Author's response to reviews

Title: Risk of obesity in immigrants compared with Swedes in two deprived neighbourhoods

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Author's response to reviews: see over
Comments from the Editors:

1. **Informed consent**: Written informed consent was obtained from the participants. This information is now included in the Methods section.

2. **References and style**: The references and style of the article is now structured according to the BMC Public Health instructions to authors.

Comments from Reviewer 1:

Major Comments:
The result that immigrants have higher obesity prevalence rates and that having economic difficulties is associated with a higher likelihood of being obese does not seem to be particularly novel. In my opinion, the analysis of how three objective obesity measures: BMI, WC, and %BF perform relative to one another would be much more interesting and exciting. Most of the current research is based on BMI. Many argue that WC and %BF are better obesity measures and that BMI is imperfect and misclassifies individuals. The authors have data that can answer many fascinating questions. For instance, what percentage of individuals BMI is misclassified as being obese or non-obese? Is BMI more likely to misclassify Swedish individuals or individuals born in the Middle East or born in other European countries? Are men more likely to be misclassified than women? I wonder if the authors may want to consider changing the focus of their paper. Overall, my major point has to do with the flow and focus of the paper. There are several places where the flow of the paper seems to be interrupted. The analysis of possible differences between the three obesity measures is interesting. However, it is beyond the scope of the present study. In addition, such a study would most likely be underpowered. We believe that this study represents new knowledge because it includes three objective measures of obesity in a random sample of Swedish-born and foreign-born participants from two deprived neighbourhoods in an urban setting. We agree that the flow of the paper was a problem in the original version, particularly in the discussion section. We have now revised the manuscript and believe that the flow has been improved.

Pages 4-7; The background section starts with a very nice description of various ways in which obesity prevalence is measured in the literature. This introduction is set up in such a way that a reader expects that the paper would be devoted to the analysis of how three objective obesity measures: BMI, WC, and %BF perform relative to one another. Instead, the end of the background section tells a reader that the goal of the study is to examine how socio-demographic factors are associated with the three objective obesity measures. I think that either the goal of the paper should be changed to the analysis of relative performance of the three obesity measures or the background section should be augmented by the literature reviews that focus on how socio-demographic factors are associated with obesity. Otherwise, the goal of the paper comes as a complete surprise to a reader.

Page 15; The authors highlight that “in terms of objectively measured indices of obesity, few studies have examined specific communities or deprived neighborhoods.” It is unclear from the paper why it is important to concentrate on specific communities or deprived neighborhoods while comparing objective measures of obesity. Is there some evidence that measures of obesity tend to be
in larger disagreement, specifically for deprived neighborhoods? Is there some other reason? Again, the flow and the motivation should be improved.

We agree with the reviewer that the flow and focus of the study should be improved. Please see our answer above. We have revised all sections of the paper, especially the Background and the Discussion section. We have also moved some of the more detailed information regarding BMI, WC and BF% to the Methods section and included a review of the sociodemographic variables (including why we targeted deprived neighbourhoods) in the Background to better introduce the focus of and rationale for the study.

Page 17 contains the discussion of the effect of the neighborhood environment on obesity. This discussion seems to be relevant to the research that the authors plan to conduct in the future, but it is unclear how it is relevant to the goal of the present paper.
We apologize for being unclear. We have revised the discussion section substantially in order to improve the relevance and clarity.

Other Comments:
1. References for background section; There are several very interesting papers that the authors may want to add to their background section. First and foremost, it is the paper by Burkhauser and Cawley (J Health Econ, 2008). In this paper, Burkhauser and Cawley use NHANES III data to compare how BMI and %BF classify individuals as obese and non-obese. Also, there are several other papers not cited by the authors that compare self-reported and measured BMI. See e.g. Cawley 2000, Health Serv Res; Rowland, 1990 Am J Clin Nutr.
Thank you for this information. We have added two of the references suggested above.

2. It would be great if the authors would say a few words about the BIA method and how reliable it is in measuring %BF.
This information is incorporated in the methods section.

3. There are many different ethnic groups living in the Middle East and European countries other than Sweden. These ethnic groups may have different diets and different lifestyles. Can the authors further stratify the results by ethnicity?
We would like to stratify the results according to other ethnic groups but are unable to do so because of lack of statistical power.

4. On page 7 the authors say that the CNI for the neighborhood in Sodertalje was 50. In the next sentence they state that the CNI minimum for all neighborhoods in Sweden is 53.5. There seems to be a contradiction between these numbers.
We have now included a more detailed description of CNI in the methods section.

5. Page 8; Can the authors provide a breakdown of excluded subjects by exclusion criteria? What was the most common exclusion criteria? Many individuals nowadays do not have land line phones and rely on cell phones instead. Is this a problem for the study? Could it bias the results?
We have included detailed information about the exclusion criteria in the methods section and added to the discussion how this might bias the results.

6. Do the authors have information on the duration of residence in Sweden (not
just the neighborhood in question)? There is evidence that for immigrants, the length of residence in the country is associated with obesity (Kaplan and Huguet et al., Am. J Prev Med).

Duration of residence has been included in table 1.

7. Tables 1 and 2: It would be nice to see if the differences in descriptive statistics across different population groups are statistically significant. We have now included p-values in table 1. Possible statistical differences in table 2 are tested in the models shown in table 3.

8. Table 3; It concerns me that the standard errors are so large and that the odds ratios are so imprecisely estimated. It means that only effects of a very large magnitude are captured. We agree and have mentioned this as a limitation in the discussion section.

9. Table 4; Can the authors stratify the analysis by gender too? Then, a reader can compare how the odds ratios from Table 3 change once education, economic difficulties and residence duration are taken into account. The statistical analysis used in table 4 was tested for possible interactions by gender, and we have described this in the text. No such interaction was found and therefore it is not necessary to stratify this analysis by gender. To keep the analysis as it is also saves space in the manuscript.

Comments from Reviewer 2:

This is an interesting manuscript on a topic of considerably importance. Insufficient research has been done comparing three different measures of obesity in diverse populations. The paper, however, suffers from some serious flaws that need to be remedied before it is suitable for publication.

Major Compulsory Revisions
1. The major flaw with the manuscript is the inadequate description of the study as outlined in the Methods sections. The authors mention the care need index (CNI), but do not explain it or explain what the scores means. Furthermore, the study is evidently part of a larger study comparing two neighborhoods, one of which will be rebuilt compared with one that will not be rebuilt, but there is no information on study design or descriptions of the neighborhoods. Sample was described as “Random samples of 18-65 (n=1400) were obtained from the local government.” (p 8), but no details are provided as to how the sample selection was made. The authors list exclusion criteria but do not give a breakdown on what percentage of potential participants fall into each exclusion category. A total of 1400 residents were obtained for the sample, but the final study was limited to 306 individuals for a response rate of <22%. Such a poor response rate is a serious limitation. Some analysis was done to look at response bias, but only on the 678 subjects that meet inclusion criteria. Two exclusion criteria were not listed telephone number or an incorrect number, meaning that residents of lower socioeconomic status were likely excluded. Even so, some differences were found between participants and non-participants although the authors did not report statistical significance of the differences.
We have improved the Methods section and added more detailed information about the study design and the care need index. We have improved the description of how the sample was drawn. Exclusion criteria are now listed in detail and we give a breakdown on how many individuals that fell into each exclusion category.

We have taken a non-response bias into consideration by including a post stratified weight based on gender and age in the analysis. In addition, the ethnic profile (country of birth) of the recruited participants was similar to the ethnic profile of the actual population in the two neighbourhoods. This information has been added to the discussion section and we believe that we have recruited a reasonably representative sample of residents.

Finally, when looking at previous literature, we have found that similar studies from deprived neighbourhoods have often managed to recruit only about 10-30 percent of eligible participants.

2. There are other problems with the manuscript, particularly in the Discussion section. This entire discussion section needs to be reworked for clarity and to better address the hypotheses.
The discussion section has been substantially revised; please also see our answers to Reviewer 1.

Minor Essential Revisions
1. One problem with the manuscript is that it appears to have been written by a non-native speaker of English. One suggestion to improve the manuscript is that the authors work with an editor to address problems with punctuation, grammar and syntax problems.
Perhaps as a result of English language problems, there are problems with clarity in the writing throughout the paper.
As a result of the comments regarding lack of clarity and flow, we have thoroughly revised the manuscript. In addition, a professional editor has helped us to improve the language.

2. Furthermore, the authors often make statements of comparison without including a comparison group, for example (page 4) “Higher rates of BMI-obesity have been established in Swedish adolescents from low socioeconomic families.”
Higher compared to what families? In some cases statements of purported fact are made without supporting references being cited.
We agree and have revised the text in accordance with this comment.

3. Other less serious problems were found, most of which could be easily addressed, such as editing Table 1, which shows three educational categories while the manuscript (p 11) says education was classified as two categories, or correcting formatting problems in Table 4.
We apologize for being unclear. We have clarified the Methods section and it is now consistent with the information in the tables.