Reviewer’s report

**Title:** Clinical impact of the perioperative management of oral anticoagulants in bleeding after colonic endoscopic mucosal resection

**Version:** 0 **Date:** 11 Aug 2019

**Reviewer:** Jessica Yu

**Reviewer's report:**

The authors have addressed some of my previous comments however the manuscript still requires clarification particularly in the methods and results section. The main merit of the study is the ability to compare the outcomes of patients on continuous warfarin, HBT and one-day skip DOACs; however, these results are buried in the study as it is currently presented.

1) The article needs significant editing for grammar and English fluency.

2) The methods section remains nebulous. For example - definition of the control group should be explicit in the text - were those patients on anything (antiplatelets?). Additionally, the authors do not specify which groups are being compared in their univariate and covariate analysis explicitly (for example, that anticoagulants were combined together).

3) How was the warfarin withdraw group analyzed - was this with the anticoagulant cohort or control cohort? Again this should be explicit in the text.

4) Tables 1 and 2 present descriptive statistics of two cohorts - bleed vs. non-bleed however, it would be more useful to compare the descriptive statistics of the groups: control, HBT, continuous warfarin and one-day skip DOAC, similar to what is presented in the first 5 rows of table 3 as these are the cohorts being compared. They should then present their univariate and multivariate analysis separately with bleed as the main outcome. As GIB is the only outcome, this could be easily presented graphically with odds ratios w/ 95% CI on the X-axis and independent variables/covariates on the y-axis.

5) The strength of this paper is in the fact that the authors are able to capture actual anticoagulant use (ie continuous warfarin vs. HBT vs. 1-day doac cessation vs control), this results currently seem buried. The paper would be significantly strengthened if the univariate and multi-variate analysis were conducted using these cohorts as opposed to a combined anti-coagulant cohort. If this cannot be done, they should be discussed both as a limitation and in their methods. Currently, the multivariate analysis as presented is redundant and confusing and may be simplified by just presenting the GIB rates of continuous warfarin/HBT/1day doac cession vs control.

6) The authors conclude that 1-day DOAC skip would be clinically acceptable in patients as peri-operative management of patients not on HBT however this conclusion seems to be
an over-reach as they do not have any patients who went from continuous warfarin to 1-day skip DOAC especially as the authors themselves describe this study as a pilot study and it is not clear if patients were switched off warfarin to 1-day stop DOAC therapy. The conclusion should be rewritten with this in account.

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

No

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

Unable to assess

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

No

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

Quality of written English
Please indicate the quality of language in the manuscript:

Not suitable for publication unless extensively edited

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