Author's response to reviews

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

Authors:

Alina Dragan (dragana@mcmaster.ca)
Noori Akhtar-Danesh (daneshn@mcmaster.ca)

Version: 2 Date: 13 January 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. 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: Overall the paper is poorly organised and contains much irrelevant information. For example, page 4 discusses advantage of the data collection method used in the CCHS. This is outside the scope of the current paper's method section. The authors should include only their own methods and simply reference the methodology applied in the CCHS. The current paper need only briefly describe the data source used.

The Method Section has been revised to accommodate concerns from both reviewers.

Point 1 (Continued): Another example is on page 5 where the authors include a lengthy discussion of BMI. Again, a description of the variable used and how is was included in the analysis would suffice.

Again, the Method Section has been shortened and much of the discussion about BMI has been deleted.

Point 2: The methodology used is poorly described. The SEM technique is not clearly explained. The authors need to more clearly outline their own methods.

The description of the SEM technique has been expanded. Although, we intentionally avoided inclusion of mathematical expressions in the manuscript because the methodology is well described in several books and articles such as Ullman [Ref #25] and Kline [book: Structural Equation Modeling], we would be happy to include them if suggested by reviewer and the Editor.

Additionally, the variable definitions are unclear. The authors need to clarify how their variables are defined in the CCHS and how they use them in their study.

Modifications have been made to more clarify the definitions.

Point 3: Why were only Ontario respondents included in the analysis? The authors discard over 20,000 respondents to the CCHS from the rest of Canada without any justification for the exclusion. The authors should clarify why only Ontario residents were included.

This manuscript is prepared based on the project done by the first author (AD) as part of her MSc thesis. The graduate committee of the department approved that working on the data from Ontario would be sufficient as a project for an MSc student. Secondly, the main focus of this article is the use of a new method (SEM) in modeling the relation between obesity and
depression. Thirdly, as revised on Page 4, Ontario embraces about 40% of Canadian population, we hope that this results could approximately estimate the pattern of the relation for the Canadian population.

Point 4: The results and discussion sections should be more clearly delineated. Many points in the results in section would be more appropriate for the discussion section. The results section should include only results form the data analysis. Any implications or interpretation should be in the discussion section.

The manuscript has been revised to accommodate the reviewer comment.

Point 5: On page 11, the authors allude to "causal" pathways. One major component of causality is temporal association meaning that the causal factor must precede the outcome. In this case, we have cross-sectional data thus the timing of the depression in relation to obesity is unknown. All mention of causality and statements of obesity leading to depression or visa a versa should be removed. Only an association between the two variables can be examined.

The manuscript has been revised as suggested.

Point 6: References should be included for the MET values and the EAT index score.

We now included references for MET and EAT indices.

Point 7: Lastly, the authors discuss the limitations of their statistical package. Why use this package if it is unable to complete the analysis set out? This choice needs to be better explained and clarified.

The only limitation of the statistical package (AMOS) is that it can not handle the sampling weights into the analysis and based on our knowledge at the time of analysis there was no statistical software that could handle sampling weights using SEM technique.

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

Point 1.1: Methods- The paragraph on page 6 “The weight classification system can be applied … individuals level” can be shortened by introducing only the relevant parts for the present study.

   The paragraph has been revised as suggested

Point 1.2: Results
Some tables are redundant because the same results have also been presented in figures. For example, Tables 3 can be abolished and the results can be reported in the text. Table 4 can be incorporated with figure 3, in which one can signify the level of significance. Similarly, tables 5 and 6 can be eliminated and the important results can be signified in the corresponding figures 4 and 5, respectively.

Based on the reviewer's suggestions:

- Table 3 has been deleted.
- Figure 3 has been deleted.
- Tables 5 and 6 have been deleted.
- Also, Table 1 has been deleted as suggested by Reviewer #2.

Point 2.1: Abstract
It is difficult to distinguish the Results and the Conclusion section in the present form. It would be good to place the results in the Results section and a brief conclusion based on the results in the Conclusion section.

   The Results and Conclusion sections have been modified as suggested.

Point 2.2: Methods
Some parts of the text should be moved to the Discussion section. For example, under the “Body Mass Index”, from “we used BMI as an index for the measurement of ……” in page 5 up to the end of the page, and from the beginning of page 6 to “In CCHS-2.1 dataset there were 76 pregnant women who were excluded from this analysis”, there is a long discussion about BMI, however, only parts of them are needed in this section whereas most of the content should be placed in the Discussion section.

   The Method Section has been modified as suggested.
Point 3.1: Methods
a. The level of heading is not clear. For example, it seems that “Variables” is a 2nd level heading (if the Methods is the 1st level), “Body Mass Index”, “Depression” ... and so on are the 3rd level. Aren’t they? In the present form, however, one would think they are the same level heading.

    As suggest by the reviewer, we used Bold for Methods (as the first level), **Bold italic** for the Variables (as the 2nd level), and italic for Body mass index (as the 3rd level).

b. The study design is vague. One knows from the paper that the survey is based on a complex design, but it is not clear how the sample was selected. In addition, it is difficult to find the number and age range of the studied population in the Methods.

    Now, the reference for the full study design has been included. The age range of the population is 15 and over (please see Page 4 under Subjects and Data Collection).

Point 3.2: Results
a. What is the definition of “health risk” in Table 1? With the uncleanness, I don’t understand the purpose of the Table 1 and it was not reported in the Results section. We agree with the reviewer that Table 1 was not necessary and did not contain much of relevant information, so we deleted this table.

    Table 1 has been deleted

b. Footnotes should be given when there is missing values. In Table 2, except gender, all of other variables have missing values and they should be reported in the footnote.

    Revised as suggested, in the current Table 1 there is a footnote that “the number is different from the total of 12376 because of missing values”.
Minor Essential Revisions:
- Please check the references carefully. There are some format errors.

  References have been checked and errors have been corrected.

Discretionary Revisions:
- What is the reason that RMSEA, NFI and CFI were chosen to describe the model fit whereas there is a lot of other indices, for example, GFI?

  We deleted use of NFI in this revision and only used RMSEA and CFI as they are the most reported indices (corrected on Page 8 with reference included.).

Thank you