Author's response to reviews

Title: Noninvasive Cardiac Output and Blood Pressure Monitoring cannot replace an Invasive Monitoring System in Critically Ill Patients

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Author's response to reviews: see over
Dear Editors

We would like to resubmit the attached manuscript titled “Noninvasive Cardiac Output and Blood Pressure Monitoring cannot replace an Invasive Monitoring System in Critically Ill Patients” for publication in the BMC Anesthesiology Journal. We have thoroughly addressed all the issues raised by the reviewers and are enclosing a detailed point-by-point reply to all their comments. We believe that the reviewers’ remarks helped to improve the quality of the manuscript and hope that it will be accepted for publication.

We herewith state that (1) the paper is not under consideration elsewhere, (2) none of the paper’s content has been previously published, (3) all authors have read and approved the manuscript, and (4) no potential conflicts of interests are present for any of the authors. We would appreciate if you could consider our paper for publication and are looking forward to your kind answer.

Yours sincerely

Markus Béchir, MD
Reply to the reviewer’s comments

Reviewer #1:

1. “The authors need to pay some attention to the English grammar in their report”: We carefully revised the whole manuscript. We hope that we could improve the English grammar in this revision to an acceptable level.


3. “To what extent did the use of norepinephrine (NE) contribute to the wide SD and % errors for both MAP and CO?”: Unfortunately we can only speculate that there was such an influence, but with our small sample size we cannot answer this issue.

4. “Was NE used on all patients?”: There was NE used in all patients. We have now mentioned this fact in the paper in the result section. (All patients required norepinephrine as a vasoconstrictor agent for hemodynamic stability, the mean norepinephrine dose during data assessment was 12±12µg/ min (range 2-29 µg/min),…)

5. “I realize this is speculation but might there be a place for the noninvasive technique in a selected group of ICU patients…”: As you speculate we also can speculate that the results of non-ICU patients of this device might be reproducible in less severe critically ill patients without high doses of NE. Nevertheless with our sample size we cannot give a correct answer and this issue remains speculation and must be determined in further investigations.

Reviewer #2:

1. “Question 1. The abstract needs to be significantly rewritten…”: Thank you for your comment which indeed points out a weakness of our work. We have rewritten the whole abstract and also included a specific question. Furthermore, we included more about analyzing blood pressure as you have suggested.

2. “Question 2. The method section of the abstract does not describe any methods…”: As we mentioned above, we have significantly rewritten the abstract and have also included the method. This clearly has improved the quality of the paper.

3. “Question 3. The data appear to be sound and specifically the cardiac output data is very interesting and could provide a better emphasis for this paper as there are plenty of commonly used and presumably reliable non-invasive blood pressure monitoring devices.”: This is a statement and no question. Therefore, we cannot give an answer.

4. “Question 4”: This is the same as #3 (no question).

5. “Question 5. For the amount of data presented, the discussion is too long, and a lot of results are incorrectly included in the discussion.”: In this revision we have reduced the discussion part of our manuscript; especially we reduced the amount of speculation. We feel that this has improved the clarity of our work. Nevertheless we still mention the rivers study because its importance in intensive care medicine.

6. “Question 6. There is no statement about the limitations of this work and the most significant one, in my view, is the number of patients.”: We included the following
statement into the discussion section of the paper, which surely further improved the
quality of our paper: “Study limitation: The sample size is small and therefore the
results must be interpreted very carefully. Interestingly the original work of
Bland and Altman which describes the statistical method used in this paper[16]
based of a sample size of 17 subjects with 34 data points overall. Under this point
of view our sample size is surely small but with 80 data points we included twice
as much data points into our analysis. Thus, our sample size should allow such an
analysis with a careful interpretation.”

7. “Question 7”: I think the article is generally well referenced.”: Thank you for your
statement.

8. “Question 8”: I don’t think the abstract and title accurately convey what was found.
Importantly, all their conclusions are based on the targets that have been identified as
part of early goal-directed therapy...”: As we already mentioned we have rewritten
the abstract and reduced the speculation part of the rivers study. Nevertheless we think
the title represents our finding. This is the reason we did decide not to change the title
itself.

9. “Question 9. The writing needs work in terms of clarity, the methodology and the
abstract, and many of the statements included in the discussion are not accurate. They
are not supported by the data.”: Thank you for your clear statement. As you can read
in the answers to the questions 1, 2 and 5 we have significantly rewritten the abstract
and shortened the discussion.

10. “Further suggestions: The non-essential revisions have to do with making sure that
the initial abstract better reflects the data that is presented on both cardiac output and
blood pressure. I also feel that a better description of the physiology of the patients,
specifically how many were on norepinephrine, needs to be better delineated...”: As
already mentioned we have rewritten the abstract and made many changes according
to your suggestions. Furthermore we have included a better description of NE use (see
point 4 reviewer #1).