

Is Thoracic Paravertebral Block Truly Superior to Erector Spinae Plane Block? Re-Evaluating the Evidence from a Recovery Perspective [Response to Letter]

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Dear editor

We would like to thank Dr. Ma et al for their reading of our publication and for sharing their valuable insights. Their perspectives align very closely with the conclusions and acknowledged limitations of our study.

First, as noted by the authors, nociceptive pathways in colorectal surgery are complex: somatic pain from abdominal incisions travels via the T8-T12 intercostal nerves, while visceral pain is carried by the celiac and superior hypogastric plexuses. Given that a 20 mL local anesthetic injection typically spreads over only four to five dermatomes, a single T10 block risks leaving both cephalad and caudad inputs partially unblocked.^{1,2} Furthermore, as we acknowledged in the Discussion and limitations section of our article, the distal rectum and anal canal receive partial innervation from the sacral plexus—specifically involving the parasympathetic pelvic splanchnic nerves (S2-S4) during low pelvic dissection.³ Therefore, the T10 level blocks employed in our study may not fully cover the pelvic nociceptive stimulation. We concur that future studies should explore multi-level injection strategies or lumbar-level techniques to provide more comprehensive analgesia.

Second, we acknowledge that intention-to-treat (ITT) is the gold standard for randomized controlled trials. In our article, we identified this as an important methodological limitation. The primary efficacy analyses were restricted to the per-protocol (PP) population because two patients were excluded post-randomization (one due to block failure and one due to the ICU transfer), which precluded a complete outcome follow-up. Because of this missing data, robust ITT sensitivity analyses for the primary outcomes could not be performed; however, we did report the demographic data for the ITT and mITT populations in the supplementary materials. We fully agree that future large-scale trials should adopt formal ITT designs and non-inferiority frameworks.

Finally, we completely agree with the authors' emphasis on distinguishing between statistical and clinical significance, which aligns with our perspective. This is the core message of our discussion section. We pointed out that the 3.03-point difference in 24-hour QoR-15 scores between the TPVB and ESPB groups is less than half of the minimal clinically important difference (MCID) of 8 points.⁴ We also agree that interpreting the scores in isolation from the MCID is one-sided, and disregarding clinical significance would defeat the original intent of the research. We concluded that although this difference is statistically detectable, it is unlikely to represent a clinically meaningful advantage of TPVB over ESPB. Therefore, our final conclusion remains that these two techniques are equivalent in providing postoperative recovery and analgesic benefits, with ESPB offering the practical advantages of being simpler and quicker to perform.

In summary, we are grateful to Dr. Ma et al for reinforcing the key messages of our study. We share the vision that both TPVB and ESPB are highly effective components of multimodal analgesia in enhanced recovery after surgery (ERAS) pathways,^{5,6} and we look forward to future high-quality studies to further optimize regional anesthesia strategies in colorectal surgery.

Disclosure

The authors have no conflict of interest to disclose for this communication.

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