Reviewer's report

Title: Functional and Self-Rated Health Mediate the Association between Physical Indicators of Diabetes and Depression

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Reviewer: Lindsay S Mayberry

Reviewer's report:

Summary
In this manuscript, the authors postulate that perceptions of health may mediate the associations between objective physical health indicators and depressive symptoms among adults with diabetes. They apply an existing conceptual framework to a large data set using path analysis to examine associations with individual indicators of each construct and between the constructs themselves. Findings suggest that perceived health status is more strongly associated with depressive symptoms, cross-sectionally and longitudinally, and mediates the association between objective physical health and depressive symptoms. This manuscript has several substantial strengths, including the use of a theoretical framework, the analytic techniques (multiple measures and cross-sectional and longitudinal analyses), and the consistency and strength of the effects. I am especially impressed by the sensitivity analyses done on missing HbA1c values (pg. 5). This is a very strong manuscript that makes a substantial contribution. However, several areas require clarification before it is suitable for publication. My major concerns focus on the organization of the introduction and the presentation of the results. Each of these issues is described in detail below.

Major Compulsory Revisions

1. The introduction requires some reorganization and clarification. As written, the constructs are presented somewhat randomly. I recommend the authors present the framework earlier in the introduction (i.e., third paragraph) and go into more detail on the study/framework by Whitelaw & Liang (population, constructs, outcomes, findings). Then the subsequent paragraphs can focus on each construct/relationship in the proposed framework. This will help clarify why these constructs were selected for inquiry and how, specifically, your work extends Whitelaw & Liang’s. Be clearer about what problem your paper is seeking to address.

2. The analyses are one of the clear strengths of this work. However, the presentation of the analyses and results needs to be clarified in the following ways:
   a. This is my most substantial concern about the manuscript: It is unclear if the “separate indicators” of PH, FH, or depressive symptoms are loaded onto latent variables in your path models (in which case you are running structural equation models) or if you are running multiple path models in which you switch out the
indicators. It should be and seems like it is the former, but there in some places it seems that you are switching the variables and running multiple models. For instance, “for predicting each of the four FH outcomes” (pg.8) and similar sentences using “each” imply you are running several models and switching out the constructs rather than using a latent construct. I think the authors should use latent constructs and describe the loadings of each individual indicator. It is standard notation to depict latent variables with circles and the indicator variables (measured variables) with rectangles in all figures. (Note: I think it makes sense to run two models – one for each of the outcome measures because of the differences between them – but still load the PH & FH indicators onto latent variables).

b. Pg. 6: Are the cross-sectional path models using only data from time 1 only or aggregated across time points. The former is preferable. This needs clarification in the first sentence of the analyses section.

c. On Pg. 7 there are results from ANOVAs (?) comparing all the constructs on insulin dependence. This is not described in the analyses and it is unclear why only insulin use was explored this way (why not the other covariates)?

d. If the main hypothesis is that FH & SRH mediate the association between PH & depressive symptoms, indirect effects and associated confidence intervals are needed. One of the primary strengths of path analysis/structural equation modeling is deriving bootstrapped confidence intervals for indirect effects—especially for latent constructs. These should be reported.

e. The longitudinal results are too dense and difficult to read. I recommend only highlighting how these results were consistent with/diverged from your main hypotheses and associated findings from the cross-sectional analysis. Possible the other results could be featured in an online appendix for those who are interested.

Minor Essential Revisions

3. I recommend changing the title to “Depressive Symptoms” instead of “Depression” to be more accurate

4. Please avoid describing patients as “suffering from diabetes” but rather simply state “persons with diabetes.”

5. Pg. 4 – the German health company randomly selected participants from what pool?

6. Move this sentence (pg. 8 “In terms of the measures of PH, the path coefficients…””) up to the paragraph under “Insert Fig. 2a here.” This supports the assertion that the indicators of FH were the strongest predictors of SRH—as written, there is no evidence to support that claim in the referenced figure or the paragraph.

7. The figure names do not match between the text and figures for 2b and 2c.

8. Sentences/paragraphs that need revision:

   a. Pg. 5, “Furthermore, participants insulin-dependence….height.”
b. What is meant by these sentences? Can they be deleted? Pg. 6, “According to the proposed model depressive symptoms are regarded as a distal outcome. Therefore anxiety/depression was used as an outcome variable.”

c. The ethical approval paragraph needs revision for clear language and grammar.

d. This paragraph is very unclear, pg. 11-12 “There have been few approaches…”

9. Why is there a discrepancy in this notation: sometimes “beta = coefficient” and sometimes “beta > coefficient”?

10. Some of the limitations mentioned seem tangential/unrelated to the findings (e.g., results not applying to non-clinically diagnosed diabetics—why might that matter?), whereas other more substantial issues are not mentioned. For instance, what about the disproportional number of men in the sample? What about using self-reported HbA1C levels—might results be different with lab values? The authors made great points about the potential for shared method bias.

11. Pg. 10 “As expected, the participants in our study showed a significantly higher level of depressive symptoms as compared to the general population.” This needs citations and comparisons—what is the rate in the general population vs. the sample rate?

Discretionary Revisions

12. Personally, I would have preferred to see the entire model in a single figure instead of broken into three separate figures.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests.