



Letter to the editor regarding “Ultrasonography guided erector spinae block in spinal surgery for pain management with enhanced recovery”



To the editor

We read with great interest the recently published research article that assessed the efficacy of erector spinae plane block (ESPB) in spine surgeries,¹ and wish to present our insights on that article.

While we appreciate the authors' efforts to assess the effects of ESPB in spine procedures, we do not agree with the statement that “there has been very little research on this emerging technique which can help to improve enhanced recovery after surgery (ERAS) which itself is a topic of interest for many clinicians”. This is because ESPB has already been assessed extensively in many surgeries. ESPB has also been studied in spine surgeries focusing on ERAS too.² Indeed, even systematic reviews with meta-analyses have been published for three consecutive years on ESPB particularly in this population.^{3–5} More importantly, the recently published meta-analysis⁵ observed that ESPB reduces the length of stay in the hospital besides reducing the perioperative opioid consumption and postoperative nausea and vomiting significantly, based on many previously published studies. Hence, unfortunately, this study¹ lacks novelty in our opinion.

In the “Introduction” section, Adhikari et al¹ state that the local anesthetic spreads significantly more in the epidural space when the lamina and ligaments are not intact, thus requiring more attention and cited 2 references (reference # 13,14 of Adhikari et al¹) to that. However, these referenced articles do not match that statement. Similarly, the 3 references (reference # 15–17 of Adhikari et al¹) cited to support another statement that the injectate spreads widely and “last until a period of time after the operation” do not match it.

In the “Methods” section, Adhikari et al¹ state that the needle direction is from “cephalad to caudad”. However, the figure describing it shows the needle direction from caudad to cranial.

Our main contention is, *p* values for “demographic and surgical characteristics of the patients” (Table 1 of Adhikari et al¹) were not provided in the “Results” section. However, it is stated “In this current study, the effect of the ESPB technique was compared with a control group of similar demographic and etiological characteristics” that too in the “Discussion” section. To make things worse, there is no *p*-value even for the parameter “Mean length of hospitalization”, despite “length of hospital stay” being one of the outcomes, leave alone whether it was primary or secondary.

Last, and to say the least, the “Discussion” lacks the basic standard of comparing and contrasting the already published studies on this topic with the current study. The very fact that there is not a single reference

cited in the “Discussion” is the epitome of this.

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CRediT authorship contribution statement

Raghuraman M Sethuraman: Conceptualization, Writing – original draft, Writing – review & editing. **Raj Murugan:** Writing – review & editing. **Mohammed Jassim:** Writing – review & editing.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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