Author’s response to reviews

Title: The effectiveness of computerized clinical guidelines in the process of care: a systematic review

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Author’s response to reviews: see over
The Editor,
BMC Health Services Research

Dear Editor,

We would like to thank you for revising our manuscript. Your critical comments helped us to improve our article significantly.
Please, find below our point to point responses to the comments.

Yours Sincerely
Gianfranco Damiani, Luigi Pinnarelli, Simona Carmela Colosimo, Roberta Almiento, Lorella Sicuro, Rocco Galasso, Lorenzo Sommella, Walter Ricciardi.

1) It looks like the original Table 4 is still in despite the authors saying they have dropped it (and a reviewer asking for this).

Taking into account your last advice, table 4 was dropped in the revised manuscript sent on 1st December.

2) It would also be good if they could add the list of excluded studies asked for by the reviewer as an additional file to the submission rather than just a list in their response letter.

The list of excluded studies is reported as additional file 1.

3) More importantly, I don't think the author's adequately address the reviewer's comment about using a scale to exclude studies. My interpretation of the Cochrane Handbook's recommendation for judging the methodological quality of studies is that scales and scores are discouraged. Here's some text I've just cut out of the Handbook (5.0.1):

'8.3.3 Quality scales and Cochrane reviews
The use of scales for assessing quality or risk of bias is explicitly discouraged in Cochrane reviews. While the approach offers appealing simplicity, it is not supported by empirical evidence (Emerson 1990, Schulz 1995b). Calculating a summary score inevitably involves assigning weights to different items in the scale, and it is difficult to justify the weights assigned. Furthermore, scales have been shown to be unreliable assessments of validity (Jüni 1999) and they are less likely to be transparent to users of the review. It is preferable to use simple approaches for assessing validity that can be fully reported (i.e. how each trial was rated on each criterion).'

So, if the authors have some text from the Handbook that explicitly supports their use of a threshold to reject studies then they should refer to the chapter heading and entry, at least in a response to the Biomed editorial team. They also need to give a justification of their cut-off for inclusion of 5 as opposed to, say, 6. If it is arbitrary they should say so.
The method for evaluating quality in our analysis is based on Chalmer’s tool, updated according to section 8.3.1 (Types of tools) of Cochrane Handbook. In fact, we evaluated only items related with internal validity of selected studies as suggested in the Cochrane Handbook (see page 5 Quality assessment section of our manuscript: “The methodology of each study was assessed independently by two authors according to a score assessing five potential sources of study bias”).

Every item has a reference in domain based evaluation, presented in Table 8.5.a “The Cochrane Collaboration’s tool for assessing risk of bias”

We did not calculate a summary score to determine a quantitative value for quality judgement but our score is comparable with yes/not strategy presented in Cochrane Handbook (section 8.5.3 The judgement). Our evaluation method was slightly different because we introduced an intermediate level of risk of bias, so the yes/not evaluation was converted in 0/1/2 evaluation. (0=High Risk of Bias 1=Medium Risk of Bias, 2=Low Risk of Bias).

Our summary score was determined only to differentiate studies at low risk of bias from studies at high risk of bias. We defined a threshold to identify studies with potential failure in key bias domains (less than 3 domains at low risk of bias), as indicated in Cochrane Handbook section 8.8.3.1 (Possible analysis strategies) at the point 2. “Primary analysis restricted to studies at low (or low and unclear) risk of bias. The second approach involves defining a threshold, based on key bias domains (see Section 8.7), such that only studies meeting specific criteria are included in the primary analysis.”

4) I don't think the reviewer was talking about meta-analysis when he asked about the lack of effect sizes; I think he was talking about the effect size of each individual study. I can't find this information for any of the 45 included studies and a table including this would, I suspect, be the information the reviewer is looking for. Without this it makes it hard to make judgements about the authors' classification of positive or negative effects for each study. In other words, 'defined as positive when reported improvement was more than 50% of the outcomes' is a bit hard to judge when we never see the outcomes. The authors must have this information so it odd that they don't present it.

We appreciated your clarification about the meaning of “effect size”. Taking into account your precious suggestions we enclosed the list of outcomes of each included study in additional file 2.