01-Apr-2021
BMJ-2020-063284.R1
Accuracy of the Hospital Anxiety and Depression Scale Depression Subscale (HADS-D) to Screen for Major Depression: Systematic Review and Individual Participant Data Meta-analysis

Dear Dr. Thombs

Thank you for sending us your revision. We are still interested, but important aspects of the work still need clarifying.

We hope very much that you will be willing and able to revise your paper as explained below in the report from the manuscript meeting, so that we will be in a better position to understand your study and decide whether the BMJ is the right journal for it. We are looking forward to reading the revised version and, we hope, reaching a decision.

When you return your revised manuscript, please note that The BMJ requires an ORCID ID for corresponding authors of all research articles. If you do not have an ORCID ID, registration is free and takes a matter of seconds.

Yours sincerely,

Timothy Feeney MD MS MPH
Research Editor
The BMJ
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To start your revision, please click this link or log in to your account: *** PLEASE NOTE: This is a two-step process. After clicking on the link, you will be directed to a webpage to confirm. ***

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Decision: Put points

First, please revise your paper to respond to all of the comments by the reviewer. Their reports are available at the end of this letter, below.

Please also respond to these additional comments by the committee:

There seems to be high similarity with a similar review on PHQ they published in JAMA

>>Unless I am mistaken, the limitations section is almost identical to the paper on PHQ-9, or which some were limitations you mention that you could not expect. However, given the previous study, why didn't you anticipate these limitations. Further, how were the limitations in studies so similar given you did a review of a different measure.
for instance the JAMA paper says: “Subgroup analyses based on medical comorbidities, as specified in the study protocol, and on country and language could not be conducted. This is because data on presence of non-psychiatric medical diagnoses were not available for 40% of participants, with higher percentages missing for specific diagnoses, and because many countries and languages were represented in few primary studies.”

and this paper says: “Subgroup analyses could not be conducted based on medical comorbidities with the exception of cancer diagnosis, as specified in the study protocol, and on country and language. This is because data on presence of non-psychiatric medical diagnoses were not available for 40% of participants, with higher percentages missing for specific diagnoses, and because many countries and languages were represented in few primary studies”

This seems uncanny. Please revisit this section and the remainder of the paper to avoid this and assure that the writing represents the study conditions and results from this analysis.

In your response please provide, point by point, your replies to the comments made by the reviewers and the editors, explaining how you have dealt with them in the paper.

** Comments from the external peer reviewers**

Reviewer: 1

Comments:
Thank you to the authors for their detailed responses to my comments and for edits to the manuscript. Comments are all well addressed.

Just one minor follow-up suggestion - the authors have clarified the rationale for the comparison made in Supplementary Table F and that confidence intervals were calculated by bootstrapping to account for the overlap of participants in the comparison. Please add a footnote to clarify that confidence intervals were calculated by bootstrapping. I don’t think this is mentioned elsewhere in the manuscript.

Additional Questions: 
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Institution: University of Liverpool

Reimbursement for attending a symposium?: No

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