Reviewer’s report

Title: The risk of upper gastrointestinal bleeding in patients treated with hemodialysis: A population-based cohort study

Version: 1 Date: 9 July 2012

Reviewer: Timothy Card

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My review is set out specifically to address the issues to which the journal directs reviewers. With the exception of the English language, which will be a matter of opinion, I consider all these matters to be essential revisions. The changes needed to analysis are major.

1. Is the question posed by the authors well defined?

Yes the question is clear, however it is not quite the one they have answered. The outcome assessed is not UGI bleeding, but rather a subset of non-variceal UGI bleeding. These bleeds would be a minority of UGI bleeds in many countries.

2. Are the methods appropriate and well described?

Methods seem generally reasonable if the question is rephrased to fit them, however they are not well described. When was age measured? Is the measure of medication use the total DDDs over the period of the study? Also the statistical methods are in my opinion inadequate. The authors have assumed a linear relationship between drug dose and outcome in all cases it seems. They have also apparently assumed that there is no effect modification between the groups. Given that they have failed to demonstrated that NSAIDs increase the risk of GI bleeding it seems unlikely that both these assumptions are correct. They need to rewrite the methods to allow reproduction of their results, detailing their modelling strategy as well as how and when all exposures are measured. They need to examine the possibility of non-linear relationships, and of effect modification appropriately.

3. Are the data sound?

I believe so.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?

No. The Strobe guidelines are not followed in respect to title or variable definition (as stated above).

5. Are the discussion and conclusions well balanced and adequately supported by the data?

No. For the most part the discussion is good, however the conclusion that "our
findings provide evidence that HD patients should be monitored more carefully for UGIB development and targeted with preventive intervention strategies" is not supported by the work. They present no evidence that such a strategy would be beneficial.

6. Are limitations of the work clearly stated?
   Yes

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?
   No. They need to reference the work which shows hypertension, diabetes, and hyperlipidaemia to be risk factors for GI bleed.

8. Do the title and abstract accurately convey what has been found?
   No. See above.

9. Is the writing acceptable?
   No. English is poor in places.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Not suitable for publication unless extensively edited

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

'I declare that I have no competing interests