Reviewer's report

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

Version: 1 Date: 24 April 2009

Reviewer: Irina Grafova

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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.

• 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.

• 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.

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.

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.

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?

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.

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?

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).

7. Tables 1 and 2: It would be nice to see if the differences in descriptive statistics across different population groups are statistically significant.

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.

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.

Level of interest: An article of importance in its field
Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

'I declare that I have no competing interests