

Correspondence

Laparoscopic cholecystectomy using spinal anaesthesia

Editor—We read with interest the paper by Van Zundert and colleagues¹ and wish to comment on this, and share our experience. We have published a feasibility study on laparoscopic cholecystectomy, with CO₂ pneumoperitoneum, using lumbar spinal anaesthesia in fit patients.² The preliminary results of a controlled randomized study of ours comparing spinal to general anaesthesia for laparoscopic cholecystectomy are to appear soon in *Archives of Surgery*.³ From our studies, it is evident that spinal anaesthesia is not only feasible and safe for laparoscopic cholecystectomy, but it is also associated with some advantages for the patients, mainly better postoperative pain control. We have therefore recently extended the use of spinal anaesthesia in other laparoscopic procedures like hernia repairs. On the basis of our experience, we believe that spinal anaesthesia has a place in laparoscopic procedures and could evolve as a routine method of anaesthesia in laparoscopic surgery. The technique of segmental thoracic spinal anaesthesia described by Van Zundert and colleagues theoretically constitutes an even more specific method of anaesthesia for laparoscopic cholecystectomy. However, it is more difficult to perform a technically demanding method that could cause neurological side-effects from spinal cord puncture, as is emphasized in the text by the authors. It is not a method that could be easily and safely applied by the majority of anaesthetists, in contrast to the lumbar spinal anaesthesia we used in our studies. As such, segmental spinal anaesthesia could be considered as an invasive procedure rather than a minimally invasive one. The incidence of intraoperative cardiovascular changes is similar between the two techniques; the only advantage the segmental spinal anaesthesia offers is the avoidance of urinary retention that we have observed in a small number of patients in our trial (~6%) and, also, the possibility of day case surgery for the majority of patients. However, spinal anaesthesia does not by definition preclude day case surgery. In our country in particular, laparoscopic cholecystectomy is not regularly considered as day surgery and open inguinal hernia repair under local anaesthesia usually involves an overnight stay. Taking into account the advantages and disadvantages of these two methods of spinal anaesthesia, we consider it dangerous to suggest routine use of segmental thoracic spinal anaesthesia for laparoscopic cholecystectomy in fit patients simply in order to achieve day case surgery. Any other potential advantage over general anaesthesia can be achieved by classic (lumbar) spinal anaesthesia, which has

all the characteristics of the minimally invasive anaesthesia and it is easily performed by the vast majority of anaesthetists.

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Editor—It was interesting to read Van Zundert and colleagues¹ study exploring the potential of performing laparoscopic cholecystectomy under segmental thoracic spinal anaesthesia, but I would like to share some observations on the study.

Although rare, spinal cord damage is a potentially disastrous complication of spinal anaesthesia or dural puncture.⁴ A combination of patient, equipment, and technique-related errors can lead to spinal cord damage.⁵ The pencil point spinal needles, used by Van Zundert and colleagues¹ in their study, are now widely used in comparison with traditional ‘cutting’ type needle due to lower incidence of post-dural puncture headache with the former. The safety of pencil point needles, often referred to as ‘atraumatic’, has been questioned⁶ and a higher incidence of paraesthesiae, in comparison with cutting needles, has been noted.⁷ The technique described in their study¹ involves measuring the depth of dura matter from skin at 10th thoracic interspace with a 27 SWG pencil point needle advanced through 16 SWG Tuohy needle after the epidural space has been identified, till it touches the dura but not actually penetrate it. The spinal needle was then further advanced to penetrate into subarachnoid space. One of 20 patients in the study did experience transient paraesthesia, the significance of which is difficult to ascertain but the technique carries all the risk factors, as mentioned above, and can be considered inherently dangerous with potential to cause spinal cord damage, particularly in the absence of an accurate and reliable method of assessment of length of spinal needle advanced into the subarachnoid space.

As I understood, Van Zundert and colleagues¹ did not use a spinal needle with measurement markings on it, although I believe no such spinal needles are commercially available, neither did they mark the needle by any ink or marker. In the absence of a commercially available spinal needle with marking on it, Van Zundert and colleagues should have marked the spinal needle *in vitro* by passing it through the Tuohy needle till the injection port

is visible beyond the bevel of Tuohy needle and under no circumstance the spinal needle should have been advanced beyond that marking and if no CSF is aspirated up to that mark then the procedure should have been abandoned. I strongly believe that patient safety takes precedence over unnecessary risks to be taken for the success of the procedure.

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Editor—We thank Dr Tzovaras and colleagues and Dr Ahmed for their interest in our work,¹ and the opportunity to reply. There is no doubt that clinicians have been slow, perhaps even reluctant because of the effects of pneumoperitoneum, to apply regional anaesthetic techniques to laparoscopic surgery. We are pleased to learn that Tzovaras and colleagues also are working in this field and look forward to reading their study. They criticize two aspects of our paper: use of a segmental thoracic approach to spinal anaesthesia and its application in day case surgery. On the first point, we can only refer them back to the points made in our paper; on the second, we would note that there are clearly national differences in the procedures which are judged suitable for day case surgery. Greek practice must be very conservative if patients undergoing inguinal hernia repair under local anaesthesia are kept in hospital overnight. Whether the segmental thoracic approach to spinal anaesthesia offers definitive benefit over the traditional lumbar one will require further comparison and evaluation, not its dismissal as being a dangerous method to use simply to permit day case surgery. There is more to it than that, as we tried to make clear, although the facilitation of day case surgery is a useful end in itself.

In response to Ahmed's comments, we would note that the device used for combined spinal–epidural anaesthesia in the study was from the Portex™ Regional Anaesthesia Tray (Smiths, Hythe, Kent). The hubs of both spinal and epidural (Tuohy) needles do have distance markings on them, thus allowing us to measure the distance the spinal needle is introduced once the Tuohy needle is in the epidural space. Like Tzovaras and colleagues, Ahmed seems ready to dismiss our method as dangerous without full acknowledgement of the points made by ourselves in regard to both the technique used and the caution required. If the recommendation to allow the spinal needle to project beyond the tip of the epidural needle only until the injection port is visible were to be followed, then dural puncture would almost inevitably fail, as perhaps can be seen by reference to our earlier case report.⁸ The obliquity of the needle's insertion and the anterior position of the thoracic segment of the spinal cord are key factors.

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Validation of pulse contour derived stroke volume variation

Editor—We read the article by Dr Kubitz and colleagues reporting their results comparing stroke volume variation (SVV) derived by different methods of SV determination with great interest.¹ The authors should be commended for this study, that for the first time compared SVV derived from the widely used PiCCO monitor (Pulsion Medical Systems, Munich, Germany) to a true reference standard, that is, aortic transit time ultrasound. Although their approach is ambitious, however, some methodological remarks are necessary. First, the authors state that there was good agreement between SVV derived from pulse contour analysis and that derived from the aortic flow signal. This conclusion, however, is not supported by the data presented. After the introduction of Bland–Altman² plots for method comparison in 1986, for more than a decade the judgement of bias and limits of agreement was left to the clinician, and identical values were interpreted differently. The pivotal work by Critchley³ for the first