

## Usefulness of dexmedetomidine as an intravenous adjuvant

Dear Editor,

I read the article on intravenous dexmedetomidine on spinal anesthesia and sedation<sup>[1]</sup> with interest as it addresses a day to day problem. Even though the study provides evidence of prolongation of subarachnoid block by dexmedetomidine infusion, I wish to place before you certain doubts.

The study was done in 2013-15, which means that more than 4 years have gone by, during which some concepts about intravenous adjuvants are likely to have changed. This may also be the reason for few new references and lack of registration with Clinical Trial Registry – India (CTRI).

The commonly accepted duration of intrathecal hyperbaric bupivacaine is 90–120 min,<sup>[2,3]</sup> whereas authors mention it to be 55 min based on just one study. The primary outcome measure is not mentioned clearly. The onset of block was observed to be  $2.1 \pm 0.3$  min in both the groups, indicating

that the two groups were similar in this respect. However, in the discussion section, the authors have mentioned that the onset was faster in one group than the other.

The authors have mentioned that the American Society of Anesthesiologists (ASA) status was high in Group A, but they have not mentioned the diseases the patients had. If the patients had diabetes with autonomic neuropathy, then the results would need to be interpreted differently. The type of surgery and the duration of surgery are not mentioned. Different procedures, such as hemorrhoids, tibial nailing, and hysterectomy, vary with regard to postoperative pain. Hence, valid conclusions cannot be made regarding the Visual analogue scale (VAS) scores.

Though the difference in the duration of motor block in the two groups (235 and 245 min) is statistically significant, this difference is not clinically relevant and should have been discussed. Similarly, the standard deviation of the duration of analgesia in the two groups is 92 and 52 min. This wide variation in dispersion makes interpretations difficult and the authors should have commented on this.<sup>[4]</sup>

The authors have mentioned that the hemodynamics changed from the baseline within each group, but have not mentioned the statistical test used.

Finally, the title would have been more informative and precise if the type of study was included in it. In addition, a consort flow diagram would have added the necessary punch to the article.

I request the authors to clarify these doubts for the benefit of all the readers.

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### Conflicts of interest

There are no conflicts of interest.

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