Author's response to reviews

Title: The Association of Sedentary Behaviors and Body Mass Index Classification in a Cross-Sectional Analysis: Are the Effects Homogenous?

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Revise/Re-submit Transmittal Letter

The Association of Sedentary Behaviors and Body Mass Index Classification in a Cross-Sectional Analysis: Are the Effects Homogenous?

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Dear Editor Lock,

Thank you for the kind and helpful feedback regarding our manuscript. We have attempted to address the concerns of the Reviewers in italics below, and hope that you will find our changes and explanations sufficient to warrant publication in BMC Public Health. Best regards.

Dr. Nanchahal’s Comments to Authors:

Major compulsory revisions

The abstract needs to better reflect the findings in the paper: Sedentary behavior and physical activity are not the same thing and this should be reflected in the conclusion.

The Introduction section of the Abstract now reflects the dual purpose of the study; to evaluate both physical activity and sedentary behavior as mutually exclusive variables. The conclusion now reflects specific findings of each variable.

The results section simply repeats the values given in the tables. It would be better to draw out key findings and comparisons across variables. Also, an interpretation of the beta values from the regression would be more appropriate than repeating the values already provided in the tables.

The authors customarily state their findings in the Results section and reserve analysis for the Discussion section. However, the authors recognize opportunity to incorporate this feedback to improve the readability of the manuscript. As such, the authors have eliminated the numerical values in the Results section and added several instances where results of statistical tests are explained. For example, there is now discussion about the importance of interpreting the statistic used to examine the heterogeneity of the BMI classifications according to the independent variables. The adjustment of the p-value of this test (the Bonferroni correction) is also discussed in terms of achieving a more stringent threshold for rejecting the null hypothesis of homogeneity among the BMI classifications. Further, the authors have added discussion on how to interpret the results of the simultaneous quantile regression analysis as well as the interpretation of the F statistic. These interpretations have been given as a layman’s dialogue of how to interpret the results using these tools. The authors hope this significant addition meets with Dr. Nanchahal’s satisfaction.
The same applies to the F values given later in the results section.

As stated above, the authors have improved the clarity of these results in layman’s terms. All numerical values have been removed and a new introduction paragraph has been added to discuss the utility of F tests. The authors hope this significant addition meets with Dr. Nanchahal’s satisfaction.

The Discussion should not include results already given, but put the study in context of findings by other researchers.

While the authors’ study was informed by the 30 references to the literature in the manuscript, the research question was very unique. Examining the effect of these variables according to exact BMI classification thresholds using this type of statistical technique is a new approach according to the authors’ investigation of previous studies. As such, it is not possible to have a meaningful discussion section that only focuses on comparisons to the work of others in the field.

There are multiple parts of the Discussion section. To be responsive to the Reviewer, the authors have added an additional reference for the first paragraph of the Discussion section (please see Cooper and colleagues) to provide context to the limited existing literature on this topic, and also removed numerical values and redundant language where appropriate. The authors feel the remainder of the Discussion section appropriately represents their contentions that targeted interventions toward BMI classification are needed when addressing obesity. This discussion is supplemented with several references to literature pertinent to the topic (please see; Variyam and colleagues, Frank and colleagues, Bliddal and colleagues, Cannioto and colleagues, Kohlbry and colleagues, and Rimmer and colleagues).

Minor revisions

It is unclear to me what being 'minimally classified' means - this term is used several times in the abstract.

The authors have removed the two instances where “minimally classified” was used, and replaced the term with “at least.” The intent of these changes was to indicate the identified relationship held for all individuals of at least a certain BMI. That is, if the relationship held for the obese class III category, it held for all those above it (obese class I, obese class II, etc.).

The methods state that households were sampled - clarify if this sampling method has been accounted for in the statistical analysis.

The survey methodology used a random-digit dial of households. The household was asked to provide the head of household (based on the adult with the next coming birthday to minimize bias) as the potential survey respondent. If willing and eligible, a survey instrument was mailed to the head of household. As a result, both the data collected and the statistical analyses were based on a sample of individual responses obtained from a random-digit dial survey. As the Reviewer points out, this introduces a certain response bias since living in a household was a requirement to take the survey. The authors have noted this in the limitations section. There is no risk of correlation in responses between household members because only one member of the household (the “head of household”
by adult with the next coming birthday completed the survey). The authors have also introduced substantive discussion of this topic in the Methods section.

It is unclear how a wider range of classifications allow for 'more precise estimates' (p7).

Additional language has been added to clarify this point. Better estimates are created from reduction of within group variance. If we used six distinct groups instead of four, we generally get more homogeneity in the respondents found in each BMI classification (i.e., less BMI variance per classification since there are more classifications). Reducing within group variance allows the model to detect effect changes with greater precision.

Clarify why hours watching television was measured as a categorical rather than a continuous variable.

This question is from the CDC’s National Health and Nutrition Examination Survey (NHANES). It is a validated instrument and the question is categorical.

State the units wherever values are given for variables e.g. 13.53 on p11.

The units are provided in the tables. Per the Reviewer’s comments above, the authors have removed numerical values since they are in the tables. Hence, there are no longer instances in the text to associate units with numerical values.

Dr. Buchowski’s Comments to Authors:

Major Compulsory Revisions

Abstract –

The goal of the study should be clearly stated.

The authors have made changes to the introduction paragraph of the abstract. The last sentence now reads, “The goal of this study was to determine if physical activity and sedentary behaviors have the same effect on individuals of all BMI classifications.”

Methods –

Was the PA part of the questionnaire validated?

This question is from the CDC’s BRFSS validated instrument.
More information about the questionnaire (PA part) should be provided

*The authors have greatly expanded the discussion of the survey instrument in terms of data collection methods, sources of validated questions, and rationale. The authors have specifically stated the PA question comes from BRFESS. The authors believe this should satisfy the Reviewer’s request for more information on the instrument and the PA question in general.*

It is unclear why time of TV viewing was considered a categorical variable.

*This question is from the CDC’s National Health and Nutrition Examination Survey (NHANES). It is a validated instrument and the question is categorical.*

Conclusions –

*It should concentrate on study finding. As written, is too vague and general.*

*The authors have re-written the Conclusion section to be much shorter and more focused on the findings of the study and their implications for public health practice.*

Minor Essential Revisions

Discussion –

*Authors should consider shortening paragraph 2 of the “Is time to exercise?” or refocus it to the study findings.*

*The authors have eliminated one descriptive paragraph from this section.*

Discretionary Revisions

Introduction –

*Authors may consider using an abbreviation for physical activity (PA).*

*This change has been made.*

It is unclear if moderate PA or moderate exercise was assessed.

*The survey instrument assessed moderate PA.*
Using term “BMI classification” should be reconsidered since it is rather obesity classification using BMI as a criterion.

To stay consistent with the WHO and associated literature, the authors decided to maintain the usage of the BMI classification vernacular, although the authors do understand and appreciate the Reviewer’s point of view.

Since all data collected was self-reported, emphasizing this fact at some instances is not necessary or even misleading.

The authors state the self-reported nature of the data to be transparent and allow the reader to know there are self-report biases associated with the study (which are acknowledged in the limitations section). The authors feel this is important to reiterate throughout the manuscript in order to maintain the transparent nature of the work.

Results –

The results section could be shortened.

The authors have shortened the Results section based on the feedback of the other Reviewer.

Graphs –

Some parts are repetitive (e.g. Test of Similarity Effect) and could be omitted.

This is a stylistic element the authors wish to maintain. While it may appear redundant, we have found the consistent label allows the reader to better absorb the concept of the F test and understand which two points are being evaluated.

Ethnicity graphs should be combined and "Effect of self-reporting …" should be replaced with “Effect of Ethnicity…”

The authors cannot combine the African-American and Hispanic graphs. These data need to remain separated for multiple reasons. First, a single graph would require the analysis of 8 different effect tests which would render the graph somewhat unreadable and certainly less useful. Second, the authors want to call attention to the difference in these groups as they are prominent in the literature and should warrant their own focus.