Author's response to reviews

Title: Relation between body mass index and depression: A structural equation modeling approach

Authors:

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Version: 3 Date: 21 February 2007

Author's response to reviews: see over
Dear Editor,

“Relation between body mass index and depression: A structural equation modeling approach”

On behalf of my co-author and myself, I would like to thank you for processing the above manuscript for publication in the BMC Medical Research Methodology. I also would like to thank the reviewers for their very constructive comments which improved the quality of the paper.

We revised the manuscript based on the comments from the reviewers and had the manuscript edited by a native English language research associate (Amanda Bradford-Janke).

We incorporated all comments by the reviewers in this version, except re-analyzing the entire dataset from Canada which is impossible because of other demands. However, as I explained in my first response to the reviewers our main objective is to demonstrate the use of SEM in modeling the relation between obesity and depression. In addition, because about 40% of Canadian population live in Ontario, we believe that this results could be good estimates for the Canadian population as well.

The manuscript is now revised and a point-by-point response is prepared for your consideration.

Sincerely,

Noori Akhtar-Danesh
Reviewer No. 1 (Fiona Shrive)

Compulsory revisions:

Point 1: I still have a major concern over the conclusions the authors draw. The data is cross sectional and thus no temporal relationship can be established between any of the variables. For example, on page 11 the authors state that "higher BMI results in more severe depression". Given that you do not know whether people have a higher BMI and develop depression or develop depression and their BMI increases you can not draw conclusions about one "resulting" in the other.

The manuscript has been revised to incorporate the comment, for instance the sentence on Page 11 has been changed to "higher BMI may result in more severe depression".

Point 2: I still do feel that the justification for why only the Ontario population was used. If the modeling is completely computer generated then surely it would be exactly the same to complete the same models on a larger dataset including the entire Canadian population. Was a specific variable only available for the Ontario population? Was the data more readily available? Using the Ontario population is fine, however the reasoning needs to be properly justified in the manuscript.

We totally agree with the reviewer that the entire dataset could generate more reliable results, however, as we already explained this manuscript is based on the project done by the first author (AD) as part of her MSc thesis and the main focus was the use of a new method (SEM) in modeling the relation between obesity and depression. Unfortunately, because of the other demands it is not possible for us to re-analyze the entire dataset, but as about 40% of Canadian population live in Ontario, we hope that this results could be good estimates for the Canadian population as well.

Point 3: The missing data for the variables of eating habit and relatives with depression are not significant! These variables are present in approximately 10% of cases! This is a major statistical issue. I would encourage the authors to either exclude the model using those variables or address the issue of reduced sample size in a more forward way. This is not an issue to be glossed over.

I am not sure about the message in this comment. We deleted these variables because, as the reviewer noticed, about 90% of the cases were missing. It is true that these variables are available in 10% of cases but if we keep these variables in the model 90% of the other variables will be lost because of the listwise deletion in the analysis. Nonetheless, as the reviewer suggested, we
deleted the aforementioned variables from further analysis because of missing values.

**Point 4:** The choice of figures is odd. Figure 1 is Stunkard's model which the authors adapt and figure 2 is Model 1 which the authors do not feel results in a good fit with the data. Are either of these figures necessary?

We deleted Figure 1, the Stunkard model. However, we prefer to keep Figure 2 (now Figure 1) to explicitly explain our starting point for the analysis (modeling process) to the readers.

**Point 5:** Lastly, the author do not clearly articulate their message about gender and depression. In the modeling process they exclude gender and depression as it is non-significant. Subsequently, an analysis is presented stratified by gender. The rationale for this analysis should be discussed in the methods section and more precisely in when discussing the results.

The rationale is now provided both in Method section and two references were given in Discussion for the gender dependency of the relation between obesity and depression.

**Minor Essential Revisions:**

1. The section entitled "Subjects and Data Collection" might be better described as "Data Source".

   Amended as suggested.

2. The sentence on page 4 "In CCHS-1.2 dataset there were 76 pregnant women..." should be moved to the "subjects and data collection" section

   Amended as suggested.

3. On page 5, the sentence "It was no other continuous variable in the dataset..." should be removed. If the variable selected is appropriate and valid then this sentence is unnecessary and draws into question the validity of the selected variable. Is it an appropriate proxy for depression? If so, it does not matter that another one was not available.

   The sentence has been deleted.

4. Page 5, paragraph 3 - it should be structured the same way as the other paragraphs - "Stress management: is the ability to handle ...."
Amended as suggested.

5. For the variable SES, is the construct of 4 variables a standardized, validated proxy for SES? If so, the authors may want to state that and reference it.

We revised the paragraph to incorporate the comments from both reviewers.

6. The authors say that the RMSEA statistic and CFI are used to assess the fit of the models. A brief description of how these statistics work (i.e. how do they measure fit?) and the type of values that would indicate a "good" fit should be stated and referenced.

The RMSEA and CFI have been described and references provided in Method section.

Thank you
Reviewer No. 2 (Hui-Xin Wang):

Point 1: ABSTRACT
Results: the following sentence is repeated twice: “higher BMI is associated with ....”

The paragraph has been changed to accommodate the association between BMI and depression in general and among males and females separately.

Conclusion: “the two major diseases of obesity and depression” can be written as “the two major diseases, obesity and depression” or simply “obesity and depression”.

Amended as suggested.

Point 2: METHODS
• The description of SEM is too long, some of them are unnecessary.

We revised and shortened the Method Section in general and also some parts that describes the methodology of SEM. However, we added some paragraphs to describe RMSEA and CFI indices as suggested by the first reviewer.

• There is a number of “CCHS-1.2” and “in this analysis” through the section. It is not necessary to repeat it all the time.

Amended as suggested.

• Depression: “It was no other continuous variable in the dataset to measure depression or it severity.” Is it relevant information?

The sentence has been deleted.

• Social economic status (SES): the language in the whole paragraph needs to be improved.

We revised the paragraph to incorporate the comments from both reviewers.

Point 3: DISCUSSION
• The 2nd paragraph, 2nd sentence, “It can be concluded from model 3 and table 2 …”, does this mean that the model 3 in table 2, or both of the model 3 and table 2?

Amended as “It may be concluded that higher BMI results in more severe forms of depression (see Table 2) which is consistent with some other recent findings”.
• The 4th paragraph, “In male, …..” this sentence is confusing. In the study, the direction was assigned using the SEM method, although one can not conclude any causality, the results indicate that the level of BMI was positively related to the level of depression. On the contrary, the level of depression was negatively related to the level of BMI. The possible explanation of these associations should be discussed.

Revised: as explained in Discussion Section: “A possible explanation might be that new medications used to treat patients with depression have reduced/no weight gain side effect when compared to previous treatment options. However this is only a hypothesis as information regarding the treatment of these respondents was not available”.

• The last paragraph, “This work showed these two diseases are associated but the form of the relation is different BETWEEN males and females”. Is it BETWEEN or AMONG?

Amended as "... is different among males and females”.

• Limitations should be placed before the conclusion.

Revised as suggested.

Point 4: There are a number of these kinds of phenomena through out the MS, all need to be carefully checked and modified. In addition, where does the figure on last page come from? It was not mentioned any where in the text.

We revised the whole documents again. The last figure (now Figure 3) has been referred to on the last paragraph of the Result Section.

Thank you