

Dear Editor

World Journal of Clinical Cases

Re: Ms. Ref. Manuscript NO.: 69419, titled: "Serum magnesium as an indicator of acute kidney injury prediction in acute pancreatitis patients"

Many thanks for your letter of Aug 19th, 2021, with the reviewer's constructive comments on our manuscript. Accordingly, we've responded to each of the comments and revised our manuscript. It is our view that the changes have considerably improved the quality of the manuscript, and for that, we are grateful.

Reviewer #1

The authors have raised an important point, however the study methodology is weak. AKI in SAP patients is caused by a variety of factors. In a retrospective analysis, it is difficult to take into account all of them. Furthermore, in critically ill patients, serum magnesium levels are a dynamic measure that is affected by a variety of circumstances. As a result, a study relying on a single baseline value may yield incorrect conclusions. Because I couldn't find tables and figures in the text or attachment files, I couldn't make sense of the full paper. Has it gone missed in some way? The authors should specify how many patients with low serum magnesium levels developed AKI versus those with levels above the cut-off. What was the time between the detection of low serum magnesium and the onset of AKI? The grammar of English (verbs, tenses, and so on) needs to be improved. English grammar

(phrases, tense etc) needs improvement. The title “Serum magnesium as an indicator of acute kidney injury prediction in acute pancreatitis patients” can be better represented as “Serum magnesium level as an predictor of acute kidney injury in patients with acute pancreatitis .”

Response:

Thank you for your affirmation and constructive suggestions on my research. However, there may be some weaknesses in methodology (due to the nature of the study). I have already explained and elaborated on this point in the limitation part of the discussion section of this article. In addition, I have specified the numbers of patients with low serum magnesium levels developed AKI versus those with levels above the cut-off in the results section. At the same time, I added the explanation of the time between the detection of low serum magnesium and the onset of AKI into the methodology part. Thank you for your suggestions on the title of my article. I have revised it as prompted.

Finally, I have submitted the figures and tables to the submission system. I also accept your suggestion and revise and polish the grammar in the article.

Editorial Office's comments

The study aimed to evaluate the value of serum magnesium upon presentation of hospital in correlation with the incidence of AKI in AP. It is a retrospective observational approach. The results indicates that serum magnesium on admission is independently associated with AKI in AP and may serve as a potential prognostic factor. The study theme is very relevant. However, the methodological approach has weaknesses (due to the nature of the study.).

Thus, the results generated lose impact. I believe it is necessary for the authors to discuss the limitations of the study. What are the possible biases arising from the methodological approach? Moreover, the discussion of the study needs more elements to support the interpretation of the results.

Response:

Thank you for your constructive suggestions and full affirmation. I have already explained and elaborated on this point in the limitation part of the discussion section of this article. At the same time, I added relevant data in the results and discussion section to support and explain the analysis results.

Finally, I fully revised each part of the manuscript (such as references, tables, figures, etc.) and grammar problems according to your requirements and prompts, and polished the article.

We trust that the manuscript will now be acceptable for publication. Thank you again for all the editor/reviewers' efforts.

Yours Sincerely

Li Weiqin

Medical School, Southeast University