Author’s response to reviews

Title: A Comparison of Depression Prevalence Estimates in Meta-Analyses based on Screening Tools and Rating Scales versus Diagnostic Interviews: A Meta-research Review

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Author’s response to reviews:
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Alessandro Recchioni, BSc
Senior Editor
BMC Medicine

Dear Mr. Recchioni:

Please accept for review the revision of our manuscript “A Comparison of Depression Prevalence Estimates in Meta-Analyses based on Screening Tools and Rating Scales versus Diagnostic Interviews: A Meta-research Review”.

We have responded to the editor and reviewer comments, and the manuscript has been revised to reflect these responses. Point-by-point responses to each of the comments are noted in our author response document, as are notations of the places in the text where changes have been made to address the reviewers’ suggestions. Per your instructions, we have submitted a marked copy of the revised manuscript that shows the changes we have made. Please note that references to line numbers in our responses refer to the clean version and not the track changes version of the revised manuscript.
We believe that the revised manuscript has addressed the reviewer comments and hope that you agree that it is now ready for publication in BMC Medicine. Thank you for your consideration of our manuscript.

Sincerely,

Brett D. Thombs, Ph.D.
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Editorial Board Member

1. In my view, the authors make an interesting and valid point to start with (overestimation of prevalence). However, they make an error regarding the format in which they present this point. Viewing this as an empirical study, they should use terms like "comparing prevalence rates of self-report versus interview-based depression ratings".

We thank the member of the editorial board for pointing out the scientific validity of the work that we have presented, as well as its importance. We have edited the title, abstract and text to address the point about how it is presented. In all places, we have carefully edited to underline that theoretically screening tools and screening cutoffs would be expected to overestimate depression prevalence, but that this has not been shown, and that we have examined published research studies to determine the degree to which findings are consistent with theoretical expectations.

2. I agree with the authors that this is an interesting and relevant point to make, and with the reviewer that there can be drawbacks to interviews as well and that sometimes self-report instruments can identify relevant cases that are not picked up in interviews.

We agree fully that there are important uses of self-report questionnaires and drawbacks to interviews. Self-report questionnaires have important uses, although based on simple calculations based on sensitivity and specificity, and borne out in our study, they should not be used to estimate disorder prevalence. To underline that they do have important uses, we have added the following text to the abstract, “Depression symptom questionnaires are commonly used to assess symptom severity and as screening tools to identify patients who may have depression. They are not designed to ascertain diagnostic status, and, based on published sensitivity and specificity estimates, would theoretically be expected to overestimate prevalence.”

In addition, in the introduction (Lines 78-81), we now state, “Depression symptom questionnaires have important uses. They are commonly used for the assessment of symptom
severity, regardless of diagnostic status, and as screening tools to identify people who may have depression based on scores above cutoff thresholds.”

3. I would invite the authors to resubmit a revised manuscript. They should present the study more cleanly in title (not: ”misuse”), abstract and intro, giving an empirical view of the findings throughout the manuscript, but are free to give interpretations in the discussion.

We thank the member of the editorial board for this suggestion. We have revised the title to read “A Comparison of Depression Prevalence Estimates in Meta-Analyses based on Screening Tools and Rating Scales versus Diagnostic Interviews: A Meta-research Review”.

In addition, we have carefully edited the abstract and introduction to present what we know theoretically and what we have examined in the present study. We believe that this accurately presents what is known theoretically based on expected prevalence using known sensitivity and specificity values, why one would thus expect self-report questionnaires to overestimate prevalence, and that we have examined this in published meta-analyses.

Reviewer 1

1. In this revised manuscript and the accompanying author responses, a number of minor issues identified in review are addressed, and I appreciate this attention to detail. Major concerns remain, however, regarding the appearance of an a priori foregone conclusion regarding the relative merits of structured diagnostic interviews versus rating scales as case ascertainment methods. I see great value in the comparisons that are made between these methods, and the data do indeed appear to support this comparison. However, the dominant theme of the paper, reflected still in the revised title, is one of a judgment of quality, appropriateness, and validity that is not directly assessed by the data presented.

We have modified our title to more accurately reflect the research question that our manuscript addresses. We have also added statements in the abstract and introduction about the value of self-report questionnaires when used for their intended purposes. In addition, we have underlined in the abstract and introduction that evidence from screening accuracy studies (sensitivity and specificity) provides a theoretical rationale to expect that screening questionnaires would overestimate the prevalence of disorders. Finally, we highlight that no previous studies have tested this, and that we examined whether theoretical expectations were borne out.

Reviewer 2

1. I think the authors did a great job in responding to the reviewers' comments overall. Regarding the response to my request of a graphical representation of the mean with-in difference across assessment methods, I would still like to see a forest plot or some other
graphical presentation of the with-in mean differences (12 studies). I think this will benefit the manuscript and its readers.

We thank Reviewer 2 for this suggestion. As suggested, we have added forest plots to illustrate the within meta-analysis comparisons of (a) screening tools or rating scales versus interviews and (b) screening tools or rating scales versus a combination of methods.