Author's response to reviews

Title: Socio-demographic and behavioural determinants of weight gain in the Swiss population

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Author's response to reviews:

Reviewer: Barry Prof. Bogin
Reviewer's report:

This article appears to be in very good shape and is, in my opinion, acceptable. I do suggest a few Minor Essential Revisions that the authors can be trusted to make. I attach a copy of the PDF of the manuscript which I reviewed and on which I added comments for these Minor Essential Revisions. Some of my suggestions are to improve the English and are marked only on the PDF.

The changes indicated in the pdf file have been performed in the revised version of the manuscript. As indicated by reviewer Ilse Swinkels (item 4), one sentence in the statistical methods was out of topic and was subsequently deleted.

I list the other types of comments here with addition details:

1. page 4-5 - the authors write that, "...the 5 Kg cut-off was chosen according to The recommendations of the World Health Organization that weight gain in adulthood should not exceed 5 Kg [7]." Does this mean that adults should not gain more than 5 Kg during their entire adult life or no more than 1 Kg a year, or something else.

According to WHO, adults (#18 years) should maintain their weight and do not gain more than 5 kg during their entire adult life. The text has been modified to “...of the World Health Organization that weight gain in adulthood should not exceed 5 Kg over the entire adult life [7]"

2. page 5 - the authors write that they excluded participants, "...who reported involuntary weight loss during the last 12 months at follow-up." Please explain why this was decided in more detail. Involuntary weight loss might be from illness, but could be due to many other factors. Excluding anyone with such weight loss may bias the results.

We agree with the reviewer that involuntary weight loss may also be due to factors other than illness. However, illness is likely to be the major driver of
involuntary weight loss and we feel that analyses are more biased with the inclusion of these individuals than with their exclusion. To explain the rationale behind this exclusion, we modified the text as follows: “As illness could lead to involuntary weight loss, participants who reported involuntary weight loss during the last 12 months at follow-up were also excluded.” Moreover, analysis including all participants yielded similar results (see our response to item 3).

3. page 7 - I ask the authors to explain what they mean by, "Repeating the same analyses on the whole sample." What constitutes the 'whole sample’?

By “whole sample” we meant including the participants who were initially excluded from the analysis, for example those who reported involuntary weight loss during the last 12 months. It is now indicated in the text: Repeating the same analyses on the whole sample (i.e. including participants who were initially excluded from the analysis) led to similar findings, except that the difference in rate of weight gain between physically active and non-active participants was no longer significant, and that the increased likelihood of gaining #5 kg was borderline significant among participants receiving social help (Supplementary table 1).

I thank the authors for their care in carrying out and reporting this valuable study. Thank you very much.

Reviewer: Ilse Swinkels

Major Compulsory Revisions

1. The authors were able to perform longitudinal analyses on the relation between weight gain and several socio-demographic factors. With the high prevalence of obesity worldwide, this might definitely be a relevant study. Furthermore, they argue that this is the first study in the Swiss population in which socio-demographic factors have been studied. However, for me it is not fully clear what the relevance is of this research. A lot of work has already been done on the relation between socio-demographic factors and obesity. This might not have been done in Switzerland, but then I would like to invite the authors to add information in the Introduction why it is important to add knowledge on the Swiss’ situation. Furthermore, more recent work on this topic can be used for the references.

We agree with the reviewer that social differences in weight gain have already been reported in other studies. However, this is the first study to evaluate social differences in weight gain in a Swiss population. As social differences in health generally vary between countries and even change over time because of cultural and epidemiological factors, these results are thus important for public health professional and policy makers (see also our answer to item 2 below). We added new references and changed the introduction as follows: “Worldwide prevalence of obesity almost doubled between 1980 and 2008 [1] and a similar trend has been observed in Switzerland [2]. Several socio-demographic and behavioural factors have been shown to influence weight gain. A consistent positive association between marital status [3], occupational position [4], low educational level, economic difficulties [5] and weight gain has been reported. Still, the impact
of SES on weight gain might differ according to gender [6] or to the country's level of socioeconomic development – while in high income countries a high socioeconomic status (SES) is generally related to a lower prevalence of obesity, the opposite association is found in low income countries [7].

In Switzerland, several cross-sectional studies have shown an inverse association between obesity and socio-demographic and behavioural factors [8, 9], but whether socio-demographic and behavioural factors have an impact on weight gain has never been investigated prospectively. Indeed, one of the main objectives of the Swiss national programme on healthy eating and physical activity (PNAAP) is achieving a health weight [10] and such data are important for adequately designing health promotion policies and to evaluate their impact in the target population.”

2. Elaborating on the first remark, I would be interested in the consequences of the findings for policy makers. One of the findings is that financial difficulties are positively associated with weight gain. What are the implications of this finding for prevention or treatment strategies?

We now provide a new paragraph untitled “Policy implications”. The following text has been added: “Our results are important for public health professionals and policy makers for several reasons. First, being physically active was negatively associated with weight gain, supporting the importance of promoting physical activity, also through environments favouring the practice of physical activity [24, 25] Second, participants with financial difficulties had a higher tendency to gain weight over the follow-up, probably due to their intake of caloric-dense, less expensive foods [27, 28]. Importantly, several randomized controlled trials have shown that education alone does not impact the purchase of healthy foods, and that cost reduction and/or promotions are needed to increase fruit and vegetable intake [29-31]. Thus, efforts should be made in the promotion of healthy eating, namely by decreasing the costs of healthy foods rather than just implementing food education campaigns. Finally, the fact that current smokers have an increased risk of gaining over 5 kg could be used as an additional argument for promoting smoking cessation.”

3. In the analyses the relation between obesity and a range of socio-demographic and behavioral factors was studied by multivariable models. The baseline data of the range of factors were used in these models. As the persons were followed for five years changes in these factors might have occurred. Did the authors consider to add these changes to the model?

We agree with the reviewer that changes in some sociodemographic or behavioural factors during follow-up might have influenced weight gain. Still, the analysis would be considerably complicated as only two observations per participant are available and whether other changes occurred during the follow-up are unknown. For instance, a participant might have divorced and married again, which would leave his/her marital status unchanged; the same applies to smoking or physical activity. Further, even if only the two observations are considered, this would lead to a large number of possibilities. For example for physical activity, four categories could be created
No # No; No # Yes ; Yes # No and Yes # Yes

Some of which would have a small number of participants, leading to wide confidence intervals of the estimators. The same applies to marital status, receiving social help and smoking status: their cross products would lead to a large number of groups, some of which with a very small number of participants, Thus, we chose to use a “classic” analytical method, i.e. using only the baseline information. The following text has been added in the discussion, in the Strengths and limitations paragraph: “Finally, changes in some sociodemographic or behavioural factors during follow-up might have influenced weight gain. Still, taking into account the multiple possibilities (for instance, physical activity would be split in four groups depending on baseline and follow-up status) would considerably complicate the model and increase the risk of small sized groups, leading to nonsignificant associations due to large confidence intervals of the estimators. Thus, and also considering the relative short follow-up period, we chose to apply a classic analytical method, taking into account only the baseline data.”

4. In the paragraph on ‘statistical analyses’ is stated that stratified analyses were conducted for age groups 35-54 and 55-74 years. However, in the methods and in the tables this is not shown as such.

We thank the reviewer for noting this error. Indeed no such analysis was performed. The sentence has been deleted.

5. The authors thoroughly analyzed their data. Which I appreciate on the one hand. On the other hand, some outcomes differ by performing different analyses and this raised questions. Especially the presentation of results with mean weight gain and with weight gain > 5 kg raised questions as different factor appeared to be related to one or the other. I think it would be better to choose one of both outcome measures as, at least for me, it is hard to understand what the differences in results mean for the interpretation. Moreover, in the discussion and conclusion factors are mentioned that have been found to be related to only one of both outcome measures. I would choose for not presenting that results as main conclusions.

Based on the findings for BMI categories and on the comment expressed in item 9, we believe that presenting the data either as a quantitative measure (rate of weight gain or weight gain #5 kg) provides important information and adds further interest to our manuscript. Further, with the notable exception of BMI categories, most associations between socioeconomic or behavioural factors and markers of weight gain were similar irrespective of the marker used: see for instance the results of table 2. Thus, we propose to maintain both the quantitative and the qualitative analysis and to add a paragraph on the apparent discrepancy regarding BMI categories (see response to item 9 below).

Minor Essential Revisions

6. The background in the abstract confused me as it states that the focus of the study is the impact of SES on weight gain. But reading the manuscript it appeared that the focus is a broader range of socio-demographic and behavioural factors. This should be used consequently in the manuscript.
We agree with the reviewer that our study was not restricted to socioeconomic factors but also included behavioural factors. Accordingly, we changed the background of the abstract to “In Switzerland, socio-demographic and behavioural factors are associated with obesity, but no study ever assessed their impact of SES on weight gain using prospective data.” We also changed the last sentence of the first paragraph of the introduction to “…but whether socio-demographic and behavioural factors have an impact…”

7. Persons aged 35 to 75 years were included in the cohort. Please change the highest age-category in 65-75 instead of 65 or older.
The change has been made in the tables and online material.

8. I assume ANOVA for repeated measures were used?
We compared the rate of weight gain, computed as the difference between the follow-up and baseline weight divided by the number of years of the follow-up, so multivariable ANOVA was used to compare groups.

9. The authors conclude from the results in Table 1 that obese people had a lower rate of weight gain (page 6 lines 130-131). When looking at the mean scores this is true, however, the percentage of persons with weight gain > 5 kg is higher in this group than in the others. I think this means that besides the substantial group of obese patients with weight gain, there was also a large group of obese patients with weight loss. Probably more weight loss occurs in the obese patients than in the group of overweight and normal weight as the medical need is higher. This results in a lower mean. However, I believe this should be distinguished in the results.

We thank the reviewer for providing this important information which improved the value of our manuscript. We added the following statement in the discussion. “No difference in weight gain was found between normal weight, overweight and obese participants; conversely, obese participants had an increased likelihood of gaining #5 kg. A possible explanation is that besides the substantial group of obese patients with weight gain #5 kg, there was also a large group of obese patients with weight loss, most likely due to medical reasons. Indeed, the prevalence of obese subjects with weight loss (40%) was higher than in overweight (36%) or normal weight (29%) participants (<0.001). Our results thus suggest that many obese participants tend to lose weight, but that this trend is overcompensated by a significant fraction of obese participants who gained more than 5 kg during the study period. Also, many normal weight or overweight participants tend to gain weight, but less than 5 kg (Figure 1).”

10. How was physical activity measured?
Physical activity was self-reported and participants were considered as physically active if they reported practicing leisure time physical activity at least twice per week. This sentence has been added in the “clinical and anthropometric data” sub-chapter.

11. The number of patients can be added to the titles of the tables
The total number of participants is now provided in the titles. We chose not to put it at the end as it might confound readers, making them falsely believe it
corresponds to the number of participants reporting involuntary weight loss at follow-up

Discretionary Revisions

12. In lines 165-168 is argued why smoking at baseline was positively associated with weight gain. An explanation might also be that these people in general live less healthy than non-smokers resulting in less healthy eating habits and less physical activity.

We changed the text to “Other explanations include a less healthy lifestyle of smokers (i.e. less healthy eating and less physical activity) and also the positive association between...”