Reviewer’s report

Title: Abnormal Cisatracurium Pharmacodynamics and Pharmacokinetics among Patients with Severe Aortic Regurgitation during Anesthetic Induction

Version: 1 Date: 23 Nov 2019

Reviewer: Issam Tanoubi

Reviewer's report:

Thank you for requesting my review of the manuscript "Abnormal Cisatracurium Pharmacodynamics and Pharmacokinetics among Patients with Severe Aortic Regurgitation during Anesthetic Induction". The objective of the study was to compare the pharmacokinetics and pharmacokinetics of cisatracurium in patient with severe aortic regurgitation, during the anesthetic induction.

Here are my comments/suggestions

The second paragraph of the background section discusses the pathology of aortic insufficiency and does not necessarily explain what led to the research project. I suggest deleting this part.

Please better clarify the relevance of the four measures of TOF, knowing that they are separated by 15 seconds and that at T1, TOF=0, theoretically the other measures will also be equal to zero. Also, this data is not presented in the results section and is not discussed later.

Statistical analysis

The statistical description is "generic". It is suitable to describe for each tested endpoint, how it is presented and which statistical test is used, beginning with the main result (or the one that answers the most to the research hypothesis), then the secondary results.

If a sample size calculation was not performed, it is desirable to calculate the effect size (r) to estimate whether the sample chosen is appropriate or very small.

Also, if there was no sample size calculation, please specify that it is a convenience sample.

Please explain why did the authors choose a significant threshold of p at 0.01 (why not at 0.05)

Table 1 and 2, please specify how the parameters are presented? Mean (SD)? Table 1 and Table 2 could be summarized in the text and deleted.

Figures 2 and 3 are also not mandatory. I suggest to only keep Figure 4, adding to means the standardized deviations (column mean/error bars).
Figure 5 is also unnecessary.

Thank you for better explaining the added limit of the study.

The last paragraph of the discussion could be deleted too. The clinical impact of the study is not major. The study suggests that there is a significant delay of muscle relaxation in patients with aortic insufficiency, but in the presence of a monitoring of the curarization, the practitioner will most likely wait for a deep curarization. I think the importance of the study is to encourage the monitoring of curarization at induction and not to rely on the curare delay in patients with aortic insufficiency.

I agree with the reviewer’s suggestion who proposed to rewrite the manuscript in the form of short communication. The introduction includes parts that can be deleted, and the discussion has been enriched by a limitation of the study, but the explanation of this limit is unclear. The discussion too could be simplified. In addition, several figures and table are not essential.

I hope this will help.

Issam.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

Yes

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

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