



## LETTER TO THE EDITOR

### Comment on “Comment on: Effect of magnesium sulfate with ketamine infusions on intraoperative and postoperative analgesia in cancer breast surgeries: a randomized double-blind trial”



Dear Editor,

Thanks for the valuable comments.<sup>1</sup>

First, the index study relies on the hemodynamic parameters as surrogates for intraoperative nociception and hence, fentanyl administration. Alongside the debatable sensitivity and specificity of the former in nociception monitoring, the matter is compounded by the lack of comparative account of hypertensives in the two study groups (albeit, the authors describe uncontrolled hypertension as an exclusion criterion).

This method was used after exclusion of other causes (e.g., tachycardia and hypotension due to blood loss) that may affect the increase in mean arterial blood pressure (MAP) and heart rate (HR).

This method has been used in many trials such as the study of Abdelraheem et al.<sup>2</sup>

The equipment for monitoring of depth of anesthesia (such as bispectral index) is not available in our institute.

Second, the authors fail to present any details on whether/or not any form of depth of anesthesia monitoring was employed.

The equipment for monitoring of depth of anesthesia (such as bispectral index) is not available in our institute.

Third, the comparable postoperative pain and sedation scores between the two groups are difficult to explain, in background of a substantially lower postoperative morphine requirement and/or consumption in the magnesium sulfate + ketamine group as opposed to the ketamine alone group.<sup>1</sup>

In our study,<sup>3</sup> PCA on demand was used for postoperative analgesia. NRS pain score was comparable between both groups as we didn't wait to administer analgesia at times of

recording only. Therefore, postoperative morphine requirement and/or consumption was lower in the magnesium sulfate + ketamine group as opposed to the ketamine alone group with the same level of NRS.

Lastly, while the ability of the study to detect any statistically meaningful differences in chronic pain could have been precluded by a small sample size, the incorporation of patient satisfaction and/or postoperative recovery would have added incremental value.

Patient satisfaction is a subjective method and affected by other factors.

Postoperative recovery was out of our scope (not primary nor secondary outcomes). Further studies are needed to focus on this item.

### Conflicts of interest

The authors declare no conflicts of interest.

### Reference

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