
**POSTOPERATIVE INTERCOSTAL BLOCK**

Sir,—I read with interest the account by Baxter and colleagues [1] of their use of continuous intercostal nerve blockade after cardiac surgery. Their expressed intention was to study the effect of intercostal nerve blockade on pain relief and pulmonary function after cardiac surgery and to document the occurrence of any adverse effects they encountered. Whereas the excellent conduct of their trial allowed them to document their information accurately, I feel they have fallen prey to a very common fault of deriving incorrect conclusions based on the data they have collected.

It is well documented that patients undergoing major surgery suffer a reduction in respiratory capacity and that this may persist for quite some time after surgery [2–6]. To infer, as Baxter and colleagues did [1], that there is little to recommend intercostal analgesia over opioid analgesia merely because pulmonary function is not improved is quite erroneous. Based on their data, they may claim that intercostal nerve blockade provided significantly better pain relief than opioid analgesia and that, by their criteria, no improvement in pulmonary function was attended by this improved quality of analgesia. They may, therefore, conclude that there are factors other than improvement in pain relief which affect postoperative pulmonary function. This is already well documented. If they had looked at pulmonary function in the postoperative period before and after intercostal nerve blockade “top-ups” were required, these authors would have been able to document the precise effect of the nerve blockade on respiratory function, as this would have been the only variable.

In my first paper on continuous intercostal nerve blockade [7], I measured respiratory peak flow in the postoperative period when further analgesia was requested and 30–40 min after the “top-up” was given. In these patients the peak flow improved by a mean of 37%. This is in direct conflict with the results of Baxter and colleagues but, unlike these authors, I can conclude that this effect must be a direct result of intercostal nerve blockade.

With regard to complications of intercostal nerve blockade, I feel that it should be stressed again that, based on the studies of Nunn and Slavin [8], it is clear that intercostal nerve blockade should be performed posteriorly at the site where the potential for pneumothorax is least, that is, where the rib is at its thickest. One can not recommend the injection in the mid axillary line as used by Baxter and colleagues.

Finally, the acceptability of performing intercostal nerve blockade in a patient in whom extradural analgesia is considered unsuitable because of prior administration of heparin must seriously be questioned, particularly in the light of the complication described by Baxter himself [9].

**REFERENCES**


Sir—Thank you for the opportunity to reply to Dr Murphy’s comments regarding our study on Continuous Intercostal Blockade after Cardiac Surgery [1]. Our intention was to assess this new technique objectively, as previous reports, including Murphy’s own [2], have not been controlled or double-blind and are therefore subject to problems of bias, etc.

Pulmonary dysfunction after major surgery is already well documented, and we studied the effects of 36 h of continuous intercostal analgesia on the recovery of postoperative pulmonary function after cardiac surgery. From this point of view, it is irrelevant if there is a 37% improvement in peak flow after each “top-up” as Murphy found in his unblinded study (in a different patient population) which he quotes if, overall, this confers no significant improvement in the pattern of recovery, incidence of complications, and so on. Indeed, Murphy suggested in his paper that more studies are required to assess whether the technique actually reduces pulmonary complications; in this patient population it would appear that it does not.

We agree with Murphy that individual intercostal blocks are usually performed at the angle of the rib, and we changed our technique only for the purpose of convenience for this study. This did not appear to affect the efficacy of the blocks and, indeed, satisfactory analgesia was obtained. However, several reports have shown that good analgesia is produced with an intrapleural catheter [3–5]. Indeed, we have seen a few of our catheters within the pleural cavity and good analgesia was obtained after operation. In view of the controversy regarding the spread of local anaesthetic within the chest wall [6,7], could intrapleural spread of local anaesthetic be the mechanism of action of this technique? In any case, it does not appear to matter whether the catheter is in the “extrapleural space” or intrapleural to produce analgesia, but pneumothorax has been documented with a catheter in both sites.

Similarly, we do not advocate performance of these blocks in patients with a coagulopathy from any cause. All blocks in our study were performed after the coagulation profile had