Reviewer's report

Title: Development and Evaluation of a Quality Score for Abstracts

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Reviewer: Prof David Moher

Level of interest: A paper of considerable general medical or scientific interest

Advice on publication: Unable to decide on acceptance or rejection until the authors have responded to the compulsory revisions

In responding to the authors I have reviewed my original peer review, their responses and the revised draft.

I don't think I indicated that the instrument was developed in the context of systematic reviews. The points I made in my original peer review were relevant to issues of quality assessment and not specifically to quality in the context of systematic reviews. The concept of quality is important inside and outside of systematic reviews. My comments are directly related to the development of the instrument itself regardless of where it is used.

The authors keep making the point that the issues I brought up are more relevant in the context of systematic reviews. This is inaccurate. The conceptual issues around quality are important in and of themselves. It so happens that examples of the conceptual problems have been nicely demonstrated within systematic reviews. This should not be construed with the basic issues/problems of assessing quality.

There were a number of issues I made at the bottom of page 2 of my original comments (starting with "The authors are silent on how they ............") that do not appear to have been addressed by the authors. I think they are relevant comments and need comment on. Additionally, if I am an average reader I think these points need to be incorporated in to a revised discussion section. Am I alone in finding it unusual to give similar weighting to random allocation and reporting of p values?

The authors have not addressed issues of combining assessing "methods" and "reporting" adequately. This issue is relevant conceptually to assessing quality be it in the context of systematic reviews or not. The Juni paper is an example of the problem within the context of systematic reviews. The concept of combining methods and reporting in a single instrument is not new (even though the Juni paper was published after their research). The authors should be more explicit about this issue in the revision.

The authors point out in their discussion that "appropriate allocation concealment could not be evaluated...". That is correct. Given the authors sample (1992 t o1995) was prior to the publication of the Schulz et al paper it would not be possible to evaluate. Can't the authors simply state this fact?

The authors should add the first few lines of point 7 (in their covering letter) to their methods section
of the manuscript. Ditto for point 9 of the cover letter.

Dr. Cho is not in Philadelphia. I made this point in my original review.

**Competing interests:**

None declared.