Reviewer’s report

Title: Adaptation of Chinese and English versions of the Ankylosing Spondylitis Quality of Life (ASQoL) scale for use in Singapore

Version: 1 Date: 08 Jun 2017

Reviewer: Peter Klooster

Reviewer’s report:

The authors have reasonably dealt with most of the previous issues raised in the reviews. Moreover, the addition of the Rasch analysis makes the manuscript more interesting and informative. However, I am still of the opinion that the aim of the study (as mentioned in the abstract) is too ambitious and the overall conclusion much too far-reaching given the current study design, analyses performed and findings. I believe these need to be formulated more conservative before the manuscript can be considered for publication.

As I also mentioned in my previous review, to develop versions of the ASQoL that are really "equivalent" to the original requires much more rigorous analyses (e.g., CFA or IRT analysis of measurement invariance or absence of DIF between both the original UK and the Singapore Chinese and Singapore English versions of the scale) which are probably not feasible given the current sample size.

I think that based on the current study design and analyses you could claim at the max something like that the aim was to develop culturally appropriate Singapore Chinese and Singapore English versions of the scale and to provide a preliminary evaluation of the construct validity and reliability of the translated scales in AxSpA patients. Likewise, concluding that the scales can be used in Singapore for clinical trials or longitudinal studies based on the current analysis and results is still overly optimistic. For both applications, it needs to be shown that the scale, besides reliable and having adequate construct validity, is responsive to real changes in the construct of interest, and this has not been examined in the current study.

The addition of Rasch analysis (and comparison of DIF between the Singapore Chinese and Singapore English only) that the authors use as an argument in their reply for having dealt with these two issues, does not in my opinion change these fundamental issues.

Additionally, I have two concerns about the section that was added about the sample size considerations on page 11-12. This section includes a support for a "decision to include" 100 patients for the cross-sectional study (based on rules of thumb for the Rasch analysis only, which was not even part of the previous version of the manuscript) and a "consideration of" 30 patients for the test-retest study.

First of all, I'm not convinced that these sample size calculations that are now reported were really performed a-priori, while the authors did write these down as if this is the case. It seems highly likely that these are actually more pragmatic, "post-hoc" power analyses trying to support
the adequacy of the relatively small number of patients they recruited for both studies. This is in itself not necessarily wrong, but if this is indeed the case, it is important that the authors make this explicit (i.e., that no formal a-priori power analyses were performed) and move this section for instance to the discussion where this can be discussed in relation to other potential strengths and/or limitations of the sample(s).

Second, the sample size calculation added for the test-retest study does not make sense. The authors added a sample size calculation based on determining if a certain correlation (in this case an "expected" $r=0.85$) is significantly different from zero. Obviously, this is not appropriate in this context (it would be very disturbing if any test-retest coefficient would NOT be significantly different from zero!) and results in an extremely low required sample size of $n=8$. This is clearly not an appropriate sample size for any reliability study. A more appropriate sample size calculation would be for instance based on a reasonable width of the confidence interval around a certain value. Such a more realistic calculation would result in a required sample size much closer to the minimum standards generally suggested for test-retest studies (usually $n>50$).

**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 am able to assess the statistics

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

Needs some language corrections before being published

**Declaration of competing interests**
Please complete a declaration of competing interests, considering the following questions:
1. Have you in the past five years received reimbursements, fees, funding, or salary from an organisation that may in any way gain or lose financially from the publication of this manuscript, either now or in the future?

2. Do you hold any stocks or shares in an organisation that may in any way gain or lose financially from the publication of this manuscript, either now or in the future?

3. Do you hold or are you currently applying for any patents relating to the content of the manuscript?

4. Have you received reimbursements, fees, funding, or salary from an organization that holds or has applied for patents relating to the content of the manuscript?

5. Do you have any other financial competing interests?

6. Do you have any non-financial competing interests in relation to this paper?

If you can answer no to all of the above, write 'I declare that I have no competing interests' below. If your reply is yes to any, please give details below.

I declare that I have no competing interests

I agree to the open peer review policy of the journal. I understand that my name will be included on my report to the authors and, if the manuscript is accepted for publication, my named report including any attachments I upload will be posted on the website along with the authors' responses. I agree for my report to be made available under an Open Access Creative Commons CC-BY license (http://creativecommons.org/licenses/by/4.0/). I understand that any comments which I do not wish to be included in my named report can be included as confidential comments to the editors, which will not be published.

I agree to the open peer review policy of the journal

Do you want to get recognition for reviewing this manuscript? Add a record of this review to Publons to track and showcase your reviewing expertise across the world's journals. Signing up is quick, easy and free!

No