Reviewer’s report

Title: Low dose of flurbiprofen axetil decrease the rate of acute kidney injury after operation: a retrospective clinical data analysis of 9915 cases

Version: 0 Date: 22 Oct 2019

Reviewer: Khaled Shawwa

Reviewer's report:

In this study Wang et al investigated the impact of different doses of flurbiprofen axetil on the rate of perioperative AKI utilizing a large cohort of 9915 patients.

1. In the introduction, I suggest that the authors be more specific when describing the pathogenesis of AKI. It is unclear whether the authors are referring to AKI in the general population, hospital-acquired AKI or perioperative AKI.

2. The authors should report the exclusion criteria more clearly. If they excluded everyone who had a urologic or cardiac surgery, then the word "some" should be removed as it is confusing.

3. The authors looked at AKI rate within the first 7 post-operative days; however, exposure to flurbiprofen axetil was considered for the period from the beginning of anesthesia to 48 hours post surgery. It would be helpful to know the timing of the AKI as it is important to know the temporality between exposure and outcome to ascertain association.

4. It is helpful to know the stages of AKI, I suggest the authors do a sensitivity analysis for the different stages of AKI.

5. I suggest the authors define "renal insufficiency". Also along those lines, I suggest that the baseline serum creatinine be reported in all the groups.

6. The authors did not test for goodness of fit when using logistic regression.

7. While the authors mentioned that they excluded cardiac and urologic surgeries, there was no mentioning of type of surgeries done in each group. This can affect the use of flurbiprofen axetil and/or AKI.
8. I am not sure what is the rationale behind dividing the groups (exposure) to the respective dose ranges. I suggest adding a justification to the chosen groups. It might also help to see the distribution of the dosages among the population.

9. It is unclear if the groups represent cumulative dosage of flurbiprofen axetil, if so, the duration of repeated administration would be helpful to know. For example, would a person who receives 50 mg once a day for three days be considered in the low dose group or the middle group.

10. I understand that the authors have used the ASA classification; however, since AKI is affected by severity of illness, I wonder if the authors considered any score that can capture this potential confounder.

11. The authors mentioned that: "What we were more interested in was that low dose of flurbiprofen axetil perioperatively was an independent protective factor for postoperative AKI". This is an overstatement. It is important to report any subsequent use of any type of NSAIDs beyond the specified 48 hours. One might argue that the use of low dose flurbiprofen axetil controlled the pain and prevented the use of subsequent dosages beyond 48 hours. Again, it is important to report when the AKI occurred after administration of flurbiprofen axetil.

12. While I understand that the authors included fluid balance, bleeding and transfusion, they did not mention intra-operative hypotension as that can affect the rate of AKI.

Minor edits

1. I suggest rewording of the second sentence in "Data collection". As it stands, it is a run-on sentence.

2. Suggest the authors review the manuscript for grammatical errors.

3. It seems that the group who did not get flurbiprofen axetil were much more likely to have undergone emergent surgery. Yet, patients who had AKI were also more likely to have undergone emergency surgery. I suggest consider checking for potential interaction between emergency surgery and use of flurbiprofen axetil.

4. The authors refer to table 5, but this is probably an error.
5. It is unclear what is the grading criteria for the "surgery grade".

6. In some instances, Fisher's exact test was the appropriate test to do rather than Chi square.

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