Reviewer’s report

Title: Reliability of pleth variability index in predicting preload responsiveness of mechanically ventilated patients under various conditions: a systematic review and meta-analysis

Version: 0 Date: 25 Feb 2019

Reviewer: Alberto Fogagnolo

Reviewer’s report:

The authors conducted a meta-analysis focused on Reliability of pleth variability index in predicting preload responsiveness; even if the topic is widely studied in literature, this meta-analysis offers an update from latest available (25 studies vs 18 described in DOI: 10.1007/s10877-015-9742-3) I have some major comments for authors; Firstly, many phrases are difficult to follow due to English problems (ex "Our new discovery is the result of patients without undergoing surgery subgroup (AUC=0.86, Youden index=0.65) was reliable"). Please consider an extensive review by a native speaker. Secondly, the advantage offered by an other meta-analysis in this topic needs to be better underline

Other comments

Introduction

Page 3 line 52. Please change "Inappropriate rehydration" with inappropriate fluid administration

Page 3 line 56. Please briefly specify how patient prognosis can be improved (i.e. which outcomes were considered)

Page 4. Please add a reference to the phrase "non invasive monitoring instrument is more acceptable to the patient than invasive monitoring"; furthermore, I would change the word "acceptable"; invasive monitoring is still "acceptable" indeed (particularly minimally invasive hemodynamic monitoring).

Please delete any refer to brand or industries from introduction

Results

Please do not report p =0.000 but p<0.001 (or as many zero as calculated)

Page 8: "The Cochrane-Q of the AUC was 39.175". I can not understand, please revise (typo?)

Page 9: "however, following the exclusion of the 17 studies". The reason of this exclusion is unclear
It is totally unclear to me how heterogeneity grown up with the exclusion of the studies which contributes to heterogeneity (from 84 to 95%). How do the authors justify this result?

Discussion

Page 11: "which may be because of the small changes of the patient's peripheral perfusion." Please better clarify this concept

Page 11. "There was no significant heterogeneity in the subgroups of patients without undergoing surgery (I²=33%), which may be because there was no effect of surgery on patient's hemodynamic changes." How do the authors reach this justification? It is quite strange that in the time of the responsiveness tests (usually very short) surgery can affect so much patient's hemodynamic

Page 11. "potentially because of the small sample size (n=4)" please change with the number of study rather than sample size

Page 12. Please revise "perfusion situation"; I propose adequacy of perfusion

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?
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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