different observations may arise from the different techniques used to record the pressures. The reference [4] quoted by Dr Serpell compared intracranial extradural pressure with lumbar extradural pressure in neurosurgical patients and is not relevant to the present study. Insertion of an extradural catheter in an anaesthetized patient may carry an increased risk of nerve damage. However, many anaesthetists regard it as a safe and acceptable practice. For example, in a recent study [5], extradural catheters were inserted either before or after induction of anaesthesia. To avoid nerve damage, an extradural catheter should only be advanced if there is no resistance to its insertion.

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**Echocardiography and chest trauma**

Sir,—We read the letter by Fleyfel and colleagues [1] with interest. They stated that the use of echocardiography in the diagnosis of traumatic intrapericardial diaphragmatic hernia was unhelpful because of extensive surgical emphysema over the left hemithorax. They did not state the mode of echocardiography used but we assume it was transthoracic and not transoesophageal. While accepting that they may not have had access to either a transoesophageal echocardiograph or the availability of an experienced operator, there is no doubt that transoesophageal echocardiography would have clearly demonstrated the presence of a significant intrapericardial lesion despite subcutaneous emphysema. It would also have confirmed the absence of other forms of cardiac or pericardial trauma.

Transoesophageal echocardiography is a minimally invasive investigation which should be considered in all cases of major blunt chest trauma as it reliably identifies the presence of pericardial effusions and tamponades, atrial thrombi, traumatic valvular disease, myocardial contusion and usually ascending aortic dissection [2]. These lesions are often not visible using transthoracic echocardiography and more than 10% of these lesions may be missed at subsequent surgery [3]. Transoesophageal echocardiography may be performed in the emergency department or operating theatre and reduces the need for patients to be transferred to remote locations for examinations such as computerized tomography (CT) or aortography.

We suggest that in the case described, transoesophageal echocardiography would have been extremely useful and may have speeded up the diagnosis and reduced the need for a CT scan. The case demonstrates well the difficulties encountered in diagnosing injuries in blunt chest trauma and is a good example of a case in which transoesophageal echocardiography may well have been of great value in providing a clue as to the cause of the life-threatening cardiac tamponade encountered. We feel that awareness of its potential in such situations should be promoted and its use encouraged.

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**Alkalination of local anaesthetic for intra-articular injection during arthroscopy**

Sir,—I read with interest the article by Richmond [1] on the effect of prilocaine on pain after arthroscopic surgery. However, I wish to point out a factual error in the discussion. Dr Richmond suggested that the results of our investigation [2] were similar to those of Chirwa, MacLeod and Day [3] who reported reduced pain scores, morphine requirements and time to first request for analgesia with bupivacaine. In fact, the only similarity in the results was that we also demonstrated that bupivacaine (compared with saline) resulted in fewer patients requiring postoperative analgesia (in our case fentanyl). In addition, bupivacaine-treated patients who requested fentanyl required a significantly smaller dose than control patients. However, the mean time to requesting analgesia did not differ between the two groups in our study. In contrast with Chirwa, MacLeod and Day, we did not show a difference in VAS pain scores between bupivacaine-treated patients and controls. Pain scores in our study were assessed in all patients, in contrast with Chirwa, MacLeod and Day who only reported pain scores in those patients receiving postoperative analgesia.

The other major difference between our results and those of Chirwa, MacLeod and Day was that we demonstrated significantly earlier mobilization and discharge in bupivacaine-treated patients. We considered this to be the most important benefit resulting from the use of bupivacaine. The current study has not surprisingly failed to demonstrate any effect of (short-acting) prilocaine on mobilization after discharge, but does not appear to have reported the time to mobilization in the early recovery phase.

Similarly, conclusions cannot be drawn regarding discharge times as no discharge criteria are stated in the manuscript.

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Sir,—In response to Dr Smith’s letter, I agree that the specific results were different in the studies by Smith and colleagues [1], from those of Chirwa, MacLeod and Day [2]. However, both studies demonstrated some benefit in terms of postoperative analgesic requirement and it was this general overview I intended to convey.

Prilocaine was used in this study to allow significant alkalinization of the local anaesthetic and to reduce the risks of toxicity associated with bupivacaine. Although it is slightly shorter acting that bupivacaine, neither are present during “mobilization after discharge”. Discharge times were not reported as these were day-case procedures and the patients were routinely discharged 3-4 h after operation with pain not being the only determining factor.

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