Author's response to reviews

Title: Association between socioeconomic status and obesity in a Chinese adult population

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

We have responded positively to the comments of the reviewers, taking on board most of their suggestions and hope that the revised manuscript now meet with your approval. We certainly feel that the revised version is improved considerably. Detailed responses follow:

REVIEWER 1:

Major Compulsory Revisions:

- The authors claim that the relationship between SES and obesity may not be very well differentiated because few studies have controlled for multiple behavioral factors such as physical activity and diet. This appears to be the major novelty of the manuscript and what I consider to be a major limitation to the manuscript. It is very likely that physical activity and diet lie within the causal pathway between SES and obesity, and it is therefore a major over adjustment to include such variables in the model. Furthermore, the authors adjust for individual SES (I assume income?) and education in the same model, and conclude in the discussion that education does not show an independent influence on obesity. However, given the likely temporal sequence of education and income (ie. Education is likely to determine income), adjusting for income will likely underestimate some of the true impact of education.

Response: We agree with the comment that income and education are sequentially related, actually both of them are common indicators of individual SES, thus put them into the same model is not proper. So we RE-FITTED the models, and RE-WROTE the corresponding parts of our MS. This time, we chose income and education as different SES indicators, and included them into the logistic regression models separately. Major changes in manuscript include: line 126-131 (variables and definitions), line 159-169 (statistical analysis), line 216-235 (model fitting results), line 258-270 (discussion), table 3 and table 4.

The reviewer also mentioned it is very likely that physical activity and diet lie in the pathway between SES and obesity, and to include them in the model
will underestimate the SES-obesity association. We also agree with this point. The main reason that we put those lifestyle factors into the model is that, in this study, we do not want to miss the opportunities to estimate adjusted ORs of these lifestyle factors, and based on literature review, we learned that only a part of SES-obesity association will be explained by those factors, so after trade-off, we decided to include those factors into the model, and by doing so, we probably underestimated the real association between SES and obesity, we think it is one limitation of our study, and we have put it in the discussion in revised MS, please see in line 316-321.

- It is unclear why Caucasian cut-points are used for BMI (for international comparison) but Asian specific cut points are used for WC.

Response: Thank you! After further literature review, we also perceived it might be improper to use different ethnicity-based obesity cut-offs in one population, so we corrected it. In revised MS, we used Chinese-specific BMI and WC cut-offs which forwarded by the Working Group on Obesity in China (WGOC), and RE-DID all related calculations in our manuscript. But we also kept corresponding rates based on WHO BMI cut-offs as comparisons. Major revisions in the MS include: line 115-125 (variables and definitions), line 181-188 (characteristics of study population), line 199-213 (distributive features of obesity).

- There appears to be major differences in the prevalence of obesity when defined by BMI or WC. This discrepancy should be addressed in the discussion.

Response: Thank you! After re-selecting the cut-offs of BMI and WC, this discrepancy has been considerably attenuated, but still significant, so we have addressed it in the discussion, please see in line 244-257.

Minor Essential Revisions:

- Introduction. The authors state that obesity rates have more than doubled since 1980 from 5% to 10% in men and 8% to 14% in women. Please state what year the doubled estimates are derived from.

Response: Thank you. This sentence is ambiguous, and some information is not accurate. We have RE-WRITTEN it. Please see in line 47-50.

- Blood pressure is not an anthropometric measure. Please revise.

Response: Thank you. We have revised this already.
• Please state survey response rate in methods. Without this information we have no indication as the generalisability of results.

Response: We have added response rate. Please see in line 87-91.

• Please include the WC cut-point for overweight.

Response: Thank you. We have included WC cut-point for overweight by WGOC’s criteria. Please see in line 121-123.

• Please provide a rationale as to why there was a focus on oil, meat and fruit and vegetables.

Response: Thank you! To be more accurate, we changed “oil intake” into “total fat intake” in revised MS. And the reasons for focusing on these three categories of ingredients were elucidated in line 135-139.

• Please state the variables that were controlled for in each model in the written methods section.

Response: Thank you! We have enumerated those variables in the written methods section. See in line 166-169.

• Please add units to BMI values throughout.

Response: Thank you! We have added units to BMI values throughout the revised MS.

• It is inappropriate to refer to ‘hazards’ of overweight/obesity when logistic regression analyses were used. Please refer to ‘odds’ of overweight/obesity.

Response: Thank you! We have replaced “hazards” into “odds”.

• Please add 95% CI’s when referring to point estimates in the results section.

Response: We have added 95% CIs to all point estimates throughout the revised MS.

• Individual SES and education are included in the regression models. I assume that individual SES is income? Education is also an individual measure of SES.

Response: Thank you. Actually this comment is similar to the second part of the first comment in “major compulsory revisions” section. We agree that both education and income are common indicators for individual SES, thus put them
into the same model is not proper. So we re-fitted the models by including them separately, and re-wrote the corresponding parts of our manuscript. Major changes in manuscript include: line 126-131 (variables and definitions), line 159-169 (statistical analysis), line 216-235 (model fitting results), line 258-270 (discussion), table 3 and table 4.

REVIEWER 2:

Major Compulsory Revisions:

1. The authors should provide more details about the participation rate. They mentioned that 17,437 participants were recruited (study population section) and that data were completed for 16,013 individuals (results section), but they did not display the number of eligible subjects and the number of refusals (and potential reasons for refusing to participate).

Response: Thank you! We have added related information. See in line 87-91.

2. In the characteristics of study population section, the authors mentioned that the participants (16,013) were comparable to those included (17,437), but they did not compare the participants to the eligible population. Were they comparable regarding educational level, marital status and individual SES? Was the sample representative of the Zhejiang general population? One should expect a description, even short, of the potential participation bias. Moreover, the potential consequences of the participation bias should be mentioned in the limitations section of the discussion.

Response: Thank you! We have compared the representativeness of our sample to the general population of Zhejiang in revised MS (please see in line 190-192), and discussed the potential participation bias in limitations section of the discussion (please see in line 321-323).

3. Since, as mentioned by the authors in the introduction section, the Zhejiang province is an economically prosperous region in a developing country, it is not sure that the sample is representative of the Chinese general population (particularly regarding education, marital status and SES). It could be of interest to discuss this point in the discussion section, particularly regarding generalization of the findings to the Chinese population. If the sample is too different from the Chinese general population, it could lead to modify the title of the manuscript by adding “in a Chinese adults population”.
Response: Thank you. We totally agree with this comment. So we have discussed generalization of our findings in revised manuscript (please see in line 326-329). And we also modified the title into “Association between socioeconomic status and obesity in a Chinese adult population”.

4. In the Measurements section, the authors should give more details on measurement devices (stadiometers and scales).

Response: Thank you! We have added detailed information on measurement devices. Please see in line 105-106, and 110-111.

5. In the Statistical analysis section, and in the rest of the manuscript, it is isampling process. The authors should clearly mention that data were weighted to take into account the probability of inclusion. Did the Table 1 and the table 2 display raw or weighted data? This point is also unclear in the manuscript. If the studied sample was too different from the Zhejiang general population (see point 2), did the authors adjust data to take into account the observed discrepancies?

Response: Thank you! We have further specified all rates mentioned in revised MS (either in the paragraphs or tables). And we have calculated adjusted rates based on the information of Zhejiang general population, please see in line 190-196.

6. In the discussion section, the authors should recall that the prevalence of overall obesity observed in this survey is low compared to those observed in western countries.

Response: We have mentioned this discrepancy in revised MS, please see in line 240-244.

7. The regression models took into account different covariates including fruits and vegetable intake, but they did not take into account the total daily energy intake, which is commonly done when data on energy intake are available. In the context of a developing country, this can explain the positive association between obesity and fruits and vegetable intake. This point should be discussed by the authors in the discussion, particularly in the limitations section.

Response: Thank you! In revised MS, we have mentioned about the reason for choosing fruits and vegetables intake as the surrogate of daily energy intake, see in line 135-138. And we have discussed the potential bias to study results by doing so in limitations, see in line 324-326.

8. In developed countries, several surveys have highlighted the existence of an inverse association between SES and obesity, particularly among women. In developing
countries, the positive association seems to be more observed among men. This discrepancy might be discussed by the authors.

Response: Thank you! We have elaborately discussed this discrepancy in revised MS. Please see in line 271-288.

Minor Essential Revisions:

According to the reviewer, the authors should prefer the terms "men" and "women" instead of “males” and “females” in the manuscript.

Response: Thank you! We have replaced most “males” and “females” into “men” and “women” based on the reviewer’s suggestion. Just kept several of them that we think might be better.

1. Abstract – Results – 2nd sentence: the term “patrial” is unclear.

Response: Thank you! In revised MS, this word no longer exists.


Response: Thank you. In revised MS, the 3rd sentence doesn’t exist.

3. Variables and definitions section – 2nd sentence: replace “(... in this study , we (...)” by “(...) in this study, we (...)”.

Response: We have already changed the order of this sentence, please see in line 117-118.

4. Model fitting results: The second part of the first sentence or the next sentence is useless.

Response: Since we have re-wrote most parts of “model fitting results” section, this sentence no longer exists in revised MS.

5. Discussion section – 4th sentence: replace “Based on IDP cut-off (…)” by “Based on IDF cut-off (…)”.

Response: Thank you! Since we have changed WC cut-off criteria, this sentence no longer exists in revised MS.

6. Title of the tables : the authors should mention the year the survey was carried out.

Response: Thank you! We have added year information in the title of each table.