Title: Gender-Dependent Associations between Socioeconomic Status and Metabolic Syndrome: A cross-sectional study in the Adult Saudi Population

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Author's response to reviews: see over
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Respected Editor:

I am pleased to resubmit our revised manuscript for publication entitled “Gender-Dependent Associations between Socioeconomic Status and Metabolic Syndrome: A cross-sectional study in the Adult Saudi Population” MS: 3455926141074046. I appreciate the constructive criticisms and remarks of the reviewers, which have greatly enhanced the scientific quality of the paper. I have addressed each of their concerns as outlined below point by point.

We hope the revisions done will merit eventual acceptance in your respected journal.

Best regards,
Nasser
Corresponding author

Reviewer: David Rehkopf

1. Although there are strong priors for gender differences in metabolic syndrome, these differences should be statistically tested as they are central to the arguments made in the manuscript. That is, it should be tested whether, for example, there are gender differences in the relationship of income and metabolic syndrome, not just whether there are associations within men and within women.

   Response: Based on reviewer’s comment, we have added a new table showing the statistical relationship between gender and SES variables.

2. While it is generally appropriate to treat medication use as having the metabolic syndrome, there is an important implication for practice for whether high rates of metabolic syndrome in some groups are actually due to high clinical levels, or due to high use of appropriate medications. So sensitivity analyses of all results should be presented without counting medication use toward metabolic syndrome definition.

   Response: It is now highly practiced that if the subjects are on medication (antihypertensive or anti-diabetic drugs) then they are considered to have evidence of elevated blood pressure or raised fasting glucose and could be included as MetS representative regardless of measured blood pressure or fasting glucose at the time of the examination. Moreover, this inclusion is never considered as any amendment to the MetS definition.

3. The tests of trend with age that are significant should be reported. If a change or difference is talked about, a related test statistic should be presented, even if the trends appear visually clear.
Response: Evaluation of trend between age groups was analyzed using the Cochran-Armitage trend test and is now introduced in the updated manuscript.

Minor Essential Revisions

1. Most of the time physical activity is not considered an indicator of socioeconomic status. This should be justified or changed.

Response: It has been changed in the revised manuscript as “behavioral factor”.

2. What is meant in the last sentence of the second paragraph of the background section is unclear.

Response: The sentence is modified to make it more clear and meaningful.

3. In the discussion the statement that higher income classes are more likely to have a "lack of nutritional awareness and proper food choices" should be cited to support it as this runs counter to literature in other countries.

Response: The statement that higher income classes are more likely to have a "lack of nutritional awareness and proper food choices" is replaced to make it more meaningful, but provides the same message. However, keeping in mind that it is contradictory to literature in other countries, we have now supported our statement with few more citations (Ref# 47, 48 and 50-53) and discussed properly in revised manuscript.

4. It is not clear why housewives would have almost negligible involvement in any type of physical activity. The reasons for this should be explained and cited if literature exists.

Response: The reason for unemployed females involved in almost negligible physical activity is explained with the support of few references (Ref#65, 66).

Reviewer 2: Ana Santos

Comment

1. My main point has to do with some of the statistical methods applied by the authors. When describing the sampling procedures, a complex sampling design was chosen, as a cluster sampling strategy was applied, and the population of each PHCC was taken as a cluster. Thus, data analysis should consider this complex structure of the sample. Was this considered by the authors? Why is this not explained in the methods?

Response: We have considered all possible strata available from the population. Each PHCC represents each region/district. Particular region represents special socio-economic structure, which corresponds to family status and education level. Analysis
based on family status and education level could possibly solve the diversity/complex social structure.

2. In the discussion, why do the authors state that the higher income males are more likely to follow sedentary lifestyles and lack of awareness and proper food choices? Isn’t it expected to be the other way around? It has been describe that higher income strata of population have healthier lifestyles when compared to lower income people. How do the authors explain these contradictory results?

Response: - We agree with reviewers comments regarding the contradictory statement “higher income males are more likely to follow sedentary lifestyles and lack of awareness and proper food choices” but we don’t agree that it should be other way round as far as our present study is concerned. We have deleted the original phrase to replace it with more meaningful and clear message in the revised manuscript. More references (Ref#44, 45, 46) are cited in the discussion part to support our contradictory results equipped with a proper discussion as well. The main contributing factor for the high prevalence of MetS with high income class is possibly the economic transition and developmental level of a specific country (Ref#43, 47, 48). The ability of males as a purchaser with an increase in earnings are also discussed (Ref#52, 53) to support that there is possibly a direct association between MetS and higher income class in males due to the increased ability to purchase food. In addition the references supporting the increase in sedentary behavior and lack of proper food choices of Saudi males in higher income class is also explained (Ref#50, 51).

3. I think that the differences regarding gender should be better discussed by the authors. In fact, the difference in the association regarding the socioeconomic variables in males and females should be the major results of this study and properly discussed why the authors think that this happens.

Response: -According to reviewer’s comment, more references about the gender differences and its association with socioeconomic variables (income and education) are included and properly discussed in the revised manuscript (Ref# 54-58).

Minor Compulsory Revisions

This is a general remark that the authors should carefully revise the whole text and look for the use of words such as incidence (e.g. line 162) and risk (e.g. line172) as this is a cross-sectional study and as such, it does not allow the estimate of incidence and the outcomes produced are not related to the risk of MetS but to the odds or prevalence of MetS. Also, authors should be careful regarding the quality of written English.

Response: - According to reviewer’s general remark, we replaced the words “incidence and risk” by “prevalence”. - The quality of English has been revised for better understanding.
Methods: Page 6 – regarding the assessment of socio-economic status why were variables such as physical activity included in this definition? Do the authors consider that physical activity is a component of socio-economic status?

Response: I agree with the reviewer’s remark about physical activity being wrongly denominated as socio-economic status. In the revised manuscript the physical activity is treated as a behavioral factor different from SES.

Page 6 – regarding the categories of education – as this type of qualitative classification can vary substantially by population, can the authors provided the number of years of schooling included in each category? Also, physical activity was not in fact evaluated, as the only evaluated variable was the frequency of physical exercise, as such the authors such refrain from using the denomination of physical activity and use “exercise”.

Response: The number of years of schooling is included in each category in text as well as in tables: uneducated (<6 years), precollege: (7–12 years), and higher education (>12 years). Based on reviewer’s comment the denomination “Physical activity” is replaced by “Physical Exercise” wherever needed (in the text and tables).

Statistical analyses

Page 6, line 142 – the authors state that multivariate logistic regression analysis were conducted, and models were adjusted for potential confounders, such as age. This is puzzling as in fact age was the only potential confounder of the association included in the models, as illustrated in the tables. Thus, what were the others variables tested, and why they were not included in the final model. Also, I think this section (Statistical analyses) should be moved toward the end of the Methods section.

Response: Age was the only factor included in the analysis. No other variables were included in the logistic regression analysis.

Results:

Line 155 – the results described here are referred to female participants, but in table 1 there is no gender stratification. Results regarding age and the distribution of MetS prevalence are in my opinion excessive. In fact, these results are not new and virtually in every population this pattern can be observed, thus authors should consider decreasing this paragraph. Throughout the text please revise the sentences where state that the risk of MetS increases or decreases, and previously said, the authors using this study design cannot estimate risk. Also, when describing odds ratio, sentences should reference the class to which these results are compared. It would make the description of results clearer.

Response: The sentence in the result section “As regards the marital status, the females (unadjusted) in the widow class presented higher prevalence of metabolic syndrome (74.2%) as…..” is corrected and replaced by “As regards the marital status, the widow class presented higher prevalence of metabolic syndrome (74.2%) as…..” in the revised
manuscript. The paragraph in the result section regarding age and the distribution of MetS prevalence is cut short. According to reviewer's comment, we have replaced the word “risk” throughout the text with “prevalence” and odds ratios were described along with their referred classes/groups.

Discussion: Line 188: Please revise words such as direct relationship and consider replacing it by positive. Also, it is uncommon to see the expression full MetS, what do you mean by that? Please, clarify. Again, I think that the discussion on the results regarding age distribution of MetS is excessive, and authors should revise it. Why do the authors state that the higher income males are more likely to follow sedentary lifestyles and lack of awareness and proper food choices? Isn’t it expected to be the other way around? It has been describe that higher income strata of population have healthier lifestyles when compared to lower income people. How do the authors explain these contradictory results?

Response: The words such as “direct” relationship are replaced by “positive”. The expression full MetS represents all characteristic/criteria (not any few of them) of metabolic syndrome definition. The discussion on the results regarding age distribution of MetS is revised and cut short, accordingly.