Reviewer’s report

Title: Dual anti-platelet therapy following percutaneous coronary intervention in a population of patients with thrombocytopenia at baseline: A meta-analysis

Version: 0 Date: 12 Sep 2019

Reviewer: Serban Maierean

Reviewer's report:

Dear Authors,

Thank you for submitting this paper to BMC Pharmacology and Toxicology. It was truly a pleasure to read and addresses a very important topic. Many of our patients who suffer myocardial infarctions also have some degree of thrombocytopenia, and data on post-PCI management is crucial. The following are some of my thoughts and suggestions for changes that I think may improve your paper. I sincerely hope you find them helpful:

1. Introduction: Lines 24-33 - "It seems that the research community has neglected this high risk group of patients… proof for this ignorance by the research community till date" It may sound a bit more pleasant and perhaps more accurate to simply say that this group of patients have not yet been investigated as much, or something to that effect. Please bear in mind that we have only relatively recently demonstrated DAPT to be superior to single therapy in PCI patients, and even more recently studied DAPT in other diseases (e.g. stroke). Patients who are at a theoretically high risk in any intervention are generally studied last after all risks and how to mitigate them are clearly understood.

2. Discussion: Last paragraph (Pg13, lines 12-22) - Paragraph is one run-on sentence and it may benefit from being divided into a few sentences for easy readability. Also, do you feel comfortable suggesting which patient populations / clinical situations might warrant giving a thrombocytopenic patient aspirin or clopidogrel? If not, it would be helpful to know that more research is needed before you can draw a more firm conclusion about this.

3. One major issue that I feel needs highlighting is that patients who are thrombocytopenic (excepting perhaps mild ITP) are generally very sick at baseline, usually sicker than the average patient who suffers an MI (my thoughts are leaning towards patients with cirrhosis, leukemia, autoimmune disease, sepsis, taking medications / chemotherapy which reduce platelet count, etc.) and for whom risk of bleeding/stroke/all-cause mortality can be higher at baseline. I think it would be great if we had more data on these individual patient populations in order to better individualize therapy. For example, perhaps in patients with ITP alone, using DAPT might be overall beneficial, but not so in patients with cirrhosis. Does the data you display stratify by cause of thrombocytopenia? If not, how would you feel about addressing this point in the discussion of the paper?

Thank you again for your submission.
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?
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I am able to assess the statistics

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