Reviewer's report

Title: Body Mass Index increase in middle life is not linked to deprivation status: a cohort study

Version: 1 Date: 1 February 2005

Reviewer: Karri Silventoinen

Reviewer's report:

General

This paper studies whether short-term weight change (the follow-up time about 5 years) is associated with baseline deprivation. The authors found that weight change was not associated with deprivation, which suggests that socio-economic differences in BMI are formed already before age 35. As the authors emphasize, this finding has important public health implications since it suggests that decreasing of socio-economic differences in BMI needs early interventions. Especially because the results are based on large data and because a publication bias favoring to publish positive findings probably exists, these negative results are interesting. However, I have a few comments the authors may want to consider.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

1) In Background (both in Abstract and the main text) the authors seems to make a distinction between previous studies on socio-economic differences in weight change and their current study. However, I am not sure what this difference actually is since in this paper the analyses are based on two measurements. This study design can give information on socio-economic differences in weight change but surely not, for example, on short-term individual weight fluctuation. The authors should make the difference to the previous studies more clear or, if there is not this kind of fundamental difference in the study designs as I expect, better integrate their results to the previous discussion on this topic.

2) The reference list is not very comprehensive. In addition to the study referred by the authors, I am aware at least three recent publications which have studied socio-economic differences in weight change (Lahmann PH et al. IJO 2000, 24, 685-94, Hardy R et al. IJO 2000, 24, 725-34, Silventoinen K et al. AJCN 2004, 80, 815-822). The authors may want to consider whether these articles would be relevant to this paper as well.

3) The data description is not very detailed. It would be useful if the authors would give the number of people in the target population (Stockport residents), how many were excluded because of a baseline disease, how many of the invited actually took part in the baseline screening, and how many of them took part in the follow-up examination. It should also be explained how the baseline diseases, used as an exclusion criterion, were diagnosed. It should be said what is the definition of hypertension used as one exclusion criterion: is it based on the measurement of blood pressure, hypertension medication or, as in the optimal case, both.

4) A short description on the quality of the data would be helpful. Now the authors just say that the quality is excellent and refer to an unpublished document, which is probably unreachable by the reader.
5) A description of the Townsend Deprivation Score would be very helpful, especially since the source the authors refer to is probably not readily available for most of the readers. To my mind the fact that the deprivation scale is based on aggregate level data is so important that it should be mentioned already in the abstract. This may also explain why the results differ from the previous studies on this topic, which have used individual level indicators of socio-economic status. The authors should discuss this possibility, i.e. how definition of deprivation / socio-economic position may modify the results.

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

6) In the tables p-values should never be 0.000 but p<0.0001. Additionally, I am not sure whether p-values should be presented at all if confidence intervals are given.

7) The author name in the reference 11 is misspelled (it should be Martikainen).

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Discretionary Revisions (which the author can choose to ignore)

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: No

Declaration of competing interests:

I declare that I have no competing interests