

relaxants to mask inadequacies of regional anaesthesia, and all failing to test their hypothesis.⁴ The time is long overdue to review the design of experimental protocols in this very difficult area of nociception and clinical outcome, and to throw away the deceptive crutch of muscle relaxants, as some of us have done, with great satisfaction for major intrathoracic and abdominal surgery and ultra-early ambulation.⁵ Clearly, there is always a need for brief use of a short-acting muscle relaxant, such as succinylcholine, while diathermy-cautery is being used directly on the muscles of the abdominal or chest wall, but that is the only concession that should be made to reliance on the integrity of regional blockade for effective muscular relaxation, a quiet operative field and ultra-early mobilisation.

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REPLY

Thank you for the opportunity to respond to the comments/criticisms raised by Dr. Bromage. My reply, not retort, to Dr. Carli¹ that "nothing was further from the truth," is valid.² The most stimulating event in surgery is the incision. In our experience, all patients undergoing aortic surgery under general anaesthesia respond to the incision and require more opioids or deeper inhalational anaesthesia.³ None of our epidural patients required additional opioids at the time of incision. To me, this is the acid test and demonstrated that the epidural was working and the block extended to the xyphoid process. Pancuronium may mask motor responses to inadequate segmental analgesia, but all anaesthetic drugs mask response to pain in some way.

We did not verify the height of the block before or after surgery. We relied on patient comfort as the measure of success as I am sure Dr. Bromage does in his clinical prac-

tice. Demonstration of a segmental level of blockade does not guarantee success as frequently demonstrated at caesarean section. Postoperatively, it may be impossible to demonstrate segmental block using bupivacaine 0.1% in our geriatric patient population. In fact, we reduced the concentration of bupivacaine from 0.125% to 0.1% because of sometimes persistent motor block. Analgesia continued, the patients were comfortable, and this was the aim.

If I were to repeat this study, the one change would be to demonstrate a T5 block before induction of anaesthesia. I am sure we achieved this level, but not to verify it leads to unanswerable questions.

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Combined spinal/epidural in neonates and children

To the Editor:

I have two concerns regarding the technique of Drs Williams *et al.* regarding the use of combined spinal and epidural anesthesia in the awake neonate and infant.¹ First is the recommendation of performing an epidural block after a spinal anaesthetic. The authors failed to mention the possibility of total spinal block from the passage of local anaesthetic through the hole in the dura.²

Second, I wonder about the advisability of performing extensive surgical procedures without airway protection although the authors previously described the use of spinal anesthesia as the sole anaesthetic for repair of gastrosciscis³ and closure of meningomyelocele.⁴ The maintenance of spontaneous ventilation may be further compromised by the surgical procedure as well as the need for supplemental intravenous sedation. The authors state that extubation may not be possible with the combined technique. We performed continuous caudal epidural anaesthesia with chloroprocaine combined with general anaesthesia in 25 neonates and