Reviewer's report

Title: The assessment of the quality of reporting of meta-analyses in diagnostic research: a systematic review.

Version: 4 Date: 28 October 2011

Reviewer: Rob J Scholten

Reviewer's report:

Dear Dr Aldcroft,

You asked me to have a look at the manuscript "The assessment of the quality of reporting of meta-analyses in diagnostic research: a systematic review" by Willis and Quigley and its history and to provide advice with regard to the disagreement between one of the reviewers and the authors.

I first looked at the second revision of the manuscript which I found well written and easy to follow. The authors clearly indicate what choices they have made for their review process and to me, the discussion and conclusions are in line with the presented results and methods applied. (The only (minor, discretionary) issue which I struggled with was the label “meta-analysis” instead of “systematic review”. A systematic review may include more than one meta-analysis, so the label “meta-analysis” confuses me somewhat and I would suggest to use “systematic review” throughout.)

Then, I looked at the peer-reviewers' comments regarding the first version and the two revisions (only assessed by referee 1).

I fully understand the issues that the referee 1 raises. The authors applied selection criteria that may have been associated with three PRISMA items and thus may have lead to an overestimate of the quality of the included reviews. I may have missed the point completely, but notwithstanding this “biased” selection, the quality of reporting of the included reviews still is rather low and there seems to be much room for improvement. So to me, this “bias to the null” may make the conclusions regarding reporting quality even stronger (which conclusions to me are a useful message for both authors of reviews and journal editors). The authors, however, may wish to discuss the effect of this selection on the RR near 1 with a very small 95% CI for item 7 (and others), as raised by referee 1.

I don’t think that referee 1 was implying this, but to me, it is not fair to ask from the authors to change the eligibility criteria and stop with step 2 and include 751 reviews. This won’t alter the conclusions of the study.

I feel that the authors have adequately addressed the concerns of reviewer 1. They have replied in a sensible way to the concerns of referee 1 and when they could mention the above-mentioned issues in their paper. In that way they can
provide the reader with some more guidance to the validity of the research findings so that the reader is aware of the limitations as raised by the referee.

Finally, to me the manuscript is sufficiently sound.

I hope that my comments are useful.

Kind regards,

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Declaration of competing interests:

I declare that I don't have any conflicts of interest.