Reviewer's report

Title: Leveling off of prevalence of obesity in the adult population of Sweden between 2000/01 and 2004/05

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Reviewer: Pedro Marques-Vidal

Reviewer's report:

An interesting and important article in the field of obesity epidemiology, indicating that the prevalence of obesity (and also overweight?) might be leveling off in Sweden. Still, there are several methodological issues, namely regarding the statistical analysis and the public health implications that should be issued before the paper can be accepted for publication.

Major compulsory revisions

1. In the title and the text, the period of interest is 2000/1 to 2004/5, and the authors present data in the tables only for those periods. Still, figures 1 and 2 present data from previous studies (namely 1980/1 to 1996/7) and fail to present the data for 2000/1. The figures should either be deleted or at least corrected to correspond to the period of interest. The same applies for the last paragraph of page 12, where the authors indicate “our finding that … increased from 4.7% to 10.8% between 1980/1 and 2004/5”. The authors should present and discuss data only pertaining to the study period.

2. The authors have conducted a considerable number of tests: just taking into account the data presented in the tables, at least 23 (table 1a) + 23 (table 1b) + 22 (table 2a) + 22 (table 2b) + 3 × 23 × 2 (tables 3a and 3b) = 228 statistical tests were performed. Considering the usual 0.05 significance level, this mean that 5% of the tests (228 × 0.05 = 11) could be due to chance. Either the authors correct for the number of tests performed (for instance by Bonferroni, i.e. 0.05/228 = 0.0002 as the level for statistical significance), or they can perform fewer tests to check for a change in the prevalence of obesity, such for example conducting ANOVA tests (adjusting for age and the explanatory variable of interest) to check if BMI increased during the study period for each explanatory variable as a whole.

3. The rationale to adjust only on age should be discussed. No such adjustment is possible when data is stratified on age, such as tables 2a and 2b. In tables 2a and 2b it would be better to perform an overall adjustment on all explanatory variables, as some of them have also changed during the study period. For instance, there appears to be a change in the population distribution according to urbanization and also an improvement in educational level (tables 1a and 1b). As educational level is usually related to overweight and obesity, an adjustment on educational level should be performed. Please make a logistic regression on obesity stratifying on gender but adjusting on all explanatory variables and using
also the study (2000/1 as the reference) as a covariate. For tables 1a and 1b, please adjust on age for all strata except age (see above).

4. The rationale to split immigrants is weak: most comparisons are not significant, and the only ones (upon which the authors put a considerable emphasis in the discussion, abstract and conclusion) are for the immigrants from Southern European countries, which represent less than 1% of the overall sample, i.e. circa 53 women in 2000/1 and 43 women in 2004/5 (data from table 1b). Are those very small numbers really representative and do they carry such a heavy burden for Swedish Public Health, or do the authors focus on them because they are just significant? This might also explain why the prevalence of obesity among immigrant men from Southern European origin increased from nil to 23.6% during the study period (table 2a), a finding rather hard to explain. Overall, it might be better to group all immigrants in a single group or split immigrants into two groups at most, so to have an adequate sample size.

5. In the results, a paragraph indicating the changes which occurred in the Swedish population between the two studies should be presented. As indicated previously, significant changes have occurred (better education, higher urbanization) and those changes might impact the prevalence of overweight and obesity.

6. The conclusions arising from data presented in the different tables are not consistent: in table 1a, it can be inferred that BMI increased among subjects aged 45-54, but this had no effect on obesity levels (table 2a), while the results from table 3a are similar to table 1a, which is somewhat expected, as linear regression can also be used to compare between groups. Similar comments for other strata (educational level, urbanization, etc.). Overall, the interest of tables 3a and 3b is rather reduced, and the manuscript could be considerably shortened and improved if those tables were deleted.

7. The authors indicate in the statistical analysis (page 7, line 7) that tables 2a and 2b were analyzed using individual weights, but it seems that no such weighting was performed for the other tables (1 and 3). The authors should either weight all the statistical analyses, or present the results from unweighted data only.

8. The discussion could be shortened. In the second paragraph of page 12, it is not necessary to show individual results for each country. If the authors really want to, they can summarize the results in a graph. Also, the reasons for the leveling off of obesity levels in Sweden (discussed in the third paragraph of page 13 and again in the third paragraph of page 14) could be grouped.

9. Also in the discussion (page 15, third paragraph) the authors suggest that physical activity levels might partly explain the leveling off of obesity levels. As they have the data, they could present it in the tables (1 and 2) and adjust for it.

Minor essential revisions
1. Page 6, explanatory variables: please indicate how former smokers were
classified, i.e. if only subjects who had stopped smoking for at least 6 months were considered as former smokers. Same for “medium-sized towns”, i.e. provide the town size.

2. Page 7, statistical analysis: please do not state the tables in this part. Tables should be stated in the results.

3. Page 13, line 6: the authors indicate that BMI increased by 2.2 units during the study period among immigrants from Southern Europe. Still, data from table 1a suggest that this value is only 1.8.

4. Table 2a, line 45-54 years, last column: “- 1.61”.

**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