Malignant hyperpyrexic syndrome

Editor,—The letter to the Lancet by Denborough and Lovell in 1960 and the report of Denborough and colleagues in 1962 are, as pointed out in the commentary by Ellis, landmarks for the recognition of malignant hyperthermia (MH) in medical anaesthesia. However, it may be of interest that the syndrome in pigs was, in fact, first described by Hall and colleagues and subsequently investigated by the same group before Harrison’s publication in 1975. Gainsford Harrison visited Cambridge to discuss these investigations and it became obvious that there was a significant difference between the pigs he used (Landrace breed) and the LandraceX pigs in the UK. For example, in South Africa, the experimental condition could be set up in the pure-bred Landrace pigs after induction of general anaesthesia with thiopental whereas in the LandraceX pigs, thiopental appeared to protect from the induction of the syndrome. This led to difficulties in the detailed study of the syndrome in the UK, but indicated that genetic makeup might be implicated in its manifestation. Unfortunately, lack of funding then prevented further work in the UK.

It is greatly to Harrison’s credit that he recognized the potential of dantrolene in the prevention and treatment of MH in both pigs and humans but it is perhaps a sad reflexion of our times that it took more than 5 yr before the place of this drug came to be appreciated in medical anaesthesia. Could this have been related to the cost of the drug and its relatively short shelf-life coupled with the failure of those on UK grant-awarding bodies to recognize the need to investigate the potential of this drug in saving lives?

L. W. Hall
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2 Hall LW, Woolf N, Bradley JWP, Jolly DW. Unusual reaction to suxamethonium chloride. BMJ 1966; ii: 1305

Editor,—I’m pleased Dr Leslie Hall has put on record his own important contributions to our knowledge of malignant hyperthermia (MH), especially in pigs. His description of succinylcholine-induced MH in 1966 was of course a landmark in this field, although difficult to interpret in the light of later findings. He points out the discrepancies in the pig research and this indeed highlights that there are important discrepancies between the pig syndrome and MH in humans.

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Early clinical experience with a newly formulated hydroxyethyl starch—Hextend

Editor,—We would like to report our early experience with a newly formulated hydroxyethyl starch (HES), Hextend (BioTime Inc, Berkeley, CA, USA). Currently, clinicians are re-evaluating their blood transfusion policies with both economic and safety issues driving the reduction in use of donated human blood products. Increasing surgical demands have increased the volumes of artificial colloids to which patients are exposed. New concerns about the uses of human albumin solution intensify the search for a ‘colloid of choice’.

Hextend is a medium molecular weight HES (450 kDa) formulated with mixed electrolytes (sodium 141 mmol litre⁻¹, chloride 124 mmol litre⁻¹, potassium 3.0 mmol litre⁻¹, calcium 2.55 mmol litre⁻¹, magnesium 0.9 mmol litre⁻¹), lactate and glucose to preserve a more ‘plasma-like’ physiological milieu than in other comparable artificial colloids. In previously presented HES solutions, the suspending medium was 0.9% saline solution with chloride 154 mmol litre⁻¹. It is becoming apparent that our ability to tolerate a high chloride load is imperfect and that, particularly in the presence of renal compromise, such a load can lead to the development of acid–base disturbance.

In an open label study, Hextend was administered for the first time to humans to evaluate its efficacy and safety. Five patients have so far received Hextend 500–3150 ml, administered according to a treatment algorithm. It proved to be an effective volume expander achieving the algorithm goals which triggered infusion. Changes in measured electrolytes outside normal limits were not seen in any patient. We performed detailed in vitro coagulation studies and observed predictable changes in factor VIII moieties from previous experience with HES of molecular weight 450 kDa. There were no idiosyncratic effects observed.

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The HES component of Hextend represents a return to higher molecular weight moieties. Lower molecular weight HES have been reported to have less effect on the coagulation system, particularly factor VIII associated proteins, although the clinical significance of this in terms of bleeding is vague.\(^1\) The prolonged intravascular persistence of a higher molecular weight HES may offer advantages in isovolaemic haemodilution which outweigh its poorly defined effect on blood coagulation in this type of application.

We suggest that this agent may be a useful adjuvant to volume therapy and that further evaluation may identify the importance of physiological balance in large volume transfusion of artificial colloid.

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Pre-emptive analgesia—why the evidence is conflicting

Editor,—We read with interest the article by Millar, Mansfield and Kinsella\(^1\) and would like to raise some points for discussion. They have given several reasons for failure to demonstrate pre-emptive analgesia in various studies. Another potential reason may be the inclusion of nitrous oxide in the design of all clinical studies. The analgesic effect of nitrous oxide is probably a result of involvement of the proenkephalin derived family of endogenous opioids.\(^2\) This finding is further supported by the fact that naloxone antagonizes the analgesic effect of nitrous oxide.\(^3\)

Nitrous oxide has been shown to produce pre-emptive analgesia in rats.\(^4\) Interestingly, in the same study, it was reported that halothane antagonized the analgesic effect of nitrous oxide. However, another animal study\(^5\) showed inhibition of nociception-induced spinal sensitization by most anaesthetic agents, except halothane. Whether a similar effect occurs in humans is not known.

It is not inconceivable that the use of 66% nitrous oxide has a pre-emptive effect which is not greatly improved by additional opioids in the doses mentioned. Any differences between study groups may be too small to detect in a small number of patients.

We agree with the authors\(^1\) that a clinically useful application of pre-emptive analgesia with morphine may not be possible because of the need for the return of spontaneous respiration at the end of surgery. Whether use of drugs such as remifentanil, which would allow a greater dose and antinociceptive effect during operation, will demonstrate pre-emptive analgesia remains to be seen.

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5. O’Connor TC, Abram SE. Inhibition of noiception induced spinal sensitisation by anaesthetic agents. Anesthesiology 1995; 82: 259–66
Cisatracurium in a patient with atypical plasma cholinesterase

Editor,—Cisatracurium is one of the 10 stereoisomers that constitute atracurium. As with atracurium, cisatracurium undergoes spontaneous non-organ-dependent Hofmann degradation at physiological pH and temperature. Unlike atracurium, non-specific ester hydrolysis does not appear to play a significant role in the degradation of cisatracurium1–5; 80% of a dose of cisatracurium is cleared by non-organ-dependent Hofmann elimination. With both drugs, the liver and kidneys play only a minor role in elimination.1–5

The non-specific ester hydrolysis of atracurium is independent of plasma cholinesterase activity. In vitro experiments have shown that the rates of breakdown of atracurium are not different when the drug is incubated in normal plasma or in plasma obtained from genetically deficient homozygous patients with virtually no plasma cholinesterase activity. This finding has been confirmed clinically; we have shown, using electromyography, a normal response to atracurium in a homozygous patient with atypical cholinesterase activity.8 The following case report investigates the neuromuscular blocking effect of cisatracurium in a patient with atypical plasma cholinesterase activity.

A 57-yr-old male, weighing 100 kg, was scheduled for nasaloplasty. During previous surgery, he received succinylcholine 75 mg for tracheal intubation, which was followed by prolonged neuromuscular block lasting 7 h. Because of this history, plasma cholinesterase concentrations and dibucaine number were estimated before the present operation. Plasma cholinesterase activity was 2.3 units (normal value 2.5–4.5 units) and dibucaine number was 35, suggesting homozygote atypical plasma cholinesterase activity. The patient was premedicated with glycopyrrolate 0.2 mg i.m. and diazepam 5 mg orally. Anaesthesia was induced with propofol 2 mg kg\(^{-1}\), lidocaine 1 mg kg\(^{-1}\) and fentanyl 1.5 µg kg\(^{-1}\). Neuromuscular transmission was monitored using a Datex electromyograph. The ulnar nerve was stimulated supramaximally at the wrist every 20 s using the train-of-four (TOF) twitch technique at a stimulus rate of 2 Hz. A bolus dose of cisatracurium 0.15 mg kg\(^{-1}\) (3 × ED\(_{50}\)) was then given. Complete neuromuscular block was achieved after 120 s, when tracheal intubation was performed easily. Anaesthesia was maintained with 1% isoflurane and 66% nitrous oxide in oxygen. After 20 min, 25% recovery of the first twitch of the TOF was observed, and a maintenance dose of cisatracurium 0.05 mg kg\(^{-1}\) was given. At the end of surgery, which lasted 60 min, 50% recovery of the first twitch of the TOF was observed. Residual neuromuscular block was antagonized successfully using neostigmine 0.05 mg kg\(^{-1}\) and glycopyrrolate 0.01 mg kg\(^{-1}\). The twitch response was restored completely, and tetanus was maintained with no post-tetanic facilitation.

This report confirms that cisatracurium, in common with atracurium,8 can be used safely in patients with atypical plasma cholinesterase.

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4 Eastwood NB, Boyd AH, Parker MA, Hunter JM. Pharmacokinetics of 1R-cis 1’R-cis atracurium besylate (51W89) and plasma laudanosine concentrations in health and chronic renal failure. Br J Anaesth 1995; 74: 400–4

Epidural anaesthesia for Caesarean section in an achondroplastic dwarf

Editor,—We were interested to read the case report by Morrow and Black1 describing the anaesthetic management of an achondroplastic dwarf undergoing Caesarean section. As the authors stated, such patients present a significant challenge for the anaesthetist, and yet there is little information in the literature to guide effective management.

Most of the published work consists of case reports and, in common with the present account, relates to anaesthesia for elective Caesarean section where time is available to

Correspondences

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Most of the published work consists of case reports and, in common with the present account, relates to anaesthesia for elective Caesarean section where time is available to
performed central neural block using an epidural or microspinal catheter.

Having recently found ourselves in a similar situation, we would be interested in how the authors might have managed their case for an emergency Caesarean section, when time constraints may preclude the use of an incremental technique, and yet the patient is keen to be awake for delivery.

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Editor,—We appreciate the interest in our article shown by Drs Ravenscroft and Rout and are grateful for the opportunity to reply.

There are few guidelines on the use of epidural or spinal anaesthesia in the gravid achondroplastic dwarf; most of the available data are derived from a small number of case reports. In one such letter, Crawford and Dutton reported that infusion of 0.5% hyperbaric bupivacaine 0.5 ml intrathecally resulted in bilateral block to T6 within 20 min, and that the block was associated with significant hypotension. The intra-abdominal position of the uterus in patients with achondroplasia may result in particularly severe aorto-caval compression. When this problem is compounded by the sudden onset of central neural block, there is the possibility that catastrophic hypotension may occur. In a series of patients of normal stature undergoing elective Caesarean section, Robson and colleagues demonstrated that a marked decrease in cardiac output occurs after spinal anaesthesia, but that such a decrease is not associated with epidural anaesthesia or an incremental spinal technique. It is debatable whether this work is applicable to patients with achondroplasia. However, in the absence of data indicating otherwise, it would seem reasonable to assume that a similar, if not greater, reduction in cardiac output may result when rapid onset of spinal block occurs in this subgroup of patients.

We considered initially the use of a ‘single-shot’ spinal technique but decided against this approach when it became apparent that the combination of factors outlined above made it potentially hazardous, particularly if general anaesthesia was required in the event of a failed spinal block. Before siting the epidural, we decided that in the event of accidental dural puncture, the operator would pass the epidural catheter into the subarachnoid space and we would perform the procedure under incremental spinal anaesthesia. Faced with a similar situation to that described by Drs Ravenscroft and Rout, our choice would be to perform an elective dural puncture using an 18-gauge Tuohy needle, pass the catheter intrathecally and establish spinal block using a small dose of 0.5% hyperbaric bupivacaine. An initial dose of 0.3 ml, followed by 0.1 ml increments as required to achieve surgical block would be our approach. Invasive arterial pressure monitoring would be advisable. We feel that the use of intrathecal opioids should be treated with extreme caution in this situation, in view of the potential for unpredictable spread of the injectate within the cerebrospinal fluid in achondroplastic patients.

The relatively small time delay involved in the use of this approach would, in our opinion, outweigh the potential risks of severe hypotension and unpredictable block height (with the consequent need for general anaesthesia) that could result from the use of a ‘single-shot’ spinal injection. We believe that the use of an incremental technique should not necessarily be ruled out in the emergency situation. The amount of time spent establishing an incremental surgical block should account for a relatively small proportion of the overall anaesthetic time spent in patient preparation and accessing the subarachnoid space, and would add to the overall safety of the procedure. Obviously, the use of such an approach would result in a higher risk of post-dural puncture headache and the use of standard guidelines aimed at minimizing its occurrence should be adopted. Should the need arise, epidural blood patching may be attempted at a later date under less stressful conditions.

Adequate preparation time for any form of central neural block is fundamental to the successful outcome of the procedure, and the role of good communication between all members of the team involved in the perinatal care of this group of patients is of the utmost importance.

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3 Brimacombe JR, Caunt JA. Anaesthesia in a gravid achondroplastic dwarf. Anaesthesia 1990; 45: 132–4
4 Cohen SE. Anaesthesia for Cesarean section in achondroplastic dwarfs. Anesthesiology 1998; 81: 54–9
Inadvertent inhalation anaesthesia during surgery under retrobulbar eye block

Editor,—I read with concern Dr Smith’s case report on inadvertent inhalation anaesthesia during surgery under retrobulbar eye block. It is axiomatic that equipment should not be used without first being checked. It is clear that the anaesthetic machine used in the case described had not been checked before use. It is just as clear that this arose from its being used by someone other than an anaesthetist. This must be identified as the sole cause of the near fatal accident.

We do not allow anyone to drive or fly without a licence. We should not allow anyone to use an anaesthetic machine except an anaesthetist. A wall mounted oxygen flow meter, such as is used routinely in recovery and ICU, should be the sole method by which non-anaesthetists are allowed to administer oxygen in theatre. I would not even allow its mounting on the anaesthetic machine, as shown in Dr Smith’s photograph. There should be no exceptions.

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Reduce dose of NSAIDs in the elderly

Editor,—I read with interest the article by Kostamovaara and colleagues comparing ketorolac, diclofenac and ketoprofen for pain relief after total hip surgery. The authors’ use of the ‘recommended maximal daily doses provided by the manufacturers’, even though the majority of patients recruited were elderly (age range 54–81 yr) and some had severe disease (ASA III), warrants comment.

The authors stated that ‘short-term NSAIDs for surgical patients does not seem to induce irreversible renal failure’. However, NSAIDs are implicated in up to 15.6% of drug-induced reports of acute renal failure (ARF), and although most patients recover with appropriate management, the relatively rare complications of papillary necrosis and interstitial nephritis can be irreversible. NSAID-induced ARF is often pre-renal. Known risk factors include old age, volume depletion from diuretics and other causes such as postoperative bleeding, congestive heart failure, diabetes mellitus and renal impairment; risk is also dose-dependent. For these reasons, the manufacturers of all three drugs recommend a reduction in dose when these drugs are used in the elderly. For instance, the maximum recommended dose of ketorolac is 60 mg in 24 h, which is half the dose used in this study. Indeed, the incidence of serious and fatal adverse events reported with ketorolac has decreased since revision of dosage guidelines. Finally, recent guidelines on the use of NSAIDs in the perioperative period advise that they should be used with caution in these conditions and that renal function should be monitored regularly in patients after major surgery.

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observation of analgesia and vital functions, including haemodynamic variables and urinary excretion. We have since substantially reduced the dose of NSAID we use in elderly people to those given in the Royal College of Anaesthetists’ guidelines.

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Epidural analgesia during labour

Editor,—While reading the article by James and colleagues, 1 comparing 0.25% bupivacaine with 0.1% bupivacaine–0.0002% fentanyl for epidural analgesia during labour, it would seem in their enthusiasm for the lower concentration of local anaesthetic mixture, they have overlooked the importance of some of the more negative results of the study.

They stated that a total of seven patients were excluded from the study because of failure to maintain analgesic requirements. Five of these were from the mixed bupivacaine and fentanyl group (12.5%); only two were from the plain bupivacaine group (5%). All patients needed bolus doses of 0.5% bupivacaine to obtain satisfactory analgesia, and had deliveries complicated by intervention.

Recent emphasis has been placed increasingly on using less local anaesthetic, in combination with an opioid, to produce good or better quality analgesia for the pain of labour. But we must be cautious that we do not ignore the times when the lower doses do not work, and make plans in advance for that inevit able and relatively common event. Almost all articles on the subject show a degree of failure which may be as high as 10–15%, 2,3 although this is rarely emphasized.

In this study, where the assessment of patient satisfaction was one of the primary goals, it would seem crucial that failures in either technique should be included in the final analysis. Otherwise, we risk advocating analgesic regimens that have an increasing need for the provision of bolus doses of high concentrations of local anaesthetics for increasingly common episodes of breakthrough pain. This would have the potential to re-introduce all the problems and risks that we are striving to avoid.

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Editor,—We read with interest the paper by James and colleagues. 1 Ever since the popularization of the mobile epidural by the Queen Charlotte’s group, 2 it has been anticipated that the lack of motor block would lead to a lower rate of instrumental deliveries. Up to now this has been hard to demonstrate. Our own study, 3 which was very similar in design to that of James and colleagues, 1 found no difference, nor did Russell and Reynolds 4 or Nageotte and colleagues 5 in the USA. Before accepting the conclusion that minimal motor block ‘may influence the progress of labour . . . and incidence of instrumental delivery’ we would raise several points.

First, obstetricians are becoming increasingly reluctant to perform mid-cavity or rotational forceps and tend to perform Caesarean section instead. The important outcome is therefore the intervention rate (Caesarean section or forceps) or conversely, the spontaneous vaginal delivery rate (SVD). Second, withdrawing patients from a study such as this may be confounding. In this study, seven women were withdrawn because they requested more than two top-ups within 1 h. In effect, this is selecting out those with particularly painful labours and it is no surprise that six went on to Caesarean section and one to forceps delivery.

It would have been better to analyse the data on an intention-to-treat basis, in which case Table 3 would read:

<table>
<thead>
<tr>
<th></th>
<th>Group A</th>
<th>Group B</th>
</tr>
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<tbody>
<tr>
<td></td>
<td>(n = 40)</td>
<td>(n = 40)</td>
</tr>
<tr>
<td>SVD</td>
<td>27</td>
<td>22</td>
</tr>
<tr>
<td>Instrumental</td>
<td>2</td>
<td>10</td>
</tr>
<tr>
<td>Caesarean section</td>
<td>11</td>
<td>8</td>
</tr>
</tbody>
</table>

In deriving a P value of 0.03 for the instrumental delivery group, the authors appeared to have compared that group with SVD and Caesarean section combined. Even if this is statistically valid it makes no sense clinically. We would suggest that a better way to analyse their data is to compare SVD with interventions (Caesarean section plus instrumental). When analysing all data on an intention-to-treat basis, and hypothesizing that group B will have a higher intervention rate, Fisher’s exact test (one-tailed) yields

\[ P = 0.1794, \]

The studies quoted 3–5 all compared a low dose ‘mobile’ epidural with a standard one in a randomized manner and studied 197, 399 and 761 women, respectively. James and colleagues studied only 80 women. While we would con-
maternal satisfaction is not open to meaningful inter-
measurement by visual analogue scales when assessing
the dimension of the whole process is misleading and erroneous.
Experience of labour is enhanced when assessing only one
aspect of the overall process.

D. N. Lucas
D. J. A. Vaughan

Dr Aveling and colleagues expressed concern that we are
mistakenly implying that our technique reduces the
instrumental delivery rate. This is an altogether different statement; indeed, we could have also
selectedly quoted the literature as they have done, to
support our conclusions. We should like to point out that
our procedure was quite rigid and women were removed
from the study if they requested more than two epidural
top-ups within a 1-h period, even if they initially had
reported pain-free contractions. When these women were
removed from the study they were treated quite differently
from the remaining study patients and therefore it makes
sense not to include them in the final analyses.

Our original aim in this study was to compare
the analgesic efficacy of the two epidural regimens and not the
rate of obstetric interventions. We chose to study 80 patients
because our power calculation predicted that we needed 70
patients to have 90% power of detecting a difference of
10 mm in VAS scores for pain during labour. The reduction
in instrumental delivery rate was an incidental finding which
we thought merited mentioning. If we had undertaken a
much larger study it would have reduced our ability to
support our conclusions. We should like to point out that
our procedure was quite rigid and women were removed
from the study if they requested more than two epidural
top-ups within a 1-h period, even if they initially had
reported pain-free contractions. When these women were
removed from the study they were treated quite differently
from the remaining study patients and therefore it makes
sense not to include them in the final analyses.

Finally, can we assure Nickells and colleagues that
maternal arterial pressure and CTG traces were monitored
throughout labour and there was no difference in the
incidence of maternal hypotension or CTG abnormalities,
despite using a 15-ml initial bolus dose. It was necessary to use 15 ml in both arms of the study to facilitate blinding of the operator. We believe that 15 ml of 0.25% bupivacaine is not an excessive dose and that up to 30% of women require this quantity to establish good analgesia. We would also argue that the results of our study demonstrated that 15 ml of a low-dose solution is equipotent to 0.25% bupivacaine 15 ml as all women in the study reported pain-free contractions by 30 min.

The primary aim of our study was not to compare maternal satisfaction between the two groups; this was a secondary issue and although there are undoubtedly limitations when interpreting VAS, it is still a commonly used technique. If the end-point had been maternal satisfaction, then more detailed analysis would have been appropriate.

Again, we would like to state that our reasons for setting up this study were to demonstrate if it was possible to both establish and maintain adequate epidural analgesia using low concentrations of bupivacaine and fentanyl.

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Epidural bupivacaine and morphine on stump sensation in lower limb amputees

Editor,—I was surprised to read the strongly negative conclusion of the recent article on the effect of preoperative extradural bupivacaine and morphine on stump sensation in lower limb amputees.1 The same group, in the same extradural bupivacaine and morphine on stump sensation conclusion of the recent article on the effect of preoperative epidural analgesia on stump sensation was studied on a mean number of 19 patients for each modality (range 15–28 patients) with correspondingly small numbers in both the block and control groups.

Patient characteristics before operation may also have affected the outcome in both studies. A larger number of patients in the block than in the control group had previous contralateral amputations (seven vs three), a greater median pain score on visual analogue scale (51 mm vs 44 mm) and greater median opioid consumption on admission (50 vs 30 mg morphine/day). This raises the possibility that the block group had greater preoperative central neural sensitization than the control group, which could influence the degree of postoperative pain.

Previous studies have suggested a reduction in post-operative phantom limb pain from pre-emptive epidural analgesia.2–7 Although these studies can be criticized for their design and small patient numbers, the study by Nikolajsen, Ilkjaer and Jensen did not provide conclusive evidence against the use of preoperative epidural block in the prevention of phantom limb pain.

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1 Vertommen JD, et al. The effects of the addition of sufentanil to 0.125% bupivacaine on the quality of analgesia during labour and on the incidence of instrumental deliveries. Anesthesiology 1991; 74: 809–14

6 Bach S, Noreng MF, Tjellden NU. Phantom limb pain in amputees during the first 12 months following limb amputation, after preoperative lumbar epidural blockade. Pain 1988; 33: 297–301

Editor,—Dr Skelton’s main concern seems to be the statistical power of our studies.1,2 Sixty patients were included in the study which examined the effect of preoperative epidural block on phantom pain.1 This number was based on an estimate that a sample size of 27 patients per group would be required (type 1 error rate 0.05; type 2 error rate 0.2; power = 0.8). After 1 week, 54 patients
were followed-up: 14 (52%) patients in the epidural block group and 15 (56%) patients in the control group had phantom pain. Mainly because of deaths, a smaller number of patients were followed-up in the later interviews (37, 36 and 28 patients after 3, 6 and 12 months, respectively). At all later interviews, the incidence of phantom pain was slightly higher, although not significantly, in the block group compared with the control group.

Dr Skelton quotes four studies which have suggested a reduction in phantom pain from preoperative epidural analgesia.3–6 Katz3 presented no original new data in his commentary but only a short review of previous studies on the subject. In a letter to the editor, Schug and colleagues4 presented data based on 23 patients. The incidence of phantom pain was lower among eight patients who received epidural analgesia before, during and after operation compared with eight patients who received systemic analgesia. Jahangiri and colleagues5 followed prospectively 24 patients who received epidural analgesia before, during and after amputation (n = 14) or conventional analgesia (n = 11). The incidence of phantom pain was reduced in the epidural treatment group. Bach, Noreng, TjellVden6 carried out the only randomized study of 25 patients. None of 11 patients who had received epidural bupivacaine and morphine for 3 days before amputation had phantom pain after 6 months, whereas five patients in the control group had pain. Calculation of statistical power was not presented in any of these studies.

We realize that the number of patients was small in our study on the effect of preoperative epidural analgesia on stump sensation.7 Previous studies which claimed an effect of pre-emptive treatment on hyperalgasia and allodynia after surgery examined 20–27 patients.7–9

We cannot exclude the fact that patient characteristics before operation may have influenced the outcome in both studies. However, patients in the block group were treated to freedom from pain during the epidural treatment period, and after amputation the consumption of opioids was similar in both groups.

Our studies1 2 do not present conclusive evidence against the use of preoperative epidural block in the prevention of post-amputation pain, hyperalgasia and allodynia. We cannot exclude the possibility that preoperative epidural block for a longer period (1–2 weeks) would prevent these phenomena from developing, but this is not realistic.

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6 Bach S, Noreng MF, TjellVden NU. Phantom limb pain in amputees during the first 12 months following limb amputation, after preoperative lumbar epidural blockade. Pain 1998; 33: 297–301

Postoperative cognitive deficit in the elderly surgical patient

Editor,—We read with interest the review article on postoperative cognitive deficit in the elderly surgical patient.1 We agree with the authors that cognitive dysfunction after surgery and anaesthesia is common and persistent, and should be considered seriously.

Among other risk factors, cardiopulmonary bypass (CPB) has been blamed for these neurological and neuropsychological deficits. The incidence of neurological abnormalities is quoted to be as high as 61% in the early postoperative period and 17% at the time of discharge from hospital. The incidence of neuropsychological deficits is even higher: 79% in the postoperative period and 38% at the time of discharge.1 The incidence of frank stroke is reported to be as high as 4.8–5.2% overall and in patients who are more than 75 yr of age it approaches 9%.2

In recent years, nuclear medicine has made tremendous advances in studying physiological changes in the brain. Positron emission tomography (PET) and single photon emission computerized tomography (SPECT) provide information on cerebral metabolism and regional cerebral blood flow, respectively. These techniques have been used successfully in studying pathophysiology in patients with neurological and psychiatric illnesses such as Alzheimer’s disease, dementia, Parkinson’s disease and stroke.3 They are also being used in oncology for diagnosis, determining prognosis and guiding treatment. More recently, PET and SPECT scanning have been found to be useful in studying the effects of CPB on cerebral function in patients undergoing cardiac surgery.4 It would be interesting to see if it
is possible to demonstrate any changes in cerebral metabolism and regional cerebral blood flow in CPB patients and to see how these variables are related to neuropsychological function.

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