

Letter to the Editor in response to “Evaluation of the efficacy of lidocaine and magnesium sulphate in reducing the hemodynamic effects caused after intubation/laryngoscopy”



Carta à editora em resposta a “Avaliação da eficácia de lidocaína e sulfato de magnésio para reduzir os efeitos hemodinâmicos desencadeados pela laringoscopia/intubação”

Dear Editor,

The authors of the letter titled “Evaluation of the efficacy of lidocaine and magnesium sulphate in reducing the hemodynamic effects triggered by intubation/laryngoscopy” point out some limitations of our study.¹ They report that a single center study with a small number of individuals is not representative of a larger population.

Our study was performed with a properly calculated sample size based on variance, effect difference, alpha error, and previously determined power. Despite the loss to follow-up of some patients, a greater number of patients were randomized to avoid loss of study power. Thus, as demonstrated by the analyses, it was possible to find satisfactory results with the applied method.

The fact that the study was conducted in a single center does not necessarily constitute a deficiency. The difference between single-center and multicenter studies is generally based on sample size.² The hospital in which the study was performed is a large reference center encompassing a broad spectrum of patients and clinical specialties and also provides research support. In addition, performing anesthesia and collecting data become less subject to bias.

The authors of the letter suggest that bolus infusion of the study drugs would have reached peak effects more quickly; however, the option for pump infusion was made to prevent the occurrence of side effects, including hypotension, which still occurred in 54% of lidocaine patients and 28% of magnesium sulphate patients, even under slow infusion.³

It was not our intention to administer drugs whose effect lasted longer than six minutes. However, all patients were followed after this period and all maintained adequate hemodynamic stability. At the end of the study time, mean SBP of patients in Group L was 97 mmHg, representing 18% reduction in the mean SBP measured after anxiolysis (SBP = 119 mmHg). The mean SBP of patients in Group M was 105 mmHg, which represented 14% decrease in mean SBP measured after anxiolysis (SBP = 123 mmHg). Arterial hypotension was considered when BP values were lower than 20% of baseline values or SBP <90 mmHg.⁴ Therefore, the studied patients had blood pressure values compatible with those of the intraoperative period.

The study was carried out in Brazil, a country of great miscegenation whose African ancestry may reach 86% in certain regions.⁵ Ethnicity was mentioned by the authors of the letter as a risk factor for systemic arterial hypertension (SAH) and they suggested that determining the ethnicity of participants would provide useful information for the study. However, this stratification does not apply to Brazilian sampling because, unlike European countries where this determination can be very clear, in our country skin color does not denote ancestry and, obviously, genetic predispositions associated with that ethnicity. Moreover, studies using mitochondrial DNA show a prevalence of up to 30% of African ancestry in a white population.⁵ Thus, there is no point in stratifying the sample in ethnicities, which could even overcome ethical barriers.

The authors of the letter point out inconsistency in the indication of antihypertensive drugs and infer that patients may have received such treatment to ensure similar pre-surgery blood pressure. However, the Demographic Data Table is clear in describing the number of patients with a history of SAH and their respective chronic use medications. In no item was it reported that the drugs were indicated for acute preoperative pressure control. The only preanesthetic medication prescribed was midazolam, as reported in the study. In addition, patients with decompensated blood pressure would be in the ASA-III physical status classification and would therefore be excluded from the selection protocol. We acknowledge that the best term for what was stated in the last paragraph of the discussion, where it is stated that “Our study was performed with healthy patients”, could have been “healthy or controlled disease patients”, since only patients with physical status ASA I and II were selected, as described in the Method.³

The authors of the letter point out that the study title does not clarify that its purpose is to compare the effects of lidocaine and magnesium sulphate. We assume the attitude of gently disagreeing because in clinical trials the word “comparison” is not always explicit in the title, as exemplified in other studies.⁶⁻⁸

We thank the authors of the letter for their attention to our study and hope to have resolved all the questions.

Conflicts of interest

The authors declare no conflicts of interest.

References

1. Gnanapragasam R, Gomaa A, Patil V. Avaliação da eficácia de lidocaína e sulfato de magnésio para reduzir os efeitos hemodinâmicos desencadeados pela laringoscopia/intubação. *Braz J Anesthesiol*. 2017; <http://dx.doi.org/10.1016/j.bjan.2017.07.006> [in press].
2. Goodman NW. Conducting research in anaesthesia and intensive care medicine. *Resuscitation*. 2001;51:325-6.
3. Mendonça FT, de Queiroz LM, da GM, et al. Effects of lidocaine and magnesium sulfate in attenuating hemodynamic response to tracheal intubation: single-center, prospective, double-blind, randomized study. *Braz J Anesthesiol Engl Ed*. 2017;67:50-6.

4. Oliveira Filho GR de, Garcia JHS, Goldschmidt R, et al. Fatores de previsão de hipotensão arterial precoce em anestesia subaracnóidea. *Rev Bras Anesthesiol.* 2001;51:298–304.
5. Pena SDJ, Bortolini MC. Pode a genética definir quem deve se beneficiar das cotas universitárias e demais ações afirmativas? *Estud Av.* 2004;18:31–50.
6. Turan A, Memiş D, Karamanlioğlu B, et al. Intravenous regional anesthesia using lidocaine and magnesium. *Anesth Analg.* 2005;100:1189–92.
7. Narang S, Dali JS, Agarwal M, et al. Evaluation of the efficacy of magnesium sulphate as an adjuvant to lignocaine for intravenous regional anaesthesia for upper limb surgery. *Anaesth Intensive Care.* 2008;36:840–4.
8. Galgon RE, Strube P, Heier J, et al. Magnesium sulfate with lidocaine for preventing propofol injection pain: a randomized, double-blind, placebo-controlled trial. *J Anesth.* 2015;29:206–11.

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Sugammadex to prevent postoperative nausea and vomiting?



Sugamadex para prevenir náusea e vômito no pós-operatório?

Dear Editor,

We read with great interest the article of Yagan et al. in a recent issue of the journal.¹ The authors should be congratulated for performing a well-designed randomized clinical trial. Sugammadex has become a popular drug in perioperative medicine with not only important effects on patients' safety (e.g., residual paralysis) but also on other critical outcomes such as hospital discharge.^{2,3}

In order to further establish the validity and reproducibility of the study results, we had some questions regarding the trial that require some comments from the authors. First, since the authors used different types of surgical procedures, it would be important to demonstrate that the postoperative opioid consumption was similar between the groups as this can alter the primary outcome.⁴ Secondly, it seems that the authors did not administered routine antiemetic drugs as commonly done in routine practice and recommended by PONV guidelines.^{5,6} Finally, it is not clear who collected the data and why the authors did not use a double-blinded design to avoid measurement bias.

We would welcome some comments from the authors. This would help to further establish the significance of this clinical trial in this very important topic.

Conflicts of interest

The authors declare no conflicts of interest.

References

1. Yağan Ö, Taş N, Mutlu T, Hancı V. Comparison of the effects of sugammadex and neostigmine on postoperative nausea and vomiting. *Br J Anaesth.* 2017;67:147–52.
2. Herring WJ, Woo T, Assaid CA, et al. Sugammadex efficacy for reversal of rocuronium- and vecuronium-induced neuromuscular blockade: a pooled analysis of 26 studies. *J Clin Anesth.* 2017;41:84–91.
3. Carron M, Zarantonello F, Lazzarotto N, Tellaroli P, Ori C. Role of sugammadex in accelerating postoperative discharge: a meta-analysis. *J Clin Anesth.* 2017;39:38–44.
4. Gacio MF, Lousame AM, Pereira S, Castro C, Santos J. Paravertebral block for management of acute postoperative pain and intercostobrachial neuralgia in major breast surgery. *Br J Anaesth.* 2016;66:475–84.
5. Yang GZ, Xue FS, Li HX, Liu YY. Perioperative use of 5% dextrose to decrease postoperative nausea and vomiting. *J Clin Anesth.* 2017;41:63–4.
6. Gan TJ, Diemunsch P, Habib AS, et al., Society for Ambulatory Anesthesia. Consensus guidelines for the management of postoperative nausea and vomiting. *Anesth Analg.* 2014;118:85–113.

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