Author’s response to reviews

Title: Associations between family factors and body weight gain from 20 years old

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Author’s response to reviews:

Reviewer 1:

I do have the following comments/concerns:

Q1- Title: I would suggest removing the name of the study from the title "the Shizuoka-Sakuragaoka J-MICC Study" since this study was originally designed to study a different topic (the relationships between genetic and lifestyle factors for risks of lifestyle-related diseases, such as cancer, heart and cerebrovascular diseases, and diabetes) and to make the title more simple.

A1. As per the comment, the title has been changed to make it more simple. Please see if the revised title is suitable.

Q2- Major concern: BMI at 20 years of age was based on a questionnaire. The population being questioned was > 35 years old. This means that at least over 15 years have passed which makes it very difficult for most people to recall. Although the authors addressed this issue in the limitation section, I find it a major limitation that affects the validity of the findings

A2. We are immensely thankful to you for this comment. To facilitate understanding for readers, the recall bias for BMI at 20 years of age was consistently mentioned in the text (lines 26-16, pages 4-5; and lines 25-27, page 7).
Reviewer 2:

Major

Q1. I do understand that the authors have manifested the concern due to recall bias about the information of weight from 20 years old and have highlighted this aspect in the limitation section. However, it is difficult to assume that only because there were very good correlations (r were 0.98 and 0.99, p <0.001 for both in men and women, respectively) between the current BMI according to actual measurement and a self-administrated questionnaire, this would infer that the same correlation on the BMI from 20 years old is true. Moreover, because this information is the crucial data of the study, and consequently of its results, I think that the accuracy of the weight information from 20 years old remains questionable and the results may have been affected.

A1. Taking this comment into consideration, the recall bias and limitation were appropriately mentioned. Moreover, we mentioned that the reasons why the two models (i.e., the two cut-off values) were set to help readers understand better, and the outcomes were properly stated in the Discussion section. Please see the corresponding sentences in the manuscript (lines 26-16, pages 4-5; and lines 25-27, page 7).

Minor

Q2. Page 6, Lines 39-44, Definition and cases of BWG,

It is not clear why there were stablished two cut-off (≥ 2.5 kg/m² and ≥3.5 kg/m²) as case definition for BWG and so two different models used on the logistic regression. Is there any difference between the BWG ≥2.5 to <3.5 and ≥3.5? If not, why then the two models instead one based only on BWG ≥2.5?

A2. The two cut-off values (i.e., the two models) were better explained to help readers understand, appropriately. Please see the corresponding sentences in the manuscript (lines 26-16, pages 4-5; and lines 25-27, page 7).

Q3. Tables 1 and 2, correct контorls to controls; Footnote: wight to weight

A3. Thank you for pointing this out. The misspelled words have been corrected. Please see the Tables and Footnote.

Q4. Page 9, Lines 1-6, Discussion
As we can see in the table 4, on the multivariate logistic regression analyses of family factors for body weight gain in women other variables such as Pregnancy and Childbirth were also significant. Why only the marital status and family structure were referred as being associated with BWG?

A4. Regarding the two variables, the response “nothing” in both pregnancy and childbirth were strongly related to the “unmarried” response for marital status, and the proportion of divorce in women was small. Therefore, marital status and family relationships were summarized early in the first paragraph in the Discussion section, and the two variables were mentioned in the later part of the same paragraph. Please see the corresponding text in the manuscript (lines 22-5, pages 7-8). ‘Offspring’ in line 5 page 8 was corrected to ‘childbirth’.

Reviewer 3:

Comments

Q1. My primary concern with this study is recall bias. I know the authors discuss a bit of this in the discussion. So, for instance, a participant who was aged 56 years at survey date would have been asked to recall her body weight when she was age 20 years (36 years ago)? This is somewhat problematic. Did the authors investigate any potential problems with this? Otherwise, the reported estimates could be very misleading.

A1. Taking the comment into consideration, the limitation of recall bias was appropriately mentioned. Please see the corresponding text (lines 26-16, pages 4-5; and lines 25-27, page 7).

Q2. On page 6, lines 19-28, I do not see any action taken by the authors to limit recall bias. They simply state that the Pearson's correlation was calculated and then what? Otherwise, what the authors are doing is an assessment, not "to limit". Limiting entails decisive action to address it, which I did not get from the authors' description. This issue needs to be carefully addressed or at least described appropriately.

A2. Thank you for the advice. We wanted to show how degree of health conscious with respect to body weight even on the current body weight. The words 'to limit' were changed to help readers understand better, and the limitation of recall bias was appropriately mentioned. Please see the corresponding text (lines 23-24, page 5; and lines 16-18, page 10).
Q3. How representative is the sample used in this study? Did any sampling weights applied to ensure representativeness?

A3. To facilitate understanding for readers, the text in the ‘study participants’ section was more clearly mentioned. Please see the corresponding text (lines 18-1 pages 3-4).

Q4. On page 3, the description of the methods need to tell us more concerning the target area. Was the analysis based on data from a particular (single) region or multiple regions? I might have missed this, but I think its not clear from the description.

A4. To facilitate understanding for readers, the methods (especially for the study participants) were more clearly mentioned. Please see the corresponding text (lines 10-16, page 3; lines 18-1 pages 3-4; and lines 3-4, page 4).

Q5. Did the authors control for other variables such as household or individual income that might influence weight gain or it was not collected?

A5. Thank you for the advice. Although we also thought it is would be better to adjust the risks with such variables, we could not collect data for them because of the nervousness Japanese individuals with respect to participating in epidemiological studies. If we would have asked the study participants about such variables, the participation rate would have been much lower. Further, such variables are factors that change throughout one’s life/lifetime, and it is thought that many factors are complicatedly related to family factors (i.e., marriage and childbirth) after the age of 20 years. This was briefly mentioned as a limitation, please see the text (lines 3-5, page 11).

Q6. The results reported in Tables 3 and 4, there is no mention of inclusion of controls for the survey year, region/city/place of residence. Are these controlled for in the analysis?

A6. Such variables were not used for controlling the risks because the study participants were recruited for a short period and in a limited area. As per this comment, this was mentioned in the text (lines 18-1 pages 3-4; and lines 3-5, page 11).

Q7. I think its still important to clearly define the outcome variable and mention unambiguously that it's binary.
A7. We mentioned that the reasons why the two cut-off values (i.e., the two models) were set to help readers understand better and the outcomes were thought to be cleared. Please see the sentences in the text (lines 26-16, pages 4-5; and lines 25-27, page 7).

Q8. The authors should also show the results for the overall model that combines both males and females and includes a dummy indicator for gender (i.e. overall sample).

A8. As described in the text (2nd paragraph, page 2), many previous studies have pointed out gender differences in the risks for BWG. Considering the number of study participants and gender differences (i.e., pregnancies, childbirth and miscarriages only in women), our results were stratified by gender. To facilitate understanding for readers, the sentence explain our study aim was clarified as follows; the words ‘both genders’ was changed to ‘each gender’ in the text (lines 27-5, pages 2-3).

Q9. Given the possibility of recall bias as mentioned earlier, its important that the authors conduct a series of sensitivity checks. For example, do these results hold for specific regions in Japan; by educational level; or by city, rural/urban locality (if applicable)? The authors need to stratify the results by age groups as well.

A9. Thank you for the comment. The possibility of recall bias was mentioned earlier, and was appropriately revised (lines 26-16, pages 4-5; and lines 25-27, page 7). Regarding stratified analyses of age groups, the distributions of male and female cases were not adequately large for ‘unmarried’ and ‘bereavement/divorce’ in marital status, ‘single’ and ‘couple’ in family structure, and ‘not so good/not good’ in family relationships. Therefore, we stratified our analysis only by gender, but not other variables such as education level, cities and living location (rural/urban). Please see lines 25-26, page 10, and lines 3-7, page 11, with respect to the candidate variables for stratified analyses.

Q10. On page 8, lines 28-34, the interpretation of the results here is not clear. Certainly, an OR=1.00 neither implies an increase nor a decrease, and yet authors suggest "decrease and increase...". The authors must carefully check their interpretation of the results especially where they appear to suggest that an OR of 1.00 implies decrease...

A10. The two values for ‘p for trend’ were <0.05 for both in Model 1; however, those were only for one, but not for both, in Model 2. The sentence was appropriately modified to be understood by readers (line 12, page 7).
Q11. Overall, the results interpretation needs a bit of improvement.

A11. Taking this comment into consideration, the limitation of recall bias was appropriately mentioned. For instance, the two cut-off values (i.e., the two models) were better explained to readers understand appropriately. Please see the corresponding text (lines 26-16, pages 4-5; and lines 25-27, page 7).

Q12. On page 13, lines 41-47, this sentence needs rephrasing to clarify, it's not very clear.

A12. As per this comment, the sentence was rephrased appropriately (lines 11-12, page 11).

Q13. General, the paper needs English language editing throughout to clarify points.

A13. Thank you for this comment. After proofread in English by a native speaker, we did not appropriately correct the text in English. In the revised version, we carefully checked.