Subarachnoid spread of isobaric tetracaine in adolescents

Sir,—In a recent report, Harabayashi and colleagues [1] showed that subarachnoid administration of hyperbaric amethocaine (tetracaine) produced an unexpectedly higher level of analgesia in adolescents than in adults. The authors postulated that when adolescents lie in the supine horizontal position, the thoracic kyphotic curvature decreases because of the flexibility of their vertebral columns which become straighter at the upper part of the thoracic vertebrae.

In our recent investigation [2], we studied 50 patients (17 adolescents, aged 13–16 yr and 33 adult patients aged 17–68 yr) who received isobaric tetracaine 10 mg in 2 ml. The anaesthetic solution was prepared from 1.0 % tetracaine solution with CSF. As in the study of Hirabayashi and colleagues, the drug was administered with the patient in the lateral position, at the L3–4 interspace, at a speed of 0.2 ml s⁻¹, with the patient remaining in the supine horizontal position during the study. The extent of analgesia was assessed by loss of sensation to pinprick, and motor block according to the Bromage scale at 2-min intervals for 20 min and then every 15–20 min until full recovery.

There were no differences in physical characteristics between the adolescent and adult groups; the median peak/highest sensory levels were T7 (range T11–3) and T7 (T10–3), respectively. This difference was not statistically significant (P > 0.5).

We also used 2 ml of anaesthetic solution, as did Hirabayashi and colleagues but the tetracaine dose that we used was 2 mg greater. When a patient is in the supine horizontal position, a hyperbaric solution spreads cephalad because of gravity and is therefore affected by the anatomical configuration and flexibility of the vertebral column. Adolescent patients have greater flexibility and thus a flatter thoracic curvature, which leads to a higher anaesthetic level. As gravity has little, if any, effect on the spread of isobaric solutions, the peak/highest anaesthetic levels in our study did not differ significantly. The difference in anaesthetic levels between adolescent and adult patients observed by Harabayashi and co-workers did not occur in our patients with isobaric tetracaine. A review of the literature has shown some correlation between patient age and subarachnoid spread of isobaric anaesthetic solution [3, 4]. However, these studies did not compare adolescents with adults, but young (<30 yr) with elderly adults (>70 yr). In addition, they were performed with 0.5 % plain bupivacaine solution which is slightly hypobaric at body temperature [5] and the results were poorly predictable [4, 5].

Although not a formal study, our data support the results of Harabayashi and colleagues who concluded that the higher anaesthesia level in adolescent patients with hyperbaric spinal anaesthesia was probably caused by a flatter thoracic curvature. This problem does not arise by an isobaric anaesthetic solution which may therefore be preferable in adolescent patients.

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Sir,—We agree with the conclusion of Drs King and Wooten that the difference in the extent of spinal anaesthesia with hyperbaric tetracaine between adolescent and adult patients [1] does not occur when isobaric tetracaine is used [2]. In addition to hyperbaric tetracaine, we have previously studied the effect of age on spread of spinal anaesthesia with 0.5 % plain bupivacaine 3 ml in 185 patients aged 12–94 yr [3]. The maximum spread of analgesia did not differ between adolescents (median T8 (range T3.5–T12)) and adults (T8 (T3–L1)). The overall range of analgesia was very wide at all ages (fig. 1). The unpredictability of isobaric (or slightly hypobaric at body temperature) anaesthetic solution is of clinical importance. We therefore believe that this anaesthetic solution produces reliable anaesthesia for perineal and lower limb surgery.

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Gravity has no effect on the spread of isobaric solutions, while hyperbaric solutions do spread under the influence of gravity [4]. As isobaric anaesthetic solutions injected into the midlumbar subarachnoid space may be expected to stay in the vicinity of the puncture site, the difference in curvature of the thoracic hollow between adolescents and adults does not affect the cephalad spread of isobaric solutions. On the other hand, with respect to hyperbaric anaesthetic solutions, when a patient is turned to the supine position after midlumbar subarachnoid injection, the injectate splits into a part migrating cephalad and a part spreading caudal under the influence of gravity. The solution migrating cephalad can be expected to pool in the thoracic hollow. Accentuation or elimination of thoracic kyphosis affects the distribution of this pool [5]. A flatter thoracic hollow in adolescent patients, the lowest point of which is located in the higher thoracic region, may allow the drug to spread more easily to higher thoracic segments. In addition, the reduced incline in the upper thoracic spinal canal may also enhance cephalad spread.

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Figure 1 Analgesic spread of subarachnoid injection of 0.5 % plain bupivacaine 3 ml with age (from [3] with permission).


### Aprotinin therapy

Sir,—We were interested to read the editorial by Royston [1] in which he considered the evidence for a thrombotic potential for aprotinin in the light of our report of serious arterial thrombosis in a patient participating in a prospective study of aprotinin in knee replacement surgery [2]. As we stated in our report, we think it is unlikely that aprotinin was the cause of the arterial occlusion: the patient underwent technically difficult surgery and the extent of pre-existing arterial disease had been masked by severe limitation of the extra-articular joint. Nonetheless, the possibility remains that the drug may have contributed and if the complication was serious, even a small risk becomes significant.

The author suggests that comprehensive prediction of the clinical effect of aprotinin can be made on the basis of published laboratory and clinical studies to date. Whatever the mechanism of haemostatic action in surgical patients may be, it is clear that under some conditions aprotinin favours haemostasis over fibrinolysis. The risk of a thrombotic effect in otherwise heavily predisposed patients therefore cannot be easily discounted. While the existing data documenting the lack of thrombogenic potential is encouraging, evidence for the role of aprotinin under tourniquet conditions is lacking and can only be inferred. In view of the serious nature of this complication, we consider it appropriate to wait for additional safety and efficacy data for aprotinin in surgical haemostasis. However, we are hopeful that additional data will allow us to consider resuming this study.

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### Comparison of ropivacaine and bupivacaine for extradural analgesia

Sir,—I read with interest the article on ropivacaine and bupivacaine for extradural analgesia for the relief of pain in labour [1]. The use of adrenaline in a test dose produces a transient tachycardia if i.v. injection occurs, but requires additional monitoring (ECG, pulse oximetry) to detect any transient change in heart rate. Adrenaline may also affect motor block and was thus undesirable in this study. Use of air injected down the extradural catheter and precordial Doppler recording of heart sounds is controversial and is not part of our routine practice.

The total loading dose of 50 mg of ropivacaine or bupivacaine was not considered excessive and the top-up dose of 25 mg was given only at maternal request, whenever contractions became painful again.

Hypotension occurred in six patients in each group. Mild hypotension was defined as a decrease of 20% from baseline systolic arterial pressure, moderate 20–30% decrease and severe > 40% decrease. In the eight patients classified as having moderate or severe hypotension, the majority were associated with a high level of block. Concomitant nausea occurred in 50% of these patients. In none of the six patients reported was bradycardia associated with maternal hypotension. In three patients bradycardia occurred at the beginning of the second stage of labour.


Animal toxins: Scorpaenidae and stingrays

Sir,—I read with interest the review article on animal toxins by Karalliedde [1] and was surprised to find no mention of the venomous bony fishes of the family Scorpaenidae, which are considered the most dangerous in both the number and severity of injuries produced [2], or of the various genera of stingrays. Scorpaenids envenomate victims by erecting spines on their dorsal, anal and pelvic fins that pierce the victim’s flesh and release a heat labile protein that differs in potency amongst genera [2]. Species familiar to the tropical or temperate scuba diver include the beautiful and majestic lionfish (genus Pterois, commonly seen in aquaria), the masterfully camouflaged scorpionfish (genus Scorpaena) and the deadly innocence of the aptly named stonefish (genus Synanceja) indigenous to the Indo-Pacific region only and responsible for several fatalities [3]. Envenomation produces intense local pain, swelling, paraesthesiae and discoulouration that may lead to tissue necrosis and subsequent sloughing. Systemic symptoms may be mild (syncope, weakness, nausea, vomiting, sweating, fasciculations, abdominal or chest pain) or may culminate in bradycardia, hypotension, shock, respiratory distress, pulmonary oedema and death, which usually results from stonefish envenomation [4].

Treatment of the affected limb by immersion in hot water up to the highest temperature tolerated for at least 1 h inactivates the venom and relieves pain. This effective and simple treatment is now well known to lifeguards in the United States, and is responsible for the decrease in the number of cases presenting to hospital emergency departments. Irrigation of the wound, tetanus prophylaxis and reassurance is usually sufficient in mild cases [4]. Further treatment with vasopressors and i.v. fluids for circulatory support may be required in severe cases of envenomation when the use of stonefish antivenin (Commonwealth Serum Laboratories of Australia) is recommended [4].

Envenomation by weever fishes (family Trachinidae) which are found off the coast of the United Kingdom cause excruciating pain and a clinical picture resembling Scorpaenidae envenomation.

The management, too, is the same [4].

While one can indulge in the amazing experience of stroking, petting, feeding and frolicking with the tame, giant stingrays of the Caribbean island of Grand Cayman in shallow waters, it is important to realize that stingrays are a common cause of morbidity in bathers, snorkellers and divers. There are approximately 750 stingray envenomations reported off the US coast every year [5]. As they commonly lie partly hidden on sandy bottoms, bathers inadvertently step on them causing the tail with its venomous spine and backward-pointing serrations to lash forwards and puncture the skin, envenoming tissues and causing laceration. The wound usually needs irrigation, surgical exploration and support may be required in severe cases of envenomation when the use of stonefish antivenin (Commonwealth Serum Laboratories of Australia) is recommended [4].

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mended by the Association of Anaesthetists of Great Britain and Ireland [1]. Its low utilization in anaesthetic rooms reported by NCEPOD [2] undoubtedly reflects over-reliance on clinical signs to detect oesophageal tube placement and the misconception that capnography is not necessary at induction. We also disagree with Dr Calder that false positive tests are fail safe as the unnecessary removal of a correctly placed tracheal tube after difficult intubation can be hazardous.

As we originally stated in our article, the placement of a tracheal tube should be followed by auscultation over the trachea anilla and epigastrium, and confirmed with a reliable technical test. Ideally this should be capnography. When this is unavailable, then the oesophageal detector device is a simple and suitable alternative.

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Ambulatory extradural analgesia

Sir,—I believe Drs Buggy, Hughes and Gardiner [1] may have made a mistake in stating that the Queen Charlotte’s group used an intrathecal dose of bupivacaine 25 mg with fentanyl 25 µg before extradural administration. This dose most probably results in an extremely high spinal block which certainly prevents the mother from mobilizing. The dose of bupivacaine is 10 times lower; that is 2.5 mg intrathecally with fentanyl 25 µg.

While it is true that it is not known what the effect of an intrathecal dose of bupivacaine 2.5 mg and fentanyl 25 µg has on the neurological status of mothers, the majority of these mothers could mobilize if they wished [2]. The extradural dose of bupivacaine 15 mg containing fentanyl 30 µg was given only after the return of discomfort, which occurred at a mean time of 90 min (range 20–245 min). Therefore, at no time was a total dose of bupivacaine 30 mg given initially, as was the case in their study [3].

I therefore repeat the contention that while their article quite rightly cautions the safety of giving bupivacaine 30 mg and fentanyl 30 µg extradurally, they have not demonstrated that repeat doses of 10–15 ml of 0.1 % bupivacaine with fentanyl 2 µg ml⁻¹ present the same dangers.

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Gastric mucosal pH and splanchnic blood flow

Sir,—I read with interest the two recent articles by Takala’s group [1, 2]. The authors used dopexamine [1] and dobutamine [2] to increase cardiac index and splanchnic blood flow after cardiac surgery in an attempt to improve splanchnic tissue oxygenation. The latter was assessed by tonometric estimation of gastric mucosal pH (pHᵢ) and by regional oxygen consumption data. Many factors are known to affect pHᵢ such that in some circumstances it is not a reliable index of the adequacy of splanchnic perfusion. Unfortunately, the presence or absence of several important factors was not described in these articles.

Evolving systemic metabolic acidosis is not an uncommon phenomenon during or after cardiac surgery. In these circumstances, a low pHᵢ would be expected to develop and may not necessarily indicate a state of regional dysoxia. Coexistent systemic alkalosis is another potential source of error as it may mask tonometric detection of low pHᵢ. In many cardiac surgical units, metabolic acidosis is treated with i.v. sodium bicarbonate but it is unclear whether or not patients in these studies received such treatment during or after operation. Administration of sodium bicarbonate is known to introduce artefact into tonometric estimation of pHᵢ [4].

Although Takala’s group found that pHᵢ decreased significantly during infusion of dopexamine, there was a comparable reduction in mean values for this variable in the control group (0.04 vs 0.05 with dopexamine), and the reason for the lack of statistical significance for the decrease in the control group may have been related to the smaller number of patients. A downward trend in pHᵢ after cardiac surgery has been documented previously [5].

“Flow rapidly should low pHᵢ be expected to recover in response to an effective therapy?” is a question pertaining to study design which remains unanswered at present. Takala’s group assessed the effect on pHᵢ after 90 min of inodilator therapy. It is conceivable that this is an insufficient length of time for restoration of local acid–base balance. Nevertheless, the apparent significance for the decrease in the control group may have been artefactual due to local acid–base changes in the proximal stomach that cannot be detected with gastric tonometry [4].

Sir,—Certainly intrathecal bupivacaine 25 mg is a mistake and not what the Queen Charlotte’s group prescribed [1], nor indeed did we interpret it as such. We suggest that this must surely be a misprint or typographical error, as our original letter of reply correctly noted the intrathecal dose of bupivacaine as 2.5 mg [2]. The return of discomfort during labour may not necessarily correlate with the return of neurological function and, in particular, with recovery of posterior column sensory modalities. Nevertheless, further studies are indeed indicated to determine the effect and duration of both the intrathecal dose of bupivacaine and the extradural dose of bupivacaine 15 mg on posterior column sensory function.

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Sir,—We thank Dr Trinder for his interest in our two articles [1, 2]. It is true that systemic acid-base status does also affect pH interpretation and a pH-gap (difference between systemic and gastric mucosal pH) rather than absolute pH values should be used. We are also aware of the possible hazards (among others sodium bicarbonate administration) concerning gastric mucosal pH (pHi) determinations. However, we do not administer sodium bicarbonate to our cardiac surgery patients during or after operation and none of our patients in the two published studies received sodium bicarbonate. Metabolic acidosis after cardiac surgery is indeed very rare in our department. A likely cause for metabolic acidosis in postoperative cardiac patients is insufficient tissue perfusion, and increasing oxygen delivery would be our choice of therapy, not sodium bicarbonate.

Metabolic acidosis or alkalosis was not observed in either of our studies. In the study of Parviainen, Ruokonen and Takala [2], mean arterial pH was 7.42 (sem 0.01) and base deficit −1.4 (0.4) during the baseline measurement and 7.40 (0.01) and −1.4 (0.4) during the vasoactive period, respectively. In the study of Uusaro, Ruokonen and Takala [1], arterial pH was 7.41 (0.01) and base deficit −1.5 (0.4) during the baseline measurement and 7–40 (0.01) and −1.6 (0.6) during the vasoactive period, respectively. In addition to systemic acid-base balance and sodium bicarbonate administration, sample handling, delay in analysis of tonometric saline Pco2 and determination of Pco2 with different blood-gas, analysers affect calculated pH [3, 4] and an effort was made to minimize these sources of error in both of our studies. It is indeed possible that if the number of patients had been larger in the control group in the dopexamine study, a significant finding in this study was that gastric mucosal pH decreased to its lowest value in 40 min during partial superior mesenteric artery occlusion and increased to the pre-occlusion level 80 min after occlusion [5]. After total occlusion there was a dissociation between changes in splanchnic blood flow and gastric intramucosal pH after cardiac surgery is certainly not a new finding. However, the main finding in this study was that gastric mucosal pH decreased during the vasoactive period in response to increased splanchnic oxygen delivery by dopexamine; a situation in which an increase in pH, would logically have been expected.

We do not agree with Dr Trinder that the time was too short for gastric mucosal pH to improve in response to vasoactive therapy and there are previous reports to support our view. In an experimental study by Antonsson and colleagues when tonometry was actually validated, tonometrically determined gastric pH decreased to its lowest value in 40 min during partial superior mesenteric artery occlusion and increased to the pre-occlusion level 80 min after occlusion [5]. After total occlusion there was a >0.1 pH unit increase in 40 min. In a recent study in patients with septic syndrome, gastric mucosal pH improved significantly in response to 60 min of infusion with dopexamine at the maximum dose studied [6]. We believe that 90 min of effective therapy should be long enough to improve gastric mucosal acidosis, if any improvement occurs.

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Positive pressure ventilation and the laryngeal mask airway in ophthalmic anaesthesia

Sir,—The controversy over whether or not to use positive pressure ventilation (IPPV) is safe with the laryngeal mask airway (LMA) ranges on, with the publication of the article by Valentine, Stakes and Bellamy [1] and the reply by Brain and colleagues [2].

In ophthalmic anaesthesia, the LMA has a particular advantage. Lamb, James and Janicki [3] demonstrated the beneficial effect of substituting the LMA for a tracheal tube on both intracocular pressure and pressor responses, not only at insertion but also at removal. The LMA has other advantages in ophthalmic anaesthesia, such as lack of a tendency to cough, either if the neuromuscular blocker is wearing off or at removal, resulting in a smooth transfer from IPPV to spontaneous ventilation. There is also markedly less discomfort in the throat after operation.

However, because the LMA was not designed to provide a perfect seal [4], there have been various attempts to assess the safety of IPPV and the LMA [5, 6] and in particular to assess the potential dangers of gastric aspiration [7–13]. There have also been several case reports [14–18]. Only Verghese, Smith and Young [19] surveyed a large number of patients to try to show the safety of the LMA, reporting on 2389 patients of whom 41 had undergone ventilation. Only two patients exhibited any regurgitation. Haden, Pinnock and Campbell [20] reported 593 cases with no significant problems.

I present a series of 1958 patients given a general anaesthetic for ophthalmic surgery. No patient had any clinical signs of inhalation of gastric contents at any time, before, during or after the recovery room or after operation. A total of 1502 patients received IPPV, of whom 1424 received IPPV by means of an LMA. For 77 patients a tracheal tube was used, and two of 1958 patients had a pre-existing tracheostomy; 455 patients breathed spontaneously by means of an LMA. The numbers for each group of operations is shown in table 1.

The high proportion of patients having a tracheal tube for oculoplastics shows the author’s preference because of the (as yet unseen) potential for heavy blood loss with dacrocyto-Rhinostomy. The author has used a tracheal tube for dacryocystorhinostomy. This was found to be unsatisfactory as, during work on encircling the globe on the medial side, stimulation of the nerves in this area caused sudden, unpredictable sneezing and during work on encircling the globe on the lateral side, stimulation of the nerves in this area caused sudden, unpredictable sneezing. The author was not satisfied with the seal provided by the LMA.

A high inflation pressure and (3) if the airway is not well sealed. The author has used an arbitrary insufflation pressure of 20 cm as the upper limit, partly because of the adverse effect of increased intracocular pressure on the retina may be relatively unsupported and coughing is therefore a serious problem in this group of patients. This is less of a problem in ophthalmic anaesthesia as local anaesthetic techniques are used for this group of patients.

The smooth change from IPPV to spontaneous ventilation is an important factor in the success of the LMA in ophthalmic anaesthesia. This is particularly so in vitreoretinal work where the retina may be partially supported and coughing is therefore highly undesirable. When I first started using the LMA for ophthalmic anaesthesia, spontaneous ventilation was used for vitreoretinal anaesthesia. This was found to be unsatisfactory as, during work on encircling the globe on the medial side, stimulation of the nerves in this area caused sudden, unpredictable sneezing even when deep levels of anaesthesia were being maintained.

Problems with laryngeal spasm at the time of antagonism of neuromuscular block can usually be overcome with an i.v. dose of dexamethasone.

Provided care is taken to use a tracheal tube: (1) in patients with reflux or with a history suggestive of a full stomach, (2) if there is a high inflation pressure and (3) if the airway is not well sealed.
maintained for any reason, the LMA is not only safe, but a great improvement for patients undergoing general anaesthesia for ophthalmic surgery.

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Optimum opioid for extradural administration

Sir,—We read with interest the editorial by Chrusbasik and Chrusbasik on the optimum choice of opioids for extradural administration [1]. The use of extradural opioids alone for postoperative analgesia is rare in British practice. A recently completed survey of 251 United Kingdom hospitals [Cook TM, Eaton JM, Goodwin APL, unpublished observations] showed that only 4% of departments use opioids alone via this route after major surgery whereas 96% of the departments use a combination of opioids with local anaesthetic agents. This practice is supported by both laboratory [2] and clinical evidence [3, 4]. The technique of extradural analgesia is widespread: 97% of departments use extradural analgesia after laparotomy and 81% after thoracotomy. The choice of opioid used in this way varies: some departments use several opioids, with fentanyl being used by 61% of departments, diamorphine by 52% and morphine by 12%.

The authors emphasize the side effects of various opioids via the extradural route but the critical issue of efficacy is less well addressed. McQuay and colleagues have demonstrated the inverse relationship between efficacy and lipophilicity of opioids used spinaly [5]. In this respect diamorphine (which is not mentioned in the editorial but is used by more than 50% of departments) is particularly effective via the extradural route. Fentanyl as a single agent produces poor results [6]; this may be a result in part of the fact that it is readily absorbed in extradural fat. The other opioids mentioned in the editorial may have theoretical advantages over fentanyl, diamorphine and morphine but are rarely used in practice in the United Kingdom and these advantages have not been supported in clinical trials [7]. However, conclusions on clinical utility cannot be drawn without considering procedural details, most notably site of insertion of the extradural catheter, which is known to influence efficacy and side effects of extradural opioids [8]. The survey suggests that 86% of departments preferentially site extradural catheters at a spinal level which corresponds with the surgical incision.

It would appear that at least for British practice the question should not be which is the best extradural opioid but which is the best opioid to use in combination with local anaesthetic agents via the extradural route and at what site.

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4. Dahl JB, Rosenberg J, Hansen BL, Hjortso NC, Kehlet H. Differential analgesic effects of low dose extradural morphine and morphine-bupivacaine at rest and during mobilisation
A second criterion which favours the use of more lipophilic opioids is speed of onset. This may not be important for the start of an extradural infusion if it is commenced during operation or if the first hour or so is covered by the intraoperative local anaesthetic effect persisting into the early postoperative period. It may be more important if the opioid is to be given by extradural PCA, for which the patient needs to be able to perceive an analgesic effect within a reasonable time from making a demand. We have tried combining fentanyl with morphine in PCA, to attempt to yet produce a rapid onset effect with a low requirement for opioid but have found that the demand rate produces concentrations of fentanyl that increase into the systemic analgesic range [4].

Diamorphine may be unique in being a lipophilic pro-drug that acts through less lipophilic metabolites (monoacetylmorphine and morphine itself). It is not available for therapeutic use in Germany, although we have studied it in collaboration with colleagues in Bristol. We found systemic concentrations of morphine during extradural PCA that were well below the range for systemic analgesia (as for extradural PCA with morphine; 1), and derived an informal estimate of between 2 and 3:1 for the ratio of i.v. to extradural dose requirements. A recent randomized study after Caesarean section produced an estimate close to 6:1 [5] (although it should be noted that the extradural doses were given on-demand by a nurse rather than self-administered by PCA).

Accurate placement of the tip of the extradural catheter is logically and practically important if the best effect is to be obtained from the local anaesthetic component of an extradural infusion (large volumes can compensate to some extent for inaccurate placement whether the injectate contains local anaesthetic, opioid or both). We all wish to see convincing evidence that accurate placement is important for extradural infusions of opioid alone. We could then be reassured that the opioid effect was as localized as we should like to the target segments of the spinal cord. The evidence thus far has not been very convincing.

Sir,—We congratulate Drs Cook and Stannard on the results of their survey. It is undoubtedly very interesting to know what is actually occurring in at least one part of Europe. However, it is equally interesting that what becomes popular at any time or place is not necessarily supported by evidence that it is the best in all circumstances.

Whereas there are undeniable situations in which combining local anaesthesia with extradural opioids has improved effectiveness, there are also undeniable reports of situations in which no significant benefit has been demonstrated [1]. The failures to show benefit are partly, but not entirely, explicable in terms of concentration of local anaesthetic, volume and rate of infusion, type of opioid used in the combination, type of surgery or small numbers in studies that failed to show effect. The almost universal use in Britain of local anaesthetic—opioid combinations, rather than extradural opioids alone may not be accompanied by universal improvement in effectiveness, but it is probably excusable nonetheless on the grounds that there is some safety in standardizing the regimens for postoperative analgesia to avoid confusing those who have to put them into effect, and that the users are well enough attuned to the side effects of local anaesthetics to be able to avoid the worst of the potential harm.

The single most useful practical feature of opioid-only infusions is that extradural opioids do not need to be given in the relatively large volumes that are required to ensure spread of local anaesthetics in the extradural space, and 24 or 48 h supplies of opioid can be contained in small infusion devices that are easily portable.

On the theoretical side, attempts to identify the best opioid for extradural use (if it is possible) had to begin with the study of opioids alone, without the potential complications of having to decide the type, infusion rate of local anaesthetic with which to combine them, and the type of surgery in which the combination might be more effective than the opioid alone. Any study to determine the best opioid for extradural use in combination with local anaesthetic would have to be based on a postulate that the rank order of extradural use of opioids in combination differs from the rank order of opioids alone, which implies that there might be some specific interactions between some opioids and some local anaesthetics. This is certainly not impossible, but is it likely enough to warrant what would clearly be a huge investigitive effort?

We suspect that the popularity of fentanyl and diamorphine in Britain is largely pragmatic, because of their ready availability in preservative-free form. We have argued elsewhere [2] that the prime criterion by which opioids should be ranked for use by extradural infusion or extradural patient-controlled analgesia (PCA) is the ratio of i.v. to extradural dose requirements. By this, morphine should be the best, with a ratio of about 9:1, and sufentanil, buprenorphine and methadone should be the worst with a ratio that is not convincingly different from 1. This is reasonably consistent with the inverse relationship between lipophilicity and effectiveness demonstrated by McQuay and colleagues [3] for the discharge of nociceptive neurones in the exposed spinal cord of the rat. However, there are major inconsistencies in the relationship in the middle order of lipophilicity, which probably reflect the added complexities of extradural administration in clinical situations [2].
and Spence who comment "...cannabinoids may be useful in some patients with chronic pain". Recently, I have found an increasing number of patients "outing" about cannabis and telling me that they find that it is more effective in relieving pain than prescribed drugs (including opioids). All of these patients are desperate enough to break the law to obtain analgesia for better symptom control. Some do not like having to smoke it nor do they like some of the side effects. Others find its use pleasant. However, is this any different to the experience of patients taking morphine for pain?

Morphine is now used commonly by pain specialists in the management of most types of pain, both acute and chronic. Over the last few years it has been liberated, without evidence of an associated increase in the number of drug misusers. Is cannabis likely to be any different?

Pain specialists are well used to using drugs in situations for which they have not been formally evaluated and are not licensed. For the majority of their patients, they conduct single-patient trials of treatment. To stop this practice would stop pain relief services in their tracks. Therefore while we continue to use amitryptiline, Depo-Medrone, sodium valproate, clozabam, flecaainide and many other drugs, unlicensed for use in pain relief, there is little justification for not also having cannabis available.

Doyle and Spence see little use for cannabis in acute pain although they cite no evidence. It may have a use in the management of acute back pain with severe muscle spasm, for example, as an alternative to diazepam and opioids. Could there also be a place for its use for analgesia and sedation in the ITU?

Clearly from a humanitarian point of view, there is little logic in denying patients a drug that we know will relieve some or all of their pain, particularly when there is no effective alternative. This is different from Doyle and Spence’s final comment implying that cannabinoids should be studied "...in comparison with optimal conventional treatment". To await the results of comparative trials before releasing cannabis for use in chronic pain would fail many seriously ill and dying patients.

The drug will remain unavailable until the Department of Health agrees to re-classify it to schedule 2 and until a pharmaceutical company produces cannabis as a medicine in preparations that satisfy the Medicines Control Agency. Until then many patients are being denied treatment for pain that is untreatable by other methods.

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Cardiac arrest after unrecognized dynamic inflation

Sir,—I read with interest Drs Myles, Madder and Morgan’s article on cardiac arrest after unrecognized dynamic hyperinflation [1] and describe a similar case.

A 59-yr-old woman with asthma of 20 years’ duration was admitted with a severe infective exacerbation leading to type 2 respiratory failure. This necessitated paralysed, tracheal intubation and ventilation of her lungs in the casualty unit, with consequent transfer to intensive care. Her arterial pressure was 150/85 mm Hg. Initial ventilation was pressure controlled to 25 cm H2O with a rate of 18 b.p.m., primarily to treat hypercapnia. Oxygenation was not a problem. By the next morning expiration was continuing up to inspiratory triggering and so expiratory time was increased. However, over the course of the following night the patient gradually and inexplicably became hypotensive. Systolic pressure, measured directly, had decreased from 110 mm Hg to 65 mm Hg while the ECG was unchanged showing sinus rhythm. It was only on disconnection from the ventilator with manual inflation that the chest x-ray show pneumothorax or sinus rhythm alter. By day 4 it was possible to dispense with this regimen and increase ventilation to combat hypercapnia. The problem did not recur.

The patient spent a further 6 weeks in the ICU and 5 weeks in the medical ward before being discharged home.

Cardiovascular collapse caused by dynamic lung hyperinflation is a well documented phenomenon [2–5]. I agree with Myles, Madder and Morgan that this should be included as a cause of electromechanical dissociation in resuscitation guidelines.

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Sir,—The case described by Dr Mercer is not uncommon in the intensive care unit, although it usually remains unrecognized. Modern ventilator management of airway obstruction and adult respiratory distress syndrome (ARDS) highlight the potentially adverse effects of positive pressure ventilation, such that many intensivists now accept a degree of permissive hypercapnia [1]. In fact, adverse haemodynamic effects and barotrauma cause more mortality than a mild to moderate degree of respiratory acidosis. If respiratory acidosis is thought to be causing complications such as arrhythmias or pulmonary hypertension, then a slow infusion of i.v. bicarbonate solution can be used.

We have had a large amount of experience in managing single and bilateral sequential lung transplantation in patients with severe end-stage obstructive lung disease, with less than 20% requiring cardiopulmonary bypass. One of the main reasons for our success has been the use of permissive hypercapnia and appreciation of the existence of dynamic hyperinflation.

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General anaesthesia for day-case cataract surgery

Sir,—I read with interest the paper by Moffat and Cullen [1] in which two techniques of general anaesthesia for day-case cataract surgery were compared.

They stated that "In the UK the majority of cataract operations are performed under general anaesthesia on an inpatient basis" [2], but that "It is estimated that between 20% and 40% of cataract patients in the UK are acceptable for day-case surgery" [3]. They also felt that, in the short term, there was little likelihood of an increasing use of local anaesthesia techniques for such procedures.

Recent experience in my own unit, however, is in marked contrast with these observations. Cataract surgery has been performed routinely under local anaesthesia for the past 15 months. The local blocks are carried out by two different anaesthetists using an identical technique, consisting of careful explanation, local anaesthetic eye drops, and intracorneal and medial
periconic injections of 0.375 % bupivacaine, 2 % lignocaine, hyaluronidase 7.5 μl−1 and adrenaline 1: 200,000.

In the past 6 months, 121 of 124 cataract operations were carried out under local anaesthesia. The indications for general anaesthesia were patient confusion in one case and two patients requested general anaesthesia. There were no complications with any block and patient satisfaction was very high, with excellent surgical operating conditions being provided.

Almost all of these patients would have been suitable for day-case surgery and it is anticipated that this will soon become the norm. The only reason why many stayed in hospital overnight appears to be that we are still in a “transition period” between general and local anaesthesia.

There has been overwhelming acceptance of this technique by patients, surgeons and nursing staff, and we feel that with greater motivation from anaesthetists and surgeons, the vast majority of cataract surgery is suitable for local anaesthetic as day-cases.

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**Design of nitric oxide delivery systems**

Sir,—We were interested to read Young’s article [1] on the development of a universal nitric oxide (NO) delivery system. The aim of any such delivery system is two-fold. First, to ensure a constant concentration of NO throughout the whole inspiratory phase (\(P_{\text{NO}}\)), as high concentrations of NO are toxic. Second, to minimize the formation of highly toxic nitrogen dioxide (NO2), which is produced by the rapid reaction between NO and oxygen. The rate of this reaction is proportional to the square of the NO concentration, so that the concentration of NO2 formed by the rapid reaction between NO and oxygen.

The rate of this reaction is proportional to the square of the NO concentration, so that the concentration of NO2 formed by the rapid reaction between NO and oxygen. The aim of any such delivery system is two-fold. First, to ensure a constant concentration of NO throughout the whole inspiratory phase (\(P_{\text{NO}}\)), as high concentrations of NO are toxic. Second, to minimize the formation of highly toxic nitrogen dioxide (NO2), which is produced by the rapid reaction between NO and oxygen. The rate of this reaction is proportional to the square of the NO concentration, so that the concentration of NO2 formed by the rapid reaction between NO and oxygen.

We suggest that in order to fully evaluate an NO delivery system it is important to both calculate the peak \(P_{\text{NO}}\) using a carbon dioxide tracer technique [3] and to directly measure the formation of NO2.

Unfortunately, Young has not presented these data for his “universal NO delivery system”. Although the “area ratio” published offers a crude measurement of the ability of NO flow to “track” the total gas flow during inspiration, it bears little relation to the peak \(P_{\text{NO}}\). Furthermore, the accuracy of the calculated “area ratio” must be questioned, because it is based on the measurement of NO flow by a pneumotachograph at flow rates of 0.2–0.4 litre min−1. It is doubtful if the pneumotachograph can accurately measure such low flows, particularly if they vary rapidly with time.

We agree that, theoretically injection of NO after the ventilator should minimize NO2 formation in an ideal system. Unfortunately, if the flow detector and valves have an inadequate dynamic response time, this results in a high peak \(P_{\text{NO}}\) and in paradoxically high NO2 concentrations.

Until such systems are fully evaluated it may be safer, at present, to opt for a constant flow NO blender, such as the Nomius (Sweden) [4], and to accept the limitations of using a Siemens “Servo” ventilator with its low pressure input. The Manley ventilator, although much less sophisticated, is a true minute volume divider; this minimizes the residual volume and the gas transit time, leading to less NO2 formation than with the Servo [5]. The Nomius has been evaluated and shown to be safe with both of these ventilators.

Whatever the NO delivery system, it is mandatory to monitor continuously the inspiratory concentrations of both NO and NO2.

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Sir,—The nitric oxide delivery system described was tested with two pneumotachographs, one to sense flow from the mass flow controller and one to sense flow from the ventilator. The pneumotachograph that Powroznyk, Latimer and Oduro are concerned about (measuring flow from the mass flow controller) has a resistance of 0.034 cm H2O ml−1 s, which implies that using a Validyne transducer with a 2–0.2 cm H2O sensitivity, the maximum range of flow measurement is about 0–3 litre min−1.

Thus flows of 0.2–0.4 litre min−1 are measured easily, and the measurement range of the pneumotachograph/Validyne transducer combination matched the full control range of the mass flow controller. The “area” method I used does not therefore suffer from inaccuracies caused by inappropriate measuring techniques.

As explained in the article and highlighted in subsequent correspondence [1], the mass flow controller I used does not have an ideal response time; it is too slow. As a result the mass flow controller tends to deliver less nitric oxide than desired, not more, and high peaks (which are characteristic of systems with a fast response) do not occur.

With the capnograph in the position indicated, any “peaks” would be detected during inspiration, but none was seen.

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