Author's response to reviews

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

**Authors:**

Jan Sundquist ([jan.sundquist@med.lu.se](mailto:jan.sundquist@med.lu.se))
Sven-Erik Johansson ([sven-erik.johansson@med.lu.se](mailto:sven-erik.johansson@med.lu.se))
Kristina Sundquist ([kristina.sundquist@ki.se](mailto:kristina.sundquist@ki.se))

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**Author's response to reviews:** see over
Title: Levelling off of prevalence of obesity in the adult population of Sweden between 2000/01 and 2004/05

Editor’s comments:

We would suggest slightly revising the background section of the abstract to give some wider context to the study (i.e. what is the importance of studies like this?) to introduce the reader to the study.

We agree and have added the requested revision including a revised aim. ‘The escalating global epidemic of obesity is of worldwide concern because of its association with several chronic diseases and premature mortality. Some subgroups seem to be more affected than others. The aim of this study was to examine whether the mean BMI (adjusted for age) and the prevalence of obesity (adjusted for all the explanatory variables) changed between 2000/01 and 2004/05 in different subgroups of the Swedish population.’

Response:
Version: 1 Date: 12 January 2010
Reviewer: Pedro Marques-Vidal

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.

Thank you for your positive response. We have revised the paper in accordance with the suggestions.

Major compulsory revisions
#1a. 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.

We agree and have deleted the figures.

#1b. 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.

We have deleted the figures (see #1a) and changed that paragraph to the following new text: ‘Our finding of a 14.3% prevalence of obesity in men aged 44-54 in 2004/05 agreed with the findings of another recently published Swedish study, i.e. the Gothenburg study of men aged 50. The Gothenburg study population had a prevalence of obesity of 13.8% in 2003.’

(Rosengren A et al., Obesity and trends in cardiovascular risk factors over 40 years in Swedish men aged 50. J Internal Medicine 2009 Sep;266(3):268-76. Epub 2009 Apr 7.)
#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 x 23 x 2 (Tables 3a and 3b) = 228 statistical tests were performed. Considering the usual 0.05 significance level, this means that 5% of the tests (228 x 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.

Thank you for this comment. We have rerun all analyses. We have now performed fewer tests according to the reviewer’s suggestion (some tables were deleted). We have also adjusted mean BMI for age and the prevalence of obesity for all the explanatory variables. Please see also #3 below.

#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).

We agree and have revised the analyses and tables, stratifying on gender. Tables 1a and 1b include new results that are adjusted for age (not the age strata). The new results in Tables 2a and 2b are based on logistic regression models that are adjusted for all the explanatory variables according to the reviewer’s suggestion.

#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.

We agree and have now grouped the first generation immigrants into two groups and the second generation immigrants into one group.

#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.

Thank you for this comment. In the revised analyses, we have adjusted the results for all the explanatory variables (please see #3 above).

#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.

We have excluded Tables 3a and 3b.

#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.

We agree. Table 1a and Table 1b now include weighted data.

#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.

We agree and have shortened and revised the Discussion in accordance with the suggestion. We have also used fewer immigrant groups in the revised analyses (see #4 above).

#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. Unfortunately, it is not possible to include physical activity in the analysis because this variable it is not available in the survey from 2000/01. We did, however, check for changes in levels of leisure-time physical activity in persons in the sample. These data are available from the SALLS 1996/97 and 2004/05, as both surveys include a question about physical activity. We found that the percentage of subjects in our sample who took part in leisure-time physical activities at least once a week remained high during 2004/05 (60%) compared with 1996/97 (55%) (data not shown).

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.
We have added the following new sentence:
Smoking habits: ‘Respondents were divided into three groups based on tobacco consumption: (1) those who had never smoked, (2) former smokers (regardless of when they stopped smoking), and (3) daily smokers.”

We have also provided more information about town size in the Method section.

2. Page 7, statistical analysis: please do not state the tables in this part. Tables should be stated in the results.
   We have deleted these statements in the statistical section.

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.
   We have revised the categorization of immigrants (please see #4 above).

4. Table 2a, line 45-54 years, last column: “-1.61”.
   Thank you for this information. We have corrected the typo.
The objective stated in the abstract is vague; it shall be revised according to the objectives provided in page 4.
We have revised the aim in the first paragraph of the Abstract:
‘The aim of this study was to examine whether the mean BMI (adjusted for age) and the prevalence of obesity (adjusted for all the explanatory variables) changed between 2000/01 and 2004/05 in different subgroups of the Swedish population.’

We have also given the aim some wider context for the study, which is in accord with the comment by the editor.

-By considering the rapid rise in the prevalence of obesity worldwide, the rationale of selecting these two relatively old studies shall be justified. Was that only because of availability of these data?
Yes, the most recent Swedish nationwide data are from 2004-05.

-The methods, especially the sampling framework needs to be described in more details.
We apologize for being unclear. We have now mentioned the sampling procedure up front in the Methods section.

-The interpretation of the current findings with existing literature shall be more strong.
We have revised the Discussion and added new references in accordance with this suggestion.

-The study limitations are not well described.
Thank you for this comment: We have expanded the discussion of the study limitations.

-The conclusion does not directly refer to the study objectives and findings.
We have revised the Conclusion in accordance with this suggestion. ‘The 2000/01 and 2004/05 prevalence estimates presented in this study indicate that the previously observed trend towards increasing obesity and BMI is levelling off in Sweden. However, the prevalence rates of obesity among men and women are still higher than in previous generations, which is of concern because the national burden of chronic diseases in the population is highly associated with obesity in both men and women.’

-The figures are not necessary.
We have deleted the figures.

-Some abbreviations are not presented in full on the first time of use.
Thank you for this comment. We now present the abbreviations in full at the first time of use.
The English writing needs to be improved.
The English has been revised by a professional language editor.