Author’s response to reviews

Title: Validation of the Imperial College Surgical Assessment Device for spinal anesthesia.

Authors:

Marcia Corvetto (marciacorvetto@gmail.com)
Carlos Fuentes (cfuentes812@gmail.com)
Andrea Araneda (aranedaandrea@hotmail.com)
Pablo Achurra (achurrapablo@gmail.com)
Pablo Miranda (pablofmh@gmail.com)
Paola Viviani (paviviani@uc.cl)
Fernando Altermatt (fernando.altermatt@gmail.com)

Version: 2 Date: 16 Aug 2017

Author’s response to reviews:

August 16th, 2017.

Dear Dr. Schaefer,

Thank you so much for the opportunity to revise again the manuscript. Your helpful comments and suggestions have been essential to improve it. We have addressed each of your concerns systematically, reiterating each comment and following it with our response.

Manuscript revisions have been highlighted in color, as requested.
Point-by-point response to specific comments:

Lidia Mora Miquel, M.D.:

1. - There are still some few typographical errors in the manuscript (Introduction, above all).

   Thanks. We have required proofreading and edited the manuscript in order to correct typographical errors.

2. - I think the description of the Post Hoc Power Analysis could be better added to the Methods and just mention it in the Results section.

   We have changed the explanation of the Post Hoc Power Analysis to Methods.

3. - The discussion has improved significantly, and now it is much better understood the purpose of the study, and better illustrated. So the conclusion meets the ultimate goal of improving the quality of medical education in Anesthesiology.

   Thank you for the input.

4. - The bibliography and the tables are clear and well organized, with better format after the revision. There is just one mistake in the Figure legends: Figure 3 description is missing and then there must be corrected in the right order, because a picture was included and now they must be reordered.

   We have edited the legends of figures.

5. - In my opinion, the Appendix 1-2 will be appended to the text as long as the editor complies. They are merely descriptive and could be obviated without subtracting information from the manuscript as they are supplementary material. In case they could be added, their names need to be attached to the tables. Appendix 1 name is missing.

   We have added the names to the tables.

6. - I still think that this work could emphasize more its usefulness to improve the quality of teaching of residents based on competences and not be so focused so much on the methodology of the validation of an assessment device which is not widespread or within
reach of all training centers. Anyway, it brings a kind of interesting novelty and it is well done. So in my opinion, after all the revisions, it could be published.

Thank you for the input.

Dan Benhamou (Reviewer 2):

To the Editor

I have read the revised version of this article.

The authors have done a nice job and have adequately answered to questions and comments from the reviewers.

I believe that the paper can now be accepted for publication.

Best regards. Pr Dan Benhamou

Thank you for the input.

Klaus Hofmann-Kiefer (Reviewer 3):

1. Concerning novelty, the authors were able to clarify the most important differences between the current study and previous investigations, especially Hayter's and Chin's more clearly. Though one cannot deny the presence of various similarities, the reviewer now agrees that the design of the present study implies a number of new features (first time test of spinal anesthesia, use of a simulated torso, some new statistical comparisons) rectifying publication.

Thanks for the input.

2. Concerning statistics:

It may be appropriate to assume a not normally distribution of data given the small number of participants in the study without doing an adequate test; however, if doing so, this should be mentioned in the beginning of the statistics section.

We have added this explanation in the statistics section.
3. The "power analysis" problem has been extensively commented by the authors and the manuscript's text has been improved. Nevertheless, to state that a power analysis could not be done because there were no pre-existing data, conducted by other investigators (Item 6. in the comments to the reviewer) is not a valid argument in my eyes. Results published by others can never be the base of a power analysis when planning a study, simply because usually one does not have the original data sheets. To compensate for the missing power analysis the authors now present an improved post hoc power analysis. This seems to be a good idea on first site. However, the reviewer's statistical expertise is not sufficient to judge whether the addition of a rank estimation (TPL-rank) really improves this post hoc power analysis and suggests that the author's search the help of a professional statistician of their institution and include this person in the authors list. Further statistical problems mentioned in the first review have been satisfactorily answered.

We have included in the author list one of the statistician of our institution who helped us to perform the analysis.

3. Concerning figure legends and graphics

The authors claim to have improved the figure legends, however I cannot see any difference to the first version. Normally, there is a certain number of "readers", who simply take a look on an article's graphics before deciding whether to read the text or not. Thus, in the reviewer's opinion it would be an improvement if statistical differences between groups would not only pointed out in a table, but would be symbolized in the figures, too.

We have added the statistical differences between groups in figures.

4. Concerning the interpretation of the results

All in all the interpretation of the study results has improved in the current version. However, the most serious flaw the reviewer has pointed out remains: The ICSAD failed to differentiate between the study groups. Though the authors extensively referred to this problem in their comments, as well as in the text, it has not been sufficiently addressed in the reviewer's opinion. To explain the phenomenon, the authors claim that the group of "intermediates" was already too well trained (experienced non-experts) to generate ICSAD-detectable differences in comparison to the real experts. After three years of training this might be the case - but if so, there should be a clearly detectable difference to the group of novices. However, when looking thoroughly at the data it becomes clear that ICSAD also failed to detect a difference between novices and intermediates during "needling phase" (Table 4). ICSAD did not find differences in any of the three dexterities in this period, which certainly is the most important phase of the whole process.
Significantly, this is not mentioned in the text, neither in the results, nor in the discussion. As a consequence, in the reviewer's opinion it is indispensable that a possible failure of ICSAD as a method to differentiate between skill levels has to be included in the discussion. And again: That's not a limitation, but an important result.

A possible failure of ICSAD as a method to differentiate between skill levels was included in the discussion.