Author's response to reviews

Title: Psychometric properties of the Cardiac Depression Scale in patients with coronary heart disease

Authors:

Litza A Kiropoulos (litzak@unimelb.edu.au)
Ian Meredith (Ian.Meredith@myheart.id.au)
Andrew Tonkin (Andrew.Tonkin@monash.edu)
David Clarke (David.Clarke@monash.edu)
Paul Antonis (Paul.Antonis@monash.edu)
Julie Plunkett (Julie.Plunkett@monash.edu)

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Author's response to reviews: see over
Dear Professor Majithia,

I am enclosing the revised version of the manuscript entitled ‘Psychometric properties of the Cardiac Depression Scale in patients with coronary heart disease’ including all the revisions made to this revised manuscript suggested by the three reviewers which are also listed below. We hope the manuscript will now be considered for publication in the BMC Psychiatry journal.

Yours sincerely,

Dr Litza Kiropoulos

Lecturer in Clinical Psychology

University of Melbourne and Royal Melbourne Hospital
Major Compulsory Revisions:

Reviewer 1:

As noted in the discussion, one of the limitations of the paper is the comparison to other depression scales rather than to diagnose based on structured interview. Supplemental Table 3 might suggest the CDS over-diagnoses depression as less than ½ the sample was identified as non-depressed. This lends some question regarding the specificity of the test, or at least the selected threshold, and should be discussed.

We have addressed the issue of specificity of the CDS in the discussion and limitations sections.

Conclusion, final sentence, please reframe given the limitations of the study. Given the lack of an interview to confirm diagnosis, it does not seem prudent to comment on the sensitivity or specificity of the scale. It would seem the unique contribution of this study relates more to the application of this scale to a slightly different population with heart disease than other studies.

The final sentence has now been edited.

Minor Essential Revisions:

Background, paragraph 1, please rephrase the ‘as a consequence’ following reference [18] as it implies that this has been established to mediate the relationship between depression and cardiovascular events.

We have now rephrased this sentence.

Methods, Measures, Depression. Please add clause to end of sentence stating “The CDS has fewer items that refer to somatic symptoms of depression” to indicate what scales the CDS is being compared to. It may be worth also specifying that this is relatively fewer given the large number of total items on the CDS relative to other scales. Please also specify the cut-off for mild/moderate depression on the CDS.

We have now indicated was scale the CDS is being compared to in this statement.

We have specified the cut-off for mild/moderate depression on the CDS in the Measures section.

Results, last paragraph, please rephrase the statement regarding the “CDS being more sensitive to detecting severely depressed participants” as it implies we know whether these participants are truly severely depressed. This higher percentage could reflect more false positives.

We have now rephrased this statement.

Discussion, paragraph 5, last sentence. The proportion identified has more to do with the cutoff’s employed than the scale of the items. A higher CDS cutoff for severe would certainly result in fewer cases identified as severe depression. Consider deleting or at least rephrasing this sentence.

We have deleted this statement.

Discussion, paragraph 6, Wise, Harris and Carter found 38% of their sample as mild, moderately or severely depressed. The current analysis finds 52.6%. Please reframe the results to the context of the literature. This paragraph could perhaps be integrated with paragraph 4 which provides a potential explanation for the high mean CDS scores.
We have now integrated this paragraph into the Discussion section.

*Table 1, please list actual p-values. It is not clear what testing was done for ‘medical history’ and would probably be most appropriate to contrast groups on each individual component of the medical history.*

We have now listed the actual p-values in Table 1 and Table 2,

*Table 2 will require some copy-editing (e.g., i instead of I, im instead of I’m). The authors may want to comment on the assignment of items 11 an 14 which appear to load with anhedonia.*

These typographical errors have now been corrected. We have also included some discussion around the loading of the items 11 and 14 on the anhedonia factor in the Discussion section.

*It seems unusual that the correlation between the CDS and the BDI-2 approximates the correlation between the CDS and the STAI. How well is it targeting the construct of depression? What was the correlation between BDI-2 and STAI? This warrants some discussion.*

We have now included the correlation between the BDI-2 and the STAI in the Results section and included some discussion about the high correlations found between the BDI-2 and the STAI also found in previous research.

*Discretionary Revisions:*

*Methods, Participants and Procedure. Were there any difference noted between those who declined participation and those who agreed to participate in the study.*

Unfortunately we did not collect information from those that did not participate in this study and we have made a statement about this in the limitations section.

*The discussion section seems to devote a lot of space to reiterating the results. It might be helpful to devote more space to placing in the context of the existing literature, particularly given the emphasis in the background of this paper focusing on those who are “medically stable and had settled into their community surroundings”*  

We have now included discussion of the results in the context of some of the existing literature.

*Table 3. Please clarify statistical reporting for group differences.*

We have now clarified the statistical reporting for group differences.

*Reviewer 2:*

*The background section could be smaller and some parts regarding the effectiveness of CHD could be moved to the Discussion section.*

We have integrated relevant sections from the Background into the Discussion section.

*The HADS is referred only once. In my opinion, HADS, would be more suitable for gold standard than the BDI-2 used. This is something that can not be changed, but the authors could give some reasons why they have not used HADS.*
We have added a statement in the limitations section stating that future research should also compare the CDS with other more commonly used depression and anxiety scales that do not rely on somatic symptoms.

**Reviewer 3:**

**Major compulsory revisions:**

The major limitation of this study was the use of a small sample size. In particular, it seems likely that the factor analysis conducted in this study was underpowered. The researchers should provide evidence that this was not the case or discuss this limitation and its effects on the interpretation of their data in the discussion section of the manuscript.

We have now discussed this limitation and the effects of this on the interpretation of our data in the discussion section.

**Minor essential revisions:**

In the Measures section, under the heading “Quality of Life” the term “a coefficients” should be replaced with “alpha coefficients”.

This has now been amended.

Similarly, in the Measures section, under the heading Social Support the term “a coefficients should be replaced with “alpha coefficients”.

This has now been amended.

In the second line of Table 2 the “i” need to be capitalized in the statement “Things I regret.”

This has now been amended.

**Discretionary Revisions:**

More elaborations is needed explaining why the authors believe it is important to validate the CDS in a sample that has been discharged for 3.5 months- e.g., Is there data suggesting the depression changes in patients after cardiac hospitalizations?

We have now made a statement justifying why it is important to validate the CDS in a people who have settled into the community 3.5 months after having a cardiac procedure in the Background section.