr>
<td>Confidence interval (%)</td>
<td></td>
<td>4.36</td>
<td>5.99</td>
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Low-dose naloxone in the treatment of urinary retention during extradural fentanyl causes excessive reversal of analgesia

Editor,—Urinary retention is a complication of the use of extradural opioids.1 The mechanism is thought to be inhibited by opioids of sacral parasympathetic outflow, although a supraspinal inhibitory effect at the level of the pontine micturition centre has been proposed.2 Ravel and colleagues demonstrated a rapid decrease in detrusor muscle function after injection of extradural morphine in volunteers.3 The effect of morphine on detrusor muscle function in this group was antagonized by administration of naloxone. Husted and colleagues also demonstrated antagonism of the effects of extradural morphine on bladder dysfunction with administration of naloxone 0.4 mg in patients after hysterectomy.4 Interestingly, the urinary effects were antagonized without reversal of analgesia. This lack of reversal of analgesia was also noted after administration of naloxone 5 µg kg⁻¹ h⁻¹ to patients receiving extradural morphine after abdominal surgery.5

Fear of delayed respiratory depression with hydrophilic opioids has led to a reduction in the use of morphine for extradural analgesia. More commonly a mixture of a lipophilic opioid (fentanyl or diamorphine) with very low concentrations of local anaesthetic is used.6 Therefore, we evaluated the use of low-dose naloxone for the treatment of the urinary retention in the thoracotomy patients receiving thoracic extradural fentanyl for postoperative analgesia. After obtaining approval from the Ethics Committee, 30 patients received an extradural bolus of 0.1 ml kg⁻¹ of a mixture of fentanyl 10 µg ml⁻¹ and 0.1% bupivacaine. This was followed by infusion at a rate of 0.1 ml kg⁻¹ h⁻¹ of the same mixture. Those patients (n = 9) who had not passed urine the morning after operation were allocated randomly to receive either naloxone infusion 5 µg kg⁻¹ h⁻¹ for 1 h or an equal volume of normal saline infused over the same period. Pain score using a 0–10 cm visual analogue scale was recorded at the start of infusion of naloxone and every 15 min thereafter. If the subsequent pain score increased by 3 cm or more above baseline, a bolus of 5 ml of the extradural mixture was given.

The incidence of urinary retention in this population was 30% (n = 9). Pain score increased by more than 3 points at 30 min after commencement of infusion (total dose of naloxone received = 0.2 mg) in all five patients in the naloxone group. In one patient, pain score increased from 1 to 10 and remained at 9 despite three 5-ml extradural bolus doses. There was no change in pain score in the four patients who received saline. None of the patients passed urine during infusion of naloxone or saline or in the subsequent hour. Based on these early results we abandoned the study.

A change in pain score as a result of extradural catheter migration is unlikely. All extradural catheters were sutured to the skin. The temporal relationship between the start of naloxone infusion and increase in pain score and termination of naloxone infusion and decrease in pain score also supports correct catheter position. Although bolus doses of naloxone have been recommended to reverse the side effects of extradural morphine, our concern over the potential for reversal of analgesia led us to choose a low-dose infusion over 1 h.7 None the less, reversal of analgesia in our patients was rapid and unpleasant. This appears to be markedly different to the published experience with extradural morphine. In support of our experience, Gueneron and colleagues noted partial reversal of extradural fentanyl analgesia with naloxone 0.2 mg i.v. followed by infusion of 5 µg kg⁻¹ h⁻¹ after lipotripsy.8

The difference between extradural morphine and fentanyl may be caused by different study populations (i.e. our study investigated thoracic rather than abdominal operations) or by the different lipid solubilities of the two drugs. Fentanyl being more lipophilic than morphine may have a greater systemic component to its analgesic effect and may thus be more susceptible to reversal by naloxone. In patients receiving thoracic extradural analgesia, we found previously no difference in pain scores 4 h after operation and thereafter between infusion of a mixture of fentanyl 10 µg ml⁻¹ and 0.1% bupivacaine and fentanyl 10 µg ml⁻¹.9 Reversal of analgesia with naloxone again supports the importance of fentanyl in thoracic extradural analgesia.

In summary, even very low doses of naloxone can reverse the analgesic effect of extradural fentanyl. Naloxone would appear to have no place in the management of non-life threatening side effects related to this analgesic technique.

L. M. BRUMLEY
Division of Anaesthesia
Department of Surgery
University College London Medical School
London

Extradural catheter-related infections in patients with infected cutaneous wounds

Editor,—Bengtsson, Nettelblad and Sjöberg described severe extradural infections in three patients who had infected cutaneous wounds in the lower limbs and suggested that extradural analgesia with catheter placement was unsafe in such patients.

Continuous extradurals for postoperative or post-trauma analgesia are common. Therefore, it is likely that a significant proportion are inserted in patients who develop superficial wound infection. Extradural infections are however relatively rare. It is surprising to have three severe cases reported from one institution. Unfortunately, studies or case reports on extradural infections do not generally cite the location or method of making up the extradural solutions.

It has been demonstrated that the environment in which solutions of drugs are made up is the most important variable affecting the microbial contamination rate. A study of contamination of syringes prepared by trained nurses using a standard aseptic technique in the usual i.v. area of a ward found a 15% contamination rate of syringes prepared by trained nurses using a standard aseptic technique in the usual i.v. area of a ward found a 15% contamination rate. There was no contamination when pharmacy personnel performed the same procedure in a clean room with full protective clothing (personal communication, unpublished data). It has been suggested that the risk of infection may be reduced by maintaining a closed system for the duration of the infusion.

Contamination of analgesic solution may be a significant causative factor in extradural infections, particularly where small volumes of analgesic solutions are repeatedly made up in ward areas. In this scenario aseptic technique may be imperfect and there is ample potential for contamination of the extradural infusions with pathogens.

In our institution more than 3000 extradurals have been performed for postoperative or post-trauma analgesia and we are yet to experience a case of extradural space infection, although inflammation at the insertion site and culture of skin organisms from catheter tips are common. The extradural solution we use consists of commercially produced sterile 500-ml bags of dilute bupivacaine to which opioid is added in a laminar flow sterile production facility in the hospital pharmacy by trained staff. The delivery system is attached in theatre recovery at the time of extradural insertion and its integrity is generally not broken until removal. Using 500-ml bags ensures that few patients require connection of a second bag. We believe that this is an optimal system for avoiding infection caused by solution contamination.

It is well established that TPN infection rates are minimized by limiting disconnection and reconnection of the delivery systems. The same considerations may apply to continuous extradural infusion. It would be unfortunate if the potentially erroneous conclusion of the case report leads to patients being deprived of extradural analgesia in this clinical setting.

B. NEWMAN
Department of Anaesthesia
Poole Hospital NHS Trust
Dorset
Correspondence

procedures were discussed. In view of the patient’s medical history and active hiatus hernia, a regional technique was preferred to general anaesthesia. However, in view of the microbiological problems, insertion of an indwelling extradural catheter left for a period of days was discounted because of the probably low but real risk of extradural abscess formation. Also, over a 12-day period it was likely that at least two extradurals would have to be sited.

Thus intrathecal anaesthesia was the option chosen as the patient was afebrile and receiving antibiotics. The patient had 12 further intrathecal anaesthetics performed over a 19-day period, 10 performed on consecutive days. The patient suffered no post-puncture headaches, backache or other neurological sequelae. CSF was obtained at the last dural puncture which had a leucocyte count of 1 and was negative for bacteria. She was happy with her management, although expressed that she would have preferred an extradural technique for pain relief after operation.

Repeated diagnostic lumbar punctures have been described in paediatric cases suspected of meningitis, but a patient having so many intrathecal anaesthetics over such a short time period has not. It may be an option in those patients in whom the risk of extradural abscess formation with an indwelling catheter is increased.

J. A. Cooney
Department of Anaesthesia
Royal Perth Hospital
Perth, WA, Australia