Reviewer’s report

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

Version: 1 Date: 09 Jul 2017

Reviewer: Klaus Hofmann-Kiefer

Reviewer's report:

Comment on the first revision of manuscript:

Since the first version the manuscript has remarkably improved in many ways and the majority of the reviewer's requests have been fulfilled.

Nevertheless, there remain some topics to discuss:

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.

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.

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.

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.

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.

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.
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 recommend additional statistical review

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Please indicate the quality of language in the manuscript:

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